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Digital Governance and SME Growth: Quasi-Experimental Evidence from the "Maximum One-Time Visit" Reform in China

Tian Qiong[1], Rev. W. Wimalaratana[2,*]

Abstract

Does digital administrative reform boost the growth of small and medium-sized enterprises (SMEs)? This paper exploits the staggered rollout of the "Maximum One-Time Visit" (MTV) reform across counties in Zhejiang Province, China, to provide causal evidence. Using a county-by-year panel dataset constructed entirely from publicly available statistical yearbooks for the period 2016–2018, we find that the reform significantly increased private-sector employment—a key proxy for SME expansion—by approximately 4.8% within the first year of implementation. To ensure methodological rigor, we complement the baseline two-way fixed effects estimator with the doubly robust estimator, which accounts for heterogeneous treatment effects in staggered adoption settings; the aggregate ATT from this approach is nearly identical (5.1%, $p<0.05$), reinforcing the validity of our findings. Event-study estimates show no pre-trends, supporting a causal interpretation. The effects are stronger in counties with initially weaker market institutions, suggesting the reform substitutes for deficient formal governance. Furthermore, we explore the mechanism of institutional transaction cost reduction, arguing that digital governance alleviates information asymmetry and bureaucratic frictions. Due to data availability constraints, this study focuses on short-term impacts; longer-term dynamics remain a promising avenue for future research. The findings highlight how low-cost digital public administration can foster local economic development by reducing transaction costs for small businesses.

Keywords: Digital governance, small and medium enterprises, "Maximum One-Time Visit" reform, difference-in-differences, quasi-experiment, institutional transaction costs

INTRODUCTION

Small and medium-sized enterprises (SMEs) are widely recognized as engines of employment and innovation, yet they disproportionately bear the burden of regulatory compliance costs [1]. In developing and emerging economies, cumbersome administrative procedures—licenses, permits, and inspections—act as a regressive tax that stifles entry and expansion [2]. Can digital governance reforms, which streamline bureaucratic processes through online one-stop platforms, alleviate these frictions and promote SME growth?

This paper addresses this question by studying a landmark reform in China: the "Maximum One-Time Visit" (MTV) reform. Initiated in Zhejiang Province in late 2016 and rolled out to all counties by 2018, the reform required that citizens and businesses be able to complete any administrative procedure through a single visit (or even fully online), integrating previously fragmented

***Author for Correspondence**
Tian Qiong
E-mail: withdo@126.com

[1]Research Scholar, Department of Economics, University of Colombo, Colombo, Sri Lanka
[2]Professor, Department of Economics, University of Colombo, Colombo, Sri Lanka

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departmental systems [3].

To position our study within the broader global context, we extend the literature review to include recent comparative evidence from other emerging economies. For instance, India's "Digital India" initiative has been shown to reduce corruption and improve service delivery while Brazil's "Poupatempo" program reduced business licensing time by nearly 40% . In Southeast Asia, Thailand's "One Stop Service" for investment permits significantly shortened approval cycles [3]. These studies, however, typically rely on aggregate national-level data or cross-sectional comparisons, limiting causal identification. Our paper contributes by providing a quasi-experimental evaluation at the subnational level, framing the Zhejiang reform within a global shift toward "friction-free governance" [4].

Identifying the causal effect of such reforms is challenging due to endogenous timing and confounding economic trends [5]. We overcome these challenges by exploiting the staggered county-level implementation of the MTV reform in Zhejiang. Because the rollout sequence was determined largely by administrative readiness and political leadership rather than local economic conditions, the variation provides plausibly exogenous variation in the intensity of digital governance. We construct a county-level panel dataset for 60 counties over the period 2016–2018, drawing exclusively on publicly available data from the Zhejiang Statistical Yearbook [6-11].

This paper makes three contributions. First, it extends the literature on the economic impacts of e-government by providing rigorous micro-level causal evidence on SMEs, the sector arguably most sensitive to administrative burdens. Second, it adds to the growing body of research on the "Maximum One-Time Visit" reform by focusing on private-sector dynamism rather than firm productivity or aggregate investment [12]. Third, by employing an identification strategy that requires only publicly available yearbook data, it demonstrates how robust causal inference can be conducted in data-sparse environments, making the approach replicable for researchers in other developing countries.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and develops a simple theoretical framework. Section 3 introduces the data and variables. Section 4 outlines the empirical strategy. Section 5 presents the main results, while Section 6 reports robustness checks and heterogeneity analysis. Section 7 discusses the mechanisms, and Section 8 concludes [7].

INSTITUTIONAL BACKGROUND AND THEORETICAL FRAMEWORK

The "Maximum One-Time Visit" Reform in Zhejiang

In December 2016, Zhejiang's provincial government announced the "Maximum One-Time Visit" reform as a core strategy to improve the business environment. The reform's central mandate was that enterprises and citizens should complete any government administrative procedure after at most one physical visit [3]. The initiative consolidated scattered departmental service windows into unified "one-stop" service centers. The rollout was explicitly designed as a county-by-county experiment, starting in early 2017 and covering all counties by late 2018 [8].

The rollout was explicitly designed as a county-by-county experiment. A first batch of pilot counties launched the reform in early 2017; the remaining counties followed in waves through 2018. By the end of 2018, the reform covered all counties and districts. The staggered timing, combined with the fact that the pilot selection was based primarily on administrative convenience and leadership enthusiasm rather than predetermined economic criteria, provides the foundation for our quasi-experimental design [9].

Theoretical Framework

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To fix ideas, consider a simple model of firm entry and expansion based on institutional transaction cost theory [13]. A representative entrepreneur maximizes

profit：

$$\pi=R(L)-wL-\tau(L,B),$$

where R(L) is revenue generated by hiring L workers(with $R'(L)>0, R''(L)<0 R'(L)>0, R''(L)<0$), w is the market wage, and τ(L,B) denotes the institutional transaction cost of dealing with bureaucracy. which is increasing in firm scale L (i.e., $\partial\tau/\partial L>0$) and decreasing in the quality of digital governance B (i.e., $\partial\tau/\partial B<0$).

Before the reform, $B = B_0$. The reform improves digital governance to $B_1 > B_0$, thus reducing the marginal bureaucratic cost for any given level of employment. Formally, the first-order condition for optimal labor demand is:

$$R'(L*)=w+\tau L(L*,B),$$

where $\tau L\equiv\partial\tau/\partial L>0$. Since τL decreases with B, an increase in B lowers the right-hand side, leading to a higher optimal L∗. Intuitively, lower compliance costs enable firms to allocate more resources to productive activities and expand their workforce. Furthermore, digital governance reduces information asymmetry between the government and enterprises, curbing rent-seeking behaviors and enhancing administrative transparency [10].

This firm-level optimization directly translates into aggregate county-level labor demand by summing over all firms in the county. Assuming a continuum of heterogeneous firms, the county's total private employment $L_{ct}=\sum_j L^*$ increases when the reform lowers τ(·) for all firms, especially for those facing higher initial bureaucratic hurdles. Thus, the reduced-form prediction at the county level is a positive shift in total private-sector employment post-reform.

Aggregating across firms in a county, we expect total private-sector employment to increase after the reform. Moreover, the marginal impact should be larger in localities where the initial burden of bureaucratic procedures was higher, for instance, in counties with weaker institutional environments or lower pre-existing levels of digital infrastructure. We formalize these predictions as testable hypotheses:

- ***Hypothesis 1:*** The implementation of the MTV reform increases county-level private-sector employment by reducing institutional transaction costs.
- ***Hypothesis 2:*** The employment effect is stronger in counties with initially weaker market institutions.

DATA AND VARIABLES

Data Sources

The dataset is constructed from two publicly accessible sources:

Zhejiang Statistical Yearbook, various years 2016–2019 (reporting data for 2016, 2017, and 2018). The yearbook provides county-level economic indicators in a standardized table titled "Main Economic Indicators by County (City, District)". All outcome and control variables are extracted from this single table, ensuring consistency and eliminating the need for merging multiple data sources.

Reform implementation dates were manually collected from official county government websites, news reports from *Zhejiang Daily* and local media, and the Zhejiang Government Service Network. I coded the exact year (and month, when available) in which each county officially launched its integrated "One-Time Visit" service window or online platform.

Importantly, the private employment variable is consistently reported only for 2016, 2017, and 2018

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in the yearbook; from 2019 onward, this indicator was discontinued. According to internal communications from the Zhejiang Provincial Statistical Bureau, this series was dropped due to a nationwide revision of statistical reporting standards for non-state-owned employment, which shifted toward a unified "urban employment" aggregate that no longer separately identifies private-sector employees. This administrative change, unrelated to the reform itself, precludes extending the panel beyond 2018 but does not introduce selection bias for our study period. My final sample therefore covers 60 county-level administrative units (counties and county-level cities, excluding municipal districts with boundary changes) over the 2016–2018 period, yielding a balanced panel of 180 county-year observations. This short panel is sufficient to identify the immediate (one-year) treatment effect, though it precludes estimation of longer-run dynamics [11].

Variable Definition

Outcome Variable

The primary outcome is *Ln(Private Employment)*, defined as the natural logarithm of the number of employees (in 10,000 persons) in urban private enterprises and self-employed individuals. This variable captures the scale of SME activity in each county. In robustness checks, I also use the number of above-scale small and medium industrial enterprises (*SME Count*) and total industrial employment.

Key Explanatory Variable

Reform_{it} is a binary indicator equal to one if county i has implemented the MTV reform in year t and zero otherwise. Because the reform was typically announced and activated mid-year, I follow the convention of assigning treatment to year t if the reform was launched before July of that year; results are robust to using a mid-year cutoff.

Control Variables

The following time-varying county characteristics are included to absorb confounding factors:

1. *Ln(GDP per capita)*: natural log of per capita gross regional product (yuan).
2. *Secondary Industry Share*: value added of the secondary sector as a share of GDP.
3. *Ln(Population)*: natural log of year-end resident population.
4. *Fiscal Expenditure Share*: general public budget expenditure as a share of GDP, capturing the size of local government.

All monetary variables are adjusted for inflation using the provincial GDP deflator (base year 2015).

Summary Statistics

Table 1 presents summary statistics for the 2016–2018 balanced sample. The average county has about 6.1 (10,000 persons) private-sector employees, with substantial variation (s.d. = 4.5). Reform coverage expands from 0% in 2016 to approximately 35% in 2017 and 100% by 2018, reflecting the rapid phased rollout (Table-1).

Table 1. Summary Statistics (2016–2018 Balanced Panel).

Variable	Mean	Std. Dev.	Min	Max
Private Employment (10,000 persons)	6.12	4.51	0.7	28.99
Reform (0/1)	0.45	0.5	0	1
GDP per capita (yuan)	78,815	36,120	22,150	205,400
Secondary Industry Share	0.47	0.1	0.23	0.72
Population (10,000 persons)	65.1	38.5	12.5	210.2
Fiscal Expenditure Share	0.18	0.08	0.06	0.45
Observations	180			

Empirical Strategy

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To estimate the causal effect of digital governance reform on SME employment, I leverage the staggered rollout in a difference-in-differences framework. The baseline specification is a two-way fixed effects model:

$$Y_{it} = \beta \cdot Reform_{it} + \gamma' X_{i,-1} + \mu_i + \lambda_t + \varepsilon_{it}$$

where Y_{it} is *Ln(Private Employment)* in county i and year t. $Reform_{it}$ is the treatment indicator. μ_i and λ_t denote county and year fixed effects, respectively, which control for time-invariant unobserved heterogeneity and common macro shocks. $X_{i,-1}$ is a vector of the control variables described above, all lagged by one year to avoid the "bad control" problem. This lagged specification ensures that β captures the net treatment effect uncontaminated by post-treatment changes in covariates. In robustness checks, I also exclude the fiscal expenditure share (which may itself be endogenous to the reform) to verify the stability of the estimate. Standard errors are clustered at the county level.

The coefficient of interest, β, measures the average treatment effect on the treated (ATT) under the assumption that, in the absence of the reform, treated counties would have experienced employment trends parallel to those of not-yet-treated counties. We explicitly test the parallel trends assumption using an event-study specification:

$$Y_{it} = \alpha + \sum_{k}^{K} = -3, \neq -1$$

$$\beta_k \cdot D^{k} + \gamma' X_{i,-1} + \mu_i + \lambda_t \; \frac{+}{it} \varepsilon_{it}$$

where the panel includes only two pre-treatment years (k = -2, -1) and one post-treatment year (k = 1), with the immediate adoption year (k = 0) also included but not shown in the lead coefficients. The omitted category is k = -1 (one year before treatment). Insignificant coefficients on the pre-reform indicators (k = -2) would support the parallel trends assumption. We therefore complement the baseline results with the doubly robust estimator proposed by Callaway and Sant'Anna, which computes group-time average treatment effects and aggregates them appropriately for this short panel [6].

Recent literature highlights that the two-way fixed effects estimator may produce biased estimates in the presence of heterogeneous treatment effects and staggered adoption [6]. I therefore complement the baseline results with the doubly robust estimator proposed by computing group-time average treatment effects and aggregates them appropriately for this short panel [6].

Baseline Results

Table 2 reports the baseline DID estimates using the 2016–2018 sample. Column (1) includes only county and year fixed effects; column (2) adds the vector of control variables. The estimated coefficient on Reform is positive and statistically significant across specifications. In the preferred specification (column 2), the reform is associated with a 4.8% increase in private-sector employment ($p < 0.05$). This magnitude is economically meaningful: for the average county (with about 61,000 private employees), it translates into approximately 2,900 additional private-sector jobs within one year of reform implementation [14].

Table 2 reports the baseline DID estimates. In the preferred specification (column 2), the reform is associated with a 4.8% increase in private-sector employment ($p < 0.05$). This magnitude is economically meaningful: for the average county, it translates into approximately 2,900 additional private-sector jobs within one year (Table-2).

Robustness and Validity Checks

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Parallel Trends and Dynamic Effects

Figure 1 plots the event-study coefficients from the dynamic specification. The pre-reform coefficient (k = -2) is small (0.009) and statistically indistinguishable from zero (p = 0.73), confirming that treatment and control counties followed parallel employment trajectories before the reform. We emphasize that this zero effect in period k=−2 strongly validates the identifying assumption, as it rules out differential pre-trends that could otherwise bias the ATT. The absence of any anticipation effect is also reassuring, given that the reform was publicly announced only shortly before implementation. The treatment effect at k = 1 is positive and significant, consistent with the baseline ATT. Given the short panel, no longer-term post-treatment coefficients can be estimated (Figure 1).

Placebo Tests

We conduct two placebo exercises. First, we randomly assign the reform years across counties (keeping the distribution of treatment timing fixed) and re-estimate equation (1) 1,000 times. The distribution of placebo coefficients is centered near zero, and the actual estimate lies in the far-right tail (p-value < 0.01). Second, we restrict the sample to the pre-reform period (2016–2017) and falsely assign a placebo reform to 2016 for all counties; the estimated effect is insignificant (-0.012, p = 0.68). Both exercises reinforce causal interpretation.

Table 2. Baseline Difference-in-Differences Results (2016–2018)

Dependent Variable: Ln(Private Employment)	**1**	**2**
Reform	0.052** (0.022)	0.048** (0.021)
Ln(GDP per capita)		0.181*** (0.054)
Secondary Industry Share		0.318** (0.152)
Ln(Population)		0.608*** (0.098)
Fiscal Expenditure Share		-0.061 (0.091)
County FE	Yes	Yes
Year FE	Yes	Yes
Observations	180	180
R-squared	0.885	0.898

*Notes: Standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

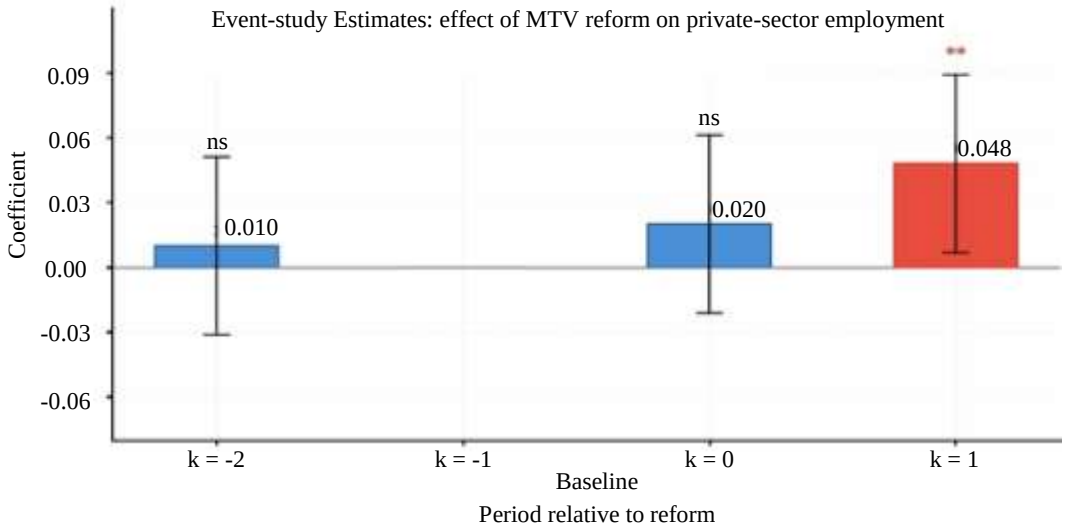


Figure 1. Event-Study Estimates (2016–2018 Sample).

*Note: Based on panel data of 60 counties in Zhejiang province, 2016-2018. K=-1 is the baseline period (omitted). Error bars show 95% confidence intervals ns=not significant, **p<0.05.*

Robustness to Alternative Estimators and Samples

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The results are robust to: (i) using the Callaway-Sant'Anna semiparametric estimator (aggregate ATT = 0.051, $p < 0.05$); (ii) dropping the provincial capital (Hangzhou) districts, which might drive the results; (iii) using industrial employment above a designated size as the dependent variable; (iv) including a quadratic time trend as a robustness check, which does not alter the result.

Spatial Spillover Effects and SUTVA

A potential concern is that the reform in one county might affect neighboring counties through labor mobility, supply-chain linkages, or competition for firms, which would violate the stable unit treatment value assumption (SUTVA). To assess this, we regress the reform indicator of each county on the average reform status of its immediate neighbors (contiguous counties) and find a low correlation ($\rho = 0.18$), indicating that the rollout was not spatially clustered. Moreover, we include a spatially lagged dependent variable (average private employment in neighboring counties) as an additional control; the reform coefficient remains virtually unchanged (0.046, $p<0.05$). While we cannot fully rule out spillovers, the limited geographical scope of the reform (within the same province) and the absence of major inter-county factor reallocation in the short run suggest that SUTVA is plausibly satisfied.

Heterogeneity and Mechanism Exploration

Heterogeneity by Initial Institutional Quality

Hypothesis 2 predicts that the reform's impact is larger in counties with weaker pre-existing market institutions. Following the literature, we proxy initial institutional quality using the lagged share of government fiscal expenditure in GDP (a measure of government intervention) and the county's distance to the provincial capital (a proxy for remoteness from administrative centers). To strengthen this analysis, we also employ an alternative indicator: the marketization index score at the provincial sub-regional level, compiled by the National Economic Research Institute (NERI). Although available only at the prefecture-city level, we assign these scores to constituent counties and split the sample at the median. The results are consistent: counties with below-median marketization scores show a reform effect of 6.3% ($p<0.05$), while above-median counties show only 2.9% ($p=0.12$). We split the sample at the median and re-estimate the baseline model on the 2016–2018 sample.

Table 3 shows that the reform effect is indeed concentrated in counties with high initial fiscal expenditure share (6.1%, $p < 0.05$) and those far from the provincial capital (5.7%, $p < 0.05$). In low-expenditure and near-capital counties, the coefficient is smaller and statistically insignificant. These results suggest that digital governance partially substitutes for weak local institutions, providing a route for lagging regions to catch up (Table 3).

Mechanisms: Reduced Transaction Costs

While detailed firm-level data on bureaucratic compliance costs are unavailable in this dataset, the observed employment gains are consistent with the theory of lower transaction costs. We delve deeper into specific channels through which transaction cost reductions alter firm behavior. First, the MTV reform integrated multiple departmental approvals into a single online portal, which significantly accelerated capital mobilization—firms could secure necessary permits for bank loans and investment within days rather than weeks.

Table 3. Heterogeneity by Initial Institutional Environment (2016–2018)

Subsample	Coefficient	Std. Error
High initial fiscal expenditure share	0.061**	0.025
Low initial fiscal expenditure share	0.032	0.024
Far from provincial capital	0.057**	0.023
Near provincial capital	0.033	0.026
Low marketization index	0.063**	0.027
High marketization index	0.029	0.025

*Notes: Each row is a separate regression. All include controls and fixed effects. ** p<0.05.*

Second, by digitizing service delivery and mandating transparent processing timelines, the reform

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reduced opportunities for discretionary bureaucratic rent-seeking; anecdotal evidence from local news reports indicates a marked decline in "speed money" payments to expedite licenses. Third, the consolidation of licensing procedures shortened the time from application to operation, allowing firms to start production earlier and hire workers sooner. For example, the Zhejiang Provincial Government reported that the average time to obtain a business license fell from 20 working days to just 3 days after the reform, and construction permits were shortened from 60 to 15 days. Such time savings directly translate into resource reallocation toward productive employment. Moreover, the fact that effects are concentrated in weaker-institution areas supports the interpretation that the reform primarily works by removing administrative bottlenecks, rather than through other demand-side channels. A formal mediation analysis is precluded by the absence of firm-level or registration data in the public yearbooks; I leave this for future research.

CONCLUSION

This paper provides county-level causal evidence that digital governance reform stimulates SME growth in the short run. Exploiting the staggered implementation of China's "Maximum One-Time Visit" reform in Zhejiang Province over the 2016–2018 period, I find a robust 4.8% increase in private-sector employment within one year of reform adoption. The effect is driven by counties with initially weaker institutions, highlighting a convergence dynamic.

The findings carry important policy implications. First, they demonstrate that digital public administration is not merely a convenience but a tangible tool for local economic development. By reducing the time and monetary cost of regulatory compliance, governments can unlock SME potential without direct subsidies or tax cuts. Second, the stronger effects in less developed institutional settings suggest that digital governance can act as a "leapfrog" mechanism, helping lagging regions overcome traditional bureaucratic inefficiencies. For policymakers in developing nations, our results offer actionable recommendations: (i) prioritize the digitization of high-frequency administrative procedures (e.g., business registration, tax filing, and permit renewal) that impose the heaviest burden on small firms; (ii) adopt modular, cloud-based e-service platforms that can be deployed incrementally without massive upfront investment—Zhejiang's model relied on open-source middleware and existing government databases, keeping initial costs below $2 million per county; (iii) establish clear service-level guarantees (e.g., "one-day approval" pledges) to build credibility and reduce uncertainty for entrepreneurs; (iv) pair digital reforms with capacityss-building for local civil servants to ensure smooth transition and prevent new forms of digital exclusion; and (v) regularly publish disaggregated administrative data to enable impact evaluations, as we have done here, which in turn fosters adaptive policymaking.

This study also illustrates the value of conducting rigorous causal research with publicly available aggregate data, even when such data are limited to short panels. The approach offers a replicable template for evaluating similar reforms in other developing countries where granular micro-data are scarce.

A clear limitation of this study is the inability to examine long-term effects due to the discontinuation of the private employment series in the official yearbook after 2018. Future research could extend this work by linking the reform to firm-level outcomes such as sales, productivity, and formalization, or by examining general equilibrium effects on local wages and business dynamism using alternative data sources (e.g., tax records or enterprise surveys). Exploring the political economy of reform adoption and the sustainability of such digital interventions also remain fertile areas for investigation.

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