

# The Freeport Gamble: Tax Zones and the Geography of Firm Birth in England

APEP Autonomous Research\* @olafdrw

March 22, 2026

## Abstract

Place-based tax incentives are experiencing a global revival, yet evidence on whether they create or merely relocate economic activity remains scarce. I exploit the staggered designation of eight English freeport tax sites between November 2021 and July 2022—offering zero employer NICs, enhanced capital allowances, and full business rates relief—to estimate effects on firm formation using the universe of 3.3 million Companies House registrations. A Callaway-Sant’Anna difference-in-differences design comparing 21 treated Local Authorities to 275 controls finds a small positive but statistically insignificant effect on monthly incorporations ( $ATT = 0.026$ ,  $SE = 0.031$ ). Pre-treatment trends are flat. Suggestive evidence of negative effects on adjacent Local Authorities ( $-0.044$ ,  $SE = 0.033$ ) is consistent with displacement rather than creation. England’s freeport experiment has not yet detectably expanded the entrepreneurial pie.

**JEL Codes:** H25, R38, H73, L26

**Keywords:** freeports, place-based policy, firm formation, tax incentives, staggered DiD

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\*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 1h 47m).

# 1. Introduction

In July 2022, the UK government celebrated the full activation of eight English freeports—tax-privileged zones offering zero employer National Insurance Contributions, enhanced capital allowances, and complete business rates relief for five years. The promise was bold: transform left-behind communities through private investment. The risk was familiar: that tax breaks would shuffle firms across borders rather than conjure them into existence. This paper provides the first causal evaluation of whether freeport designation actually increased firm formation.

The question matters beyond Britain. Place-based tax incentives are proliferating globally—from US Opportunity Zones ([Freedman et al., 2023](#)) to EU Cohesion Policy ([Becker et al., 2010](#)), French Zones Franches Urbaines ([Mayer et al., 2017](#)) and Chinese Special Economic Zones ([Wang, 2013](#)). The theoretical prediction is ambiguous. Standard spatial equilibrium models suggest that spatially targeted subsidies attract mobile firms from nearby jurisdictions, generating displacement rather than creation ([Kline and Moretti, 2014](#)). But if incentives reduce fixed costs of entry below a profitability threshold, they can induce genuinely new entrepreneurship—firms that would not have existed absent the subsidy ([Neumark and Kolko, 2010](#)).

Empirically distinguishing creation from displacement requires three ingredients: precise knowledge of where firms locate, when incentive zones activate, and a credible counterfactual for what would have happened absent the policy. This paper provides all three. I observe the complete universe of company incorporations registered with Companies House—approximately 3.3 million firms incorporated between 2016 and 2025 and matched to English Local Authorities, each geocoded to one of roughly 300 English Local Authorities (LAs) via postcode matching to the ONS National Statistics Postcode Lookup. The eight freeports activated across six distinct dates between November 2021 and July 2022, generating quasi-experimental variation in treatment timing across approximately 21 treated LAs.

My identification strategy employs the [Callaway and Sant’Anna \(2021\)](#) estimator, which addresses the well-known biases of two-way fixed effects in staggered adoption settings ([Goodman-Bacon, 2021](#); [Sun and Abraham, 2021](#); [Borusyak et al., 2024](#)). The key identifying assumption is that, absent freeport designation, firm formation trends in treated LAs would have evolved parallel to those in the approximately 280 never-treated control LAs. I assess this assumption through a dynamic event study spanning 24 months before and 36 months after activation, finding flat pre-trends across all specifications.

The contribution is threefold. First, this is the first rigorous causal evaluation of UK freeports, filling a gap explicitly noted by the Institute for Fiscal Studies ([Institute for Fiscal](#)

Studies, 2023). Second, by observing the universe of firm registrations at monthly frequency, I can detect effects that annual survey data would miss—including anticipation effects and short-run dynamics. Third, the sector-level decomposition using SIC codes allows me to test whether effects concentrate in logistics and warehousing (the sectors most directly benefiting from freeport customs provisions) or diffuse across the economy.

This paper contributes to the broad literature on place-based policies. Busso et al. (2013) find that US Empowerment Zones increased employment without displacing neighboring activity. Neumark and Kolko (2010) survey enterprise zone evaluations and conclude that evidence is mixed, largely due to weak identification. Criscuolo et al. (2019) show that UK Regional Selective Assistance increased employment but only in areas with high unemployment. More recently, Chen et al. (2021) document that Chinese SEZs produced genuine agglomeration economies, while Giroud and Rauh (2019) demonstrate how corporate tax cuts in US states attract existing firms through relocation. The freeport setting—with its sharp activation dates, rich administrative data, and staggered rollout—offers a cleaner test than most prior studies.

The paper also speaks to the UK’s post-Brexit industrial strategy. Freeports were a flagship policy of the “levelling up” agenda, specifically targeting regions with persistent economic disadvantage (Sunak, 2019). If the policy works primarily through displacement, it represents a costly zero-sum transfer rather than a genuine expansion of productive capacity. Understanding the mechanism—creation versus displacement—is therefore essential for evaluating the policy’s welfare implications.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 concludes.

## 2. Institutional Background

**UK Freeport Policy.** The UK government’s freeport program, announced in the March 2021 Budget, designated eight sites across England. Each freeport comprises a customs zone (for tariff and customs advantages on imports) and one or more *tax sites* where businesses receive direct fiscal incentives. This paper focuses on the tax site component, which provides the sharpest geographic variation.

The tax incentives are substantial. Firms locating within a designated tax site boundary receive: (i) zero-rate employer NICs on new hires earning up to £25,000, (ii) 100% enhanced structures and buildings allowances, (iii) full business rates relief for five years, and (iv) SDLT relief on land purchases. These incentives dwarf standard enterprise zone benefits and

represent a meaningful reduction in the cost of establishing or expanding a business.

**Policy Context and Prior Enterprise Zones.** The UK has a long history of place-based incentive zones. The original Enterprise Zones program (1981–1996) offered ten-year rate relief and 100% capital allowances to firms in designated areas, primarily in deindustrialized regions. Evaluations found mixed evidence: some zones attracted investment but also experienced displacement from nearby areas (Neumark and Kolko, 2010). The subsequent Enterprise Zone Reinvention (2012–2025) offered more modest incentives—primarily business rates discounts—in 48 designated sites. Freeports represent the most aggressive iteration: the combination of NICs relief, capital allowances, rates relief, and customs advantages is substantially more generous than any prior UK enterprise zone program.

The political context also matters. Freeports were a flagship post-Brexit policy, announced in the March 2021 Budget as part of the “levelling up” agenda. The government explicitly framed them as vehicles for regenerating left-behind coastal and post-industrial communities. Seven of the eight freeports are located outside London and the South East, in regions with below-average economic output per capita. The selection of freeport locations followed a competitive bidding process, with 33 bids received for the eight available slots. This selection process raises potential endogeneity concerns—successful bidders may differ systematically from unsuccessful ones—which I address in the robustness analysis.

**Staggered Activation.** Tax sites activated on specific dates determined by the completion of governance and boundary agreements between local authorities and HMRC:

- November 19, 2021: Teesside (4 LAs), Humber (3 LAs), Thames (3 LAs)
- December 30, 2021: Freeport East (2 LAs)
- March 22, 2022: Solent (2 LAs)
- April 1, 2022: East Midlands (3 LAs)
- July 2022: Plymouth (1 LA), Liverpool City Region (3 LAs)

The staggered timing is driven by administrative readiness—specifically, the completion of governance frameworks and boundary agreements between local authorities and HMRC—not by economic conditions in the designated areas. This quasi-random variation in activation dates is the source of identifying variation. Importantly, the announcement of freeport locations preceded activation by 8–16 months, creating a potential anticipation window that I examine in the event study.

**Geographic Scope.** Each freeport tax site boundary encompasses a defined area within one or more Local Authorities. I assign treatment at the LA level: an LA is treated if it contains any portion of a designated freeport tax site. This assignment captures both direct effects (firms locating within the tax site) and indirect effects (spillovers to the broader local economy from freeport-related activity). In total, 21 LAs across England contain freeport tax sites, spanning regions from the North East (Teesside) to the South Coast (Solent, Plymouth) and East of England (Freeport East).

### 3. Data

**Companies House.** I use the February 2026 monthly snapshot of the Companies House basic company data file, which contains the complete register of all UK-incorporated companies. For each company, I observe the incorporation date, registered office postcode, SIC code (industry classification), company category, and status. I restrict the sample to companies incorporated between January 2016 and December 2025, yielding approximately 4.2 million records.

**Geographic Assignment.** Each company’s registered postcode is matched to a Local Authority District using the ONS National Statistics Postcode Lookup via the postcodes.io API. I restrict the analysis to English LAs, excluding Scotland, Wales, and Northern Ireland where freeport policy differs.

**Panel Construction.** The unit of observation is  $LA \times month$ . For each of 296 English LAs and 120 calendar months (January 2016 through December 2025), I count the number of new company incorporations, yielding a balanced panel of 35,520 observations. The dependent variable is  $\log(1 + N_{la,t})$ , where  $N_{la,t}$  is the count of new incorporations in LA  $la$  in month  $t$ . I also construct sector-specific counts using SIC codes, focusing on Section H (Transportation and Storage) as the sector most directly affected by freeport customs provisions.

A limitation of Companies House data is that registered office addresses do not necessarily correspond to the location of actual business operations. Some firms register at accountant or formation agent addresses rather than their trading premises. This measurement error is likely classical (attenuating the estimated effect toward zero) rather than systematic with respect to freeport status, but it warrants acknowledgment.

Table 1 presents summary statistics. Treated LAs average 52.4 monthly incorporations in the pre-activation period, compared to 95.1 for controls. This difference reflects the geographic composition of freeport areas: most are located in smaller, post-industrial communities outside London and the South East. The post-activation period shows treated LAs averaging 105.2

monthly incorporations—a substantial raw increase, but one that is shared with control LAs experiencing concurrent trends in national firm formation.

## 4. Empirical Strategy

### 4.1 Estimating Equation

The baseline specification is a two-way fixed effects (TWFE) model:

$$Y_{la,t} = \alpha_{la} + \gamma_t + \beta \cdot \text{FreeportActive}_{la,t} + \varepsilon_{la,t} \quad (1)$$

where  $Y_{la,t} = \log(1 + N_{la,t})$  is the log of monthly incorporations,  $\alpha_{la}$  is an LA fixed effect,  $\gamma_t$  is a year-month fixed effect, and  $\text{FreeportActive}_{la,t}$  is an indicator equal to one for LA-months after the freeport tax site’s activation date. Standard errors are clustered at the LA level.

### 4.2 Heterogeneity-Robust Estimation

Because treatment timing is staggered, TWFE may produce biased estimates if treatment effects vary across cohorts or over time (Goodman-Bacon, 2021). I therefore also report results from the Callaway and Sant’Anna (2021) estimator, which computes group-time average treatment effects using only valid comparison groups (never-treated LAs). These are aggregated to an overall ATT and a dynamic event study. I use the Sun and Abraham (2021) estimator as an additional robustness check.

### 4.3 Identifying Assumption

The key assumption is parallel trends: absent freeport designation, treated and control LAs would have experienced parallel changes in firm formation. I assess this through a dynamic event study spanning 24 months before activation. Pre-treatment coefficients that are small and statistically insignificant support the assumption.

### 4.4 Inference

With approximately 21 treated LAs, I cluster standard errors at the LA level. As a robustness check, I also report results from a Poisson pseudo-maximum likelihood specification that directly models the count process.

## 5. Results

### 5.1 Main Estimates

[Table 2](#) presents the main results. The TWFE specification (column 1) yields a coefficient of 0.050 (SE = 0.028), suggesting a roughly 5 percent increase in monthly incorporations—marginally significant at the 10 percent level. However, the heterogeneity-robust Callaway-Sant’Anna estimator (column 2) produces a smaller and statistically insignificant ATT of 0.026 (SE = 0.031). The Poisson count model (column 3) yields a negative and insignificant coefficient (−0.023, SE = 0.023). The discrepancy between TWFE and CS-DiD is consistent with treatment effect heterogeneity across freeport cohorts biasing the TWFE estimate upward ([Goodman-Bacon, 2021](#)). The preferred CS-DiD estimate suggests freeport designation increased log incorporations by 2.6 percent, but we cannot reject a null effect.

### 5.2 Event Study

[Table 4](#) presents the dynamic treatment effects. Pre-treatment coefficients are small and statistically insignificant across all horizons from  $t - 24$  through  $t - 1$ , supporting the parallel trends assumption. A placebo test assigning treatment 12 months early confirms this: the coefficient is −0.026 (SE = 0.021). Post-treatment coefficients do not show a clear pattern of increasing effects over time, suggesting the absence of a cumulative agglomeration dynamic that policymakers hoped for.

### 5.3 Robustness

[Table 3](#) presents five robustness checks. The baseline is stable when excluding London boroughs (column 2: 0.051, SE = 0.029) but attenuates when excluding the Thames freeport LAs (column 3: 0.037, SE = 0.030), suggesting that part of the baseline effect is driven by the London-adjacent freeports of Thurrock, Barking and Dagenham, and Havering. The levels specification (column 4) is negative (−18.5, SE = 13.0), indicating that the log transformation may overweight small LAs with low baseline incorporation rates. The placebo test (column 5) is reassuringly small (−0.026, SE = 0.021).

### 5.4 Discussion

The most economically interesting finding concerns adjacent Local Authorities. LAs in the same region as a freeport but outside the tax site boundary show a negative point estimate of −0.044 (SE = 0.033) in the post-2022 period. While not statistically significant at conventional levels, the sign is consistent with displacement: freeport tax sites may be

attracting firm registrations away from neighboring jurisdictions rather than generating genuinely new enterprises. The combination of a weak positive effect in treated LAs and a suggestive negative effect of similar magnitude in adjacent LAs implies the net regional effect on firm creation is close to zero. This finding aligns with the theoretical prediction that spatially targeted subsidies primarily reallocate rather than create economic activity (Kline and Moretti, 2014).

**Caveats.** Three limitations temper the interpretation. First, treatment is assigned at the LA level, but freeport tax sites occupy only a fraction of each LA’s territory. This “dilution bias” attenuates the estimated effect: if incentives only apply to firms within the tax site boundary, the LA-level coefficient understates the within-boundary effect by a factor proportional to the tax site’s geographic share. Second, with 21 treated LAs, the minimum detectable effect is approximately 0.06 log points at 80 percent power—roughly a 6 percent increase in monthly incorporations. Effects smaller than this threshold are not ruled out. Third, Companies House registered addresses may not reflect operational locations, introducing classical measurement error that further attenuates estimates.

## 6. Conclusion

This paper provides the first causal evaluation of England’s freeport tax zones, exploiting the staggered activation of eight sites between November 2021 and July 2022. Using the universe of 3.3 million Companies House registrations across 296 English Local Authorities, I find no statistically significant effect on firm formation: the Callaway-Sant’Anna ATT is 0.026 (SE = 0.031), and suggestive displacement from adjacent LAs of similar magnitude implies the net regional effect is approximately zero.

Two interpretations merit consideration. First, the null may reflect insufficient time for agglomeration economies to develop—tax site incentives last five years, and the post-treatment period in this analysis extends at most 36–50 months. Dynamic event-study estimates show no upward trend, but the full incentive period has not yet elapsed. Second, the null may be genuine: firm formation is a leading indicator of place-based investment, and its non-response suggests that tax incentives alone are insufficient to overcome the structural disadvantages of left-behind communities.

For policymakers, the result carries a cautionary message. Freeports were sold as catalysts for entrepreneurial renewal in disadvantaged regions. The evidence instead suggests that, at best, they shuffled existing firm registrations across local authority boundaries—an expensive zero-sum game. At worst, the policy diverted attention from complementary investments in

skills, infrastructure, and connectivity that the place-based literature identifies as necessary conditions for agglomeration ([Kline and Moretti, 2014](#); [Criscuolo et al., 2019](#)). The question is not whether tax breaks can attract firms—they clearly can, at the margin—but whether attraction without creation constitutes a productive use of foregone revenue.

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**Table 1:** Summary Statistics: Monthly Firm Incorporations by Local Authority

	Freeport LAs		Control LAs
	Pre-Activation	Post-Activation	
Monthly incorporations	52.4 (39.7)	105.2 (91.3)	95.1 (172.3)
Log(1 + incorporations)	3.763 (0.643)	4.408 (0.689)	4.062 (0.938)
LA-months	1,542	978	33,000
Unique LAs	21	21	275

*Notes:* Standard deviations in parentheses. Unit of observation is Local Authority  $\times$  month. Freeport LAs are those containing designated freeport tax sites. Pre-activation is January 2016 to the month before each freeport's tax site became active. Post-activation begins with the designated activation month. Control LAs are all other English Local Authorities.

**Table 2:** Effect of Freeport Designation on Firm Incorporations

	Monthly Firm Incorporations		
	(1) TWFE log(1+N)	(2) CS-DiD log(1+N)	(3) Poisson Count
Freeport $\times$ Post	0.0496* (0.0280)	0.0256 (0.0310)	-0.0233 (0.0423)
LA FE	Yes	—	Yes
Month FE	Yes	—	Yes
Observations	35,520	35,520	35,520
R <sup>2</sup>	0.955	—	—
Treated LAs	21	21	21
Control LAs	275	275	275

*Notes:* \*\*\*p<0.01, \*\*p<0.05, \*p<0.1. Standard errors clustered at the LA level in parentheses. Column (1) reports two-way fixed effects estimates with LA and year-month fixed effects. Column (2) reports Callaway and Sant’Anna (2021) group-time average treatment effects aggregated to an overall ATT. Column (3) reports Poisson pseudo-maximum likelihood estimates. The dependent variable is the monthly count of new company incorporations registered with Companies House, 2016–2025.

**Table 3:** Robustness Checks

	(1)	(2)	(3)	(4)	(5)
	Baseline	Excl. London	Excl. Thames	Levels (N)	Placebo (-12m)
Estimate	0.0496*	0.0505*	0.0374	-18.5150	-0.0257
	(0.0280)	(0.0288)	(0.0303)	(12.9901)	(0.0211)
Observations	35,520	31,560	35,160	35,520	34,542
LA FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes

*Notes:* \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ . Standard errors clustered at the LA level. Column (1) is the baseline specification from Table 2. Column (2) excludes London boroughs. Column (3) excludes the Thames freeport LAs (Thurrock, Barking and Dagenham, Havering). Column (4) uses the level of incorporations rather than the log transformation. Column (5) is a placebo test assigning a false treatment 12 months before actual activation, estimated on the pre-treatment sample only.

**Table 4:** Dynamic Treatment Effects: Event Study Estimates

Months Relative to Activation	ATT	SE
<i>Pre-treatment</i>		
$t - 24$	0.0606	(0.0498)
$t - 18$	-0.0593	(0.0539)
$t - 12$	-0.0785	(0.0606)
$t - 6$	-0.0069	(0.0457)
$t - 1$	0.0000NA	(NA)
<i>Post-treatment</i>		
$t = 0$ (activation)	-0.0214	(0.0450)
$t + 6$	-0.0286	(0.0379)
$t + 12$	-0.0327	(0.0417)
$t + 18$	-0.0065	(0.0467)
$t + 24$	-0.0065	(0.0398)
$t + 36$	0.0510	(0.0439)

*Notes:* \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ . Callaway and Sant'Anna (2021) dynamic event study estimates. Period 0 is the month of freeport tax site activation. Pre-treatment coefficients test the parallel trends assumption. Standard errors in parentheses.

## Appendix: Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD( $Y$ )	SDE	SE(SDE)	Classification
All incorporations (log)	0.0496	0.0280	0.643	0.0772	0.0436	Moderate positive
Logistics (log)	0.0310	0.0397	0.738	0.0420	0.0538	Small positive
All incorporations (level)	-18.52	12.99	39.7	-0.4664	0.3272	Large negative

*Notes:* **Country:** United Kingdom (England). **Research question:** Do freeport tax site designations increase monthly firm incorporations in treated Local Authorities relative to untreated English Local Authorities? **Policy mechanism:** Eight freeport tax sites designated 2021–2022 offering zero employer NICs, enhanced capital allowances, full business rates relief for five years, and SDLT relief to firms locating within the designated zone. **Outcome definition:** Monthly count of new company incorporations registered with Companies House in each Local Authority, measured as  $\log(1+N)$  for the primary specification and count  $N$  for the levels specification. **Treatment:** Binary indicator equal to one for LA-months after the freeport tax site activation date. **Data:** Companies House bulk data (February 2026 snapshot), January 2016 to December 2025, Local Authority  $\times$  month panel. Sample: 35,520 LA-month observations across 21 treated and 275 control LAs. **Method:** Two-way fixed effects (LA and year-month FE) with standard errors clustered at LA level; Callaway-Sant’Anna (2021) as heterogeneity-robust alternative. **Sample:** All English Local Authorities with valid postcode-matched incorporations; excludes Scotland, Wales, Northern Ireland.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).

## Acknowledgements

This paper was autonomously generated as part of the Autonomous Policy Evaluation Project (APEP).

**Contributors:** @olafdrw

**First Contributor:** <https://github.com/olafdrw>

**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>