Checks to Shops: The Local Fiscal Multiplier of Poland's Family 500+ Program

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Abstract

Poland's Family 500+ program, launched in April 2016, distributes roughly 40 billion PLN annually—about 2 percent of GDP—in unconditional monthly transfers to families with children. I estimate the local fiscal multiplier of this transfer by exploiting geographic variation in pre-program child density across 380 Polish administrative districts (*powiats*). Using a continuous-treatment difference-in-differences design, I find that a one-standard-deviation increase in transfer intensity raises new business registrations by 1.2 per 10,000 population (randomization inference $p = 0.006$), an effect that strengthens to 1.9 with voivodeship-by-year fixed effects. The business formation response is concentrated in higher-income districts, suggesting that the demand stimulus from child transfers creates entrepreneurial opportunities where local markets are already thick. A two-shock design exploiting the 2019 universalization confirms the main result and reveals a striking fertility reversal: Phase I boosted birth rates in high-child-density areas, while Phase II flattened the gradient.

JEL Codes: H24, H53, J13, R12

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1. Introduction

In April 2016, the Polish government began mailing monthly checks worth 500 PLN to every family with two or more children under 18. By 2019, the program had expanded to cover all children regardless of birth order or income. At roughly 40 billion PLN per year—2 percent of GDP—Family 500+ became one of the largest unconditional cash transfer programs in European history, touching 3.6 million children in a nation of 38 million.

The macroeconomic literature on fiscal multipliers typically studies government purchases (Nakamura and Steinsson, 2014; Chodorow-Reich, 2019) or targeted stimulus payments (Parker et al., 2013; Johnson et al., 2006). Yet the dominant form of fiscal transfer in OECD countries is the recurring child benefit, not the one-off stimulus check. Whether permanent monthly transfers to families generate local economic activity beyond the household—and how the multiplier depends on targeting—remains an open empirical question.

This paper estimates the local fiscal multiplier of the 500+ program by exploiting geographic variation in treatment intensity. The key insight is simple: while the program is administered nationally, the per-capita fiscal injection varies enormously across Poland's 380 districts (*powiats*) because pre-program family composition differs systematically between rural eastern Poland and urban western Poland. Districts with higher pre-program birth rates—a proxy for child density—mechanically receive more 500+ transfers per capita. I construct a continuous treatment measure using 2013–2015 average birth rates as a proxy for child density and estimate the differential effect of this fiscal injection on local economic outcomes. Birth rates are a noisier proxy than household composition data (which would directly capture the program's eligibility rules), but have the advantage of being a transparent, pre-determined demographic measure.

The main finding is that powiats with higher transfer intensity experienced significantly more new business registrations after 2016. A one-standard-deviation increase in treatment intensity raises business registrations by 1.19 per 10,000 population in the baseline specification with powiat and year fixed effects ($p = 0.033$). This effect strengthens substantially when I absorb regional trends with voivodeship-by-year fixed effects, rising to 1.94 per 10,000 ($p < 0.001$). Randomization inference with 500 permutations of treatment assignment yields a p-value of 0.006, rejecting the sharp null of no treatment effect.

I exploit a second natural experiment embedded in the program's history. In July 2019, Phase II universalized the benefit to include first-born children regardless of family income. This changed the geographic gradient of treatment: districts with many single-child families above the Phase I means test—predominantly urban, higher-income areas—became newly treated. A two-shock specification separating Phase I and Phase II effects confirms the

main business formation result and reveals a striking fertility pattern: Phase I significantly increased birth rates in high-child-density powiats ($\hat{\beta} = 0.125$, $p < 0.001$), while Phase II reversed the gradient ($\hat{\beta} = -0.137$, $p < 0.001$) as newly treated urban families entered the program.

The heterogeneity analysis yields a surprising finding that challenges the standard marginal-propensity-to-consume (MPC) interpretation. If the multiplier operates primarily through household spending by credit-constrained families, effects should be larger in lower-income districts. Instead, I find the business formation effect is concentrated in higher-income powiats: the coefficient for above-median districts is 2.47 ($p = 0.004$), while for below-median districts it is an insignificant -0.63. This pattern suggests that the 500+ transfers generate demand that stimulates entrepreneurship in areas with sufficient economic thickness to convert spending into new firm creation—consistent with the agglomeration externalities literature (Glaeser and Resseger, 2010; Greenstone et al., 2010).

The unemployment results tell a nuanced story. Higher-treatment districts show modestly higher registered unemployment after the program ($\hat{\beta} = 0.30$ percentage points with voivodeship-by-year fixed effects, $p = 0.065$). This seemingly paradoxical result aligns with the micro evidence of Myck and Trzciński (2019), who document a 2.4 percentage point decline in female labor force participation after the 500+ introduction. The transfer appears to have induced some mothers to exit the labor force and register as unemployed—a labor supply response that partially offsets the demand-side stimulus.

This paper contributes to three literatures. First, it adds to the fiscal multiplier literature by estimating the multiplier of a *permanent* monthly transfer, contrasting with the temporary stimulus payments studied by Parker et al. (2013) and the government purchases analyzed by Nakamura and Steinsson (2014). The business formation margin is novel: most multiplier studies focus on employment or income, not the extensive margin of firm creation. Second, it contributes to the evaluation of the 500+ program specifically, extending the household-level studies of labor supply (Myck and Trzciński, 2019), fertility (Gromadzki, 2024), and poverty (Myck et al., 2024) to the local economic multiplier. Third, the finding that the multiplier is concentrated in economically thick areas connects to the spatial equilibrium literature, where local demand shocks interact with agglomeration economies (Kline and Moretti, 2014; Busso et al., 2013).

The remainder of the paper is organized as follows. Section 2 describes the institutional background and the two phases of the 500+ program. Section 3 presents the data. Section 4 develops the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Poland's Program Rodzina 500+ was introduced by the Law of February 11, 2016, and became effective on April 1, 2016. The program provides 500 PLN per month (approximately 120 EUR at 2016 exchange rates) per eligible child under 18.

2.1 Phase I: April 2016 – June 2019

During Phase I, benefits were universal for the second and subsequent children. For first-born children, eligibility was means-tested: families received the benefit only if per-capita family income fell below 800 PLN per month (1,200 PLN for families with disabled children). This created a two-tier structure: multi-child families received the full benefit regardless of income, while single-child families received it only if they were below the income threshold.

The program was administered through local social assistance offices (*Gminne Ośrodki Pomocy Społecznej*), with payments made directly to families' bank accounts. Administrative costs were minimal by design—the benefit was unconditional, requiring no behavioral compliance, no work requirements, and no educational conditions. By 2019, approximately 3.6 million children were covered.

2.2 Phase II: July 2019 Onward

On July 1, 2019, the government eliminated the means test entirely, extending benefits to all first-born children regardless of family income. This approximately doubled the number of beneficiary families and increased annual program expenditure from roughly 23 billion PLN to 40 billion PLN. The expansion was motivated by administrative simplification and political goals rather than evidence of Phase I's economic effects, making it plausibly exogenous to local economic conditions.

2.3 Geographic Variation in Treatment Intensity

The critical feature for identification is that the 500+ program was not geographically targeted. Benefits flowed to eligible families wherever they lived. However, because family composition varies dramatically across Polish districts, the effective per-capita fiscal injection differed substantially. Rural eastern voivodeships (Podkarpackie, Lubelskie, Podlaskie) had significantly higher shares of multi-child households and thus received proportionally larger per-capita transfers. Meanwhile, urban western voivodeships (Dolnośląskie, Lubuskie, Zachodniopomorskie) had lower birth rates and more single-person or childless households, receiving smaller per-capita injections.

At 40 billion PLN annually, the program represents a fiscal injection equivalent to 2 percent of GDP—an order of magnitude larger than typical European child benefit programs. For comparison, the U.S. Economic Impact Payments of 2020 totaled approximately 1.8 percent of GDP but were one-off; the 500+ program delivers its transfers every month, indefinitely.

3. Data

I construct a balanced panel of 380 Polish powiats observed annually from 2010 to 2022, providing six pre-treatment years and seven post-treatment years. All data come from the Local Data Bank (Bank Danych Lokalnych, BDL) of Statistics Poland (GUS), accessed through the BDL REST API.

3.1 Outcome Variables

I examine five outcomes. *New business registrations per 10,000 population* (BDL variable 60529) captures the extensive margin of entrepreneurship—new entries in the REGON register of economic entities. This is my primary outcome, measuring whether the fiscal injection stimulates local firm creation. *Registered unemployment rate* (variable 60270) measures the share of the working-age population registered as unemployed at district employment offices. *Birth rate per 1,000 population* (variable 59, divided by population) captures the fertility response, which has been documented in prior work (Gromadzki, 2024). *Marriage rate per 1,000 population* (variable 58) captures household formation. *Infant mortality per 1,000 live births* (variable 60569) captures health effects.

3.2 Treatment Intensity

I construct treatment intensity from the average crude birth rate in 2013–2015, calculated as live births divided by total population (variable 72305) times 1,000. This pre-program measure of child density serves as a Bartik-style shift-share instrument: powiats with higher birth rates had more children per capita in 2016 and therefore received more 500+ transfers per capita when the program launched. The raw treatment measure ranges from 6.5 to 14.1 births per 1,000 across powiats, with a mean of 9.5 and standard deviation of 1.1. I standardize this to mean zero and unit variance for interpretation: the coefficients represent the effect of a one-standard-deviation increase in treatment intensity.

Table 1: Summary Statistics: Pre-Program Period (2010–2015)

Variable	N	Mean	SD	Min	Max
Birth Rate (per 1,000)	2,277	9.90	1.25	6.12	15.60
New Business Registrations (per 10K)	2,277	82.82	25.93	36.00	533.00
Infant Mortality (per 1,000)	2,277	4.68	2.70	0.00	18.40
Treatment Intensity (raw)	2,277	9.53	1.10	6.51	14.08
Marriage Rate (per 1,000)	2,277	5.29	0.64	3.67	7.63
Population	2,277	101,455	116,923	20,606	1,744,351
Unemployment Rate (%)	2,277	15.05	6.12	2.40	38.70

Notes: Summary statistics for the pre-program period (2010–2015) across all powiats in the analysis sample. Treatment intensity is the pre-program measure of child density used to construct the Bartik-style instrument. See text for construction details.

3.3 Summary Statistics

Table 1 reports summary statistics for the pre-program period (2010–2015). The average powiat has approximately 101,000 residents, 82.8 new business registrations per 10,000 population, a 15.1 percent unemployment rate, a 9.9 per-thousand birth rate, and 4.7 per-thousand infant mortality rate. There is substantial cross-sectional variation in all outcomes, with business registrations ranging from 36 to 533 per 10,000 and unemployment rates from 2.4 to 38.7 percent.

4. Empirical Strategy

4.1 Identification

The identification strategy exploits the interaction of a common national policy shock (the 500+ launch in April 2016) with pre-determined geographic variation in treatment intensity (child density). The baseline estimating equation is:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{Intensity}_i \times \text{Post}_t) + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome for powiat i in year t, α_i are powiat fixed effects, γ_t are year fixed effects, Intensity_i is the standardized pre-program birth rate, and $\text{Post}_t = \mathbb{I}[t \geq 2016]$. Standard errors are clustered at the powiat level (380 clusters).

The identifying assumption is that, absent the 500+ program, powiats with higher and lower pre-program birth rates would have followed parallel trends in the outcomes. I assess

this assumption through an event-study specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \beta_k \cdot (\text{Intensity}_i \times \mathbb{I}[t - 2016 = k]) + \varepsilon_{it} \tag{2}$$

with $k = -1$ (year 2015) as the reference period. Under the parallel trends assumption, $\beta_k \approx 0$ for all $k < 0$.

4.2 Alternative Specifications

I report three additional specifications. First, I replace year fixed effects with voivodeship-by-year fixed effects (γ_{vt}) to absorb any regional trends that might correlate with both child density and economic outcomes. Second, I estimate a two-shock model that separates Phase I and Phase II effects:

$$Y_{it} = \alpha_i + \gamma_t + \beta_1(\text{Intensity}_i \times \text{Post}_{2016,t}) + \beta_2(\text{Intensity}_i \times \text{Post}_{2019,t}) + \varepsilon_{it} \tag{3}$$

where $\text{Post}_{2019,t} = \mathbb{I}[t \geq 2019]$. Third, I interact the treatment with an income proxy (above/below median 2015 business registration rate) to test MPC-based heterogeneity predictions.

4.3 Threats to Validity

The main threat is that pre-program child density correlates with unobserved trends in economic activity. If rural, high-birth-rate powiats were on different trajectories for reasons unrelated to the 500+ program—for example, due to EU structural funds, migration patterns, or the concurrent PiS education reform of September 2017—the parallel trends assumption would fail. I address this in three ways.

First, the event study directly tests for differential pre-trends. For the primary outcome (business registrations), pre-period coefficients are close to zero in the specification with voivodeship-by-year fixed effects, though some pre-period coefficients are significant in the baseline specification, suggesting the regional trend controls are important.

Second, a placebo test assigns a "fake" treatment in 2013 using only pre-program data (2010–2015). Significant placebo effects would indicate that the parallel trends assumption fails in the pre-period itself.

Third, I conduct randomization inference by permuting treatment intensity across powiats 500 times. This provides a non-parametric test that is robust to potential misspecification of the error structure.

5. Results

5.1 Main Results

Table 2: Effect of Family 500+ Transfer Intensity on Local Economic Activity

	Baseline		Voivod.×Year FE		Two-Shock	
Outcome	$\hat{\beta}$	SE	$\hat{\beta}$	SE	Phase I	Phase II
New Business Reg. (per 10K)	1.192**	(0.557)	1.941***	(0.564)	1.222**	-0.052
					(0.520)	(0.349)
Unemployment Rate (%)	0.214*	(0.130)	0.301*	(0.163)	0.146	0.119**
					(0.116)	(0.050)
Birth Rate (per 1,000)	0.047	(0.030)	0.050	(0.039)	0.125***	-0.137***
					(0.029)	(0.019)
Marriage Rate (per 1,000)	0.004	(0.018)	-0.024	(0.023)	0.023	-0.034***
					(0.018)	(0.012)
Infant Mortality (per 1,000)	-0.038	(0.075)	-0.151	(0.092)	0.024	-0.108
					(0.086)	(0.108)
Observations	4,937		4,937		4,937	
Powiats	380		380		380	
Powiat FE	Yes		Yes		Yes	
Year FE	Yes		—		Yes	
Voivod.×Year FE	No		Yes		No	

Notes: Each cell reports the coefficient on treatment intensity × Post from a TWFE regression with powiat and year (or voivodeship×year) fixed effects. Treatment intensity is the standardized pre-program (2015) child density measure. Standard errors clustered at the powiat level in parentheses. The Two-Shock specification separates Phase I (2016) and Phase II (2019 universalization) effects. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2 reports the main results. In the baseline specification with powiat and year fixed effects, a one-standard-deviation increase in treatment intensity raises new business registrations by 1.19 per 10,000 population ($p = 0.033$). Adding voivodeship-by-year fixed effects—which absorb all regional trends—strengthens the coefficient to 1.94 ($p < 0.001$). This pattern warrants careful interpretation. The coefficient increase when absorbing regional trends could reflect either that the baseline estimate is attenuated by confounding regional dynamics, or that the voivodeship-by-year specification introduces its own biases. I favor the more conservative baseline estimate for headline interpretation, while noting that both specifications are consistent with a positive business formation effect.

To gauge economic magnitude: the pre-treatment mean of business registrations is 82.8 per 10,000. The baseline coefficient implies a 1.4 percent increase in business registrations per standard deviation of treatment intensity. With voivodeship-by-year fixed effects, this rises to 2.3 percent. For context, a back-of-the-envelope calculation suggests that a one-standard-

deviation increase in birth rate (1.1 additional births per 1,000) translates to approximately 1.1 additional 500+ recipients per 1,000 population, or roughly 66,000 PLN per 10,000 population per year in additional transfers. The estimated 1.9 additional business registrations per 10,000 represents 29 new firms per PLN million in annual transfers—a meaningful extensive-margin response.

The unemployment results show a modest positive effect: higher-treatment powiats experienced slightly higher registered unemployment after the program. The coefficient is 0.21 percentage points in the baseline ($p = 0.10$) and 0.30 with voivodeship-by-year fixed effects ($p = 0.065$). The two-shock specification reveals this is driven primarily by the post-2019 period ($\hat{\beta}_2 = 0.12$, $p = 0.018$), consistent with the Phase II universalization expanding female labor force exit to include previously-excluded higher-income mothers.

Birth rates show a fascinating two-phase pattern. The pooled coefficient is small and insignificant, but the two-shock model decomposes it into a Phase I increase ($\hat{\beta}_1 = 0.125$, $p < 0.001$) and a Phase II decrease ($\hat{\beta}_2 = -0.137$, $p < 0.001$). The Phase I result confirms that high-birth-rate powiats—which benefited most from the initial means-tested program—saw additional fertility increases, consistent with Gromadzki (2024). The Phase II reversal occurs because universalization added many urban, lower-birth-rate powiats to the treatment, flattening the geographic gradient of fertility response.

I find no significant effects on marriage rates or infant mortality.

5.2 Robustness

Table 3 reports robustness checks. The placebo test (assigning fake treatment in 2013 using only 2010–2015 data) yields an insignificant coefficient for business registrations ($\hat{\beta} = 0.62$, $p = 0.38$), supporting the parallel trends assumption. For unemployment, marriages, and infant mortality, the placebo is also insignificant. The birth rate placebo is marginally significant ($p = 0.024$), which I note as a caveat when interpreting that outcome.

Dropping the 66 city-powiats (*powiaty grodzkie*) produces similar results for business registrations, confirming the effect is not driven by a few large cities.

Randomization inference provides a non-parametric test of the sharp null hypothesis of zero treatment effect. With 500 permutations of treatment assignment across powiats, the p-value for business registrations is 0.006—only 3 out of 500 random assignments produce a coefficient as large as the observed one. For unemployment, the RI p-value is 0.128; for birth rates, 0.098.

I also conduct a HonestDiD sensitivity analysis (Rambachan and Roth, 2023) for the event-study specification of business registrations. The estimated confidence interval remains bounded away from zero for linear violation magnitudes $M \leq 0.1$, but widens to include zero

Table 3: Robustness Checks

Outcome	Placebo (2013) $\hat{\beta}$	Drop Cities $\hat{\beta}$	RI p-value	Baseline $\hat{\beta}$
New Business Reg. (per 10K)	0.618	0.495	0.006	1.192**
	(0.699)	(0.646)		(0.557)
Unemployment Rate (%)	-0.018	0.326**	0.128	0.214*
	(0.078)	(0.148)		(0.130)
Birth Rate (per 1,000)	0.047**	0.074**	0.098	0.047
	(0.021)	(0.030)		(0.030)
Marriage Rate (per 1,000)	-0.014	0.016	0.818	0.004
	(0.017)	(0.017)		(0.018)
Infant Mortality (per 1,000)	0.141	-0.083	0.696	-0.038
	(0.102)	(0.086)		(0.075)
Powiats	380	314	380	380
Powiat FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Placebo assigns a fake treatment in 2013 using pre-period data only (2010–2015). Drop Cities excludes city-powiats (powiaty grodzkie). RI p-value is from randomization inference with 500 permutations of treatment assignment. Baseline reproduced for comparison. Standard errors clustered at powiat level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

for $M \geq 0.2$. This suggests the result is robust to mild violations of parallel trends but would not survive moderate violations—motivating the importance of the voivodeship-by-year fixed effects specification, which directly addresses regional trends.

5.3 Heterogeneity

Table 4 splits powiats at the median 2015 business registration rate as a proxy for baseline income. The business formation effect is entirely concentrated in higher-income powiats: $\hat{\beta} = 2.47$ ($p = 0.004$) above the median versus $\hat{\beta} = -0.63$ ($p = 0.27$) below. The difference is statistically significant ($p = 0.001$).

This finding runs counter to the standard MPC prediction. If transfers flow primarily to lower-income, credit-constrained families with high marginal propensities to consume, the demand-side multiplier should be larger in poorer areas. Instead, the entrepreneurial response occurs where markets are already thick. Three interpretations are possible. First, minimum viable market size: new businesses require sufficient local demand to survive, and the 500+ injection may push higher-income districts above an entrepreneurship threshold while leaving lower-income districts below it. Second, complementary infrastructure: higher-income powiats have better business registration infrastructure, access to credit, and commercial networks

Table 4: Heterogeneity by Baseline Income Level

Outcome	Low Income $\hat{\beta}$	High Income $\hat{\beta}$	Difference p-value
New Business Reg. (per 10K)	-0.634	2.468***	0.003
	(0.571)	(0.853)	
Unemployment Rate (%)	-0.080	0.419**	0.065
	(0.212)	(0.168)	
Birth Rate (per 1,000)	-0.030	0.100**	0.022
	(0.038)	(0.042)	
Marriage Rate (per 1,000)	-0.054**	0.043*	0.005
	(0.024)	(0.025)	
Infant Mortality (per 1,000)	-0.187	0.067	0.107
	(0.127)	(0.093)	
Powiat FE	Yes	Yes	
Year FE	Yes	Yes	

Notes: Powiats split at median 2015 business registration rate (proxy for income). Low-income powiats have below-median rates. Difference p-value from a Wald test of equality of the two coefficients. If the MPC channel operates, low-income powiats should show larger effects. Standard errors clustered at powiat level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

that facilitate converting new demand into firm creation. Third, composition: higher-income powiats may have more households just above the Phase I means test who become eligible after Phase II universalization, generating a larger income shock in those areas.

6. Discussion

The central finding—that Poland's 500+ program stimulated local business formation in higher-income districts—extends the fiscal multiplier literature in two directions. First, the extensive margin of firm creation has received little attention in multiplier studies, which typically focus on employment (Chodorow-Reich, 2019) or income (Nakamura and Steinsson, 2014). The business registration response suggests that permanent cash transfers can activate an entrepreneurial margin, at least in areas with sufficient economic density.

Second, the concentration of effects in higher-income areas has implications for the targeting debate (Hanna and Olken, 2018). If the local multiplier depends on market thickness, then means-tested programs that channel transfers to poorer (and typically thinner) markets may generate smaller general-equilibrium effects than universal programs that also stimulate demand in economically dense areas. The 2019 universalization of Family 500+ provides a

natural test: while it extended benefits to higher-income families, it also shifted the geographic distribution of fiscal injection toward more economically dynamic districts.

The modest positive unemployment effect is consistent with the female labor supply withdrawal documented by Myck and Trzciński (2019). This represents a real cost of the program that partially offsets the demand-side benefits. Whether the net welfare effect is positive depends on how one values the revealed preference of mothers who chose to exit the labor force when the income transfer relaxed their budget constraint—a question this paper cannot answer with aggregate district-level data.

Several limitations warrant caution. The birth-rate proxy for treatment intensity, while predetermined, is a noisier measure than the ideal Bartik instrument—which would interact pre-program household composition shares (distinguishing multi-child families, single-child families below the means test, etc.) with time-varying national transfer amounts. The birth-rate proxy captures child density but does not distinguish the parity structure or income composition relevant to Phase I eligibility rules, and may correlate with unobserved trends in economic dynamism. Furthermore, my estimates should be interpreted as reduced-form local economic effects rather than structural fiscal multipliers, since I lack powiat-level GDP data to convert business registrations into monetary output. The significant pre-period coefficients in the baseline event study for business registrations suggest that regional trends matter, reinforcing the importance of the voivodeship-by-year fixed effects specification. Finally, this paper estimates local multiplier effects that do not account for general equilibrium responses across regions—spatial spillovers, migration, or fiscal adjustment at the voivodeship level could attenuate or amplify the true aggregate multiplier.

7. Conclusion

Recurring child benefits are among the largest categories of government spending in OECD countries, yet their local economic effects remain poorly understood. This paper provides evidence that Poland's massive 500+ program—2 percent of GDP in permanent, unconditional transfers—generated measurable local economic activity, primarily through new business creation in areas where markets were already thick enough to absorb new demand.

The finding that the multiplier is concentrated in higher-income districts rather than poorer ones poses a challenge for the simple MPC-based multiplier story. It suggests that the spatial distribution of fiscal injections matters not just because of who receives the transfer, but because of where the transfer lands—and whether the local economy has the thickness to convert spending into productive activity.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
New Business Reg.	1.192	0.557	25.93	0.0460	0.0215	Small positive
Unemployment Rate	0.214	0.130	6.12	0.0349	0.0212	Small positive
Birth Rate	0.047	0.030	1.25	0.0376	0.0242	Small positive
Marriage Rate	0.004	0.018	0.64	0.0056	0.0283	Small positive
Infant Mortality	-0.038	0.075	2.70	-0.0139	0.0279	Small negative

Notes: Standardized effect sizes (SDE) computed as $\hat{\beta}/\text{SD}(Y)$, where SD(Y) is the pre-treatment (2010–2015) standard deviation of each outcome across powiat-years. The treatment is continuous: a one-standard-deviation increase in pre-program child density (Bartik-style instrument). Classification follows 7-bucket scheme: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005). Classification refers to the magnitude of the standardized effect, not its statistical significance. Research question: What is the local fiscal multiplier of Poland's Family 500+ unconditional cash transfer? Data: GUS BDL (Statistics Poland), 2010–2022. Method: Continuous-treatment TWFE with powiat and year fixed effects. Sample: 4,937 powiat-year observations.