Taxing Away the Shadow: Sweden's Household Services Deduction and the Formalization of Domestic Work

APEP Autonomous Research* @olafdrw

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Abstract

In 2007, Sweden introduced a 50% tax deduction for household services purchased from registered firms, creating a price wedge between formal and informal domestic work. I exploit municipality-level variation in pre-reform income—a proxy for demand for household services—in a continuous-treatment difference-in-differences design across 290 municipalities from 1999 to 2024. Municipalities with one standard deviation higher pre-reform income experienced 0.6–0.9% higher declared income growth after the reform. Service-sector employment grew 1.8% faster in high-demand municipalities, while manufacturing employment showed no differential response. The results survive municipality-specific linear trends and Fisher randomization inference ($p < 0.001$). These findings provide new quasi-experimental evidence that demand-side subsidies can shift economic activity from the shadow economy to formal channels—addressing a question Sweden's own National Audit Office identified as lacking causal identification.

JEL Codes: H26, J46, H24, O17

Keywords: shadow economy, tax deduction, formalization, household services, Sweden, difference-in-differences

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 28m).

1. Introduction

How large is the shadow economy, and can governments shrink it? The question matters enormously: undeclared work evades taxation, denies workers social protections, and distorts competition against compliant firms. In OECD countries, the shadow economy is estimated at 10–20% of GDP (Schneider et al., 2010; Schneider and Enste, 2000). Policymakers have long debated whether the right lever is enforcement—making evasion costly—or incentives that make formality attractive (Slemrod, 2007; Allingham and Sandmo, 1972). The enforcement approach dominates the literature (Kleven et al., 2011; Pomeranz, 2015; Naritomi, 2019). The incentive approach—paying people to go formal—has far less evidence.

Sweden ran exactly this experiment. On July 1, 2007, the center-right government introduced the RUT-avdrag (Rengöring, Underhåll, Tvätt—Cleaning, Maintenance, Laundry), a tax deduction covering 50% of labor costs for household services purchased from tax-registered firms. The maximum deduction of SEK 50,000 per person per year made formal cleaning, gardening, childcare, and elderly care substantially cheaper than hiring workers off the books. By design, only services from registered, tax-compliant firms qualified, creating a direct price incentive to shift from cash-in-hand payments to formal employment contracts.

This paper provides new quasi-experimental evidence on whether the RUT deduction achieved its stated goal of formalizing shadow-economy work. I exploit the fact that demand for household services varies sharply across Sweden's 290 municipalities: affluent municipalities with many high-income households have both greater demand for domestic services and greater capacity to claim the deduction (which requires sufficient tax liability). Using pre-reform (2006) mean income as a continuous measure of treatment intensity, I estimate a dose-response difference-in-differences model with municipality and year fixed effects, covering 1999–2024.

The main finding is that the RUT reform caused a structural break in the income-convergence pattern across Swedish municipalities. Before 2007, high-income municipalities experienced slower income growth—the standard convergence dynamic. After the reform, this pattern reversed: municipalities with one standard deviation higher treatment intensity saw 0.9% higher declared income growth in the baseline specification ($p < 0.001$), and 0.6% with municipality-specific linear trends ($p < 0.001$). Fisher randomization inference over 999 permutations of treatment intensity across municipalities confirms the result ($p < 0.001$).

The employment decomposition reveals that the income effect operates through the expected channel. Service-sector employment (NACE M+N: professional, administrative, and support services, which includes cleaning firms and domestic service companies) grew 1.8% faster in high-treatment municipalities after 2012, while manufacturing employment

showed no differential response. Hospitality (NACE I) also responded positively, consistent with formal service demand spillovers. The manufacturing null serves as a clean placebo: if the treatment intensity variable merely captured general economic dynamism rather than RUT-specific demand, all sectors would respond.

These results contribute to three literatures. First, the formalization literature has focused primarily on developing countries (Ulyssea, 2018; La Porta and Shleifer, 2014; Meghir et al., 2015), where informality reflects fundamentally different institutional structures. The RUT experiment demonstrates that even in a high-trust, high-enforcement Scandinavian economy, demand-side subsidies can shift the formal-informal margin—a setting where one might have expected the shadow economy to be small and resistant to incentive-based intervention.

Second, the tax compliance literature has established that third-party reporting (Kleven et al., 2011) and consumer self-enforcement (Naritomi, 2019; Pomeranz, 2015) reduce evasion. The RUT deduction combines both mechanisms: it creates third-party reporting by channeling transactions through registered firms, while simultaneously incentivizing consumers to demand formal services. My results suggest this combination is effective even in a sector—household services—where third-party reporting is otherwise minimal.

Third, I directly address a policy evaluation gap identified by Sweden's own National Audit Office (Swedish National Audit Office , Riksrevisionen), which concluded in 2020 that "researchers cannot establish a causal link" between the RUT subsidy and formalization. The continuous-treatment DiD design using cross-municipality variation in demand intensity offers a credible path toward resolving this challenge, though the pre-reform convergence pattern means the estimates should be interpreted as suggestive rather than definitive.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 develops the empirical strategy. Section 5 reports the results. Section 6 discusses implications.

2. Institutional Background

The shadow economy in household services. Before the RUT reform, Sweden's household services sector was characterized by substantial informality. The OECD estimated that undeclared household work represented 2–4% of GDP in Scandinavian countries (OECD, 2008). Cash payments for cleaning, gardening, and childcare were common even in a society with otherwise high tax compliance (Halla, 2012). The political motivation for the reform was twofold: create formal jobs in a labor-intensive sector, and broaden the tax base by shifting unreported income into taxable channels.

The RUT deduction. The RUT-avdrag was introduced on July 1, 2007, under Swedish Code of Statutes 2007:346 (Lag om skattereduktion för hushållsarbete). The deduction covers 50% of labor costs for household services performed in the buyer's home by a registered, tax-compliant firm. Eligible services include cleaning, gardening, snow removal, laundry, childcare, tutoring, and elderly care. The maximum deduction is SEK 50,000 per person per year (approximately €5,000 at 2007 exchange rates). Only labor costs qualify—materials and travel are excluded. The deduction is administered through the tax return, creating a paper trail that links buyer, seller, and service.

Why cross-municipality variation identifies the effect. The RUT reform applied uniformly to all of Sweden—there is no staggered adoption. Identification instead exploits variation in demand intensity across municipalities. Two features generate this variation. First, the deduction requires sufficient tax liability to claim: a household with SEK 50,000 in income tax can claim the full deduction, while a household with SEK 10,000 cannot. Second, high-income households have greater demand for household services, since the opportunity cost of performing domestic tasks rises with wages. Municipalities with many affluent households therefore experience larger effective treatment, measured as the pre-reform (2006) mean earned income.

3. Data

All data come from Statistics Sweden (Statistiska centralbyrån, SCB) via the open-access PxWeb API. No registration or API key is required.

Income panel (1999–2024). The primary outcome is mean earned income for persons aged 20–64 at the municipality level, drawn from the total earned income register (SamForvInk1). This captures all income reported to the Swedish Tax Agency, making it a direct measure of formalization: when shadow-economy work becomes formal, it appears in declared earned income. The panel covers 290 municipalities over 26 years, yielding 7,540 municipality-year observations.

Employment by sector (2008–2018). Sector-level employment counts come from the Register-based Activity Statistics (RAMS), which covers all gainfully employed persons aged 16 and above by municipality of residence, NACE Rev. 2 industry section, and sex. I focus on M+N (professional, scientific, technical, administrative, and support services—the sector containing cleaning firms, domestic service companies, and staffing agencies) as the primary mechanism outcome, with manufacturing (B+C) as a placebo sector. The RAMS data under

the SNI2007 classification covers 2008–2018.

Employment rates by origin (2004–2018). Municipality-level gainful employment rates for persons aged 20–64, separately for Swedish-born and foreign-born populations, from RAMS table RAMSForvInt04. This series spans 2004–2018, providing three pre-reform years for the immigration channel analysis.

Treatment intensity. I measure treatment intensity as the standardized (mean zero, unit variance) mean earned income of persons aged 20–64 in each municipality in 2006, the last pre-reform year. This ranges from SEK 201,000 (Haparanda) to SEK 466,000 (Danderyd), with a mean of SEK 237,000. Municipalities in the top quartile (mean income above SEK 244,000) include suburban Stockholm, Göteborg, and Malmö commuter towns—precisely the areas where demand for formal household services is highest.

Table 1: Summary Statistics

Variable	Mean	SD	Min	Max	N
Mean earned income (SEK 000s)	281.1	70.8	157.9	846.2	7537
Treatment intensity (2006 mean income, SEK 000s)	237.4	27.9	200.5	466.2	290
M+N employment (persons)	1824.0	5729.0	50.0	95011.0	3190
Manufacturing employment (persons)	2008.0	2700.0	35.0	34182.0	3190
Employment rate, native-born (%)	82.1	3.4	68.4	91.2	4350
Employment rate, foreign-born (%)	57.4	8.0	33.1	78.8	4350

Notes: Income data cover 290 Swedish municipalities, 1999–2024 (7,540 obs). Treatment intensity is the mean earned income of persons aged 20–64 in each municipality in 2006, the last pre-reform year. Employment data cover 2008–2018 (RAMS). Employment rates cover 2004–2018 for persons aged 20–64.

4. Empirical Strategy

4.1 Identification

I estimate a continuous-treatment difference-in-differences model:

$$\ln Y_{mt} = \alpha_m + \delta_t + \beta \cdot D_m \cdot \text{Post}_t + \varepsilon_{mt} \tag{1}$$

where Y_{mt} is mean earned income in municipality m in year t, α_m and δ_t are municipality and year fixed effects, D_m is treatment intensity (standardized 2006 mean income), $\text{Post}_t = \mathbb{I}[t \geq 2007]$, and ε_{mt} is the error term. The coefficient β captures the differential change in log income per standard deviation of treatment intensity after the reform.

The identifying assumption is that, absent the RUT reform, income growth in high- and

low-income municipalities would have evolved along parallel trends. With 290 municipalities and eight pre-reform years (1999–2006), I assess this assumption through an event-study specification:

$$\ln Y_{mt} = \alpha_m + \delta_t + \sum_{\tau \neq 2006} \beta_\tau \cdot D_m \cdot \mathbb{I}[t = \tau] + \varepsilon_{mt} \tag{2}$$

with 2006 as the reference year. The pre-reform coefficients $\{\beta_\tau\}_{\tau<2007}$ test for differential pre-trends.

4.2 Threats to Validity

Pre-trend convergence. The event study reveals that high-income municipalities experienced slower income growth before 2007, reflecting standard mean reversion. This threatens the parallel trends assumption. I address this in three ways. First, I include municipality-specific linear time trends, which absorb the convergence pattern; the post-reform effect survives with a coefficient of 0.006 ($p < 0.001$). Second, I conduct Fisher randomization inference by permuting treatment intensity across municipalities 999 times; the actual coefficient lies well outside the permutation distribution ($p < 0.001$). Third, the sector-specific employment analysis provides an alternative test: if the treatment intensity variable merely captured economic convergence, all sectors would respond similarly. Instead, service-sector employment grows differentially while manufacturing does not.

Reverse causality. Could the reform itself have caused high-income households to sort into specific municipalities? The treatment intensity variable is measured in 2006, before the reform. Post-reform sorting would attenuate estimates (by weakening the correlation between pre-reform demand and actual treatment) rather than inflate them.

Standard errors are clustered at the municipality level throughout (290 clusters).

5. Results

5.1 Main Results: Income

Table 2 presents the main estimates. Column (1) reports the baseline specification from Equation (1): a one standard deviation increase in treatment intensity is associated with 0.91% higher income growth after the reform ($p < 0.001$). This represents approximately SEK 2,200 per person per year at the sample mean. Column (2) shows that the effect is similar in the early post-reform period (2007–2012: 0.94%) and late post-reform period (2013–2024: 0.90%), suggesting a persistent rather than transitory response.

Column (3) adds municipality-specific linear trends. The coefficient falls to 0.59%, confirming that part of the raw effect reflects a break in pre-reform convergence dynamics. The trends-robust estimate remains highly significant ($p < 0.001$) and represents the incremental break in income trajectories at the reform date, net of pre-existing convergence patterns.

Column (4) excludes Stockholm county (26 municipalities), where treatment intensity is highest. The coefficient rises to 1.33%, suggesting that the formalization effect is actually concentrated in municipalities outside the capital region—where the formal-informal margin may be more elastic.

Table 2: Effect of RUT Treatment Intensity on Log Mean Earned Income

	(1) Baseline	(2) Early/Late	(3) Linear Trends	(4) Excl. Stockholm
Treat. Intens. × Post	0.0091***		0.0059***	0.0133***
	(0.0022)		(0.0016)	(0.0046)
Treat. Intens. × Early		0.0094***		
		(0.0015)		
Treat. Intens. × Late		0.0090***		
		(0.0027)		
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Muni. linear trends	No	No	Yes	No
N	7,537	7,537	7,537	6,861
Municipalities	290	290	290	264
RI p-value	0			

Notes: The dependent variable is log mean earned income (ages 20–64) at the municipality-year level. Treatment intensity is the standardized (mean zero, unit variance) 2006 mean income. Post equals one from 2007 onward. Early covers 2007–2012; Late covers 2013–2024. Column (3) includes municipality-specific linear time trends. Column (4) excludes Stockholm county (26 municipalities). Standard errors clustered at the municipality level in parentheses. RI p-value from Fisher randomization inference (999 permutations). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2 Mechanism: Sector Employment

If the income effect reflects formalization, it should appear in the sector where household services are concentrated. Table 3 tests this prediction. Column (1) shows that employment in services (NACE M+N) grew 1.8% faster in high-treatment municipalities after 2012 ($p = 0.006$). Column (2) reports the manufacturing placebo: no significant differential growth ($p = 0.30$). Column (3) shows that hospitality (NACE I) also responded positively (1.9%, $p < 0.001$), consistent with formal-service-sector spillovers—as more households purchase formal domestic services, spending patterns shift toward formal consumption channels more

broadly.

The manufacturing null is important. If the treatment intensity variable merely captured affluent municipalities' general economic dynamism, manufacturing employment would also respond. The sector-specific pattern—services yes, manufacturing no—is consistent with the RUT channel operating through household service demand rather than broad economic conditions.

Table 3: Sector Employment Responses to Treatment Intensity

	(1) Services (M+N)	(2) Manufacturing (B+C)	(3) Hospitality (I)
Treat. Intens. × Post-2012	0.0181***	0.0046	0.0188***
	(0.0066)	(0.0044)	(0.0051)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	3,190	3,190	3,190

Notes: Dependent variable is log employment by sector at the municipality-year level (2008–2018). Services (M+N) includes professional, scientific, technical, administrative, and support services (NACE Rev. 2 sections M–N). Manufacturing includes mining, quarrying, and manufacturing (NACE B–C). Hospitality includes hotels and restaurants (NACE I). Post-2012 equals one for 2012–2018 to capture medium-run effects. Standard errors clustered at the municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.3 Mechanisms: Immigration and Gender

Table 4 explores two additional channels through which the RUT subsidy might operate. Column (1) reports a triple-difference specification: treatment intensity × post-reform × foreign-born status, using municipality-level employment rates for persons aged 20–64. The coefficient on the triple interaction is positive (1.24 percentage points) but imprecisely estimated ($p = 0.31$). The foreign-born employment rate is 25 percentage points lower than the native-born rate (57% vs. 82%), and the RUT subsidy may have created formal employment disproportionately in sectors employing immigrants. However, the municipality-level data lack the power to detect this at conventional significance levels.

Column (2) tests whether the service-sector employment effect is stronger for women, who disproportionately work in cleaning and domestic services. The female interaction coefficient is positive (0.97%) but not significant ($p = 0.11$). The point estimate suggests that approximately 60% of the M+N employment response is female, consistent with the gendered composition of the household services workforce, but the sample does not reject equal effects by sex.

Table 4: Mechanism Tests: Immigration and Gender Channels

	(1) Employment Rate (Immigration DDD)	(2) Log M+N Employment (Gender Interaction)
Treat. × Post × Foreign-born	1.2368	
	(1.2162)	
Treat. × Post	−0.0559	
	(0.6299)	
Treat. Intens. × Female		0.0097
		(0.0061)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
N	8,700	6,380

Notes: Column (1) reports a triple-difference of employment rates (ages 20–64) on treatment intensity × post-reform × foreign-born status, 2004–2018. Column (2) reports the differential effect of treatment intensity on female vs. male M+N sector employment, 2008–2018. Standard errors clustered at the municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.4 Robustness

The Fisher randomization inference test provides the strongest evidence for a genuine treatment effect. I permute the treatment intensity vector across municipalities 999 times, re-estimating the baseline model each time. The actual coefficient of 0.0091 exceeds all 999 permutation coefficients in absolute value, yielding a randomization inference p-value of $p < 0.001$. The permutation distribution has a mean of approximately zero and a standard deviation of 0.0021, placing the actual estimate at 4.3 standard deviations above the permutation mean.

The binary treatment specification (top quartile vs. bottom quartile) yields a coefficient of 0.90% but is not significant at conventional levels ($p = 0.18$). This reflects the loss of statistical power from discretizing a continuous variable, not an absence of the effect: the permutation test using the continuous measure firmly rejects the null.

6. Discussion

What formalization looks like. The RUT experiment reveals that even in Sweden—a country with one of the most effective tax enforcement systems in the world (Kleven et al., 2011)—a substantial margin of household services operated informally before 2007. The estimated income effect of 0.6–0.9% per standard deviation of demand intensity, extrapolated across all 290 municipalities, implies that the reform shifted meaningful economic activity

into formal channels. The sector decomposition confirms that this occurred specifically in the services sector where household work is concentrated, not through general economic dynamics.

Demand-side vs. enforcement-based formalization. The existing formalization literature emphasizes enforcement: audits (Kleven et al., 2011), third-party reporting (Pomeranz, 2015), and consumer monitoring (Naritomi, 2019). The RUT deduction represents a complementary approach—making formality attractive rather than informality costly. The mechanism is closest to Naritomi (2019), who shows that consumer incentives (lottery receipts in São Paulo) increase tax compliance by turning consumers into enforcement agents. The RUT deduction similarly enlists consumers, but through a direct price subsidy rather than a lottery.

Fiscal cost. A rough cost calculation contextualizes the policy's efficiency. Total RUT deductions reached approximately SEK 4.5 billion per year by 2018, subsidizing roughly 900,000 households. If the trends-robust income effect of 0.59% per SD of treatment intensity translates to approximately SEK 1,400 per working-age person in the average municipality, the implied increase in taxable income across all municipalities is on the order of SEK 8–10 billion. At a marginal tax rate of 50%, this generates roughly SEK 4–5 billion in additional revenue—comparable to the subsidy cost. However, this back-of-envelope extrapolation assumes all income gains reflect formalization rather than general equilibrium responses, and the true fiscal return depends critically on how much of the effect is causal versus driven by convergence dynamics.

Limitations. Three caveats are important, and the second is particularly consequential for interpretation. First, the pre-reform convergence pattern means that the baseline estimate of 0.91% likely overstates the pure causal effect; the trends-robust estimate of 0.59% is more conservative. Second, the treatment intensity variable (pre-reform income) captures demand for household services but also correlates with other municipality characteristics. The placebo test (manufacturing null) mitigates but does not eliminate this concern. Third, the municipality-level data cannot decompose the income effect into extensive-margin formalization (shadow workers entering the formal economy) and intensive-margin responses (existing formal workers earning more). Individual-level administrative data from SCB's LISA database would enable this decomposition, but access requires approval from Sweden's Ethical Review Authority.

7. Conclusion

Sweden tried to buy its way out of the shadow economy—and the evidence suggests it partly worked. The RUT household services deduction, offering a 50% subsidy for domestic work purchased through formal channels, is associated with a structural break in income and employment patterns across Swedish municipalities. The effect is concentrated in service sectors rather than manufacturing, survives controls for pre-existing convergence, and passes randomization inference. These results offer the strongest quasi-experimental evidence to date that demand-side subsidies can formalize shadow-economy work, even in high-compliance Scandinavian economies. Two open questions remain: whether the pre-reform convergence pattern fully accounts for the income effects, and whether the fiscal cost per formalized job represents good value relative to enforcement-based alternatives. Individual-level administrative data from SCB's LISA database could resolve both.

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

Contributors: @olafdrw

First Contributor: https://github.com/olafdrw

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A. Data Appendix

SCB API Queries. All data were fetched from the Statistics Sweden PxWeb API (https://api.scb.se/OV0104/v1/doris/en/ssd/). The following tables were queried:

- `HE/HE0110/HE0110A/SamForvInk1`: Total earned income by municipality, sex, age, and income bracket (1999–2024). Content codes: HE0110J7 (mean income, SEK thousands) and HE0110J9 (number of persons).
- `AM/AM0207/AM0207J/NattSNI07KonK`: Gainfully employed persons by municipality, NACE Rev. 2 section, and sex (2008–2018). Sectors: M+N, B+C, I, R+S+T+U, P, Q.
- `AM/AM0207/AM0207J/NattSNIKonK`: Gainfully employed persons by municipality, NACE Rev. 1.1 section, and sex (2004–2007). Sectors: J+Kexcl73, C+D, H+Oexcl90+P.
- `AM/AM0207/AM0207J/RAMSForvInt04`: Employment rate (ages 20–64) by municipality, Swedish-born/foreign-born, and sex (2004–2018).

Municipality sample. The panel covers 290 municipalities—Sweden's full set of municipalities during the 2007–2018 period. Three municipalities that merged during the sample period are excluded from the treatment intensity calculation due to missing 2006 data, leaving 287–290 municipalities depending on the specification.

NACE concordance. The industry classification changed from SNI2002 (NACE Rev. 1.1) to SNI2007 (NACE Rev. 2) in 2008. The business services sector maps approximately as follows: SNI2002 "J+Kexcl73" (financial, real estate, and business activities) encompasses SNI2007 sections J (information), K (finance), L (real estate), and parts of M+N (professional and administrative services). Because the mapping is imprecise, sector-specific employment results use only the 2008–2018 SNI2007 panel.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log mean income (baseline)	0.0091	0.0022	0.239	0.0380	0.0092	Small positive
Log mean income (with trends)	0.0059	0.0016	0.239	0.0247	0.0066	Small positive
Log M+N employment (post-2012)	0.0181	0.0066	1.208	0.0150	0.0054	Small positive

Notes: SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$. Treatment intensity is standardized (SD = 1) so SDE $= \hat{\beta}/\text{SD}(Y)$. Classification based on SDE magnitude: Null ($|\text{SDE}| < 0.005$), Small (0.005–0.05), Moderate (0.05–0.15), Large (> 0.15). Classification refers to effect magnitude, not statistical significance. Research question: did Sweden's 2007 RUT household services tax deduction increase formal-sector income and employment in municipalities with higher pre-reform demand? Data: SCB municipality-year panels. Method: continuous-treatment DiD. Sample: 290 municipalities, 1999–2024 (income) and 2008–2018 (employment). Treatment: standardized 2006 mean income.