Sticky Cantons: Fiscal Equalization and the Limits of Tiebout Migration in Switzerland

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Abstract

Switzerland's 2008 Neuer Finanzausgleich (NFA) replaced earmarked conditional federal transfers with unconditional block grants, redistributing CHF 4 billion annually across 26 cantons based on standardized tax potential. Tiebout theory predicts that such fiscal equalization should attract migrants to recipient cantons. I test this prediction using a continuous-treatment difference-in-differences design that exploits predetermined variation in NFA transfer intensity. Naive estimates suggest that a one-standard-deviation increase in transfer intensity raises net migration by 0.32 standard deviations ($p = 0.008$). However, event study diagnostics reveal significant pre-trends ($F = 12.04$, $p < 0.001$), and placebo tests at 2004 and 2006 produce coefficients of similar magnitude. Controlling for canton-specific trends eliminates the relationship entirely. The data cannot identify a causal migration response to fiscal equalization, and the trend-adjusted point estimate is economically negligible.

JEL Codes: H77, H73, R23

Keywords: fiscal equalization, Tiebout sorting, inter-cantonal migration, Switzerland, NFA

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1. Introduction

Every year, Switzerland's federal government redistributes roughly CHF 4 billion across its 26 cantons through the Finanzausgleich—the fiscal equalization system. The stated purpose is to ensure that every canton can provide a comparable level of public services without imposing punitive tax rates. But a long-standing theoretical prediction suggests that such redistribution should do more than equalize budgets: it should move people. If recipient cantons use transfer windfalls to improve services or lower taxes, the Tiebout hypothesis implies that mobile households will vote with their feet, migrating toward cantons offering better fiscal packages (Tiebout, 1956).

This prediction carries first-order policy stakes. If fiscal equalization induces population sorting, it creates a feedback loop: migrants raise recipient cantons' tax bases, potentially reducing their need for transfers, while depleting payer cantons further (Oates, 1972). Alternatively, if equalization fails to attract migrants, the fiscal federalism architecture is doing something quite different from what textbook models predict—and the massive sums involved may be achieving equity goals without the efficiency costs that migration-based theories anticipate.

I test this prediction using Switzerland's 2008 NFA reform, one of the cleanest natural experiments in fiscal federalism. On January 1, 2008, Switzerland replaced its 1959 system of earmarked, conditional federal-cantonal cost sharing with a formula-based system of unconditional block grants. The reform created sharp, predetermined variation in fiscal treatment: cantons with below-average tax potential became net recipients of the new Ressourcenausgleich, while cantons with above-average potential became net payers. Critically, the transfer formula was based on 2003–2005 tax potential assessments, making each canton's treatment intensity predetermined at the time of the reform (Dafflon, 2004; Frey, 2008).

The identification strategy is a continuous-treatment difference-in-differences design. I measure treatment intensity as each canton's distance from the equalization threshold (Resource Index = 100), interacted with a post-2008 indicator. This design exploits the fact that the NFA's redistribution formula generated large, exogenous variation in fiscal resources across cantons—from Appenzell Innerrhoden (index 55.3, the largest per-capita recipient) to Zug (index 147.2, the largest per-capita payer)—while holding constant each canton's pre-reform fiscal trajectory through two-way fixed effects. The primary outcome is the net inter-cantonal migration rate per 1,000 residents, measured from the BFS demographic balance for all 26 cantons over 2000–2023 (624 canton-year observations, 8 pre-reform and 16 post-reform years).

The headline finding is that the causal effect of fiscal equalization on migration is

unidentified in this setting. Naive two-way fixed effects estimates suggest a statistically significant positive relationship between transfer intensity and net migration ($\hat{\beta} = 0.049$, SE $= 0.017$, $p = 0.008$). A one-standard-deviation increase in transfer intensity appears to raise the net migration rate by 0.32 standard deviations. But three diagnostic tests demolish this result. First, an event study reveals that the pre-reform coefficients are jointly significant ($F = 12.04$, $p < 0.001$), with recipient cantons already experiencing improving net migration relative to payers well before 2008. Second, placebo tests that apply the same estimator to false reform dates of 2004 and 2006—using only pre-reform data—produce coefficients of similar magnitude and significance ($\hat{\beta}_{2004} = 0.046$, $p = 0.024$; $\hat{\beta}_{2006} = 0.052$, $p = 0.001$). Third, adding canton-specific linear trends absorbs the entire effect ($\hat{\beta}_{\text{trends}} = 0.012$, SE $= 0.029$, $p = 0.684$). What appeared to be a causal response to fiscal equalization is, in fact, the continuation of a convergent migration trend that predates the NFA by at least eight years.

This finding—that the causal effect cannot be separated from pre-existing trends—matters for three reasons. First, Switzerland is arguably the best laboratory for testing Tiebout sorting: a small, wealthy country with 26 cantons, four national languages, extremely high mobility, and sharply differentiated cantonal fiscal systems (Kirchgässner, 2002; Schmidheiny, 2006). If fiscal equalization fails to move people here, it is unlikely to do so in larger, more heterogeneous federations. Second, the NFA was a large shock—the redistribution amounted to roughly 2% of cantonal spending for the most affected cantons—yet produced no detectable migration response over 16 post-reform years. This speaks to the dominance of non-fiscal amenities (language, culture, labor markets, family ties) over fiscal considerations in the residential location choice (Bayer et al., 2007; Brülhart and Jametti, 2014). Third, the diagnostic strategy illustrates how easily a standard difference-in-differences can generate spurious positive results when pre-trends are present—a cautionary tale for the broader fiscal federalism empirical literature (Roth and Sant'Anna, 2023; Sun and Abraham, 2021).

The paper contributes to several literatures. Within the fiscal equalization literature, existing work on Switzerland's NFA is exclusively descriptive, limited to government evaluation reports (EFV Wirksamkeitsberichte) and OECD working papers (Eidgenössische Finanzverwaltung, 2014). I provide the first causal analysis of the NFA's migration effects using modern difference-in-differences methods. Within the Tiebout literature, the paper joins a growing body of work finding limited residential responses to local fiscal packages (Banzhaf and Walsh, 2008; Basten et al., 2017), and offers a mechanism: in a country where inter-cantonal migrants must cross language barriers and give up culturally embedded social networks, fiscal incentives are simply too small relative to attachment costs. Within the methodological literature on pre-testing, the paper provides a vivid empirical example of how

significant pre-trends, placebo failures, and trend sensitivity collectively invalidate a naive estimate that would otherwise pass standard statistical thresholds (Rambachan and Roth, 2023; Freyaldenhoven et al., 2019).

The remainder of the paper is organized as follows. Section 2 describes the NFA reform and its institutional context. Section 3 presents the data. Section 4 lays out the identification strategy. Section 5 reports the main results, event study diagnostics, and robustness checks. Section 6 discusses the implications. Standardized effect sizes for cross-study comparability appear in Appendix B.

2. Institutional Background

Switzerland's fiscal architecture is among the most decentralized in the world. Cantons set their own income, wealth, corporate, and inheritance taxes, with effective rates varying by a factor of three across cantons (Kirchgässner, 2002). Municipal governments add a further layer, applying their own tax multiplier (Steuerfuss) to the cantonal base rate. This variation creates the conditions for Tiebout competition: households and firms can, in principle, select their canton and municipality based on the tax-and-service bundle offered (Schmidheiny, 2006).

Before 2008, the federal government's fiscal equalization system operated through a patchwork of earmarked, conditional transfers. Under the 1959 Federal Finance Equalization Law, Bern channeled subsidies to cantons for specific purposes—roads, hospitals, education, social assistance—with matching requirements and spending mandates attached. This system was widely criticized for creating perverse incentives: cantons inflated project costs to attract larger federal subsidies, and the earmarks constrained cantonal fiscal autonomy without clear evidence that they improved outcomes (Dafflon, 2004).

The 2008 NFA reform. The Neuer Finanzausgleich (New Financial Equalization, SR 613.2) took effect on January 1, 2008, following a decade of legislative development and a federal referendum in 2004 that approved the constitutional amendments. The reform was comprehensive. It replaced the entire system of conditional transfers with three unconditional instruments. First, the Ressourcenausgleich (resource equalization) redistributes funds from cantons with above-average standardized tax potential (Resource Index > 100) to cantons below the threshold, through both horizontal (inter-cantonal) and vertical (federal) transfers. Second, the Lastenausgleich (burden equalization) provides additional transfers to cantons facing structural cost burdens—geographic topography (Geographisch-topographischer Lastenausgleich) or urban agglomeration pressures (Soziodemographischer Lastenausgleich). Third,

the reform separated cantonal from federal task responsibilities, ending the co-financing arrangements that had created the earmarking problem.

The key feature for identification is that the Ressourcenausgleich transfer formula was based on each canton's *standardized tax potential*—a measure of the revenue a canton would collect if it applied a uniform tax rate to its tax base, assessed using 2003–2005 fiscal data. This creates a predetermined, continuous treatment variable: cantons with low standardized tax potential (low Resource Index) receive the largest per-capita transfers, while cantons with high tax potential make the largest per-capita contributions. At the reform's introduction in 2008, the Resource Index ranged from 55.3 (Appenzell Innerrhoden, the weakest) to 147.2 (Zug, the strongest). The redistribution totaled approximately CHF 3.5–4 billion annually.

Seventeen of the 26 cantons were net recipients (Resource Index ≤ 90), including the large cantons of Bern, Luzern, and St. Gallen, as well as smaller alpine cantons like Uri, Obwalden, Glarus, and Valais. Four cantons were clear net payers (Resource Index ≥ 110): Zürich, Zug, Basel-Stadt, and Genève. Five cantons fell in a near-zero band (Resource Index 90–110), including Schwyz, Basel-Landschaft, Schaffhausen, Aargau, and Vaud. This classification is stable over time because the formula adjusts slowly through rolling three-year tax base assessments.

3. Data

I construct a balanced panel of 26 cantons observed annually from 2000 to 2023 (624 canton-year observations). The pre-reform period spans 2000–2007 (8 years) and the post-reform period spans 2008–2023 (16 years).

Inter-cantonal migration. The primary outcome is the net inter-cantonal migration rate per 1,000 permanent residents. Data come from the BFS (Federal Statistical Office) demographic balance database (px-x-0102020000_101), accessed via the PXWeb API. This dataset records in-migration from other cantons and out-migration to other cantons for all 26 cantons and every year since 1971. I compute the net migration rate as $(M_{ct}^{\text{in}} - M_{ct}^{\text{out}})/\text{Pop}_{ct} \times 1{,}000$, where Pop_{ct} is the permanent resident population on January 1 of year t.

NFA transfer intensity. Treatment intensity is measured as $100 - \text{Resource Index}_{2008}$, where the Resource Index is the EFV's standardized tax potential measure at the reform's introduction, based on 2003–2005 fiscal data. Positive values indicate net recipients (below-average tax potential); negative values indicate net payers. This variable is time-invariant by construction: I use the 2008 initial allocation to avoid endogenous adjustment of the formula in response to post-reform migration. The cross-sectional standard deviation of transfer

Table 1: Summary Statistics

Panel A: Full Sample (26 cantons × 24 years = 624 canton-years)

Variable	N	Mean	SD	Min	Max
Population	624	305459.7	319120.4	14946.0	1579967.0
In-migration (count)	624	5134.4	4809.0	329.0	25205.0
Out-migration (count)	624	5134.1	4703.2	270.0	27713.0
Net migration (count)	624	0.3	960.4	-5123.0	3788.0
Net migration rate (‰)	624	-0.4	3.4	-12.5	7.7
In-migration rate (‰)	624	20.1	7.4	5.2	40.2
Transfer intensity	624	11.8	22.5	-47.2	44.7

Panel B: Pre-Reform Balance (2000–2007)

NFA Group	Cantons	Mean pop.	In-mig. rate	Net mig. rate	Resource index
Recipient	17	218685	17.9	-1.0	75.9
Near-zero	5	332939	20.2	0.9	96.2
Payer	4	488126	19.6	-1.4	130.3

Notes: Migration rates are per 1,000 residents. Transfer intensity equals $100 - \text{Resource Index}_{2008}$; positive values indicate net recipients. Resource index based on 2003–2005 cantonal tax potential (EFV). Recipients: index ≤ 90; payers: index ≥ 110; near-zero: 90–110.

intensity across cantons is 22.5 index points.

Table 1 presents summary statistics. The average canton has approximately 305,000 residents, with substantial variation from Uri (15,000) to Zürich (1.6 million). The mean net migration rate is -0.4 per 1,000 residents, reflecting a slight tendency toward out-migration to other cantons (offset by international immigration). In the pre-reform period, recipient cantons had lower average populations (219,000) and slightly lower net migration rates (-1.0 per 1,000) compared to payer cantons (-1.4), with near-zero cantons having the highest rates ($+0.9$).

4. Empirical Strategy

4.1 Identification

I estimate the effect of NFA transfer intensity on inter-cantonal migration using a continuous-treatment difference-in-differences design:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot (\text{Intensity}_c \times \text{Post}_t) + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the net migration rate per 1,000 residents in canton c and year t, α_c and γ_t are canton and year fixed effects, $\text{Intensity}_c = 100 - \text{Resource Index}_{2008}$ is the predetermined treatment intensity, $\text{Post}_t = \mathbb{I}[t \geq 2008]$, and ε_{ct} is the error term.

The parameter β captures the differential change in net migration for cantons with higher transfer intensity after the NFA reform, relative to cantons with lower intensity. Under the Tiebout hypothesis, $\beta > 0$: recipient cantons (positive intensity) should attract in-migrants after receiving unconditional transfers.

The identifying assumption is that, absent the NFA reform, the evolution of net migration rates would have been parallel across cantons with different transfer intensities—conditional on canton and year fixed effects. This is the standard parallel trends assumption, adapted to a continuous treatment setting.

I also estimate an event study specification:

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{k \neq -1} \beta_k \cdot (\text{Intensity}_c \times \mathbb{I}[t - 2008 = k]) + \varepsilon_{ct} \tag{2}$$

where k indexes years relative to the reform and β_{-1} is normalized to zero. The pre-reform coefficients $\{\beta_k\}_{k<0}$ directly test the parallel trends assumption.

4.2 Inference

With only 26 cantons, standard cluster-robust standard errors may understate uncertainty. I address this in three ways. First, I cluster standard errors at the canton level throughout. Second, I conduct randomization inference by permuting transfer intensity across cantons 1,000 times, generating a non-parametric p-value. Third, I perform a leave-one-out analysis, dropping each canton in turn, to assess sensitivity to influential observations.

4.3 Threats to Validity

Pre-existing trends. The primary threat is that recipient and payer cantons were already on different migration trajectories before 2008. I test for this directly using the event study and placebo cutoff tests described below.

Global Financial Crisis. The NFA reform coincided with the 2008 Global Financial Crisis. However, the GFC affected all cantons simultaneously, and its effects are absorbed by year fixed effects. The identification comes from *cross-sectional* variation in transfer intensity, which is orthogonal to the macroeconomic shock. I also estimate a short-window specification (2000–2015) to limit exposure to longer-run confounds.

Small N. With 26 cantons, the design has limited statistical power. The randomization inference and leave-one-out analyses address concerns about finite-sample inference.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Column (1) presents the baseline specification from Equation (1): the coefficient on transfer intensity × Post is $\hat{\beta} = 0.049$ (SE = 0.017, $p = 0.008$). In economic terms, a one-standard-deviation increase in transfer intensity (22.5 index points) is associated with a $0.049 \times 22.5 = 1.1$ increase in the net migration rate per 1,000 residents per year, or about 0.32 standard deviations of the outcome. Taken at face value, this would suggest that cantons receiving larger NFA transfers experienced meaningful increases in net in-migration.

Column (2) adds canton-specific linear time trends. The coefficient drops to $\hat{\beta} = 0.012$ (SE = 0.029, $p = 0.684$)—statistically and economically indistinguishable from zero. This is the first indication that the baseline result reflects pre-existing differential trends rather than a causal response to the NFA.

Column (3) examines in-migration rates separately. The positive coefficient ($\hat{\beta} = 0.046$, $p < 0.001$) mirrors the baseline result, confirming that the naive relationship operates through the in-migration margin. Column (4) uses log population as the outcome; the coefficient is small and insignificant ($p = 0.14$), indicating no detectable effect on population stocks. Column (5) replaces continuous treatment with a binary recipient indicator; the coefficient is positive but imprecise ($p = 0.18$), reflecting the loss of statistical power from coarsening the treatment variable.

5.2 Event Study Diagnostics

Table 3 reports the event study coefficients from Equation (2). The pre-reform coefficients tell a clear story: they are uniformly negative and, in most years, individually significant. At $k = -8$ (2000), the coefficient is -0.109 (SE = 0.023, $p < 0.001$). The magnitude declines monotonically toward zero as the reference year ($k = -1$) approaches, tracing out a convergent pre-trend in which high-transfer-intensity cantons were steadily catching up to low-intensity cantons in net migration rates throughout the 2000s.

The joint test of pre-trend significance decisively rejects the parallel trends assumption: $F = 12.04$ ($p < 0.001$). This means that the parallel trends assumption underlying the baseline estimate is violated. The post-reform coefficients are a mixture of positive and

Table 2: NFA Transfer Intensity and Inter-Cantonal Migration

	Continuous Treatment				Binary
	Net mig. rate (1)	Net mig. rate (2)	In-mig. rate (3)	Log pop. (4)	Net mig. rate (5)
Transfer intensity × Post	0.0491***	0.0119	0.0460***	−0.0008	
	(0.0170)	(0.0289)	(0.0122)	(0.0005)	
Recipient × Post					1.3365
					(0.9779)
Canton FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Canton trends	No	Yes	No	No	No
Observations	624	624	624	624	624
Adj. R^2	0.617	0.696	0.956	0.999	0.600

Notes: Each column reports a two-way fixed effects regression. Transfer intensity equals 100 − Resource Index$_{2008}$, interacted with a post-2008 indicator. Migration rates are per 1,000 residents. Standard errors clustered by canton in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

negative values, none individually significant at conventional levels, and show no systematic pattern—consistent with the NFA having had no additional effect on migration beyond the pre-existing trend.

5.3 Robustness

Table 4 presents additional diagnostics. Panel A confirms that the baseline coefficient is robust to winsorization ($\hat{\beta} = 0.048$), population weighting ($\hat{\beta} = 0.066$), and restriction to the short post-period 2000–2015 ($\hat{\beta} = 0.041$). But these tests all share the same flaw: they do not address the pre-trend problem.

Panel B provides the definitive evidence. Placebo tests that apply the same estimator to false reform dates of 2004 and 2006—using *only pre-reform data*—produce significant coefficients of similar magnitude ($\hat{\beta}_{2004} = 0.046$, $p = 0.024$; $\hat{\beta}_{2006} = 0.052$, $p = 0.001$). These placebos confirm that the relationship between transfer intensity and migration trends existed well before the NFA reform and is not caused by it.

Panel C reports inference diagnostics. The randomization inference p-value is 0.015 (1,000 permutations), which might appear to support the causal interpretation. However, this test permutes transfer intensity across cantons while maintaining the time series structure—meaning it tests whether the *cross-sectional correlation* between transfer intensity and migration trends is anomalous, not whether the reform caused it. Given the pre-trends, this

correlation is real but pre-dates the reform. The leave-one-out range of $[0.043, 0.063]$ confirms that no single canton drives the result, but the finding is again one of a robust pre-existing correlation, not a causal effect.

6. Discussion

The null result has three implications. First, for the theory of fiscal federalism, the finding suggests that Tiebout sorting—at least the migration margin—is weak in response to inter-cantonal fiscal equalization, even in a country with exceptionally high internal mobility, compact geography, and large fiscal variation (Tiebout, 1956; Oates, 1972). Switzerland's cantons are separated by language, culture, and deeply localized labor markets; these non-fiscal amenities appear to dominate the residential location decision (Brülhart and Jametti, 2014). The NFA's redistribution of CHF 4 billion annually—roughly 2% of affected cantonal budgets—is simply too small relative to the full bundle of place-based amenities to generate detectable sorting.

Second, for the design of equalization systems, the null is reassuring. A common concern in the fiscal federalism literature is that equalization creates a "migration externality": transfers attract residents to recipient regions, eroding the tax bases of payer regions and amplifying the demand for redistribution (Boadway and Shah, 2004). The absence of a migration response in Switzerland suggests that this feedback loop is not operative—at least at the magnitude of redistribution observed under the NFA. Policymakers designing equalization systems in other federations can draw cautious comfort from this finding.

Third, for empirical methodology, the paper illustrates the importance of pre-trend diagnostics in fiscal federalism research. The naive estimate of $\hat{\beta} = 0.049$ ($p = 0.008$) would have survived standard robustness tests (leave-one-out, winsorization, alternative treatment definitions). Only the event study, placebo tests, and trend sensitivity jointly revealed the pre-trend problem. This echoes the broader methodological literature on the dangers of relying on statistical significance without examining the dynamic structure of the treatment effect (Roth and Sant'Anna, 2023; Rambachan and Roth, 2023; Sun and Abraham, 2021).

Scope and limitations. The original research design also proposed analyzing cantonal expenditure composition and tax multipliers to test the flypaper effect. The BFS PXWeb system did not make functional expenditure data available through its API during the data collection window, and the simultaneous 2008 accounting standard change would have complicated any such analysis. Future work with direct access to EFV financial statistics could address these margins.

The small number of clusters (26) limits statistical power. Under the trend-adjusted specification ($\hat{\beta} = 0.012$, SE $= 0.029$), the minimum detectable effect (MDE) at 80% power is approximately $0.029 \times 2.8 \approx 0.08$ per unit of transfer intensity, or roughly 1.8 per 1,000 residents for a one-standard-deviation intensity change. This is large relative to the baseline migration rate, meaning the design has limited power to detect small but policy-relevant sorting responses. The correct interpretation is therefore not "the NFA had no effect" but rather "the data cannot distinguish any NFA effect from the pre-existing convergent trend, and the trend-adjusted estimate is too imprecise to rule out modest effects."

The pre-existing convergent trend itself is an interesting object: why were recipient cantons already gaining relative in-migration throughout the 2000s? This may reflect broader patterns of de-urbanization, tourism-sector growth in alpine cantons, labor market shifts, or the effects of earlier federal policies that anticipated the NFA. The treatment intensity measure also omits the Lastenausgleich (burden equalization) component of the NFA, which provided additional transfers for geographic and sociodemographic costs; incorporating this component could refine the treatment definition in future work.

7. Conclusion

Switzerland's largest fiscal reform in half a century moved money across cantons but cannot be shown to have moved people. The NFA's redistribution of CHF 4 billion annually left no detectable imprint on inter-cantonal migration once pre-existing convergent trends are accounted for—though the design lacks the precision to rule out modest effects. The finding underscores both the dominance of non-fiscal amenities in the residential location decision and the danger of interpreting standard difference-in-differences estimates at face value in settings with even mild pre-trends. For policymakers, the reassurance is cautious: the feared migration externality of fiscal equalization does not appear to be first-order in Switzerland, but the evidence base for this conclusion remains limited by the small number of cantonal units and the identification challenge posed by pre-reform trend convergence.

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A. Data Appendix

BFS PXWeb API. Inter-cantonal migration data were obtained from the Swiss Federal Statistical Office (BFS) via the PXWeb API. The specific dataset is px-x-0102020000_101 ("Demographic balance by canton"), which provides annual demographic components—including inter-cantonal in-migration (component 5) and out-migration (component 7)—for all 26 cantons, total citizenship, total sex, from 1971 to 2023. Data were accessed on April 1, 2026.

Population data (permanent residents as of January 1) come from the same dataset (component 0). The analysis panel uses years 2000–2023, yielding 26 cantons $\times$ 24 years $= 624$ canton-year observations.

NFA transfer intensity. The Resource Index at the NFA's introduction (2008) was compiled from the EFV (Federal Finance Administration) Wirksamkeitsbericht 2008–2011, which reports each canton's standardized tax potential relative to the national average (index $= 100$). The 2008 index values are based on 2003–2005 fiscal data and are thus predetermined with respect to post-reform outcomes. Transfer intensity is defined as $100 - \text{Resource Index}_{2008}$, with positive values for net recipients.

Variable definitions.

- **Net migration rate**: $(M_{ct}^{\text{in}} - M_{ct}^{\text{out}})/\text{Pop}_{ct} \times 1{,}000$, where M_{ct}^{in} and M_{ct}^{out} are inter-cantonal in- and out-migration counts and Pop_{ct} is the permanent resident population on January 1.
- **In-migration rate**: $M_{ct}^{\text{in}}/\text{Pop}_{ct} \times 1{,}000$.
- **Transfer intensity**: $100 - \text{Resource Index}_{2008}$. Range: -47.2 (Zug, strongest payer) to $+44.7$ (Appenzell I.Rh., largest recipient). SD $= 22.5$.
- **NFA status**: Recipient (Resource Index ≤ 90, 17 cantons), payer (Resource Index ≥ 110, 4 cantons), near-zero (90–110, 5 cantons).

B. Standardized Effect Sizes

Table 3: Event Study Coefficients: NFA Transfer Intensity and Net Migration Rate

Year relative to NFA	Coefficient	SE	p-value
$k = -8$	-0.1090^{***}	0.0233	0.000
$k = -7$	-0.0770^{**}	0.0337	0.031
$k = -6$	-0.0647^{***}	0.0218	0.007
$k = -5$	-0.0472^{***}	0.0159	0.007
$k = -4$	-0.0612^{**}	0.0274	0.035
$k = -3$	-0.0295	0.0183	0.120
$k = -2$	-0.0249	0.0166	0.146
$k = +0$	0.0077	0.0364	0.835
$k = +1$	0.0457	0.0323	0.169
$k = +2$	-0.0135	0.0361	0.712
$k = +3$	-0.0158	0.0282	0.581
$k = +4$	-0.0360	0.0412	0.391
$k = +5$	-0.0106	0.0317	0.740
$k = +6$	-0.0284	0.0308	0.366
$k = +7$	-0.0389	0.0256	0.141
$k = +8$	-0.0222	0.0248	0.381
$k = +9$	-0.0266	0.0292	0.372
$k = +10$	-0.0187	0.0309	0.550
$k = +11$	-0.0119	0.0376	0.755
$k = +12$	0.0166	0.0200	0.413
$k = +13$	0.0559	0.0343	0.116
$k = +14$	0.0322	0.0229	0.173
$k = +15$	0.0227	0.0298	0.454
Joint pre-trend F-test	$F = 12.04$, $p = 0.000$		

Notes: Each coefficient is from a regression of net migration rate (per 1,000) on interactions of transfer intensity with event-time indicators, with canton and year fixed effects. Reference period: $k = -1$ (2007). Standard errors clustered by canton. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Robustness Checks

Specification	Coefficient	SE	p-value	N
Panel A: Main and Alternative Specifications				
Baseline (Table 2, col. 1)	0.0491^{***}	0.0170	0.008	624
Winsorized (1%–99%)	0.0484^{***}	0.0167	0.008	624
Population-weighted	0.0661^{***}	0.0121	0.000	624
Short post-period (2008–2015)	0.0405^{**}	0.0177	0.031	416
Panel B: Placebo Tests				
Placebo cutoff: 2004	0.0456^{**}	0.0190	0.024	208
Placebo cutoff: 2006	0.0523^{***}	0.0139	0.001	208
Panel C: Inference				
Randomization inference p-value	0.015 (1,000 permutations)			
Leave-one-out range	[0.0427, 0.0630]			

Notes: All specifications include canton and year fixed effects with standard errors clustered by canton. Placebo tests use pre-reform data only (2000–2007). Randomization inference permutes transfer intensity across cantons 1,000 times. Leave-one-out reports the range of coefficients when each canton is dropped in turn. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Net migration rate	0.0491	0.0170	3.43	0.322	0.111	Large positive
In-migration rate	0.0460	0.0122	6.86	0.151	0.040	Large positive
Log population	−0.000765	0.000498	1.1162	−0.015	0.010	Small negative
Panel B: Heterogeneous (Language Region)						
German-speaking cantons	0.0452	0.0226	3.57	0.284	0.142	Large positive
French-speaking cantons	0.0669	0.0056	2.92	0.515	0.043	Large positive

Notes: **Country:** Switzerland. **Research question:** Does replacing conditional earmarked federal transfers with unconditional block grants alter inter-cantonal population sorting? **Policy mechanism:** The 2008 NFA reform abolished the 1959 system of earmarked federal-cantonal cost sharing and replaced it with formula-based unconditional equalization transfers (Ressourcenausgleich and Lastenausgleich), redistributing CHF 3.5–4 billion annually based on standardized cantonal tax potential. **Outcome definition:** Net inter-cantonal migration rate per 1,000 permanent residents, measuring the balance of in-flows from and out-flows to other Swiss cantons within the same calendar year. **Treatment:** Continuous; transfer intensity defined as $100 - \text{Resource Index}_{2008}$, measuring each canton's distance from the equalization threshold based on predetermined 2003–2005 tax potential. **Data:** BFS PXWeb demographic balance (px-x-0102020000_101), 26 cantons, 2000–2023, 624 canton-year observations. **Method:** Two-way fixed effects (canton + year) with continuous treatment intensity interacted with post-2008 indicator; standard errors clustered by canton; robustness via randomization inference (1,000 permutations) and leave-one-out. **Sample:** All 26 Swiss cantons; no exclusions. $\text{SDE} = \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation and SD(X) is the cross-sectional standard deviation of transfer intensity. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).