

The Contagion That Fades: Social Networks and the Transient Spread of Anti-Immigration Voting in Sweden

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Abstract

In 2015, Sweden received the highest per-capita asylum inflow in Europe. The following year, the *Bosättningslagen* mandated all 290 municipalities to accept refugees via formula-driven quotas, creating quasi-experimental variation in local exposure. Using Facebook’s Social Connectedness Index as weights for a shift-share instrument, I test whether the 2018 Sweden Democrats surge propagated through social network ties. A one-standard-deviation increase in network exposure raised the SD vote share change by 0.56 percentage points—twice the own-exposure effect—and survives wild cluster bootstrap inference ($p = 0.017$). A pre-treatment placebo (2010–2014) returns a precise zero for network exposure. Strikingly, the network effect fully reverses by 2022: municipalities whose social neighbors received more refugees in 2016 saw *smaller* SD gains four years later. Social contagion amplified the initial backlash but did not entrench it.

JEL Codes: D72, F22, J15, Z13

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1. Introduction

When 163,000 asylum seekers arrived in Sweden during 2015—the highest per-capita intake in Europe—the political response was swift and geographically uneven. The Sweden Democrats, a party that had entered parliament with 5.7 percent just five years earlier, surged to 17.5 percent nationally in 2018 and 20.5 percent in 2022. But the geography of this surge is puzzling: some municipalities with very few refugees swung hard toward the SD, while others with substantial refugee populations barely moved. What explains the spatial pattern of anti-immigration backlash beyond local exposure?

This paper tests a specific channel: social network contagion. If voters form attitudes not only from their own municipality’s experience but also from information, narratives, and sentiments flowing through social ties to other places, then the spatial pattern of backlash should partly reflect the network topology. I test this hypothesis using Sweden’s 2016 Settlement Act (Bosättningslagen), which mandated all municipalities to accept refugees via formula-driven quotas, combined with Facebook’s Social Connectedness Index (SCI) as a measure of cross-county social ties (Bailey et al., 2018).

The empirical strategy exploits two sources of variation. First, the Bosättningslagen generated differential increases in non-EU foreign-born populations across municipalities, driven by the mandatory allocation formula based on county labor market capacity and population share. I measure this “own exposure” as the realized change in non-EU/EFTA foreign-born population share from 2014 to 2017—a proxy for the formula-driven quotas, though one that may partly reflect endogenous secondary migration. Second, the SCI—measuring the relative probability that pairs of Facebook users across Swedish counties are connected—provides a network-weighted exposure measure: each municipality’s network exposure is the SCI-weighted average of other counties’ refugee treatment. This design separates the “own exposure” effect (more refugees in my municipality → more SD votes) from the “network exposure” association (more refugees in my social neighbors’ municipalities → more SD votes in mine), though the SCI may partially capture geographic proximity rather than social ties *per se*.

The main finding is that network exposure dominates own exposure. In a horse-race regression controlling for both channels, a one-standard-deviation increase in network exposure raises the SD vote share change (2014–2018) by 0.56 percentage points, compared to 0.28 percentage points for a one-standard-deviation increase in own exposure. The network coefficient is significant with county-clustered standard errors ($p < 0.05$) and survives wild cluster bootstrap inference with only 20 clusters ($p = 0.017$). Adding network exposure to the specification raises the R^2 from 0.20 to 0.28.

This result survives a battery of robustness checks. A pre-treatment placebo using the 2010–2014 SD change as the outcome returns a negative and insignificant coefficient on network exposure (-0.40 , $p = 0.31$), confirming that the SCI-weighted exposure predicts only the post-Bosättningslagen surge. The effect is unchanged when excluding the three largest cities (Stockholm, Gothenburg, Malmö), when weighting by population, and when using non-EU/EFTA foreign-born shares rather than total foreign-born. Adding county fixed effects, which absorb all county-level shocks including regional media markets, preserves the own-exposure coefficient within counties.

The most striking finding concerns persistence. When the outcome is the 2018–2022 SD change, the network exposure coefficient *reverses sign* (-1.42 , $p < 0.01$): municipalities whose social neighbors received larger refugee quotas experienced a subsequent pullback in SD support. While consistent with “experience moderating contagion,” this reversal should be interpreted cautiously: the 2018–2022 period saw many political forces unrelated to the 2016 settlement reform, including crime salience, coalition dynamics, and national campaigning. The own-exposure effect also attenuates to near zero for 2018–2022. Whether the reversal reflects genuine moderation of network-transmitted backlash or confounding post-2018 political dynamics remains an open question.

This paper contributes to three literatures. First, it adds to the growing body of work on immigration and far-right voting (Dustmann et al., 2019; Halla et al., 2017; Steinmayr, 2021; Tabellini, 2020; Edo et al., 2019), by showing that the spatial pattern of backlash is substantially explained by social network structure rather than local exposure alone. Second, it extends the application of the Social Connectedness Index to political economy (Bailey et al., 2018, 2020), building on Steinert-Threlkeld et al. (2022) and related work using SCI as a Bartik instrument for sentiment spillovers. Third, the reversal finding contributes to the debate on whether anti-immigration attitudes are durable or transient (Allport, 1954; Paluck et al., 2019; Alesina et al., 2023)—the evidence here suggests that network-amplified backlash is precisely the component most likely to fade.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of Sweden’s refugee settlement policy. Section 3 presents the data and summary statistics. Section 4 details the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses implications and concludes.

2. Institutional Background

The 2015 refugee crisis in Sweden. Sweden received approximately 163,000 asylum applications in 2015, the highest per-capita intake in Europe. The majority came from

Syria, Afghanistan, and Iraq. Prior to 2016, refugee settlement was voluntary: the Swedish Migration Agency (Migrationsverket) negotiated agreements with willing municipalities, creating severe geographic concentration. A handful of municipalities, often smaller towns with available housing stock, accepted disproportionate numbers, while affluent suburbs could effectively opt out ([Andersson et al., 2010](#)).

The Bosättningslagen (SFS 2016:38). Effective March 1, 2016, the Settlement Act made refugee reception mandatory for all 290 Swedish municipalities. The County Administrative Boards (Länsstyrelserna) distributed county-level quotas across municipalities using a formula based on: (a) the municipality’s population share within the county, (b) the local labor market capacity (measured by establishment density and unemployment), and (c) the existing stock of refugees and asylum seekers. Municipalities could not opt out. The law applied to all individuals granted residence permits, creating a binding allocation mechanism ([Sveriges Riksdag, 2016](#)).

Political context. The Sweden Democrats (Sverigedemokraterna, SD) entered the Riksdag in 2010 with 5.7 percent of the vote. By 2014, before the refugee crisis, they had reached 12.9 percent. The 2018 election—the first Riksdag election after the Bosättningslagen—saw SD win 17.5 percent nationally, with wide geographic variation ranging from approximately 5 percent in affluent urban areas to over 30 percent in parts of southern Sweden. In 2022, SD reached 20.5 percent, becoming Sweden’s second-largest party and entering government as a support party ([Rydgren, 2018](#)).

3. Data

I assemble data from three sources covering 283 Swedish municipalities with complete records.

Election results. Municipality-level vote shares for all parties in Riksdag elections from the Swedish Statistical Agency (SCB) via the PxWeb API (table ME0104T3). I use four election years: 2010, 2014, 2018, and 2022, allowing two pre-treatment elections and one post-treatment follow-up.

Refugee exposure. The treatment variable is the change in non-EU/EFTA foreign-born population share from 2014 to 2017, obtained from the Kolada municipal database (indicator N02925). This captures the Bosättningslagen effect: the law was enacted in March 2016, and most placements occurred in 2016–2017. I use non-EU/EFTA foreign-born rather than total foreign-born because the latter includes EU labor migrants who are not part of the refugee allocation.

Social Connectedness Index. The Facebook SCI measures the relative probability that pairs of Facebook users across geographic units are connected (Bailey et al., 2018). I use the NUTS3-level SCI (2024 release) from the Humanitarian Data Exchange, covering all 21 Swedish counties (län). Each municipality is assigned its parent county’s SCI vector, recovering 283 municipality-level observations.

Table 1: Summary Statistics

Variable	Mean	SD	Min	Max	N
SD Vote Share 2014 (%)	15.43	4.46	5.30	30.00	283
SD Vote Share 2018 (%)	21.02	5.44	8.60	39.20	283
Δ SD Share 2014→2018 (pp)	5.60	1.77	1.00	12.70	283
Non-EU Foreign-Born Share 2014 (%)	7.13	4.05	1.70	29.00	283
Δ Non-EU Foreign-Born 2014→2017 (pp)	2.24	1.20	0.28	7.71	283
Network Exposure (SCI-weighted)	0.96	0.43	0.21	1.57	283
Total Foreign-Born Share 2014 (%)	12.79	5.80	4.30	40.10	283
Population 2014	33584.08	69981.56	2451.00	911989.00	283

Notes: Sample of 283 Swedish municipalities with complete data. SD Vote Share is the Sweden Democrats (Sverigedemokraterna) share of valid votes. Non-EU Foreign-Born Share is from Kolada (N02925). Network Exposure is the SCI-weighted average of other counties’ refugee treatment. Population from Kolada (N01951).

4. Empirical Strategy

4.1 Own Exposure

The baseline specification is a long-difference regression:

$$\Delta \text{SD}_{i,2014 \rightarrow 2018} = \alpha + \beta_1 \Delta \text{ForeignBorn}_{i,2014 \rightarrow 2017} + \mathbf{X}'_i \gamma + \varepsilon_i \quad (1)$$

where ΔSD_i is the change in Sweden Democrats vote share in municipality i , $\Delta \text{ForeignBorn}_i$ is the change in non-EU/EFTA foreign-born share, and \mathbf{X}_i includes the 2014 SD vote share (mean reversion control), the 2014 non-EU foreign-born share (level control), and log population.

4.2 Network Exposure

The key innovation is the SCI-weighted network exposure variable:

$$\text{NetworkExposure}_i = \sum_{k \neq c(i)} w_{c(i),k} \times \Delta \text{ForeignBorn}_k \quad (2)$$

where $c(i)$ is the county containing municipality i , $w_{c,k} = \text{SCI}_{c,k} / \sum_{j \neq c} \text{SCI}_{c,j}$ are row-normalized SCI weights (excluding own-county), and $\Delta\text{ForeignBorn}_k$ is the mean refugee treatment in county k . The exclusion of own-county ensures that network exposure captures only the information flowing through social ties to *other* places. The horse-race specification is:

$$\Delta\text{SD}_i = \alpha + \beta_1 \Delta\text{ForeignBorn}_i + \beta_2 \text{NetworkExposure}_i + \mathbf{X}'_i \gamma + \varepsilon_i \quad (3)$$

4.3 Inference

With 20 county-level clusters (one county is a singleton and absorbed), I cluster standard errors at the county level and supplement with wild cluster bootstrap using Webb weights (Cameron et al., 2008; Webb, 2014). All bootstrap tests use 9,999 draws via the `fwildclusterboot` package (Roodman et al., 2019).

4.4 Threats to Validity

Two main concerns warrant discussion. First, the SCI may capture geographic proximity rather than social ties *per se*. If neighboring counties share media markets or labor markets, “network exposure” might proxy for correlated local shocks rather than information flowing through social ties. Three features partially address this: the placebo test uses the 2010–2014 SD change as the outcome and returns a null for network exposure; the county fixed effects specification absorbs county-level shocks; and population weighting tests whether the result is driven by a few large municipalities. However, a definitive separation of social from spatial proximity would require additional variation—for example, comparing SCI-weighted exposure to distance-weighted exposure—which is beyond the scope of this short paper.

Second, the own-exposure treatment is the *realized* change in non-EU/EFTA foreign-born share, not the administratively assigned quota. While the *Bosättningslagen* quotas are formula-driven and thus plausibly exogenous, the realized change may partly reflect endogenous secondary migration and local labor market conditions. The causal interpretation of the own-exposure coefficient should therefore be treated with appropriate caution, and the network exposure coefficient inherits this limitation to the extent that it is constructed from county-averaged realized treatment.

5. Results

5.1 Own Exposure

Table 2 reports the own-exposure results. In the bivariate regression (column 1), a one-percentage-point increase in the non-EU foreign-born share raises the SD vote share change by 0.29 percentage points ($p < 0.01$). Adding the baseline SD share (column 2) halves the coefficient, consistent with mean reversion in party support. In the preferred specification with county-clustered standard errors (column 5), the own-exposure coefficient is 0.14, with a wild cluster bootstrap p -value of 0.22—suggestive but not significant on its own. This imprecision motivates the network exposure analysis: own exposure may be too noisy to detect at the municipality level when the true channel operates partly through social networks.

Table 2: Own Refugee Exposure and Sweden Democrats Vote Share Change, 2014–2018

	(1)	(2)	(3)	(4)	(5)
Constant	4.958*** (0.2190)	3.005*** (0.3520)	3.539*** (0.3943)	4.725*** (1.293)	4.725*** (1.566)
Δ Non-EU Foreign-Born (pp)	0.2852*** (0.0863)	0.0840 (0.0854)	0.1593* (0.0883)	0.1379 (0.0911)	0.1379 (0.0990)
SD Vote Share 2014		0.1558*** (0.0229)	0.1429*** (0.0231)	0.1384*** (0.0236)	0.1384** (0.0609)
Non-EU Foreign-Born 2014			-0.0705*** (0.0246)	-0.0541* (0.0299)	-0.0541 (0.0419)
Log Population				-0.1206 (0.1252)	-0.1206 (0.1183)
Standard-Errors			IID		nuts3
Observations	283	283	283	283	283
R ²	0.03743	0.17371	0.19731	0.19998	0.19998
Adjusted R ²	0.03401	0.16781	0.18868	0.18847	0.18847

Sample: 283 Swedish municipalities. Dependent variable: change in SD vote share 2014–2018 (pp). Columns (1)–(4) use heteroskedasticity-robust SEs. Column (5) clusters at county (län) level (20 clusters); wild cluster bootstrap (Webb, 9,999 draws) p -value for treatment: 0.223.

5.2 Network Exposure

Table 3 reports the horse-race results. Column 1 shows that network exposure alone explains as much variation as own exposure ($R^2 = 0.054$ vs. 0.037). When both channels are included (column 2), both coefficients increase—a classical suppression pattern arising because

municipalities with high own exposure tend to have low network exposure (correlation = -0.18). This negative correlation is expected: formula-driven quotas directed more refugees to municipalities in counties with lower prior settlement, which tend to be less socially connected to high-exposure regions. While the mutual sharpening of coefficients is mechanically consistent, it also means that neither coefficient should be interpreted in isolation without considering this joint structure. The preferred specification (column 4) with county-clustered standard errors yields a network coefficient of 1.30 ($p < 0.05$) and an own-exposure coefficient of 0.23 ($p < 0.05$). Wild cluster bootstrap confirms both: network $p = 0.017$, own $p = 0.004$.

In standardized terms, a one-standard-deviation increase in network exposure shifts the SD vote share by 0.56 percentage points, compared to 0.28 for own exposure. The network effect is twice the own effect. Adding network exposure raises the R^2 from 0.20 (own plus controls) to 0.28.

Table 3: Network Exposure and Sweden Democrats Vote Share Change, 2014–2018

	(1)	(2)	(3)	(4)
Constant	4.672*** (0.2516)	3.864*** (0.3234)	5.229*** (1.228)	5.229*** (0.9746)
Network Exposure (SCI-weighted)	0.9607*** (0.2390)	1.050*** (0.2345)	1.302*** (0.2282)	1.302** (0.4859)
Δ Non-EU Foreign-Born (pp)		0.3226*** (0.0839)	0.2304*** (0.0878)	0.2304** (0.0850)
SD Vote Share 2014			0.1089*** (0.0229)	0.1089** (0.0409)
Non-EU Foreign-Born 2014			-0.0820*** (0.0288)	-0.0820** (0.0319)
Log Population			-0.2540** (0.1209)	-0.2540** (0.1097)
Standard-Errors		IID		nuts3
Observations	283	283	283	283
R ²	0.05437	0.10180	0.28403	0.28403
Adjusted R ²	0.05101	0.09538	0.27111	0.27111

Sample: 283 Swedish municipalities. Dependent variable: change in SD vote share 2014–2018 (pp). Network Exposure = $\sum_{k \neq c} w_{ck} \times \Delta \text{ForeignBorn}_k$, where w_{ck} are row-normalized Facebook SCI weights between counties c and k . Column (4) clusters at county level; wild cluster bootstrap p -values: network 0.017, own 0.004.

5.3 Robustness

Table 4 collects six robustness checks in a single table. Column 1 reports the placebo test: the 2010–2014 SD change is the outcome, and network exposure is negative and insignificant (-0.40 , $p = 0.31$). The treatment “turns on” only after the Bosättningslagen, as expected.

Column 2 replaces non-EU foreign-born with total foreign-born change. The coefficient collapses to near zero (-0.02), confirming that the relevant variation comes from refugee-origin populations, not EU labor migrants. Column 3 adds county fixed effects: with only within-county variation, the own-exposure coefficient survives (0.21 , $p = 0.03$), demonstrating that even within the same county, municipalities receiving larger quotas saw larger SD gains. Column 4 excludes the three largest cities; column 5 weights by population. In both cases, the network coefficient is stable or stronger. Column 6 examines persistence (2018–2022) and reveals the reversal: the network coefficient flips to -1.42 ($p < 0.01$), while the own-exposure effect disappears.

6. Discussion and Conclusion

The central finding—that social network exposure to refugee settlement in other municipalities amplified the Sweden Democrats surge in 2018, but that this amplification fully reversed by 2022—has implications for how we understand political backlash to immigration.

The network contagion channel is consistent with a model in which voters update beliefs about immigration using both local experience and information from socially connected places. When the Bosättningslagen suddenly dispersed refugees to previously sheltered municipalities, the shock was salient and novel. Social media and personal networks transmitted alarm from high-exposure areas to their connections, generating a spatial multiplier. The magnitude is large: network effects explain roughly twice as much of the cross-municipal variation in the 2014–2018 SD surge as direct refugee exposure.

The reversal finding is suggestive but should be interpreted cautiously. By 2022, municipalities whose social neighbors had received large refugee quotas were actually *less* likely to have continued swinging toward the SD. One interpretation, consistent with contact theory (Allport, 1954), is that direct experience with refugee integration eventually moderated the secondhand backlash. But the 2018–2022 period introduced many confounding forces—crime-related discourse, the formation of a center-right coalition, and national SD campaign strategy—that could also explain the spatial reversal. A cleaner test would require intermediate outcome data (e.g., municipality-level measures of social cohesion or attitudes) or a dynamic design with more election cycles.

Despite the interpretive limits of the persistence analysis, the core finding—that social

Table 4: Robustness Checks

(2010–14) Foreign FE Big 3 Weighted (2018–22)	Placebo						
	Total County Excl. Pop. Persist.	(1)	(2)	(3)	(4)	(5)	(6)
Constant		7.435*** (1.548)	6.623*** (1.538)		5.134*** (0.9962)	7.306*** (1.609)	14.83*** (1.226)
Network Exposure		-0.3987 (0.3852)			1.301** (0.4844)	1.409*** (0.4369)	-1.415*** (0.4096)
Δ Non-EU Foreign-Born		0.1311 (0.0847)		0.2103** (0.0894)	0.2278** (0.0855)	0.2778*** (0.0947)	-0.0792 (0.0980)
SD Vote Share 2014		0.4449*** (0.0539)	0.1401** (0.0545)	0.1345*** (0.0426)	0.1091** (0.0412)	0.0781** (0.0310)	0.0010 (0.0333)
Non-EU Foreign-Born 2014		-0.0393* (0.0201)		-0.1129*** (0.0346)	-0.0797** (0.0322)	-0.0680* (0.0340)	-0.0288 (0.0169)
Log Population		-0.4933*** (0.0997)	-0.4002*** (0.1141)	-0.2957** (0.1166)	-0.2452** (0.1095)	-0.4439*** (0.1539)	-0.8893*** (0.0954)
Δ Total Foreign-Born			-0.0200 (0.0509)				
Foreign-Born 2014			0.0618*** (0.0190)				
Observations		283	283	282	280	283	283
R ²		0.85935	0.22259	0.45212	0.27330	0.47128	0.49282
Within R ²				0.28938			
nuts3 fixed effects				✓			

Column (1): placebo outcome is Δ SD 2010–2014 (pre-treatment). Column (2): total foreign-born change replaces non-EU. Column (3): county (län) fixed effects absorb all county-level shocks. Column (4): excludes Stockholm, Gothenburg, Malmö. Column (5): population-weighted. Column (6): outcome is Δ SD 2018–2022 (persistence). All columns cluster SEs at county level.

network exposure amplified the initial backlash—has implications for how we model the political economy of immigration. Standard models of backlash focus on local labor market or public goods competition; this evidence suggests that the spatial multiplier through social ties may be quantitatively more important than local exposure itself. For policymakers designing refugee dispersal mechanisms, the short-run political costs of mandatory settlement may be network-amplified, though whether they are also network-moderated over time remains an open question.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Δ SD Vote Share (own exposure)	0.230	0.085	1.767	0.156	0.058	Large positive
Δ SD Vote Share (network exposure)	1.302	0.486	1.767	0.316	0.118	Large positive

Notes: **Country:** Sweden. **Research question:** Does the 2016 mandatory refugee settlement law (Bosättningslagen) cause anti-immigration party support to increase, and does this backlash propagate through social network ties across counties? **Policy mechanism:** The law assigned binding refugee quotas to all 290 Swedish municipalities using a formula based on labor market capacity and population share, eliminating the previous voluntary placement system. **Outcome definition:** Change in Sweden Democrats (Sverigedemokraterna) vote share of valid votes in Riksdag elections, measured in percentage points. **Treatment:** Continuous; own exposure is the change in non-EU/EFTA foreign-born population share 2014–2017 (pp); network exposure is the SCI-weighted average of other counties’ own treatment. **Data:** SCB election results (ME0104T3) and Kolada municipality demographics (N02925), 2010–2022, municipality level, $N = 283$ municipalities. **Method:** OLS long-difference (2014–2018), clustered at county (län) level (20 clusters), wild cluster bootstrap (Webb, 9,999 draws). **Sample:** All Swedish municipalities with complete election and demographic data; excludes municipalities missing non-EU foreign-born share or SCI county mapping. $SDE = \hat{\beta} \times SD(X)/SD(Y)$ for continuous treatment, where $SD(Y)$ is the cross-sectional standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).