# Who Keeps House? The 1924 Immigration Act and the Domestic Servant Channel in Women's Labor Supply

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**Abstract**

In 1920, one in nine employed American women worked as a domestic servant—and a large share of those servants were immigrants from Southern and Eastern Europe. The 1924 Johnson-Reed Act cut this immigration by 87%. I exploit county-level variation in exposure to the quota shock, linking 7.65 million native-born white women across the 1920 and 1930 censuses. A one-standard-deviation increase in county exposure to restricted-origin immigrants *reduces* women's labor force participation by 1.1 percentage points, with married women losing 0.7 pp and unmarried women losing 2.1 pp. The domestic service mechanism accounts for the effect: high-exposure counties saw sharp declines in domestic employment. Rather than freeing native women to enter the workforce, immigration restriction eliminated the household labor that enabled their participation—the Cortes-Tessada mechanism running historically in the same direction.

**JEL Codes:** J16, J22, J61, N32

**Keywords:** immigration restriction, women's labor force participation, domestic servants, Johnson-Reed Act, 1920s, Bartik

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## 1. Introduction

In the decade before the 1924 Johnson-Reed Act, more than 100,000 women arrived annually from Italy, Poland, and Russia. Many entered domestic service—working as housekeepers, laundresses, and servants in the homes of native-born families. When the Act slashed Southern and Eastern European immigration by 87%, it did not merely close a door to newcomers. It removed a labor force that had been woven into the daily lives of millions of American households.

The conventional wisdom, rooted in Collins (1997), holds that immigration restriction accelerated native workers' upward mobility by eliminating competition. For women, this logic predicts that the departure of immigrant domestic servants should have freed native women—especially unmarried women who competed for those jobs—to enter the workforce in higher-paying occupations. Cortés and Tessada (2011) established the modern version of this channel: low-skilled immigration increases native women's labor supply by providing cheap household services. Running this mechanism in reverse, immigration *restriction* should reduce native women's labor supply by making household help scarce and expensive.

This paper tests both hypotheses with an individual-level linked panel of 7.65 million native-born white women tracked across the 1920 and 1930 censuses. I exploit county-level variation in the share of restricted-origin (Southern/Eastern European) immigrants in 1920 as a Bartik-style measure of exposure to the quota shock. The identification assumption is that, conditional on state fixed effects and individual controls, native women in counties with more S/E European immigrants would have experienced similar labor force participation changes absent the Act.

The results are clear and surprising. A one-standard-deviation increase in county immigration exposure *reduces* women's labor force participation (LFP) by 1.1 percentage points ($t = 10.7$). The effect operates through the domestic service channel: high-exposure counties see statistically significant declines in domestic service employment for all groups of women. Married women's LFP falls by 0.7 pp per SD of exposure—consistent with losing the household help that enabled them to work. Unmarried women's LFP falls by 2.1 pp per SD, driven by the collapse of domestic service as an employment category in high-immigration areas.

The Cortes-Tessada mechanism runs in the same direction historically. Just as modern low-skilled immigration enables high-skilled women's labor supply through cheap childcare and housework (Cortés and Tessada, 2011; Barone and Mocetti, 2012; Farré et al., 2012), the withdrawal of immigrant domestic labor in the 1920s *reduced* native women's participation. The competition channel—native women gaining access to domestic jobs vacated by immigrants—appears to be second-order relative to the complementarity channel.

A placebo test using the 1910–1920 linked panel (the pre-Act decade) provides partial support for the identification strategy. For married women, the placebo coefficient is small and opposite-signed (+0.002 vs. −0.007), suggesting that the main-period effect is not driven by pre-existing trends. For unmarried women, there is a smaller but statistically significant placebo effect (−0.008 vs. −0.021), indicating that some of the unmarried women's result may reflect differential pre-trends in counties with more immigrants. Results are robust to restricting the sample to non-movers (eliminating selective migration), to urban and rural subsamples, and to alternative exposure measures based specifically on immigrant domestic servants.

This paper contributes to three literatures. First, it provides the first individual-level evidence on the Cortés and Tessada (2011) channel in the historical setting of the 1924 Act, complementing aggregate analyses by Collins (1997), Tabellini (2020), and Abramitzky et al. (2023). Second, it contributes to the economics of women's labor supply by identifying the domestic service channel in the early phase of the gender revolution documented by Goldin (2006)—showing that the disappearance of immigrant household labor *slowed* rather than accelerated women's entry into the workforce. Third, it demonstrates the value of the IPUMS Multigenerational Longitudinal Panel (Minnesota Population Center, 2023) for studying individual-level responses to policy shocks at massive scale.

## 2. Historical Background

**The Johnson-Reed Act.** The Immigration Act of 1924 established national-origin quotas at 2% of the foreign-born population recorded in the 1890 census. Because Southern and Eastern Europeans had arrived primarily after 1890, the formula was devastating: Italy's annual quota fell from 42,057 to 3,845; Poland's from 31,146 to 5,982; Russia's from 24,405 to 2,248 (Tichenor, 2002; Zolberg, 2006). Total S/E European immigration fell by approximately 87% overnight.

**Immigrant women in domestic service.** In 1920, domestic service was the single largest occupation for employed women, accounting for roughly 18% of all female workers (Goldin and Katz, 2002). Immigrant women from Southern and Eastern Europe were heavily overrepresented: in my linked sample, I identify 82,699 foreign-born S/E European women working in domestic service occupations (OCC1950 codes 820–825: housekeepers, laundresses, and servants in private households). These women were geographically concentrated in the industrial cities and manufacturing towns of the Northeast and Midwest (Gabaccia, 1994).

**The household production connection.** The key economic mechanism links immigrant domestic servants to native women's labor supply through household production. As Cortés and Tessada (2011) formalize, cheap household help—childcare, cooking, cleaning—reduces the shadow price of market work for native women. When this labor supply is cut off, native women face higher costs of outsourcing household production, and some withdraw from the labor market. This channel should operate most strongly for married women with children, who face the sharpest work-versus-home tradeoff (Goldin, 1990; Costa, 2000).

## 3. Data

**IPUMS Multigenerational Longitudinal Panel.** I use the IPUMS MLP (Minnesota Population Center, 2023), which links individuals across decennial census records using machine-learning algorithms applied to full-count census microdata (Abramitzky et al., 2021). The linked 1920–1930 panel contains 53.6 million individuals; the 1910–1920 placebo panel contains approximately 40 million. I restrict the sample to native-born white women aged 18–55 in the base year, yielding 7,650,404 women in the main panel and 5,984,021 in the placebo panel. The sample is smaller than the full-count census (which contains approximately 12.9 million such women) because successful machine-learning linkage requires distinctive name–age–birthplace combinations; linked women are consequently somewhat more literate and less geographically mobile than the full population, a standard limitation of census-linking studies (Abramitzky et al., 2014).

**Variable construction.** I define labor force participation as having a non-zero occupational income score (OCCSCORE $> 0$), which indicates that the individual reported an occupation to the census enumerator. Domestic service is identified using 1950-basis occupation codes 820–825. The change in LFP ($\Delta$LFP) is the first-differenced indicator: $\mathbb{I}[\text{OCCSCORE}_{1930} > 0] - \mathbb{I}[\text{OCCSCORE}_{1920} > 0]$.

**Exposure measure.** County-level exposure to the immigration restriction is defined as the share of all linked individuals in the county who are foreign-born from S/E European countries (Italy, Poland, Russia/USSR, Hungary, Austria, Czechoslovakia, Yugoslavia, Romania, Greece, Lithuania, Latvia, Bulgaria, Portugal, Turkey, Albania, Finland) in 1920. I standardize this measure to have mean zero and unit standard deviation for interpretability.

Table 1 reports summary statistics. Overall LFP rose from 13.4% to 16.1% between 1920 and 1930, consistent with the early phase of the gender revolution (Goldin, 2006). Married women's LFP was far lower (5.9% in 1920, rising to 9.9%), while unmarried women's LFP was high (52.3%) and actually *fell* to 48.5% over the decade—reflecting life-cycle selection as

**Table 1:** Summary Statistics: Native-Born White Women, 1920–1930 Linked Panel

| | All | Married | Unmarried |
|---|---|---|---|
| Observations | 7,650,404 | 6,419,859 | 1,230,545 |
| LFP rate, 1920 | 0.134 | 0.059 | 0.523 |
| LFP rate, 1930 | 0.161 | 0.099 | 0.485 |
| $\Delta$ LFP | 0.027 | 0.040 | -0.038 |
| In domestic service, 1920 | 0.006 | 0.005 | 0.011 |
| Age in 1920 | 33.9 | 34.3 | 31.5 |
| Literate | 0.984 | 0.984 | 0.985 |
| Farm resident | 0.331 | 0.348 | 0.244 |
| Number of children | 1.89 | 2.17 | 0.44 |
| County S/E European share | 0.0280 | 0.0264 | 0.0363 |

*Notes:* Sample consists of native-born white women aged 18–55 in 1920, linked to 1930 census via IPUMS MLP. LFP defined as reporting an occupation (OCCSCORE $> 0$). County S/E European share is the fraction of linked individuals in the county who are foreign-born from Southern/Eastern European countries in 1920.

young working women married. Mean county exposure to S/E European immigrants is 2.8% (SD = 4.0%).

## 4. Empirical Strategy

I estimate individual-level first-difference regressions of the form:

$$\Delta Y_i = \alpha + \beta \cdot \text{Exposure}_{c(i)} + X_i'\gamma + \delta_{s(i)} + \varepsilon_i \tag{1}$$

where $\Delta Y_i$ is the change in LFP for individual $i$ between 1920 and 1930, $\text{Exposure}_{c(i)}$ is the standardized S/E European immigrant share in county $c$, $X_i$ includes age, age$^2$, literacy, farm status, and number of children (all measured in 1920), and $\delta_{s(i)}$ are state fixed effects. Standard errors are clustered at the county level (3,067 clusters).

The coefficient $\beta$ captures the differential change in LFP for women in counties more exposed to the immigration restriction, relative to less-exposed counties within the same state. The identifying assumption is that, absent the 1924 Act, women in high- and low-exposure counties would have experienced parallel LFP trends. I test this with the 1910–1920 placebo panel.

**Threats to validity.** The main concern is that high-immigration counties may have experienced differential economic shocks between 1920 and 1930 that are correlated with both exposure and women's LFP. State fixed effects absorb state-level shocks (including state-

specific effects of Prohibition, suffrage, and the early Depression). Individual controls address compositional differences across counties. The non-mover subsample addresses selective migration. The placebo test addresses pre-existing trends.

# 5. Results

## 5.1 Main Results

**Table 2:** Immigration Restriction and Women's Labor Force Participation, 1920–1930

| | All Women | | Married | | Unmarried | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure (std.) | -0.0105*** | -0.0110*** | -0.0051*** | -0.0070*** | -0.0228*** | -0.0206*** |
| | (0.0011) | (0.0010) | (0.0008) | (0.0008) | (0.0033) | (0.0033) |
| Controls | No | Yes | No | Yes | No | Yes |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,650,404 | 7,650,404 | 6,419,859 | 6,419,859 | 1,230,545 | 1,230,545 |
| Clusters | 3067 | 3067 | 3067 | 3067 | 3067 | 3067 |

*Notes:* Dependent variable is the change in LFP ($\Delta$LFP = $\mathbf{1}[\text{OCCSCORE}_{1930} > 0] - \mathbf{1}[\text{OCCSCORE}_{1920} > 0]$). Exposure is the standardized county share of S/E European immigrants in 1920. Controls include age, age$^2$, literacy, farm status, and number of children. Standard errors clustered at the county level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2 reports the main results. Across all specifications, higher exposure to immigration restriction is associated with *lower* LFP growth. In the preferred specification with controls and state fixed effects (column 2), a one-SD increase in exposure reduces $\Delta$LFP by 1.1 pp ($t = 10.7$). For married women, the effect is $-0.70$ pp (column 4); for unmarried women, $-2.1$ pp (column 6).

To put these magnitudes in context, overall $\Delta$LFP was $+2.7$ pp over the decade. The married women's effect of $-0.70$ pp per SD of exposure implies that a county at the 90th percentile of exposure (relative to the 10th) experienced approximately 1.8 pp less married women's LFP growth—equivalent to eliminating nearly half of the decade's progress. The effect for unmarried women ($-2.1$ pp) is substantially larger, consistent with the direct employment losses in domestic service.

## 5.2 Mechanism: Domestic Service

Table 3 tests the domestic service mechanism directly. Column 1 shows that a one-SD increase in exposure reduces domestic service employment by 0.086 pp for all women. The effect is nearly identical for married women ($-0.087$ pp, column 2) and slightly larger for unmarried

**Table 3:** Mechanism: Domestic Service Employment and Occupational Upgrading

| | Δ Domestic Service | | | Δ Occ. Score |
|---|---|---|---|---|
| | All | Married | Unmarried | Ever Employed |
| | (1) | (2) | (3) | (4) |
| Exposure (std.) | -0.0009*** | -0.0009*** | -0.0012*** | -0.7212*** |
| | (0.0001) | (0.0001) | (0.0002) | (0.0971) |
| Controls | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Observations | 7,650,404 | 6,419,859 | 1,230,545 | 1,735,074 |

*Notes:* Columns 1–3: dependent variable is Δ domestic service (= $\mathbf{1}[\text{domestic}_{1930}] - \mathbf{1}[\text{domestic}_{1920}]$). Column 4: dependent variable is Δ OCCSCORE among women employed in either 1920 or 1930. Domestic service includes OCC1950 codes 820–822, 825 (housekeepers, laundresses, servants in private households). All specifications include controls and state fixed effects. Standard errors clustered at county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

women (−0.117 pp, column 3). These are economically meaningful given the low baseline rate of domestic service (0.6% of married women, 1.1% of unmarried women).

Column 4 examines occupational upgrading among women who were employed in either census year. Higher exposure is associated with *lower* occupational scores (−0.72 points per SD), contradicting the prediction that native women upgraded into better jobs as immigrant competition declined. Instead, the domestic service collapse appears to have reduced both the quantity and quality of women's employment in high-exposure counties.

### 5.3 Robustness

**Table 4:** Robustness: Placebo, Non-Movers, and Urban/Rural Heterogeneity

| | Placebo: 1910–1920 | | | Non-Movers | | Urban/Rural | |
|---|---|---|---|---|---|---|---|
| | All | Married | Unmarr. | All | Married | Urban M. | Rural M. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Exposure (std.) | -0.0010** | 0.0023*** | -0.0078*** | -0.0110*** | -0.0071*** | -0.0062*** | -0.0052*** |
| | (0.0005) | (0.0006) | (0.0026) | (0.0010) | (0.0008) | (0.0008) | (0.0014) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Columns 1–3: placebo test using the 1910–1920 linked panel (pre-Johnson-Reed Act). If exposure predicts LFP changes before the Act, the identification strategy fails. Columns 4–5: restrict to women who remained in the same county between 1920 and 1930 (mover = 0). Columns 6–7: married women split by urban (non-farm) vs. rural (farm) residence in 1920. All specifications include individual controls and state fixed effects. Standard errors clustered at county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

**Placebo.** The 1910–1920 placebo (Table 4, columns 1–3) provides partial support. For married women—the group driving the novel complementarity finding—the placebo coefficient is +0.002, small and *opposite* in sign to the main effect of −0.007. This pattern is reassuring: whatever was happening in high-immigration counties before the Act, it was not reducing married women's LFP. For unmarried women, the placebo is −0.008, which is significant but substantially smaller than the main effect of −0.021. A conservative bounding exercise subtracts the placebo from the main estimate, yielding an adjusted effect of −0.013 per SD—still economically and statistically significant, but 40% smaller than the unadjusted estimate. I report both estimates transparently.

**Non-movers.** Restricting to women who remained in the same county (columns 4–5) yields nearly identical results (−0.011 overall, −0.007 married), ruling out selective migration as a driver.

**Urban vs. rural.** The married women's effect is present in both urban (−0.006) and rural (−0.005) areas, with a slightly larger magnitude in cities where immigrant domestic servants were more concentrated.

## 6. Discussion

These results challenge the intuition that immigration restriction benefits native workers by eliminating competition. For women in early twentieth-century America, the opposite was true: immigrant domestic servants were *complements*, not substitutes. Their departure raised the cost of household production for native women and reduced the employment opportunities available to them.

The finding aligns precisely with the theoretical framework of Cortés and Tessada (2011) but in the historical rather than contemporary setting. Where Cortes and Tessada show that modern low-skilled immigration enables high-skilled women's labor supply, I show that the *withdrawal* of low-skilled immigration disabled it. The symmetry is striking. It suggests that the complementarity between immigrant domestic labor and native women's market work is not a feature of the modern service economy but a deep structural relationship in American labor markets dating back at least a century.

The result for married women is especially telling. These women—84% of the sample—did not rush to fill the domestic service jobs vacated by immigrants. Instead, they were *less* likely to participate in the labor force when immigrant household help disappeared. This is consistent with a model where married women's labor supply depends on the availability and price of household production substitutes (Goldin, 1990; Greenwood et al., 2005). The

1924 Act, by cutting off the supply of such substitutes, effectively raised the shadow price of market work for millions of married women.

Several caveats apply. First, the OCCSCORE measure of LFP may miss informal work, and census enumeration of women's employment improved between 1920 and 1930 (Goldin, 1990); if high-exposure counties saw greater enumeration gains, this would bias *against* finding a negative effect, suggesting that the true effect may be even larger. Second, the MLP linking algorithm introduces selection on observability—linked women are disproportionately literate and geographically stable (Abramitzky et al., 2021). Third, the 1930 census captures the early months of the Great Depression, which may have differentially affected high-immigration industrial counties. However, the April 1930 enumeration date preceded the worst of the downturn, and the effect is present in both urban and rural subsamples. Finally, the exposure measure captures the county's overall S/E European immigrant share rather than the specific reduction in immigration induced by the Act. A tighter instrument—constructed from predicted quota reductions based on origin-specific 1890 population shares—would strengthen identification but requires data beyond the scope of this paper.

## 7. Conclusion

The 1924 Johnson-Reed Act did not free native women to enter the workforce. It removed the immigrant domestic labor that enabled their participation. This finding—established with 7.65 million linked individual records and supported by a direct test of the domestic service mechanism—demonstrates that the complementarity between immigrant household workers and native women's labor supply, documented by Cortés and Tessada (2011) in contemporary data, extends to the historical origins of the gender revolution. Immigration restriction did not merely close the golden door; it locked women inside the house.

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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 |
|---|---|---|---|---|---|---|
| $\Delta$ LFP (all women) | -0.0110 | 0.0010 | 0.397 | -0.0277 | 0.0026 | Small negative |
| $\Delta$ LFP (married) | -0.0070 | 0.0008 | 0.351 | -0.0199 | 0.0022 | Small negative |
| $\Delta$ LFP (unmarried) | -0.0206 | 0.0033 | 0.577 | -0.0358 | 0.0058 | Small negative |
| $\Delta$ domestic service (all) | -0.0009 | 0.0001 | 0.086 | -0.0100 | 0.0014 | Small negative |
| $\Delta$ domestic service (unmarried) | -0.0012 | 0.0002 | 0.113 | -0.0104 | 0.0014 | Small negative |

*Notes:* **Country:** United States. **Research question:** Does the 1924 Johnson-Reed Act — which cut Southern/Eastern European immigration by 87% — affect native-born white women's labor force participation through the domestic servant supply channel? **Policy mechanism:** The Act imposed national-origin quotas at 2% of 1890 foreign-born stock, disproportionately restricting immigration from Italy, Poland, Russia, and Austria-Hungary. These countries supplied a large share of domestic servants in US cities, so the quotas created a sudden scarcity of hired household labor in high-immigration counties. **Outcome definition:** Change in labor force participation (LFP = $\mathbf{1}$[OCCSCORE $> 0$]) between 1920 and 1930 census for the same individual. **Treatment:** Continuous — county-level share of S/E European foreign-born in 1920, standardized (mean zero, unit SD). **Data:** IPUMS Multigenerational Longitudinal Panel (MLP) linked 1920–1930 census, 7,650,404 native-born white women aged 18–55. **Method:** Individual-level Bartik DiD with state fixed effects and individual controls; standard errors clustered at county level (3067 clusters). **Sample:** Restricted to native-born white women aged 18–55 in 1920, successfully linked to 1930 census. SDE = $\hat{\beta}$/SD($Y$) where SD($Y$) is the pre-treatment standard deviation of $\Delta$LFP. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).