

Does Off-Farm Work Help Farm Survival?

Evidence from U.S. Counties

Daniel Centuriao* Zheng Tian[†] Stephan Goetz[‡]

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Abstract

Using county-level Census of Agriculture panels from 2002 to 2022, this paper studies whether off-farm work stabilizes U.S. farm counts or accelerates the net loss of farms. We estimate county and Census-year fixed-effects models and instrument off-farm work with predicted private-sector labor demand accessible through commuting links. The IV results support a channel in which off-farm work stabilizes farm numbers. In the preferred specification, a one percentage-point increase in instrumented off-farm work raises five-year log farm-count change by 0.052 log points, equivalent to an implied 5.3 percent increase in farm counts over the Census interval and lowers the net loss of farm by 15.5 percentage points. The estimates imply that off-farm employment induced by commuting-linked labor-market access helps farms persist rather than facilitating exit. Policies that expand off-farm job access may therefore complement agricultural policy by supporting farm household viability.

Keywords: Consolidation; Employment; Commuting; Persistence; Labor; Agriculture.

JEL Classification: Q12, Q18, J22, R11.

*Postdoctoral Scholar at Northeast Regional Center for Rural Development, Penn State University. Email: dfc5823@psu.edu.

[†]Assistant Research Professor at The Northeast Regional Center for Rural Development, Pennsylvania State University. Email: ztian@psu.edu.

[‡]Professor of Agricultural and Regional Economics Director of The Northeast Regional Center for Rural Development, Pennsylvania State University Email: sjg16@psu.edu.

1 Introduction

The continued decline in farm numbers remains a central concern for agricultural policy and rural development. The 2022 Census of Agriculture counted 1.90 million farms in the United States, a decline of roughly seven percent from 2017 and the first Census counting fewer than two million farms in modern records (USDA Economic Research Service, 2024; USDA National Agricultural Statistics Service, 2024). Land in farms has declined more slowly than the number of farms, implying a reallocation of land toward surviving operations (Appel and Balmann, 2023), a rise in average farm scale, and continued changes in the organization of agricultural production (USDA National Agricultural Statistics Service, 2024). The relationship between farm scale, labor allocation, and productivity is affected by labor-market transaction costs and machine capacity, as emphasized by Foster and Rosenzweig (2022). If structural change has altered the composition of farms at risk of exit, then the same measure of off-farm work may no longer carry the same interpretation as in the 1980s and 1990s. This concern extends beyond the United States: because most farms remain family operated while farmland is increasingly concentrated, which farms persist and which farms exit shapes both household viability and the organization of agricultural production (Lowder et al., 2021). This paper studies one adjustment mechanism: whether access to off-farm employment stabilizes farm counts or accelerates their net loss.

Off-farm earnings can help farm households manage income risk and liquidity constraints and withstand periods of low farm returns. However, they may also accelerate transitions out of farming when nonfarm employment strengthens attachment to outside labor markets and lowers the cost of leaving farming. This ambiguity is central to Goetz and Debertin (2001), who address the issue with a predicted off-farm-employment measure and a selection model that distinguishes whether a county loses farm proprietors from how rapidly it loses them once in a net-loss state. Their results reveal opposing margins: off-farm employment appears to be associated with a reduction in the probability of county-level farm-proprietor loss, but to accelerate decline among counties already losing proprietors. Subsequent evidence shows that farm exits are associated with farm size and scale (Breustedt and Glauben, 2007; Dong et al., 2016; Foltz, 2004), specialization and production structure (Dong et al., 2016; Zorn and Zimmert, 2022), government transfers and off-farm work incentives (Mishra et al., 2014), and operator life-cycle factors (Zorn and Zimmert, 2022). Yet the ambiguity remains unresolved: little is known about which channel dominates in the modern farm economy, or about the average effect of off-farm work on farm counts.

Off-farm is an endogenous household labor-supply choice, jointly determined with

idiosyncratic farm characteristics, household preferences, financial constraints, and expectations about the future of the farm. The direction of bias in ordinary fixed-effects regressions estimations is therefore not signed a priori. If declining farm profitability pushes households into off-farm work while also increasing future farm-count loss, ordinary estimates may make off-farm work appear more exit-inducing than it truly is. If households use off-farm earnings to keep farms operating during periods of low farm income, the same estimates may make off-farm work appear more stabilizing than it truly is. Related work shows that off-farm labor decisions are influenced by policy transfers and crop insurance (Key and Roberts, 2006; Kim et al., 2020), household structure and spousal labor-force participation (El-Osta et al., 2008), operator age and succession (Gale, 2003), financial access (Kitenge, 2022), and climate adaptation and risk (Afridi et al., 2022; Huang and Kahn, 2026; Wheeler et al., 2020). Observed off-farm work bundles the effect of outside employment with the farm and household conditions that lead operators to seek it.

This paper addresses this problem by connecting off farm work to the outside employment opportunities available to farm households. This distinction follows (Chang et al., 2012) who theoretically argues that off-farm work can serve as an adjustment margin only when jobs are available and compatible with continued farm operation. It is also consistent with evidence that local labor-market conditions influence farm exits and off-farm labor supply (Foltz, 2004; Mathenge and Tschirley, 2015). Ifft and Jodlowski (2022) shows empirically that shocks to labor availability can affect farm employment, labor expenditure, mechanization, and land use. This suggests that labor-market conditions can reshape farm structure, not only labor allocation.

Our analysis combines county-level Census of Agriculture data from the USDA National Agricultural Statistics Service (NASS) for 2002–2022 with commuting-flow data from the U.S. Census Bureau’s Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES) and private-sector employment data from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW). We instrument off-farm work with predicted private-sector labor demand accessible through commuting links. The instrument combines predetermined county-to-destination commuting flows from LODES with private-sector industry employment growth from QCEW in commuting destinations. Intuitively, the IV measures the private-sector labor-market opportunities that farm households in a county are predicted to face through the places to which county residents commute. The preferred specification includes county fixed effects and Census-year fixed effects. Identification comes from within-county changes over adjacent Census intervals in commuting-linked private labor demand, after conditioning on farm-structure controls.

Ordinary county fixed-effects estimates show little relationship between observed off-

farm work and farm-count adjustment. In contrast, the IV estimates indicate that instrumented off-farm work is associated with more positive farm-count change and a lower probability of net farm-count loss, indicating a stabilization channel. The IV estimates support a stabilization channel. Off-farm work induced by commuting-linked labor-market access is associated with slower farm-count decline and a lower probability of net farm-count loss. The implied magnitudes are economically meaningful and robust across alternative specifications. The negative first stage coefficient is consistent with a composition channel: stronger private-sector outside work options pull marginal or part-time operators fully out of farming, leaving a remaining operator pool that is more attached to farming and less likely to report 200 or more days off farm. The second stage should therefore be interpreted as an instrument-specific average causal response for the component of off-farm work shifted by commuting-linked private labor-market access. The main findings are robust across a variety of alternative specifications and supporting analyses. Additional results suggest that the stabilizing effect is strongest among small farms, consistent with an income-diversification mechanism.

The paper contributes to three strands of literature. First, it contributes to work on farm exits, consolidation, and structural change by studying a mechanism of farm-count adjustment over a recent period in which off-farm income and commuting access are central to farm household viability (Appel and Balmann, 2023; Dong et al., 2016; Foltz, 2004; MacDonald et al., 2018; Zorn and Zimmert, 2022). It also contributes directly to Goetz and Debertin (2001) by using credible exogenous variation to disentangle the ambiguity of the off-farm-work adjustment mechanism, providing instrument-specific evidence on how off-farm work induced by commuting-linked private labor-market access affects county farm-count adjustment. Second, it contributes to the farm-household labor literature by providing empirical evidence consistent with the theoretical proposition in Chang et al. (2012) that off-farm labor-market access operates as an adjustment margin in farm household labor allocation. Third, the conclusions complement recent empirical work by Ifft and Jodlowski (2022).

2 Conceptual Framework

Following the farm-household labor-allocation tradition (e.g. Huffman (1976), Huffman (1980) and Kwon et al. (2006)), a farm household allocates time across on-farm work, off-farm work, and leisure, while also choosing hired labor and farm production. Consider a farm household h in county i and period t . The household has a time endowment T allocated among on-farm family labor, off-farm labor, and leisure:

$$T = f_{hit} + m_{hit} + \ell_{hit}, \quad (1)$$

where f_{hit} denotes family labor supplied to the farm, m_{hit} denotes off-farm labor, and ℓ_{hit} denotes leisure. Farm production is

$$Q_{hit} = G(f_{hit}, H_{hit}; A_{hit}), \quad (2)$$

where H_{hit} is hired labor and A_{hit} summarizes fixed farm assets, land, and production conditions. Farm operating income is uncertain because output prices, yields, weather, and production costs vary over time. Let $\tilde{\epsilon}_{hit}$ denote these farm-income shocks, with $\mathbb{E}_t[\tilde{\epsilon}_{hit}] = 0$. Household income while continuing to farm is

$$\mathcal{Y}_{hit} = p_{it}G(f_{hit}, H_{hit}; A_{hit}) - w_{it}^H H_{hit} + w_{it}^O m_{hit} - \kappa_{it}(m_{hit}) + Z_{hit} + \tilde{\epsilon}_{hit}, \quad (3)$$

where p_{it} is the farm output price, w_{it}^H is the hired-labor wage, w_{it}^O is the off-farm wage, $\kappa_{it}(m_{hit})$ captures the monetary and time costs of accessing off-farm work, Z_{hit} denotes nonlabor income, including government payments and resources related to the farm asset base, and $\tilde{\epsilon}_{hit}$ captures farm-income shocks from output prices, yields, weather, and production costs, with $\mathbb{E}_t[\tilde{\epsilon}_{hit}] = 0$.

The value of continuing to farm can be written as the value generated by expected utility over stochastic income and leisure, plus the future continuation value:

$$V_{hit}^{\text{cont}} = \max_{f, H, m, \ell} \mathbb{E}_t [U(\mathcal{Y}_{hit}, \ell_{hit})] + \delta \mathbb{E}_t [V_{h,i,t+1}^{\text{cont}}]. \quad (4)$$

For households on an interior off-farm-work margin, the choice of m_{hit} would satisfy the illustrative first-order condition

$$E_t [U_Y(Y_{hit}, \ell_{hit}) (w_{it}^O - \kappa'_{it}(m_{hit}))] = \lambda_{hit}, \quad (5)$$

where λ_{hit} is the shadow value of household time. This condition states that changes in off-farm wages or in the cost of accessing outside work shift the household's off-farm-work decision. For households at a corner, the equality becomes an inequality, so improved labor-market access may instead move households into or out of off-farm work. This expression is not estimated structurally. It is used to clarify why off-farm earnings can raise the value of continuing to farm. When off-farm opportunities are flexible and accessible, the household can adjust m_{hit} , f_{hit} , H_{hit} , and ℓ_{hit} jointly (Chang et al., 2012). In that case, the off-farm wage affects the shadow value of family labor and the composition of household income. When off-farm opportunities are constrained by distance, commuting costs,

schedule rigidity, or limited local employment, production and consumption decisions are less separable: the household may not be able to use off-farm work as a smooth margin of adjustment. This distinction is central for the present paper because our proposed empirical instrument shifts private-sector labor demand through commuting networks, not farm profitability directly.

The household compares the value of continuing to farm with the value of leaving farming. Let V_{hit}^{cont} denote the value of continuing to operate the farm, and V_{hit}^{exit} denote the value of quitting farming and moving fully into nonfarm employment, retirement, or another nonfarm activity. The latent exit index can be defined as:

$$Exit_{hit}^* = V_{hit}^{\text{exit}} - V_{hit}^{\text{cont}}, \quad (6)$$

where the household exits farming when $Exit_{hit}^* > 0$.

Off-farm work affects this exit index through two competing channels. The first is an income-stabilization channel. Off-farm earnings diversify household income, reduce exposure to volatile farm returns, relax liquidity constraints, and may allow the household to continue operating the farm when farm income is low (Kitenge, 2022; Mishra and Chang, 2012; Mishra and Goodwin, 1997). Let $B(m_{hit})$ denote this buffering value. The second is a transition or exit-cost channel. Off-farm work may increase information about nonfarm employment, build nonfarm labor-market experience, strengthen attachment to outside labor markets, and lower the cost of moving completely out of farming (De Janvry and Sadoulet, 2001; Rajkhowa and Qaim, 2022). Let $C(m_{hit})$ denote this transition value. A reduced-form representation of the exit index is

$$Exit_{hit}^* = \alpha + \psi C(m_{hit}) - \omega B(m_{hit}) + X_{it}'\gamma + \mu_i + \tau_t + \varepsilon_{hit}, \quad (7)$$

where X_{it} denotes baseline farm-structure and local controls, μ_i captures time-invariant county characteristics, and τ_t captures common Census-period shocks. The sign of off-farm work is theoretically ambiguous:

$$\frac{\partial Exit_{hit}^*}{\partial m_{hit}} = \psi C'(m_{hit}) - \omega B'(m_{hit}). \quad (8)$$

If the income-buffering channel dominates, off-farm work lowers exit pressure. If the transition-cost channel dominates, off-farm work raises exit pressure.

This ambiguity is emphasized by (Goetz and Debertin, 2001). In their framework, off-farm employment can supplement farm household income and reduce the probability that a county loses farmers, but it can also facilitate movement into full-time nonfarm work once a county is already losing farmers. They use county-level net farm proprietorship changes and a selection model to distinguish counties losing farmers from counties gaining

or maintaining farmers, and a predicted version of the model to address endogeneity.

Aggregation. To connect the household exit margin to the county-level outcomes used in the empirical analysis, let:

$$e_{hit} = \mathbf{1}\{\text{Exit}_{hit}^* > 0\}, \quad (9)$$

indicate whether farm household h exits between Census years t and $t + 5$, and let

$$d_{hit} = \mathbf{1}\{m_{hit} \geq 200\}, \quad (10)$$

denote high-intensity off-farm work. The observed county off-farm-work measure is the baseline share of operators or producers working 200 or more days off farm,

$$d_{it} = \frac{1}{F_{it}} \sum_{h=1}^{F_{it}} d_{hit}, \quad (11)$$

where F_{it} is the number of farms in county i at baseline. Let the county exit rate over the Census interval be

$$E_{it} = \frac{1}{F_{it}} \sum_{h=1}^{F_{it}} e_{hit}, \quad (12)$$

and let n_{it} denote the county entry and succession rate, scaled by baseline farms. The county farm-count accounting identity is

$$F_{i,t+5} = F_{it}(1 - E_{it} + n_{it}). \quad (13)$$

Dividing by F_{it} and taking logs gives

$$\log\left(\frac{F_{i,t+5}}{F_{it}}\right) = \log(1 + n_{it} - E_{it}) \approx n_{it} - E_{it}. \quad (14)$$

Thus, if off-farm work reduces household exit pressure, it lowers E_{it} and raises county farm-count growth. In the county regression, the coefficient on d_{it} therefore has the opposite sign of the off-farm-work effect on the latent exit index. A positive coefficient for log farm-count change indicates that the income-stabilization channel dominates, while a negative coefficient would indicate that the transition channel dominates. Similarly, for the net-loss outcome,

$$q_{i,t+5} = \mathbf{1}\{F_{i,t+5} - F_{it} < 0\} = \mathbf{1}\{E_{it} > n_{it}\}, \quad (15)$$

so a stabilizing effect of off-farm work implies a negative coefficient in the net farm-count

loss specification.

3 Data

The analysis uses a county-by-Census-interval panel covering 5-year Census of Agriculture intervals from 2002 to 2022. The unit of observation is a county-interval. The outcome interval runs from baseline Census year t to $t + 5$. The main continuous outcome is log farm-count change, defined as:

$$y_{i,t+5} = \log \left(\frac{F_{i,t+5}}{F_{it}} \right), \quad (16)$$

and the extensive-margin outcome is net farm-count, denoted as:

$$q_{i,t+5} = \mathbb{1}\{F_{i,t+5} - F_{it} < 0\}. \quad (17)$$

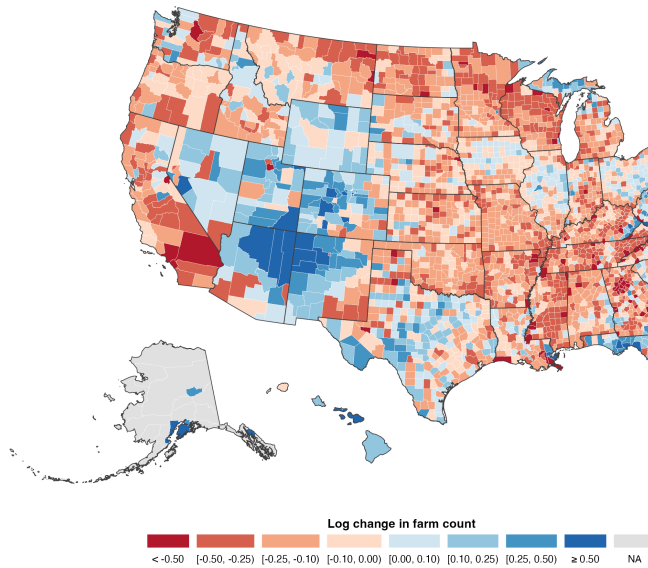
Negative values of $y_{i,t+5}$ indicate proportional farm-count decline. Values of $q_{i,t+5} = 1$ indicate net farm-count loss. Because the analysis updates the original Goetz and Debertin (2001) setting along several dimensions, Table A.1 summarizes the main measurement and interpretation differences between the earlier specification and the variables used here.

Figure 1 summarizes the raw spatial and temporal patterns in farm-count adjustment over the full 2002–2022 data panel. Panel A maps cumulative county-level log farm-count change using discrete bins. The geography of adjustment is broad but uneven: many counties experienced negative or near-zero changes, while a smaller number recorded large increases. Panel B places this county-level geography in national context by plotting farm counts indexed to 2002 for the United States and Census regions. The aggregate series shows that farm counts increased or remained stable in several regions early in the period but declined after 2012, with especially large declines by 2022.

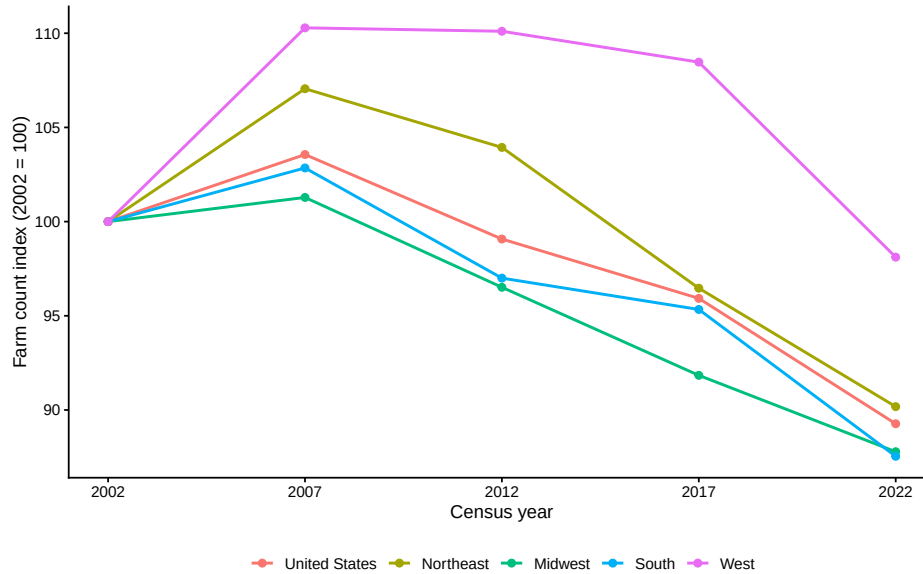
One visible feature of Panel A is the cluster of large farm-count increases in northeastern Arizona, northwestern New Mexico, and southeastern Utah. These increases should not be interpreted as ordinary farm entry. They coincide with changes in the Census of Agriculture’s treatment of agricultural activity on American Indian reservations. Historically, reservation agricultural activity was often collected through reservation-level aggregate reports, while the 2007 Census expanded efforts to collect individual farm and ranch reports from American Indian and Alaska Native operators on reservations nationwide (USDA National Agricultural Statistics Service, 2009). This change is especially relevant for counties overlapping the Navajo Nation and nearby reservation areas. For this reason, the appendix reports robustness checks excluding reservation-affected counties.

Figure 1: Farm-Count Adjustment Across Counties and Census Regions

Panel A. Cumulative Change in Farm Counts



Panel B. National and regional farm-count trends



Notes: Panel A maps the cumulative log change in the number of farms between the 2002 and 2022 Censuses of Agriculture, defined as $\log(F_{i,2022}/F_{i,2002})$, where F_{it} is the number of farms in county i and Census year t . Negative values indicate farm-count decline. White areas indicate counties not included in the estimation sample or counties with missing values. Panel B plots total farm counts for the United States and Census regions indexed to 2002, so that each series equals 100 in 2002. The large positive changes in parts of northeastern Arizona, northwestern New Mexico, and southeastern Utah partly reflect changes in Census enumeration of individual farm and ranch operators on American Indian reservations, especially around the 2007 Census, and should not be interpreted as ordinary farm entry. The figures are descriptive rather than conditional averages.

The endogenous variable is off-farm work in the baseline Census year. The measure is the share of farm operators or producers working 200 or more days off farm.¹ The series uses the historical principal-operator measure through 2012 and the producer-based measure in later Census years. The series should therefore be interpreted as a bridge measure of high-intensity off-farm work. The empirical design includes year fixed effects, so common Census-wave differences in the level of the measure are absorbed by the time effects. At the aggregate county-mean level, the integrated series does not display a visible level break across Census years: the mean is about 38 percent in 2002, 39 percent in 2007, 39 percent in 2012, 39 percent in 2017, and 38 percent in 2022 (Appendix Table A.2). Figure A.1 shows the same pattern visually, with the producer-based series close to the extrapolated pre-2017 path. The potentially problematic comparison is the 2012–2017 interval, which crosses the change from the principal-operator measure to the producer-based measure. The empirical specifications include Census-year fixed effects, and Appendix Table A.4 reports a slope-break diagnostic for intervals beginning after the definition change.

Table 1 reports summary statistics for the county–Census-interval panel used in the main analysis. The average five-year log farm-count change is negative, indicating farm-count decline over the typical county interval, and 61.3 percent of county-interval observations experience net farm-count loss. The average county has 38.8 percent of operators or producers reporting 200 or more days of off-farm work. The sample also reflects substantial heterogeneity in farm structure. The average county has about 692 farms at baseline, but the interquartile range runs from 332 to 903 farms. Small farms account for 58.2 percent of farms on average, while large farms account for 17.8 percent. Farmland represents about half of county land on average, although this share varies widely across counties.

¹The Census of Agriculture changed the reporting basis for off-farm work after 2012. The historical series reports off-farm work for principal operators, whereas the 2017 and 2022 Censuses report the measure for producers. We therefore treat the 2002–2022 measure as an integrated off-farm-work series and include Census-year fixed effects in all main specifications. Appendix diagnostics examine whether the post-2017 definition change introduces a visible level or slope break in the series.

Table 1: Summary Statistics

Variable	Mean	Std. dev.	P25	Median	P75	N
Log farm-count change	-0.029	0.155	-0.111	-0.034	0.046	12,222
Net farm-count loss	0.613	0.487	0.000	1.000	1.000	12,222
Off-farm work	38.801	6.918	34.702	39.252	43.225	12,222
Private commuting-flow IV	-0.022	0.969	-0.525	0.061	0.584	12,222
Baseline farm count	691.934	551.267	332.000	571.000	903.000	12,222
Family farms	85.744	8.113	82.395	87.363	91.115	12,222
Operator age	57.436	2.901	55.900	57.600	59.100	12,222
Land and building value	1007.434	964.302	460.460	698.113	1200.773	12,131
Farmland share	51.403	30.926	23.628	48.720	80.943	12,139
Irrigated farmland	13.501	24.300	0.307	1.889	12.755	12,194
Government payments per farm	7.959	13.791	1.051	3.420	9.730	12,126
CV of net farm income	2.674	47.503	0.361	0.590	1.104	12,023
Small-farm sales share	58.201	16.795	46.323	60.000	71.055	12,215
Large-farm sales share	17.792	14.271	6.256	13.540	26.484	12,039

Notes: The table reports summary statistics for 2002–2022 panel sample used in the main specifications. The unit of observation is a county–Census interval. Off-farm work is the integrated 200+ days off-farm-work measure, reported in percentage points. The private commuting-flow IV is standardized. Land and building value and government payments are measured in thousands of dollars per farm.

We combine the Census of Agriculture panel with two additional data sources used to construct the private commuting-flow instrument. Predetermined county-to-destination commuting links come from the U.S. Census Bureau’s LEHD Origin-Destination Employment Statistics (LODES). Private-sector employment growth is constructed from Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW) employment data, restricted to private ownership and excluding agriculture, mining and oil/gas, construction, agricultural support, and food manufacturing. The resulting instrumental variable construction is detailed in the following section.

4 Empirical Strategy

To estimate the effect of off-farm work on farm-count adjustment, we begin with a county panel model that relates baseline off-farm work to farm-count changes over the following Census interval:

$$Y_{i,t+5} = \beta d_{it} + X'_{it}\gamma + \mu_i + \tau_t + \varepsilon_{it}, \quad (18)$$

where i indexes counties and $t \in \{2002, 2007, 2012, 2017\}$ indexes Census baseline years.

The outcome $Y_{i,t+5}$ is either the log change in farms, $\log(F_{i,t+5}/F_{it})$, or an indicator equal to one if county i loses farms on net between t and $t + 5$. The variable d_{it} is the share of farm operators or producers reporting 200 or more days of off-farm work in baseline year t . The vector X_{it} includes predetermined farm-structure controls, μ_i are county fixed effects, and τ_t are Census-year fixed effects.

Equation (18) provides a useful descriptive benchmark, but β generally cannot be interpreted causally. Off-farm work is a household labor-allocation choice jointly determined with farm production, operator attachment to farming, farm profitability, and expectations about future farm viability. This simultaneity is precisely the empirical problem emphasized by Goetz and Debertin (2001), who used a predicted off-farm-employment strategy to separate off-farm labor supply from farm-exit decisions. Our design builds on that strategy but replaces predicted off-farm work from observed county characteristics with an external source of variation in off-farm work generated by private-sector labor demand in commuting-linked destinations.

County fixed effects absorb time-invariant county determinants of farm-count adjustment, including historical farm structure, geography, soil quality, persistent land-market conditions, baseline remoteness, and long-run proximity to employment centers. Year fixed effects absorb shocks common to all counties in a Census wave, including national macroeconomic conditions, agricultural policy changes, national commodity-market shocks, and common differences across Census definitions or measurement environments. These fixed effects do not remove time-varying county characteristics that evolve differentially across places and may be correlated with both off-farm work and farm-count adjustment. The preferred control vector therefore includes baseline farm count, the prevalence of family farms, operator age and age squared, land and building values, farmland share, irrigated farmland, government payments per farm, the coefficient of variation of net farm income, and the shares of farms in small and large sales classes. Land and building values are included because farm assets can affect both the continuation value of farming and the value of exit through sale, collateral, or transfer opportunities. Local economic controls, such as population growth and population density, are excluded from the preferred specification because they may respond to the same commuting-linked labor-demand shocks used for identification; they are introduced separately as robustness checks.

Private Commuting-Flow Instrument. Farm households face job opportunities in destination labor markets to which county residents are already connected through commuting flows. Changes in labor demand in those destinations shift the opportunity cost of farm labor and the feasibility of combining farm and nonfarm work. Farm-household labor models emphasize the availability of off-farm employment opportunities

as central to the allocation of labor among farm production, off-farm work, and household time (Chang et al., 2012). Our instrument operationalizes this idea by combining predetermined commuting exposure with external private-sector employment shocks.

To formalize the instrumental variable, let s_{ij}^0 denote the predetermined share of workers from county i commuting to destination county j . Let $\widehat{g}_{jt}^P$ denote predicted private-sector employment growth in destination j over the relevant pre-period. Let k index private-sector industries included in the instrument, and let ω_{jk}^0 denote industry k 's baseline share of private employment in destination county j . Predicted private-sector employment growth in destination j is constructed as

$$\widehat{g}_{jt}^P = \sum_k \omega_{jk}^0 g_{k,-s(j),t}^P, \quad (19)$$

where $g_{k,-s(j),t}^P$ is national private-sector employment growth in industry k , excluding destination j 's state. The leave-one-state-out construction prevents the predicted destination shock from mechanically using employment changes in the same state whose local farm and labor-market conditions may be related to farm-count adjustment.

The private commuting-flow instrument is

$$Z_{it}^P = \sum_j s_{ij}^0 \widehat{g}_{jt}^P. \quad (20)$$

Thus, Z_{it}^P is high when county i is connected, through predetermined commuting flows, to destinations whose baseline industry composition predicts stronger private-sector employment growth. The instrument is standard normalized; consequently, the first-stage coefficient measures the change in off-farm work associated with a one-standard-deviation increase in predicted private-sector commuting exposure.

The first stage is:

$$d_{it} = \pi Z_{it}^P + X'_{it}\rho + \mu_i + \tau_t + u_{it}. \quad (21)$$

The second stage is:

$$Y_{i,t+5} = \beta \widehat{d}_{it} + X'_{it}\gamma + \mu_i + \tau_t + \varepsilon_{it}. \quad (22)$$

Identification comes from within-county changes over Census intervals in predicted private-sector labor demand faced through predetermined commuting connections. Counties differ in the destinations to which their residents are historically connected, and those destinations differ in baseline industry composition. As national private-sector industry growth evolves over time, otherwise similar counties receive different predicted commuting-linked labor-demand shocks. The preferred specification clusters standard errors by county, allowing arbitrary heteroskedasticity and serial correlation in unobserved

shocks within counties across Census intervals. The coefficient β under the respective assumptions is interpreted as an instrument-specific weighted average causal response. A positive β for log farm-count change, or a negative β for the net-loss indicator, is consistent with the stabilization mechanism in the conceptual framework: off-farm work relaxes household income constraints and allows farms to remain in operation. The opposite pattern would be consistent with a transition mechanism in which off-farm work lowers the cost of leaving farming.

Identification. The causal interpretation of β depends on two identifying assumptions. The first is relevance:

$$\text{Cov}(Z_{it}^P, d_{it} \mid X_{it}, \mu_i, \tau_t) \neq 0. \quad (23)$$

Relevance requires that predicted private-sector labor demand in commuting-linked destinations shift measured off-farm work among farm operators or producers. This condition is economically motivated by farm-household labor allocation and is directly assessed in the first-stage estimates.

The second condition is the exclusion restriction:

$$\text{Cov}(Z_{it}^P, \varepsilon_{it} \mid X_{it}, \mu_i, \tau_t) = 0. \quad (24)$$

Conditional on county fixed effects, Census-year fixed effects, and predetermined farm controls, predicted private-sector employment growth in commuting-linked destinations must affect farm-count adjustment only through off-farm work. The design supports this restriction in three ways. First, commuting exposure is predetermined, so current farm-count adjustment cannot mechanically determine the commuting weights used to construct the instrument. Second, destination shocks are built from national private employment growth by industry with one state omitted, reducing feedback from local agricultural outcomes to the labor-demand shock. Third, county and year fixed effects remove permanent county differences and national Census-wave shocks, while farm controls² absorb time-varying differences in baseline farm structure that may predict both off-farm work and future farm numbers.

Because the instrument has a shift-share structure (Bartik, 1991), the exclusion argument is based either on the exogeneity of the shocks, the exogeneity of the exposure shares, or both (Adão et al., 2019; Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020). Our preferred interpretation is shock-based: baseline commuting links determine exposure, while national private-sector industry growth supplies external labor-demand

²Appendix Table A.3 reports diagnostic associations between the private commuting-flow instrument and the preferred explanatory variables. Several associations are statistically significant, but the magnitudes are small, and the preferred IV specifications condition directly on these farm-structure variables. The table is therefore used as a transparency check on observable covariate balance.

variation. The exposure-based interpretation is strengthened by county fixed effects and by robustness checks that replace county fixed effects with commuting-zone fixed effects.

Potential threats to identification and robustness design. The exclusion restriction would fail if the commuting-flow instrument affected farm-count adjustment directly through nonfarm growth rather than through off-farm work. For example, stronger private labor demand could increase land-development pressure, alter migration, change local wages, or shift demand for local goods and services. These channels would violate the exclusion restriction if they directly affect farm counts after conditioning on fixed effects and farm controls. The empirical strategy therefore distinguishes the preferred specification from a sequence of robustness checks that condition on observable proxies for these alternative channels. Baseline specifications already condition on farm structure, land values, farmland share, irrigation, government payments, farm-income volatility, and farm-size composition, but it does not include local economic variables such as population density or population growth because these variables may be outcomes of the same labor-demand shocks that generate off-farm work. They are therefore included only in sensitivity tests presented in the appendix. Second, we add detailed commodity-share controls to test whether the estimates are driven by differential exposure to changing agricultural production systems. Third, we estimate specifications with commuting-zone fixed effects, which absorb shocks common to broader local labor markets. Fourth, we report definition-bridge diagnostics for the integrated off-farm-work series to assess whether the later producer-based measure changes the relationship between off-farm work and farm-count adjustment. This does not "prove" the validity of the exclusion restriction, but provides test of whether the results are sensitive to observable farm structure, local economic conditions, commodity composition, labor-market geography, or the construction of the off-farm-work series.

Heterogeneity diagnostics. We examine heterogeneity using interacted IV specifications rather than split-sample IV regressions. In shift-share IV settings, split samples change both the estimation sample and the instrument-specific weighting of heterogeneous effects, while interaction terms can be treated as additional shift-share instruments (Borusyak et al., 2025). They may also generate weak subgroup first stages, a concern related to first-stage heterogeneity across observable groups (Abadie et al., 2024). We treat the heterogeneity results as diagnostics.

Let G_{it} denote either the county's Census region or the baseline Census year, and let g_0 be the omitted group. The heterogeneity specification is estimated as a base-group interaction model:

$$Y_{i,t+5} = \beta d_{it} + \sum_{g \neq g_0} \delta_g (d_{it} \times \mathbb{1}\{G_{it} = g\}) + X'_{it}\gamma + \mu_i + \tau_t + \varepsilon_{i,t+5}. \quad (25)$$

The endogenous variables are d_{it} and each interaction $d_{it} \times \mathbb{1}\{G_{it} = g\}$ for $g \neq g_0$. They are instrumented with the private commuting-flow instrument Z_{it}^P and the corresponding interactions $Z_{it}^P \times \mathbb{1}\{G_{it} = g\}$.

In this parameterization, β is the IV effect for the omitted group, and $\beta + \delta_g$ is the IV effect for group g . The interaction coefficient δ_g measures the difference between group g and the omitted group. We proceed diagnostics graphically for Census regions and baseline Census years. The South is omitted, as is the baseline year 2002. These specifications use county and year fixed effects, preferred controls, the private commuting-flow instrument, and county-clustered standard errors.

5 Results

Table 2 reports the main county and Census-year fixed effects results. Column (1) reports the preferred OLS specification, with the preferred control vector. The OLS estimates are economically small and statistically indistinguishable from zero for both outcomes. Columns (2)–(4) report the IV estimates, adding controls and ending with the preferred IV specification in column (4). Panel A reports the first stage for the IV columns, Panel B reports the second stage for log farm-count change, and Panel C the second stage for net farm-count loss. The preferred IV specification is the main focus, especially given the stability of the IV coefficients across the control sequence. In column (4), a one-standard-deviation increase in private commuting-flow exposure reduces measured high-intensity off-farm work by 0.235 percentage points, with a first-stage Wald F -statistic of 13.0. This statistic is above the conventional threshold of 10; the table also reports Anderson–Rubin confidence intervals, which are robust to weak first-stage concerns (Andrews and Stock, 2005; Keane and Neal, 2024; Stock and Yogo, 2002). The negative first stage reflects a composition margin. Because off-farm work is measured only among remaining farm operators or producers, stronger private-sector labor demand may move some marginal operators out of farming altogether, reducing the measured off-farm-work share among those who remain.

The second stage shows that the component of off-farm work shifted by the private commuting-flow instrument is stabilizing. For log farm-count change, the preferred estimate is 0.052. Because the mean log change is negative, a positive coefficient means that instrumented off-farm work is associated with slower farm-count decline. The same pattern appears on the extensive margin: the preferred estimate for net farm-count loss is -0.155 . Thus, counties with higher instrumented off-farm work experience slower farm-count decline and a lower probability of losing farms over the next Census interval. For both outcomes, the Anderson–Rubin confidence interval excludes zero, indicating that

the preferred IV estimates remain statistically different from zero under weak-instrument-robust inference.

The magnitudes should be interpreted as instrument-specific average causal responses along the margin of off-farm work shifted by commuting-linked private labor-demand shocks. In the preferred specification, a one percentage-point increase in instrumented off-farm work raises five-year log farm-count change by 0.052 log points and lowers the probability of net farm-count loss by 15.5 percentage points. Expressed in levels using the decomposition outcome, the average county loses about 1.94 farms per 100 baseline farms over a five-year Census interval, or roughly 0.39 farms per 100 per year. The preferred IV estimate implies 6.17 additional farms per 100 baseline farms over the same five-year interval, or about 1.23 additional farms per 100 per year.

In land-use terms, in the preferred sample, farmland accounts for about 52 percent of county land on average, and the average county has about 305,000 acres in farms. Using the ratio of mean farmland acres to mean baseline farms, the preferred estimate corresponds to roughly 2,650 acres of farmland associated with continued farm operations per 100 baseline farms over five years. For the average county in the preferred sample, with about 708 baseline farms, this translates into about 44 additional farms over five years, equivalent to approximately 19,000 acres of farmland tied to operating farms. These acreage calculations are intended only to provide scale and context for the magnitude of the farm-count estimates. They do not imply that all associated acreage would otherwise leave agriculture, nor do they imply that the estimates measure changes in food production directly.

Adding farm demographics and farm structure reduces the no-control magnitude, but it does not change the sign or statistical significance of the estimates. For log farm-count change, the coefficient moves from 0.080 in the fixed-effects-only specification to 0.052 in the preferred model. For net farm-count loss, the corresponding estimate moves from -0.234 to -0.155 . This pattern suggests that some of the raw IV relationship is related to observable farm structure, but not that the stabilization result is generated by a single control choice. Although the descriptive bridge analysis shows no visible break in the off-farm-work series after the Census definition changes in 2017, we also implement a regression-based slope-break diagnostic. Specifically, we allow the second-stage effect of instrumented off-farm work to differ for intervals beginning in 2017 by interacting off-farm work with an indicator for the post-2017 measurement period. The evidence does not suggest that the definition change drives the results. The post-2017 interaction is close to zero and statistically insignificant for both log farm-count change and net farm-count loss (Appendix Table [A.4](#)).

Adding population growth as a local-economy control leaves the signs and statistical

Table 2: Private Commuting-Flow IV, Farm-Count Change, and Net Farm-Count Loss

	(1) OLS	(2) IV farm demographics	(3) IV farm structure	(4) IV preferred
<i>Panel A. First stage</i>				
Private commuting-flow IV	–	-0.292*** (0.071)	-0.248*** (0.068)	-0.235*** (0.065)
First-stage F	–	16.93	13.32	13.00
<i>Panel B. Log Farm-Count Change</i>				
Off-farm work	0.00003 (0.0005)	0.047*** (0.013)	0.051*** (0.016)	0.052*** (0.016)
95% CI	[-0.001, 0.001]	[0.022, 0.073]	[0.020, 0.082]	[0.020, 0.083]
Anderson–Rubin 95% CI	–	[0.029, 0.093]	[0.031, 0.113]	[0.031, 0.116]
Implied farm-count effect over five years (%)	0.0	4.8	5.2	5.3
Outcome mean	-0.030	-0.029	-0.030	-0.030
<i>Panel C. Net Farm-Count Loss</i>				
Off-farm work	0.001 (0.001)	-0.136*** (0.040)	-0.150*** (0.049)	-0.155*** (0.051)
95% CI	[-0.002, 0.003]	[-0.214, -0.058]	[-0.247, -0.054]	[-0.255, -0.054]
Anderson–Rubin 95% CI	–	[-0.271, -0.080]	[-0.337, -0.087]	[-0.354, -0.088]
Effect on net-loss probability (p.p.)	0.1	-13.6	-15.0	-15.5
Outcome mean	0.616	0.613	0.615	0.616
Observations	11,810	12,225	12,106	11,813
County clusters	2,992	3,074	3,060	2,993
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Preferred controls	Yes	No	No	Yes

Notes: The table reports estimates using the integrated 2002–2022 off-farm-work series. The unit of observation is a county–Census-interval, where baseline years are 2002, 2007, 2012, and 2017. Column (1) reports the ordinary least-squares association between observed off-farm work and the outcome using county fixed effects, Census-year fixed effects, and the preferred control vector. Columns (2)–(4) report two-stage least-squares estimates using the standardized private commuting-flow instrument. Panel A reports the first-stage relationship between off-farm work and the instrument for the IV columns. The first-stage F -statistic is the cluster-robust excluded-instrument Wald statistic from the first-stage regression. Panels B and C report estimates for log farm-count change and an indicator for net farm-count loss, respectively. Log farm-count change is measured from the baseline Census year to the next Census year. Net farm-count loss equals one if the county has fewer farms in the next Census year than in the baseline Census year. Off-farm work is measured in percentage points. For the log-change outcome, the implied farm-count effect over five years is computed as $100[\exp(\hat{\beta}) - 1]$. For the net-loss outcome, the probability-effect row reports $100\hat{\beta}$, so values are percentage-point changes in the probability that a county loses farms over the five-year Census interval. Conventional 95 percent confidence intervals use county-clustered standard errors. Anderson–Rubin confidence intervals are reported only for the IV columns and are computed by grid inversion of the cluster-robust reduced-form test for the null value of the endogenous coefficient. Control sets in columns (2)–(4) are cumulative: farm demographics adds baseline farm count, family-farm share, operator age, and operator age squared; farm structure adds land and building value, farmland share, and irrigated farmland; the preferred model adds government payments per farm, the coefficient of variation of net farm income, and small- and large-farm sales shares. Columns use the model-specific complete-case sample implied by the listed controls. Standard errors are clustered by county and shown in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

significance of the estimates unchanged (Appendix Tables [A.6](#)). The estimates are slightly smaller, 0.050 for log farm-count change and -0.147 for net farm-count loss, which is consistent with local economic conditions being related to both commuting-linked job access and farm-count adjustment. Adding detailed commodity-share controls also leaves the estimates close to the preferred model, with coefficients of 0.056 and -0.166 .

Because the 2007 Census expanded enumeration of individual farm and ranch operators on American Indian reservations, Appendix Table [A.5](#) reports the preferred specification excluding counties in the Navajo Nation and nearby reservation areas that experienced large enumeration-related farm-count increases. The estimates are essentially unchanged: the log farm-count coefficient remains 0.052, while the net-loss coefficient becomes slightly larger in magnitude. This implies the main results are not driven by the reservation-enumeration visible in the descriptive evidence.

Replacing county fixed effects with commuting-zone fixed effects leaves the results qualitatively unchanged (Appendix Table [A.7](#)). The log-change estimate falls from 0.052 to 0.037, and the net-loss estimate falls from -0.155 to -0.111 . These estimates retain the same signs and remain about 70–72 percent as large as the preferred county fixed-effects estimates. The commuting-zone specification also strengthens the first stage: the preferred first-stage coefficient increases in magnitude from -0.235 to -0.318 , and the first-stage F -statistic rises from 13.0 to 21.3. This suggests that the result are not only restricted by idiosyncratic within-county variation and might have broader geographical implications.

The channel diagnostics show that the private commuting-flow instrument is related to some observable local conditions, but the magnitudes are small relative to the variation in those outcomes (Appendix Table [A.8](#)). A one-standard-deviation increase in the instrument changes population density, population growth, land values, farmland share, and net farm income by only -0.003 , 0.024, -0.038 , 0.003, and 0.071 standard deviations, respectively. It is suggestive evidence that the instrument is not mainly operating through large changes in land pressure, population adjustment, or measured farm-income conditions. Together with the control-sensitivity results, this evidence strengthens our main interpretation.

Holding all else in the preferred specification fixed, Appendix Table [A.9](#) decomposes the aggregate IV estimate by replacing the outcome with net farm-count changes by size and commodity category, each scaled by baseline total farms. Because the dependent variables are measured as category-specific net changes per 100 baseline farms, a positive coefficient means a larger net count in that category over the Census interval, while a negative coefficient means a smaller net count. The estimates are suggestive evidence of heterogeneous effects across farm types. The total-farm coefficient is 6.171, and almost all of this estimate comes from small farms: the small-farm coefficient is 6.136, while

the medium-farm estimate is negative and statistically insignificant and the large-farm estimate is much smaller. By commodity, positive coefficients appear for other crops, cattle/dairy, and other livestock, while the field-crop estimate is small and statistically insignificant. For example, the cattle/dairy coefficient indicates that higher instrumented off-farm work is associated with a larger net count of cattle/dairy farms per 100 baseline farms. This pattern is consistent with prior dairy-specific evidence that exit pressure is especially concentrated among smaller operations, where scale, efficiency, succession conditions shape continuation decisions (Dong et al., 2016; Foltz, 2004).

The nonlinear IV robustness check produces marginal effects that are consistent with the linear IV estimates over the relevant range of the off-farm-work distribution (Appendix Table A.10). The similar results found in the nonlinear model reduces concern that the linear estimates are masking a different relationship at typical values of off-farm work. The heterogeneity diagnostics ask whether the average result is driven by a particular region or Census interval. Region-specific estimates do not provide strong evidence that the effect differs systematically across broad Census regions (Figure A.2). Year-specific estimates also do not indicate that the average result is driven by a single interval (Figure A.3).

The main results and the additional robustness and supporting evidence support the stabilization side of the off-farm-work ambiguity emphasized by Goetz and Debertin (2001). The IV estimates suggest that the component of off-farm work induced by commuting-linked private labor-market access slows farm-count decline and lowers the probability of net farm-count loss. This interpretation is consistent with the farm-household labor-allocation framework in Chang et al. (2012): off-farm work is a viable adjustment margin only when outside employment opportunities can be combined with continued farm operation. It also complements recent evidence that labor-market shocks can reshape farm employment, labor expenditure, mechanization, and land use (Ifft and Jodlowski, 2022). The decomposition results strengthen this interpretation because the estimated stabilization is concentrated among small farms, where household income diversification is most likely to matter for persistence. At the same time, the negative first stage and the flat aggregate off-farm-work series show that off-farm employment is not an unlimited buffer against structural change. Better private labor-market opportunities may stabilize farms when they are accessible and compatible with farm operation, but they may also draw marginal operators fully out of farming when the outside option requires a more complete transition away from agriculture.

6 Conclusion

This paper studies whether off-farm work stabilizes county farm counts in recent years of the U.S. farm economy. Using county Census of Agriculture panels from 2002 to 2022, we instrument high-intensity off-farm work with predicted private-sector labor demand accessible through commuting flows. The preferred specification includes county and year fixed effects and a parsimonious set of farm-structure controls.

The evidence supports the stabilization channel. The IV estimates show a clear and robust pattern: instrumented off-farm work is associated with less negative log farm-count change and a lower probability of net farm-count loss. The preferred estimates are robust to cumulative farm controls, local economic controls, commodity controls, commuting-zone fixed effects, measurement-bridge diagnostics, and nonlinear IV specifications. Outcome-decomposition results show that the stabilizing effect is concentrated in small farms, consistent with the idea that household income diversification is most important where farms are less likely to be fully supported by farm income alone.

The results should be interpreted as county-level evidence on net farm-count adjustment, not as individual-level evidence on operator exit. Farm-count changes combine exits, entries, mergers, reclassifications, and succession. The IV estimates are also specific to the component of measured off-farm work shifted by private labor-market access through commuting networks. These qualifications clarify the economic margin: off-farm work is one way farm households adjust to agricultural risk and structural pressure, but its stabilizing role depends on whether nonfarm employment can be combined with continued farm operation.

The findings also help explain an important tension in the data. Farm counts declined substantially, yet the off-farm-work share did not rise sharply over the same period. Off-farm work is therefore not an unlimited buffer against farm decline. Its stabilizing potential depends on access to suitable nonfarm work, the cost of commuting, household time constraints, and whether nonfarm jobs complement or replace farm operation. In places where the commute to better job markets is too costly, or where nonfarm employment requires a full exit from agriculture, private labor demand may accelerate farm-count loss even as it reduces measured off-farm work among remaining operators.

The policy implication is that farm policy and rural development policy are linked. Farm persistence depends not only on agricultural returns, subsidies, or commodity conditions, but also on the labor-market opportunities available to farm households. This implication is consistent with rural-development work emphasizing that policies for rural prosperity should connect the farm sector with the broader nonfarm rural economy (Goetz et al., 2018; Partridge and Rickman, 2007). It is also consistent with evidence that off-farm work

provides many farm households with more stable income, health insurance, retirement benefits, and other forms of economic security (Whitt and Todd, 2020). Policies that improve rural labor-market access, commuting connectivity, broadband-enabled work, and local job quality may therefore help farm households diversify income without leaving agriculture entirely (Mack et al., 2024; Whitacre et al., 2014). At the same time, rural development policy should not treat nonfarm employment as a universal substitute for farm-sector support. The relevant objective is to expand the set of opportunities that allow households to combine farm operation with stable outside earnings.

Future research should move closer to the household margin. Farm-level or household-level panel data linking off-farm earnings, commuting distance, household labor allocation, and farm survival would allow sharper tests of the mechanisms suggested here. A second useful direction is to measure labor-market access more directly, distinguishing nearby employment growth from growth that is technically within commuting networks but too distant or costly to support joint farm and off-farm work. These extensions would clarify when off-farm work acts as a stabilizer and when it becomes part of the pathway out of farming.

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Appendix A - Tables and Figures

Table A.1: Variable Definitions and Measurement Differences Relative to Goetz and Debertin (2001)

Variable	Goetz and Debertin (2001)	Updated measure in this paper	Interpretation note
Farm-count change	Log change in BEA farm proprietorships, 1987–1997.	Adjacent-Census log change in NASS county farm counts, $\log(F_{i,t+5}/F_{it})$, for $t \in \{2002, 2007, 2012, 2017\}$.	Measures net county farm-count adjustment over five-year Census intervals, not individual farm exits or proprietor exits.
Net farm-count loss	County-level indicator for whether farm proprietorships declined.	Indicator equal to one if the number of farms in county i declines between Census years t and $t + 5$.	Binary measure of net county farm-count decline. It captures whether the county loses farms on net, not which farms exit.
Off-farm work	Share or predicted share of farmers working off farm 150 or more days.	Integrated off-farm-work series measuring the share of farm operators or producers reporting 200 or more days of off-farm work. The series uses the principal-operator measure through 2012 and the later producer-based measure thereafter.	Closest consistent contemporary measure available over the 2002–2022 panel. The threshold differs from the original 150-day measure, and the post-2017 definition is producer-based rather than principal-operator-based.
Instrument for off-farm work	No commuting-flow shift-share instrument.	Standardized private commuting-flow instrument constructed from predetermined county-to-destination commuting shares and predicted private-sector employment growth in commuting-linked destinations.	New source of identifying variation. The instrument shifts off-farm work through predicted private labor demand accessible through commuting networks.
Family farms	Share of farms family-owned.	Share of farms classified as family farms.	Conceptually comparable control for farm ownership structure.
Baseline farm count	Number of farm proprietorships.	Number of farms in the baseline Census year.	Controls for county farm-sector scale. The updated analysis refers to farms rather than proprietorships.
Operator age	Operator demographic controls.	Operator age and operator age squared.	Captures nonlinear differences in operator life-cycle structure across county farm sectors.
Value of land and buildings	Average value of land and buildings per farm.	County-level value of land and buildings measure used in the Census-based panel.	Controls for land-market and capital-value differences that may affect both farm viability and adjustment.
Farmland share	Percent of county land in farms.	Share of county land in farms.	Controls for the intensity of agricultural land use in the county.
Irrigated farmland	Irrigation-related farm-structure information where available.	Irrigated farmland measure included in the preferred control vector.	Controls for production-system differences and land productivity margins not captured by farm counts alone.
Government payments	Federal farm-program payments per farm, measured in thousands of dollars.	Government payments per farm, measured in thousands of dollars.	Comparable scaling; controls for public support to the local farm sector.
Farm-income volatility	Coefficient of variation of net farm income around the baseline period.	Coefficient of variation of net farm income in the updated panel, restricted to finite and valid values.	Captures instability in local farm income, which may affect both off-farm work and farm-count adjustment.
Farm-size structure	Farm-size or farm-type controls available in the original county data.	Shares of farms in small and large sales classes.	Controls for baseline farm-size composition. This is important because off-farm work may be more common among smaller or part-time operations.
Commodity composition	Sommer–Hines agricultural-dependence indicators.	Detailed commodity-share controls used in appendix robustness specifications.	Not a literal replication of the original classification. Used to test whether estimates are driven by differential exposure to agricultural production systems.
Local economic conditions	County labor-market and demographic controls in the original specification.	Population density, population growth, and unemployment included in appendix robustness specifications.	These variables may be mechanisms of private labor-demand shocks, so they are excluded from the preferred specification and added only as sensitivity checks.
Fixed effects	Cross-sectional comparison over the 1987–1997 period.	County fixed effects and Census-year fixed effects in a 2002–2022 county panel.	County fixed effects absorb time-invariant county differences; year fixed effects absorb national Census-wave shocks. Identification comes from within-county changes over time.

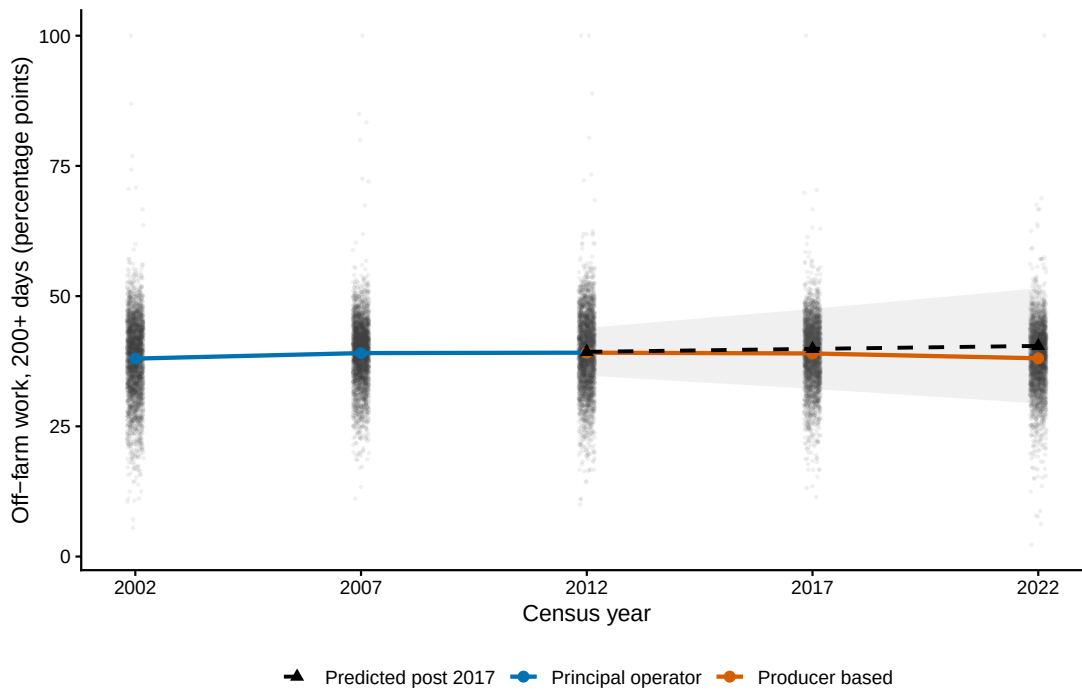
Notes: The updated analysis is not a literal replication of Goetz and Debertin (2001). The outcome is based on NASS farm counts rather than BEA farm proprietorships, the panel covers 2002–2022 rather than 1987–1997, and the off-farm-work measure changes from a 150-day measure to the available 200-day Census measure. The empirical design also differs: this paper estimates county-panel fixed-effects and instrumental-variables specifications using a private commuting-flow instrument for off-farm work. Commodity-share controls and local economic controls are included in robustness specifications rather than in the preferred specification.

Table A.2: Integrated Off-Farm-Work Series by Census Year

Year	Counties	Mean	Std. dev.	Measure source
2002	3,072	38.01	7.62	Principal operators
2007	3,075	39.06	6.29	Principal operators
2012	3,076	39.14	7.40	Principal operators
2017	3,011	38.99	6.27	Producers
2022	3,011	38.06	6.31	Producers

Notes: The table describes the integrated off-farm-work series used in the 2002–2022 panel. Values are county means of the share of operators or producers working 200 or more days off operation, reported in percentage points. The series uses the principal-operator measure through 2012 and the producer-based measure from 2017 onward. The 2022 value is shown for the measurement bridge diagnostic; in the regression panel, 2022 enters as the terminal outcome year for the 2017–2022 interval rather than as a baseline treatment year. Year fixed effects in the regression specifications absorb common Census-year level differences in measurement.

Figure A.1: Off-Farm-Work Measurement Bridge Diagnostic



Notes: Gray dots show county-level observations of the integrated off-farm-work series. The blue line reports the county mean for the principal-operator measure used through 2012. The red line reports the county mean for the producer-based measure used from 2017 onward and is anchored at 2012 to facilitate visual comparison across definitions. The dashed line shows the predicted post-2017 path obtained by extrapolating the 2002–2012 mean trend; the shaded area gives the corresponding 95% confidence interval. Off-farm work is measured as the share of operators or producers working 200 or more days off operation, reported in percentage points.

Table A.3: Private Commuting-Flow IV and Preferred Explanatory Variables

Variable	Coefficient	Std. error	Observations
Baseline farm count	0.027	(0.044)	11,810
Family-farm share	0.092***	(0.022)	11,810
Operator age	-0.099***	(0.017)	11,810
Operator age squared	-0.097***	(0.017)	11,810
Value of land and buildings	-0.113***	(0.026)	11,810
Farmland share	0.019	(0.053)	11,810
Irrigated farmland	0.107***	(0.029)	11,810
Government payments per farm	-0.061***	(0.014)	11,810
CV net farm income	-0.001	(0.004)	11,810
Small-farm sales share	-0.013	(0.029)	11,810
Large-farm sales share	-0.084**	(0.034)	11,810

Notes: Each row reports a separate county fixed-effects regression of the standardized private commuting-flow instrument on one standardized explanatory variable. All specifications include county and Census-year fixed effects and cluster standard errors by county. The table is a diagnostic for whether the instrument is systematically related to observable preferred controls. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Slope-Break Diagnostic for the Integrated Off-Farm-Work Series

	(1) Log farm-count change	(2) Net farm-count loss
Off-farm work	0.052*** (0.015)	-0.158*** (0.048)
Off-farm work $\times$ post-2017	0.001 (0.008)	-0.007 (0.025)
Observations	11,810	11,810
County clusters	2,992	2,992
County FE	Yes	Yes
Year FE	Yes	Yes
Preferred controls	Yes	Yes

Notes: The table reports a diagnostic specification that allows the second-stage effect of instrumented off-farm work to differ in the post-2017 measurement period. The interaction term equals instrumented off-farm work multiplied by an indicator for intervals beginning in 2017. All specifications use county and year fixed effects, preferred controls, the standardized private commuting-flow instrument, and county-clustered standard errors. Off-farm work is measured in percentage points. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Reservation-Enumeration Robustness of the County Fixed-Effects IV Estimates

	(1) Preferred	(2) Excluding reservation-enumeration counties
<i>Panel A. First stage</i>		
Private commuting-flow IV	-0.236*** (0.065)	-0.229*** (0.065)
First-stage F	13.10	12.44
<i>Panel B. Second stage</i>		
Log farm-count change	0.051*** (0.016)	0.052*** (0.016)
Implied farm-count effect over five years (%)	5.3	5.3
Net farm-count loss	-0.154*** (0.051)	-0.168*** (0.055)
Effect on net-loss probability (p.p.)	-15.4	-16.8
Observations	11,810	11,781
County clusters	2,992	2,984
County FE	Yes	Yes
Year FE	Yes	Yes
Preferred controls	Yes	Yes

Notes: The table reports a data-quality robustness check for the preferred county fixed-effects IV specification. Column (1) reports the preferred specification on the robustness-table sample. Column (2) excludes counties affected by large reservation-enumeration-related farm-count increases around the 2007 Census: Apache County, AZ; Coconino County, AZ; Navajo County, AZ; Cibola County, NM; McKinley County, NM; Sandoval County, NM; San Juan County, NM; and San Juan County, UT. All specifications use the integrated 2002–2022 panel, county fixed effects, Census-year fixed effects, preferred controls, the standardized private commuting-flow instrument, and county-clustered standard errors. Off-farm work is measured in percentage points. For the log-change outcome, the implied farm-count effect over five years is computed as $100[\exp(\hat{\beta}) - 1]$. For the net-loss outcome, the probability-effect row reports $100\hat{\beta}$, so values are percentage-point changes in the probability that a county loses farms over the five-year Census interval. The reservation-exclusion exercise is motivated by changes in Census enumeration of individual farm and ranch operators on American Indian reservations; it is not part of the preferred identifying specification. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Control Sensitivity of the County Fixed-Effects IV Estimates

	(1) Preferred	(2) + Local economy	(3) + Commodities
<i>Panel A. First stage</i>			
Private commuting-flow IV	-0.235*** (0.065)	-0.241*** (0.065)	-0.227*** (0.066)
First-stage F	13.00	13.56	11.84
<i>Panel B. Second stage</i>			
Log farm-count change	0.052*** (0.016)	0.050*** (0.015)	0.056*** (0.018)
Implied farm-count effect over five years (%)	5.3	5.1	5.8
Net farm-count loss	-0.155*** (0.051)	-0.147*** (0.049)	-0.166*** (0.056)
Effect on net-loss probability (p.p.)	-15.5	-14.7	-16.6
Observations	11,813	11,813	11,813
County clusters	2,993	2,993	2,993
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The table reports control-sensitivity checks for the county fixed-effects IV specification. Columns vary the set of controls while keeping the integrated 2002–2022 panel, county and year fixed effects, the standardized private commuting-flow instrument, and county-clustered standard errors. Column (1) reports the preferred specification. The local-economy specification adds population growth to the preferred model. The commodity specification adds mutually exclusive commodity-share controls to the preferred model. Off-farm work is measured in percentage points. For the log-change outcome, the implied farm-count effect over five years is computed as $100[\exp(\hat{\beta}) - 1]$. For the net-loss outcome, the probability-effect row reports $100\hat{\beta}$, so values are percentage-point changes in the probability that a county loses farms over the five-year Census interval. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Private Commuting-Flow IV Estimates: Commuting-Zone Fixed-Effects Robustness

	(1)	(2)	(3)
	Farm demographics	Farm structure	Preferred
<i>Panel A. Log farm-count change</i>			
Private commuting-flow IV	-0.255*** (0.075)	-0.274*** (0.075)	-0.318*** (0.069)
First-stage F	11.39	13.25	21.25
Off-farm work	0.050*** (0.017)	0.045*** (0.015)	0.037*** (0.010)
Implied farm-count effect over five years (%)	5.1	4.6	3.8
<i>Panel B. Net farm-count loss</i>			
Private commuting-flow IV	-0.255*** (0.075)	-0.274*** (0.075)	-0.318*** (0.069)
First-stage F	11.39	13.25	21.25
Off-farm work	-0.144*** (0.049)	-0.130*** (0.043)	-0.111*** (0.030)
Effect on net-loss probability (p.p.)	-14.4	-13.0	-11.1
Observations	12,222	12,113	11,827
Commuting-zone clusters	696	695	694
Commuting-zone FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The table repeats the main IV specification replacing county fixed effects with commuting-zone fixed effects. Standard errors are clustered by commuting zone. Control sets are cumulative and defined as in the main IV tables. Off-farm work is measured in percentage points. For the log-change outcome, the implied farm-count effect over five years is computed as $100[\exp(\hat{\beta}) - 1]$. For the net-loss outcome, the probability-effect row reports $100\hat{\beta}$, so values are percentage-point changes in the probability that a county loses farms over the five-year Census interval. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Reduced-Form Diagnostics for Observable Exclusion Channels

	(1) Population density	(2) Population growth	(3) Value of land and buildings	(4) Farmland share	(5) Net farm income
Private commuting-flow IV	-0.005*** (0.001)	0.001** (0.001)	-0.026*** (0.003)	0.001 (0.001)	0.566*** (0.092)
Effect in outcome standard deviations	-0.003	0.024	-0.038	0.003	0.071
Outcome transform	log	level	log	share	asinh
Raw outcome mean	154.277	1.014	1017.406	52.427	14908.889
Observations	11,813	11,813	11,813	11,813	11,813
County clusters	2,993	2,993	2,993	2,993	2,993
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Preferred Controls	Yes	Yes	Yes	Yes	Yes

Notes: The table reports reduced-form diagnostics for observable channels that could represent alternative pathways from the private labor-demand instrument to farm-count adjustment. Each column estimates a regression of the listed channel on the standardized private commuting-flow instrument, county fixed effects, year fixed effects, and the preferred control vector. When the channel variable is itself part of the preferred control vector, it is omitted from that column's controls. The same preferred IV sample is used across columns. Standard errors are clustered by county and shown in parentheses. The effect in outcome standard deviations divides the coefficient by the standard deviation of the transformed dependent variable; the instrument is standardized. These regressions are diagnostics for observable alternative channels and are not second-stage IV estimates. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Outcome Decomposition of Private Commuting-Flow IV Estimates

	(1) Total farms	(2) Small farms	(3) Medium farms	(4) Large farms	(5) Field crops	(6) Other crops	(7) Cattle/dairy	(8) Other livestock
<i>Panel A. Second stage</i>								
Instrumented off-farm work	6.171*** (1.920)	6.136*** (1.857)	-0.394 (0.287)	0.428* (0.253)	0.044 (0.280)	4.336*** (1.442)	2.536** (1.138)	2.373*** (0.800)
Annualized effect per 100 baseline farms	1.234	1.227	-0.079	0.086	0.009	0.867	0.507	0.475
Outcome mean	-1.942	-2.006	-0.810	0.874	-0.331	0.681	-3.923	0.377
Observations	11,761	11,761	11,761	11,761	11,761	11,761	11,761	11,761
County clusters	2,977	2,977	2,977	2,977	2,977	2,977	2,977	2,977
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preferred Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

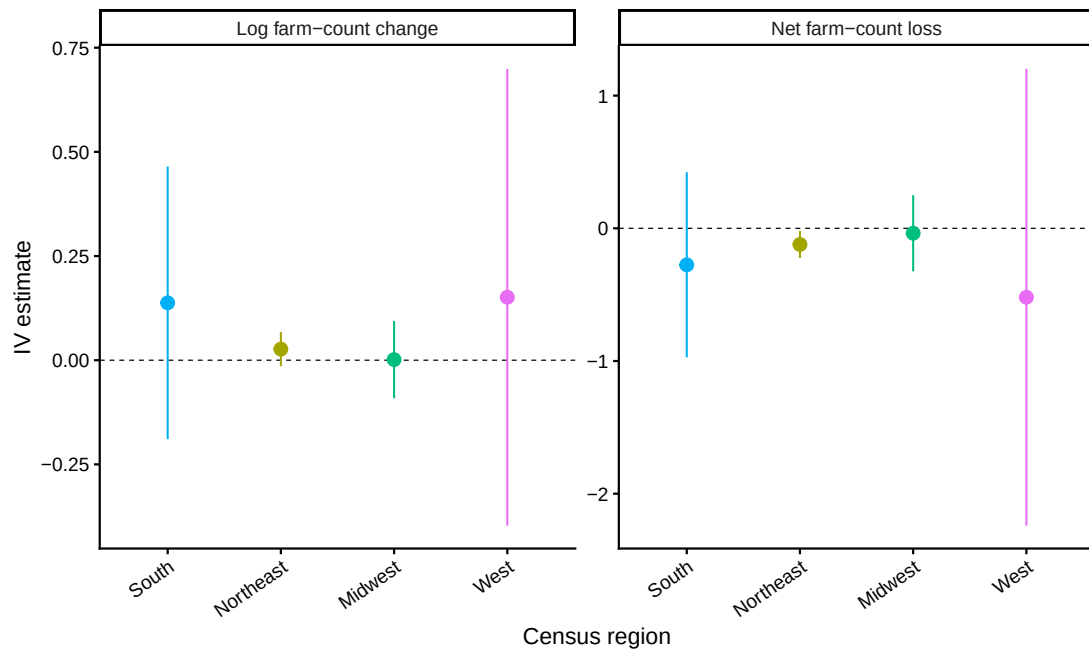
Notes: The table reports outcome-decomposition IV estimates holding the endogenous variable, instrument, fixed effects, controls, and estimation sample fixed while changing the dependent variable. Each dependent variable is the net change in the category-specific farm count from the baseline Census year to the next Census year, scaled by baseline total farms and multiplied by 100. Coefficients therefore report the effect of a one percentage-point increase in instrumented off-farm work on the number of additional farms in each category per 100 baseline farms over the five-year Census interval. The annualized-effect row divides the coefficient by five and reports the implied effect per 100 baseline farms per year. This scaling preserves counties with zero farms in a category and allows the decomposition to use the same first-stage sample across columns. Off-farm work is instrumented by the standardized private commuting-flow IV. Standard errors are clustered by county. Off-farm work is measured in percentage points. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Nonlinear Private Commuting-Flow IV Robustness

	(1)	(2)
	Log farm-count change	Net farm-count loss
Off-farm work, centered	0.053*** (0.015)	-0.154*** (0.044)
Off-farm work ² , centered	-0.000 (0.002)	0.000 (0.008)
Marginal effect at p25	0.057** (0.024)	-0.156** (0.074)
Implied effect at p25	5.9	-15.6
Marginal effect at median	0.052*** (0.015)	-0.154*** (0.045)
Implied effect at median	5.3	-15.4
Marginal effect at p75	0.048* (0.027)	-0.153* (0.085)
Implied effect at p75	4.9	-15.3
Observations	11,810	11,810
County clusters	2,992	2,992
County FE	Yes	Yes
Year FE	Yes	Yes
Controls	Preferred	Preferred

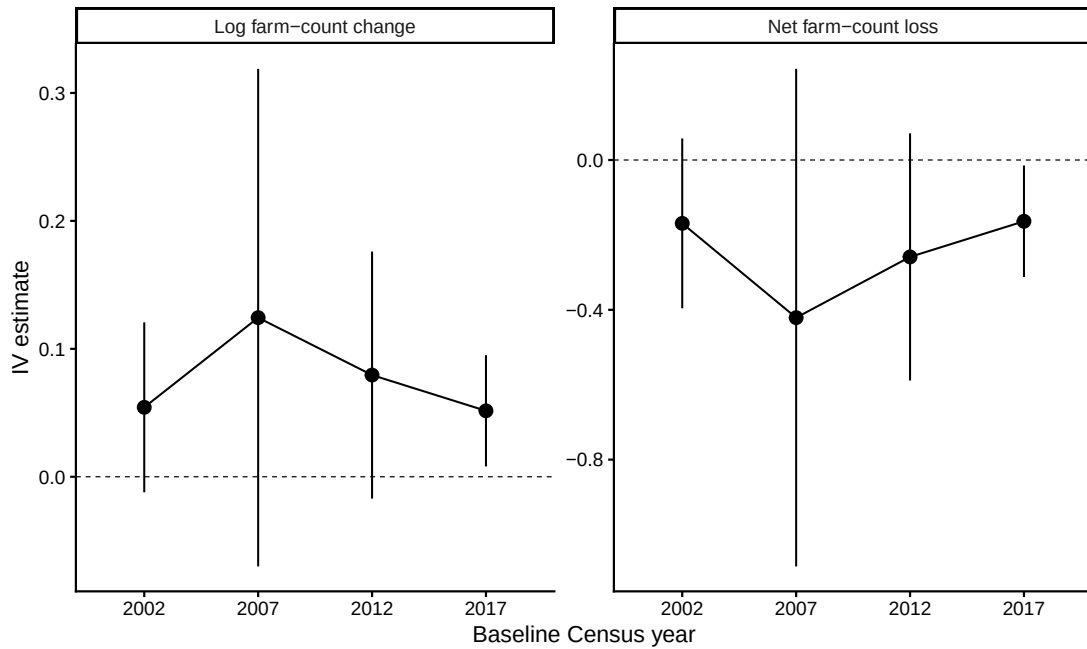
Notes: The table reports a nonlinear two-stage least-squares version of the preferred specification. Off-farm work and squared off-farm work are treated as endogenous and are instrumented with the centered private commuting-flow instrument and its square. Off-farm work is centered before squaring. Marginal effects are computed at the 25th percentile, median, and 75th percentile of off-farm work. All specifications include county and year fixed effects and preferred controls. Standard errors are clustered by county. Off-farm work is measured in percentage points. For column (1), the implied-effect rows report $100[\exp(\widehat{ME}) - 1]$, so values are percentage effects on farm counts over the five-year Census interval. For column (2), the implied-effect rows report $100\widehat{ME}$, so values are percentage-point changes in the probability of net farm-count loss over the five-year Census interval. The nonlinear specification is a robustness diagnostic; the preferred specification remains the linear IV model. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.2: IV Effects by Census Region



Notes: The figure reports region-specific IV estimates from specifications that allow the effect of off-farm work to vary by Census region. The left panel uses log farm-count change as the outcome; the right panel uses an indicator for net farm-count loss. Points show estimated effects of instrumented off-farm work, and vertical bars show 95% confidence intervals. The dashed horizontal line marks zero. Off-farm work is measured in percentage points.

Figure A.3: IV Effects by Baseline Census Year



Notes: The figure reports year-specific IV estimates by baseline Census year. The left panel uses log farm-count change as the outcome; the right panel uses an indicator for net farm-count loss. Baseline years correspond to the beginning of each five-year Census interval: 2002–2007, 2007–2012, 2012–2017, and 2017–2022. Points show estimated effects of instrumented off-farm work, and vertical bars show 95% confidence intervals. The dashed horizontal line marks zero. Off-farm work is measured in percentage points.