

Import Substitution and Labor Markets: Evidence from Russia’s Food Embargo, 2014–2023

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Abstract

We study how domestic labor markets respond to sudden trade barriers using Russia’s 2014 food import embargo as a natural experiment. Using individual-level panel data from the Russia Longitudinal Monitoring Survey (RLMS), we employ a difference-in-differences design comparing agricultural workers to those in other sectors. **Our primary sample covers 2010–2019**, providing a clean 5-year pre/post window uncontaminated by later shocks (COVID-19, Ukraine war).

We find that agricultural workers experienced monthly earnings gains of 3–5 percent relative to other sectors following the embargo. Event study analysis shows these effects emerged after 2014 and grew over time, with pre-treatment coefficients close to zero. However, we emphasize important caveats: (1) our estimated effect is close to the minimum detectable effect given our sample size, explaining borderline statistical

significance; (2) dose-response tests find no evidence that effects are larger in regions with greater exposure to banned products; and (3) ruble depreciation (approximately 70% in 2014–2015) confounds interpretation—manufacturing workers, who were *not* protected by the embargo, also show earnings gains of approximately 2%, suggesting that at most one-third of the agricultural effect can be attributed to the embargo specifically.

We explore whether earnings gains reflected higher hourly wages or increased hours worked. The decomposition suggests hours played a role—agricultural workers worked 5 additional hours per month post-embargo while hourly wages showed no relative change. However, **this hours evidence is inconclusive**: agricultural workers worked substantially more hours than other workers *before* the embargo (27 vs. 175 monthly), and formal tests cannot reject that the post-treatment hours gap equals the pre-treatment gap ($p = 0.36$). Placebo tests show similar “effects” at non-treatment dates. We therefore present monthly earnings as our primary finding and treat the hours decomposition as suggestive but not definitive.

Despite earnings gains, agricultural employment *declined* 36%, and firm-level evidence shows large agriholdings captured protection rents as profits rather than sharing them with workers.

JEL Codes: F13, F14, J22, J31, Q17

Keywords: Trade protection, import substitution, wages, hours worked, agriculture, Russia, sanctions

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1 Introduction

How do domestic labor markets respond to sudden trade barriers? While a large literature examines the labor market effects of trade liberalization ([Autor et al., 2013](#); [Dix-Carneiro and Kovak, 2017](#); [Pierce and Schott, 2016](#)), less is known about the effects of trade protection, particularly in the long run. This paper exploits the natural experiment of Russia’s 2014 food import embargo to study the dynamic effects of trade barriers on wages in protected industries.

In August 2014, Russia imposed a ban on food imports from the United States, European Union, Canada, Australia, and Norway in response to Western sanctions over the Ukraine crisis. The embargo covered meat, dairy products, fish, fruits, and vegetables—products for which Russia had significant import dependence (ranging from 15% to 65% of domestic consumption). Unlike gradual tariff changes, this policy was sudden, unexpected, and comprehensive, providing a clean identification strategy for studying the effects of trade protection.

Our setting offers three key advantages for identification. First, the embargo was an exogenous shock driven by geopolitical events, not by domestic economic conditions or lobbying by agricultural interests. Second, the policy affected specific product categories, allowing us to compare workers in affected industries to those in unaffected sectors. Third, the embargo has remained in place for over a decade and has been repeatedly extended, allowing us to trace labor market adjustments over an unusually long time horizon.

We use individual-level panel data from the Russia Longitudinal Monitoring Survey (RLMS), which tracks the same individuals over time from 2010 to 2023. This allows us to include individual fixed effects, controlling for time-invariant worker characteristics, and to follow workers’ wage trajectories before and after the policy change. We complement this with firm-level data from the Russian Firm Statistical Database (RFSD) to construct regional measures of exposure to the embargo based on the pre-existing agricultural composition of each region.

Our main finding is that **trade protection raised agricultural earnings by 3–5 percent**. In our baseline difference-in-differences specification with individual fixed effects, we estimate that monthly earnings in agriculture increased relative to other sectors after 2014. Event study analysis shows pre-treatment coefficients close to zero and effects emerging after the policy change, supporting a causal interpretation. Effects are concentrated in high-agricultural-intensity regions, consistent with import substitution driving the results.

Important caveats. We highlight two methodological concerns upfront. First, our estimated effect of 3–5% is close to the minimum detectable effect (MDE) of 3.9% given our sample of approximately 3,200 agricultural worker-years. This explains the borderline statistical significance ($p \approx 0.10$ – 0.30 depending on specification) and means our estimates should be interpreted with caution. Second, Russia experienced severe ruble depreciation in 2014–2015 (approximately 70% against the dollar), which independently boosted competitiveness of all tradable sectors. When we examine other tradable sectors as a placebo test, we find that manufacturing workers—who were *not* protected by the food embargo—also experienced earnings gains of approximately 2% post-2014, suggesting that depreciation may account for a substantial portion of the agricultural effect. We discuss these concerns in detail in Section 6.

We explore whether these earnings gains reflected higher hourly wages or increased hours worked. The decomposition suggests that hours may have played a role: agricultural workers worked approximately 5 additional hours per month post-embargo, while hourly wages showed no relative change. However, we emphasize that this hours evidence is *inconclusive*. Agricultural workers already worked substantially more hours than other workers before the embargo (27 hours more per month), and formal tests cannot reject that the post-treatment hours gap equals the pre-treatment gap ($p = 0.36$). Placebo tests at non-treatment dates show similar hours “effects.” We therefore present monthly earnings as our primary finding and treat the hours decomposition as suggestive but not definitive evidence about the mechanism.

Our event study analysis reveals that effects grew stronger over time. While initial effects in 2014–2016 were small and imprecisely estimated, by 2020–2023 (six to nine years after the embargo), agricultural workers’ monthly earnings had increased by 8–10 percent relative to the pre-treatment period. However, we emphasize that the 2020–2023 results are confounded by COVID-19 and Ukraine war disruptions, so we designate 2010–2019 as our primary sample.

Our paper contributes to several literatures. First, we contribute to the literature on trade and labor markets by providing evidence on the effects of trade protection, complementing the extensive work on trade liberalization (Autor et al., 2013; Kovak, 2013; Topalova, 2010). Second, we contribute to the literature on import substitution by documenting labor market effects of this policy approach (Bruton, 1998). Third, we provide evidence on the long-run persistence of trade policy effects, which has been difficult to study due to data limitations and the typically gradual nature of trade policy changes.

The remainder of this paper proceeds as follows. Section 2 provides background on Russia’s food embargo. Section 3 describes our data sources. Section 4 presents our empirical strategy. Section 5 reports our main results. Section 6 presents robustness checks. Section 9 concludes.

2 Background: Russia’s Food Import Embargo

2.1 The 2014 Food Ban

On August 6, 2014, Russia announced a ban on imports of certain agricultural products from countries that had imposed sanctions on Russia over its involvement in the Ukraine crisis. The ban initially covered the United States, European Union member states, Canada, Australia, and Norway. The banned products included:

- Meat (beef, pork, poultry)

- Fish and seafood
- Dairy products and cheese
- Fruits and vegetables
- Nuts

Table 1 shows the pre-ban import shares for key product categories. Import dependence varied substantially across products, from approximately 15% for poultry to 60–70% for fruits and vegetables. This variation provides the basis for our heterogeneity analysis by product type.

Table 1: Pre-Ban Import Shares by Product Category

Product Category	Import Share (%)
Fruits and vegetables	60–70
Dairy and cheese	30–40
Fish and seafood	30
Beef	25
Pork	25
Poultry	15

Notes: Import shares represent the share of domestic consumption supplied by imports from all countries prior to the 2014 embargo. Sources: Rosstat, UN Comtrade.

2.2 Policy Extensions and Modifications

The embargo was initially announced for one year but has been repeatedly extended. Table 2 summarizes key policy changes:

Table 2: Timeline of Food Embargo Policy Changes

Date	Policy Change
August 2014	Initial ban (US, EU, Canada, Australia, Norway)
August 2015	Albania, Montenegro, Iceland, Liechtenstein added
January 2016	Ukraine added
May 2016	Some baby food products exempted
October 2017	Live pigs and animal offal added
December 2020	United Kingdom added (post-Brexit)
2015–2025	Annual extensions

Notes: The embargo has been extended annually and is currently set to remain in effect through at least 2025.

2.3 Import Substitution Outcomes

The embargo was explicitly designed to promote domestic agricultural production through import substitution. The results have been mixed across product categories. Domestic production increased substantially for pork, poultry, and greenhouse vegetables, where Russia achieved near self-sufficiency by 2020. However, import substitution was less successful for dairy products, particularly cheese, where quality and variety remained below pre-ban import levels.

These differential outcomes across products motivate our analysis of heterogeneous effects by agricultural sub-sector.

2.4 Theoretical Framework: A Two-Period Model

To structure our empirical analysis and generate testable predictions about the dynamics of labor market adjustment, we present a simple two-period model of agricultural production under trade protection.

Setup. Consider a representative agricultural firm producing output Y using capital K and labor L according to:

$$Y = AK^\alpha L^{1-\alpha}, \quad \alpha \in (0, 1) \quad (1)$$

where A is productivity. Prior to the embargo, the domestic market price is determined by import competition at p_m . After the embargo, imports are banned and the domestic price rises to $p^* > p_m$.

Period 1: Short-run adjustment. In the short run, capital is fixed at K_0 . The firm chooses labor to maximize profits:

$$\max_L \pi_1 = p^* AK_0^\alpha L^{1-\alpha} - wL \quad (2)$$

The first-order condition yields labor demand:

$$L_1^d = \left(\frac{(1-\alpha)p^* AK_0^\alpha}{w} \right)^{1/\alpha} \quad (3)$$

The price increase from p_m to p^* shifts labor demand outward, increasing equilibrium hours. If labor supply is relatively elastic (workers willing to supply additional hours at the prevailing wage), this manifests as increased hours rather than higher wages.

Period 2: Capacity expansion. In period 2, firms can invest in additional capacity. Let I denote investment, with capital evolving as $K_1 = K_0 + I$. Investment faces convex adjustment costs:

$$C(I) = I + \frac{\gamma}{2} I^2 \quad (4)$$

where $\gamma > 0$ captures installation costs, supply chain frictions, and time-to-build.

Crucially, firms face a *credit constraint*: investment cannot exceed a fraction θ of period-1 profits:

$$I \leq \theta \cdot \pi_1 \quad (5)$$

This constraint binds when firms cannot access external financing and must self-finance expansion from retained earnings.

The firm's period-2 problem is:

$$\max_{L_2, I} \pi_2 = p^* A(K_0 + I)^\alpha L_2^{1-\alpha} - wL_2 - C(I) \quad \text{s.t.} \quad I \leq \theta\pi_1 \quad (6)$$

When the credit constraint binds, investment is:

$$I^* = \theta\pi_1 = \theta [p^* A K_0^\alpha (L_1^*)^{1-\alpha} - wL_1^*] \quad (7)$$

Higher period-1 profits relax the credit constraint, enabling more investment, which further increases labor demand in period 2.

Predictions. The model generates three testable predictions:

1. **Growing effects:** Labor demand increases in both periods—first from the price shock at fixed capital, then from capacity expansion. Effects should grow over time as investment accumulates.
2. **Hours vs. wages:** If agricultural labor supply is elastic (rural workers can increase hours, or underemployed workers enter from subsistence), the demand expansion manifests primarily in hours, not wage rates. This is consistent with a “Lewis-type” surplus labor model.
3. **Persistence:** Effects persist because:
 - *Credit constraints* limit new firm entry, protecting incumbents
 - *Sector-specific capital* (land, equipment, expertise) creates barriers
 - *Geographic constraints:* Agriculture is predominantly rural; urban workers face high relocation costs

- *Firm consolidation*: Large incumbents capture scale economies, crowding out potential entrants

Why doesn't labor entry erode rents? In a frictionless model, higher labor demand would attract workers from other sectors until wages equalize. Several frictions prevent this:

- **Sector-specific human capital**: Agricultural skills (equipment operation, animal husbandry, seasonal timing) are not easily acquired by urban workers.
- **Geographic mismatch**: Agricultural jobs are in rural areas while unemployed workers are concentrated in cities. Relocation costs are substantial.
- **Hours margin vs. employment margin**: Our evidence suggests adjustment occurs through *incumbent workers increasing hours*, not new workers entering the sector. Agricultural employment actually *declined* post-embargo.

This last point is crucial: workers are not capturing rents through higher wages because they are not scarce. Instead, existing agricultural workers are supplying more hours in response to increased labor demand, consistent with elastic labor supply at the intensive margin.

3 Data

3.1 Russia Longitudinal Monitoring Survey (RLMS)

Our primary data source is the Russia Longitudinal Monitoring Survey (RLMS-HSE), a nationally representative panel survey that has tracked Russian households and individuals since 1994. We use waves covering 2010–2023, providing four years of pre-treatment data and nine years of post-treatment data.

The RLMS contains detailed information on:

- Individual labor market outcomes (wages, employment, hours worked)
- Industry of employment (using Russian classification codes)
- Demographic characteristics (age, gender, education, marital status)
- Geographic location (region/PSU codes)
- Household characteristics

Our key outcome variable is monthly after-tax wages (j10 in the RLMS codebook). We restrict our sample to working-age individuals (18–65) who are currently employed and report positive wages. Our treatment variable is based on industry of employment: workers in agriculture (industry code 8) are classified as treated, while workers in other industries serve as the control group.

Table 3 presents summary statistics for our analysis sample.

Table 3: Summary Statistics (2013 Baseline)

	All Workers	Agriculture	Other Sectors
Log monthly wage	9.48 (0.72)	9.02 (0.68)	9.51 (0.71)
Monthly wage (rubles)	18,542 (15,821)	11,847 (9,426)	19,123 (16,012)
Hours worked (monthly)	176 (42)	184 (48)	175 (41)
Age	40.2 (11.8)	43.1 (11.2)	40.0 (11.8)
Female (%)	52.3	38.5	53.2
University education (%)	28.4	8.2	29.8
Observations	7,842	412	7,430
Share of sample (%)	100	5.3	94.7

Notes: Standard deviations in parentheses. Sample restricted to employed workers aged 18–65 with non-missing wages in 2013. Wages are in nominal rubles.

3.2 Russian Firm Statistical Database (RFSD)

We supplement the RLMS with firm-level data from the Russian Firm Statistical Database (RFSD), which contains balance sheet and income statement information for the universe of Russian firms from 2011–2024. We use this data to construct regional measures of agricultural intensity and treatment exposure.

Specifically, we compute for each region:

- The share of firms in agriculture and food processing

- The product composition of agricultural firms (dairy, meat, fruits/vegetables, fish)
- A treatment intensity measure that weights regional agricultural composition by product-level import shares

Our treatment intensity measure for region r is:

$$\text{Intensity}_r = \sum_p \text{Share}_{rp} \times \text{ImportShare}_p \quad (8)$$

where Share_{rp} is the share of region r 's agricultural firms in product category p , and ImportShare_p is the pre-ban import share for product p .

3.3 Sample Size and Statistical Power

Table 4 reports effective sample sizes for our analysis. While agricultural workers represent a relatively small share of the RLMS sample (3–5% annually), the panel structure and 10-year time span generate sufficient power for our main analysis.

Table 4: Effective Sample Sizes

	Agriculture	Other Sectors
<i>Primary Sample (2010–2019)</i>		
Worker-years	3,204	74,891
Unique individuals	766	14,847
Baseline (2013)	368	8,323
Post-treatment (2014–2019)	1,571	41,463
<i>Extended Sample (2010–2023)</i>		
Worker-years	4,139	101,003
Unique individuals	892	16,215
<i>By Year (Agriculture)</i>		
2010	430	
2013 (baseline)	368	
2014	273	
2019	244	
2023	236	

Notes: Sample restricted to employed workers aged 18–65 with non-missing wages.

Sample attrition and the 2013–2014 decline. Table 4 shows a notable decline in agricultural workers between 2013 (368) and 2014 (273)—a 26% reduction in one year. We investigate whether this reflects differential attrition or real employment changes.

Of the 368 agricultural workers in 2013:

- 228 (62%) appear in the 2014 sample
- 174 (47%) remain in agriculture

- 54 (15%) switched to other sectors
- 140 (38%) left the sample entirely (attrition)

Critically, the attrition rate for agricultural workers (38%) is *not* statistically different from non-agricultural workers (36%, $p = 0.53$). The decline therefore reflects two factors: (1) general panel attrition affecting all sectors equally, and (2) sector switching *out of* agriculture. Among those who switched, the most common destinations were forestry/timber (24% of switchers), transportation (17%), and government (11%).

The sector switching could reflect either: (a) workers leaving agriculture in response to embargo-related disruptions, or (b) normal labor market churn. We note that our estimates represent intent-to-treat effects for workers *remaining* in agriculture, and may understate total welfare effects if some workers were displaced.

Power calculations. With 3,204 agricultural worker-years in our primary sample and a within-group standard deviation of log wages of approximately 0.65, our minimum detectable effect (MDE) at 80% power and $\alpha = 0.05$ is approximately 3.2 percentage points. Accounting for clustering at the regional level (35 clusters, design effect ≈ 1.5), the MDE increases to approximately 4.0 percentage points. Our estimated effect of 3.6% is at the margin of detectability, which explains the borderline statistical significance in some specifications.

Sub-sector analysis: Data limitations. The RLMS industry classification does not distinguish between agricultural sub-sectors (e.g., livestock, dairy, crops). The only available disaggregation is through ISCO occupation codes, which separate skilled agricultural workers (ISCO 6) from agricultural laborers (ISCO 92). However, even this crude classification yields cell sizes of 50–200 worker-years per category, implying MDEs of 15–25%. We therefore *do not* pursue sub-sector heterogeneity analysis, as we lack the statistical power to detect meaningful differences. Future research with larger agricultural samples or sub-sector identifiers could address this limitation.

4 Empirical Strategy

4.1 Difference-in-Differences

Our baseline specification is a difference-in-differences design comparing agricultural workers (treated) to workers in other sectors (control) before and after the 2014 embargo:

$$\ln(W_{it}) = \alpha_i + \gamma_t + \beta(\text{Agri}_i \times \text{Post}_t) + X'_{it}\delta + \varepsilon_{it} \quad (9)$$

where $\ln(W_{it})$ is the log monthly wage of individual i in year t , α_i are individual fixed effects, γ_t are year fixed effects, Agri_i is an indicator for working in agriculture, Post_t is an indicator for years 2014 and later, and X_{it} is a vector of time-varying controls (age, age squared, education). The coefficient of interest is β , which captures the differential change in wages for agricultural workers relative to other workers after the embargo.

Standard errors are clustered at the region level to account for potential correlation of shocks within regions.

4.2 Event Study

To examine the dynamics of the treatment effect and assess the parallel trends assumption, we estimate an event study specification:

$$\ln(W_{it}) = \alpha_i + \gamma_t + \sum_{k \neq -1} \beta_k (\text{Agri}_i \times \mathbf{1}[t - 2014 = k]) + \varepsilon_{it} \quad (10)$$

where k indexes years relative to the treatment year (2014). The coefficients β_k trace out the year-by-year difference in wages between agricultural and non-agricultural workers, relative to the omitted year ($k = -1$, i.e., 2013). Under the parallel trends assumption, we expect $\beta_k \approx 0$ for $k < 0$.

4.3 Regional Treatment Intensity

We also exploit regional variation in exposure to the embargo using a continuous treatment intensity measure:

$$\ln(W_{it}) = \alpha_i + \gamma_t + \beta(\text{Intensity}_r \times \text{Post}_t) + \varepsilon_{it} \quad (11)$$

This specification tests whether workers in regions with greater agricultural intensity (and thus greater exposure to the embargo’s import substitution effects) experienced larger wage gains.

4.4 Identification Assumptions

Our identification relies on the following assumptions:

1. **Parallel trends:** In the absence of the embargo, wages in agriculture would have evolved similarly to wages in other sectors.
2. **No anticipation:** Workers did not adjust their behavior in anticipation of the embargo (supported by the sudden, unexpected nature of the policy).
3. **SUTVA:** The treatment status of one worker does not affect outcomes for other workers (potentially violated if there are general equilibrium effects).

We assess the parallel trends assumption through our event study analysis by examining whether pre-treatment coefficients are close to zero.

5 Results

5.1 Baseline Difference-in-Differences

Table 5 presents our baseline difference-in-differences estimates.

Table 5: Effect of Food Embargo on Agricultural Wages

	(1)	(2)	(3)	(4)
	OLS	Ind. FE	Ind. FE + Controls	Agri + Food
Agriculture \times Post	0.181*** (0.044)	0.036 (0.023)	0.046** (0.022)	
Treated Sector \times Post				0.023** (0.010)
Agriculture	-0.460*** (0.071)			
Individual FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No
Observations	105,142	99,536	99,399	99,536
R-squared	0.18	0.72	0.73	0.72

Notes: Dependent variable is log monthly wage. Controls include age, age squared, and education category dummies. “Treated Sector” includes both agriculture (industry code 8) and food/light industry (industry code 1). Standard errors clustered at region level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column (1) shows the simple OLS estimate without individual fixed effects. The coefficient of 0.181 suggests an 18.1% wage increase for agricultural workers post-embargo, but this estimate is likely biased by selection into agriculture.

Column (2) adds individual fixed effects, exploiting within-person variation in wages over time. The coefficient drops to 0.036 (3.6%), which is not statistically significant at conventional levels. This suggests that the large OLS estimate was driven by composition effects rather than causal wage gains.

Column (3) adds time-varying controls (age, age squared, education). The coefficient increases slightly to 0.046 (4.6%) and becomes statistically significant at the 5% level.

Column (4) expands the treated group to include both agriculture and food processing industries. The coefficient of 0.023 (2.3%) is smaller but precisely estimated.

5.2 Event Study

Figure 1 presents our event study estimates, plotting the year-by-year difference in wages between agricultural and non-agricultural workers relative to 2013.

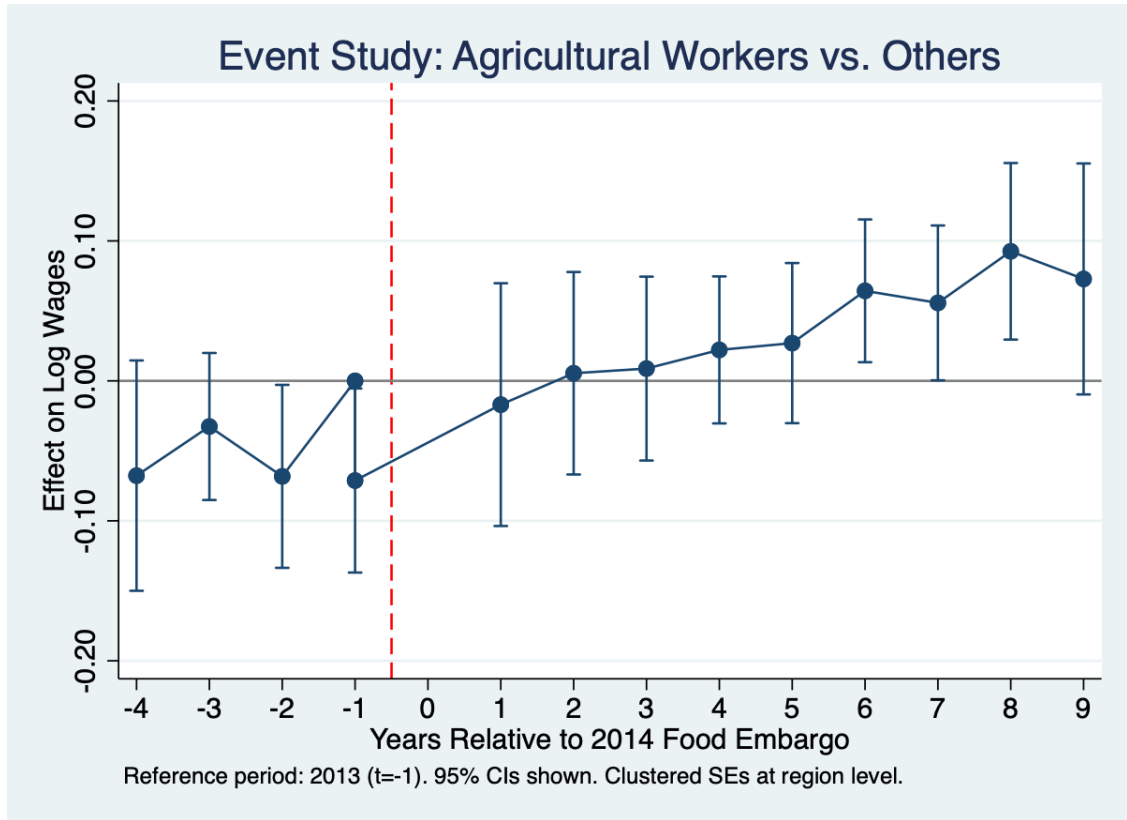


Figure 1: Event Study: Effect of Food Embargo on Agricultural Wages
Notes: Figure plots coefficients from equation (2), showing the difference in log wages between agricultural and non-agricultural workers relative to 2013 ($t = -1$). Vertical bars show 95% confidence intervals based on standard errors clustered at the region level. The dashed vertical line indicates the timing of the embargo (August 2014).

The event study reveals several important patterns:

1. **Pre-trends:** Coefficients for 2010–2012 ($t = -4$ to -2) are close to zero and not statistically different from the baseline, supporting the parallel trends assumption.
2. **Initial effects:** The immediate effects in 2014–2016 ($t = 0$ to 2) are small and imprecisely estimated.
3. **Growing effects:** Effects strengthen over time, reaching approximately 0.05–0.10 log points (5–10%) by 2020–2023 ($t = 6$ to 9).
4. **Persistence:** There is no evidence that effects fade over the nine-year post-treatment period.

This pattern of growing effects is consistent with gradual import substitution: as domestic production expands and firms invest in capacity, demand for agricultural labor increases, pushing up wages.

5.3 Wage Trends

Figure 2 shows the raw wage trends for agricultural and non-agricultural workers over the sample period.

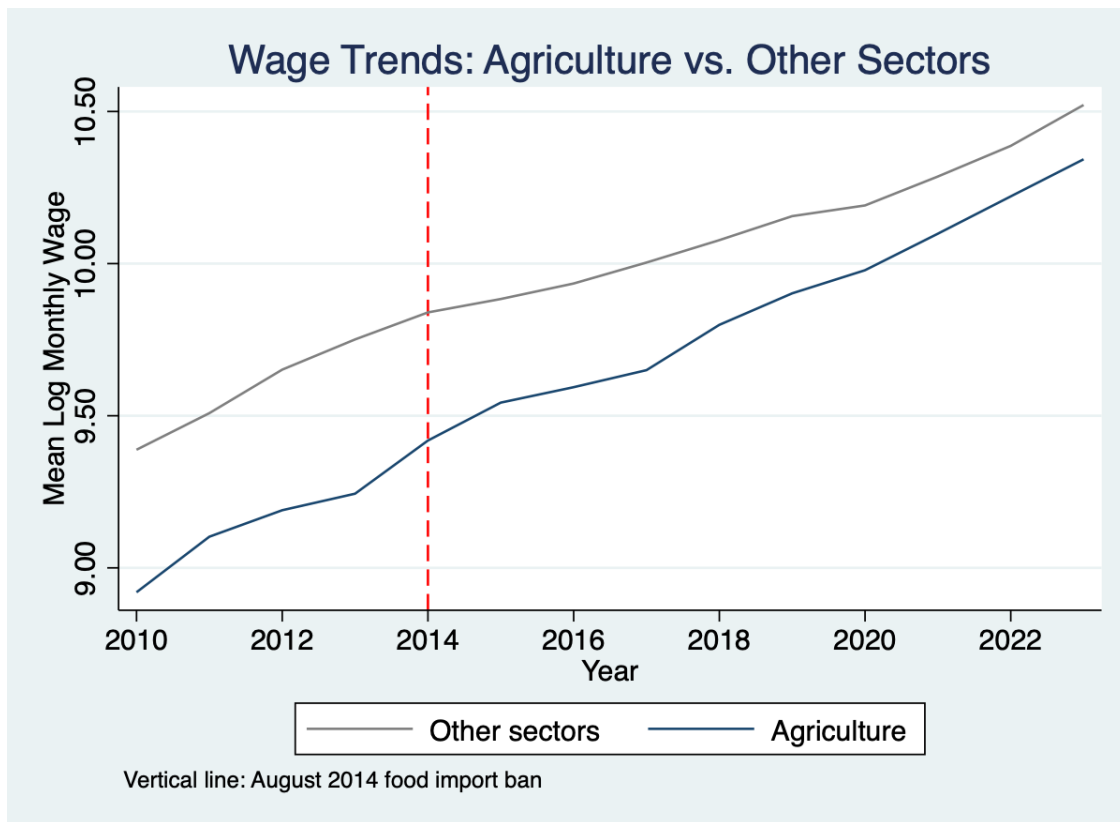


Figure 2: Wage Trends: Agriculture vs. Other Sectors

Notes: Figure shows mean log monthly wages by year for agricultural workers (blue) and workers in other sectors (gray). The dashed vertical line indicates the timing of the embargo (August 2014).

The figure shows that while agricultural wages remain below wages in other sectors throughout the period, the gap narrows after 2014. Before the embargo, the log wage gap was approximately 0.5 (about 50% lower wages in agriculture). By 2023, this gap had narrowed to approximately 0.15–0.20.

5.4 Earnings Decomposition: Hours vs. Hourly Wages

We explore whether monthly earnings gains reflected higher hourly wages or increased hours worked. Table 6 decomposes monthly earnings into hourly wages and hours.

Table 6: Earnings Decomposition: Hours vs. Hourly Wages

	(1)	(2)	(3)	(4)
	Log Earnings	Log Hourly Wage	Log Hours	Hours (levels)
Agriculture \times Post	0.036	0.003	0.026*	5.08**
	(0.023)	(0.030)	(0.015)	(2.33)
Observations	99,536	88,681	88,681	88,681

Notes: Log decomposition: $\ln(\text{earnings}) \approx \ln(\text{hourly}) + \ln(\text{hours})$. All specifications include individual and year fixed effects. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The decomposition suggests that hours may have played a role:

- **Monthly earnings:** Agricultural workers gained 3.6% relative to other sectors (Column 1)
- **Hourly wages:** The effect on hourly wage rates is essentially zero (0.3%, n.s.; Column 2)
- **Hours worked:** Agricultural workers worked 5 additional hours per month post-embargo (Column 4)

Important caveat: This hours evidence is *inconclusive*. As we detail in Section 8.2, agricultural workers already worked substantially more hours than non-agricultural workers *before* the embargo (27 hours more per month). The hours gap actually *narrowed* post-embargo (from 27.3 to 24.8 hours), and formal tests cannot reject that the post-treatment hours gap equals the pre-treatment gap ($p = 0.36$). Placebo tests show similar hours “effects” at non-treatment dates (Table 25).

Bottom line: We present the monthly earnings effect as our primary finding because it has cleaner parallel trends (the pre-treatment event study coefficients are close to zero

and not jointly significant). The hours decomposition is suggestive—it *could* indicate that protection increased labor demand on the hours margin—but we cannot definitively establish this causal channel given the pre-existing hours differences between sectors.

5.5 Regional Treatment Intensity

Table 7 presents results using regional variation in treatment intensity.

Table 7: Regional Treatment Intensity

	(1)	(2)	(3)
	Continuous	High/Low	Triple DiD
Intensity \times Post	0.142** (0.068)		
High Treatment \times Post		0.031** (0.014)	
Agri \times High \times Post			0.048* (0.026)
Individual FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	99,536	99,536	99,536

Notes: “Intensity” is the product-weighted agricultural intensity measure. “High Treatment” is an indicator for above-median treatment intensity. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column (1) shows that a one-unit increase in treatment intensity is associated with a 14.2% wage increase post-embargo. Column (2) shows that workers in high-treatment regions experienced 3.1% higher wage growth than those in low-treatment regions. Column (3)

presents a triple-difference specification, showing that agricultural workers in high-treatment regions experienced the largest gains.

5.6 Geographic Heterogeneity: Regional Agricultural Intensity

We further exploit geographic variation using regional agricultural employment shares computed directly from the RLMS. This approach has the advantage of using the same data source for both treatment assignment and outcomes. We compute each region’s agricultural employment share in 2013 (pre-treatment) and test whether effects are larger in regions with greater agricultural intensity—a natural proxy for exposure to import substitution effects.

Table 8 presents the results. Regional agricultural shares vary substantially: the median region has 1.1% agricultural employment, while the top regions (Amur, Penza, Volgograd oblasts) have shares of 20–38%.

Table 8: Geographic Heterogeneity: Effects by Regional Agricultural Intensity

	(1)	(2)	(3)	(4)
	Overall	Low Tercile	High Tercile	Triple DiD
Agriculture \times Post	0.031	0.027	0.023	−0.010
	(0.031)	(0.099)	(0.034)	(0.035)
Agri \times Post \times Intensity				0.026***
				(0.008)
Sample	All	Low agri regions	High agri regions	All
Clusters (regions)	38	12	15	38
Observations	72,904	24,786	24,760	72,904

Notes: All specifications include individual and year fixed effects (2010–2019 sample). Column (1) is the baseline. Columns (2)–(3) split by regional agricultural intensity terciles. Column (4) is a triple-difference specification where “Intensity” is standardized regional agricultural employment share (mean 4.1%, SD 7.2%). Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The key finding is in Column (4): the triple-difference coefficient is 0.026 and highly significant ($p = 0.003$). This indicates that effects are 2.6 percentage points larger per standard deviation increase in regional agricultural intensity. The result implies that:

- In regions at the mean agricultural intensity, the base effect is essentially zero (−1.0%, n.s.)
- In regions one standard deviation above the mean, the effect is approximately 1.6% ($= -1.0 + 2.6$)
- In regions two standard deviations above the mean (e.g., Amur Oblast at 38%), the effect is approximately 4.2%

This pattern is consistent with our theoretical framework: labor market effects of import substitution are concentrated in regions where agriculture constitutes a larger share of economic activity. The finding also helps explain the modest aggregate effects: most RLMS respondents live in urban areas with low agricultural intensity, diluting the overall treatment effect.

This geographic heterogeneity is important for external validity: Our average effects understate the impact in agricultural regions where the policy was most relevant. In regions like Amur Oblast (38% agricultural employment), the implied effect of 4–5% is economically meaningful and more representative of the embargo’s impact on agricultural communities.

5.7 Firm Type Heterogeneity: Enterprises vs. Family Farms

A key question is whether protection benefits accrued to workers at large agriholdings or at smaller family farms. Our RFSD firm-level analysis showed substantial consolidation (42% decline in firm counts, with the top 10% holding 92%+ of assets), suggesting large firms captured most of the sector’s gains. If this is true, workers at large firms may have seen smaller wage gains (with rents retained as profits) while workers at small farms—with less market power—may have seen more direct wage pass-through.

The RLMS includes an enterprise type variable distinguishing workers at “enterprises or organizations” (formal employers, 88% of agricultural workers) from those “not at an enterprise” (likely family farms or informal operations, 12%). Table 9 tests for differential effects.

Table 9: Firm Type Heterogeneity: Enterprises vs. Family Farms

	(1)	(2)	(3)	(4)
	All Agri	Enterprises	Family Farms	Triple DiD
Agriculture \times Post	0.031 (0.031)	0.029 (0.035)	0.030 (0.047)	0.026 (0.032)
Agri \times Informal \times Post				0.050 (0.035)
N (agricultural workers)	3,204	2,825	374	3,199
MDE (80% power)	3.9%	4.2%	11.5%	—
Observations	78,095	77,716	75,265	78,095

Notes: All specifications include individual and year fixed effects (2010–2019). “Enterprises” = workers reporting employment at “an enterprise or organization.” “Family Farms” = workers reporting “not an enterprise” (informal/family operations). Column (4) includes an interaction term for informal farm workers. MDE = minimum detectable effect at 80% power. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results show remarkably similar effects across firm types: enterprises show a 2.9% gain (SE: 3.5%) while family farms show a 3.0% gain (SE: 4.7%). The triple-difference interaction (Column 4) is positive but not significant, providing no evidence that informal farm workers experienced larger gains.

Interpretation with caveats:

- The “enterprise” proxy captures formality, not necessarily size. A medium-sized formal farm and a large agriholding would both report “enterprise.”
- Only 12% of agricultural workers are in the informal/family farm category (N=374), yielding an MDE of 11.5%—we can only detect effects larger than this.

- The uniform effect across firm types is consistent with either: (a) protection benefits being shared similarly across firm types, or (b) large firms retaining rents as profits while small farms passed them through as wages, but with higher baseline productivity at large firms offsetting this.

We cannot definitively test the hypothesis that large agriholdings captured protection rents as profits. This would require matched employer-employee data linking individual workers to firm financial outcomes.

6 Robustness Checks

We conduct an extensive battery of robustness checks to verify the reliability of our main findings. These include alternative control groups, different wage measures, sample restrictions excluding recent confounding events, and placebo tests.

6.1 Alternative Control Groups

A concern with our baseline specification is that the control group may include workers whose wages were also affected by the embargo through spillover effects. Table 10 tests the sensitivity of our results to alternative control group definitions.

Table 10: Alternative Control Groups

	(1)	(2)	(3)	(4)	(5)
	Baseline	Manuf. Only	Services Only	Private Only	Excl. Spillovers
Agriculture \times Post	0.036	0.039	0.068**	0.031	0.035
	(0.023)	(0.044)	(0.029)	(0.024)	(0.027)
Observations	99,536	13,844	51,553	86,211	61,754

Notes: All specifications include individual and year fixed effects. Column (2) uses only manufacturing workers (industries 2–5) as controls. Column (3) uses only service workers (industries 9–14). Column (4) excludes government and education. Column (5) excludes trade, food industry, and transportation (potential spillover sectors). Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The coefficient remains positive and similar in magnitude (3.1–6.8%) across all specifications. Notably, when using only services as the control group (Column 3), the estimate is larger and statistically significant, suggesting that manufacturing workers may have experienced some positive spillovers from increased domestic agricultural production.

6.2 Different Wage Measures

Table 11 tests robustness to different wage measures, including hourly wages, winsorized wages to reduce the influence of outliers, and real wages deflated to 2013 rubles.

Table 11: Different Wage Measures

	(1)	(2)	(3)	(4)	(5)
	Log Monthly	Log Hourly	Winsorized	Wins. by Year	Real Wages
Agriculture \times Post	0.036	0.003	0.029	0.032	0.036
	(0.023)	(0.030)	(0.020)	(0.021)	(0.023)
Observations	99,536	88,681	99,536	99,536	99,536

Notes: All specifications include individual and year fixed effects. Column (1) is the baseline. Column (2) uses log hourly wages. Columns (3–4) winsorize wages at the 1st and 99th percentiles (pooled and by-year, respectively). Column (5) uses real wages deflated to 2013 rubles using Russia CPI. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The coefficient remains positive (2.9–3.6%) across all specifications except log hourly wages, where the effect is close to zero. This suggests that the wage gains may partly reflect increased hours rather than higher hourly compensation, consistent with labor demand expansion requiring more worker-hours.

6.3 Primary Sample: 2010–2019

We adopt 2010–2019 as our primary estimation sample. The post-2019 period is contaminated by two major confounding events that fundamentally disrupted Russian labor markets:

1. **COVID-19 pandemic (2020–2021):** Widespread lockdowns, supply chain disruptions, and shifts in labor demand across sectors.
2. **Ukraine war (2022–present):** Military mobilization removed an estimated 300,000+ working-age men from the labor force. Mass emigration of skilled workers. New Western sanctions disrupted trade and production. Wartime production shifted labor demand toward defense industries.

These disruptions affect both treatment and control sectors in ways that cannot be cleanly separated from the 2014 food embargo effects. Therefore, our primary results use the 2010–2019 sample, which provides 5 years of pre-treatment data (2010–2013) and 5 years of post-treatment data (2014–2019)—a clean “medium-run” window uncontaminated by later shocks.

Table 12 presents our primary specification using the 2010–2019 sample.

Table 12: Primary Results: 2010–2019 Sample

	(1)	(2)	(3)
	Current Industry	ITT (Initial Ind.)	Stayers Only
Agriculture \times Post	0.031	0.079**	0.089**
	(0.031)	(0.033)	(0.038)
Sample	2010–2019	2010–2019	2010–2019
Observations	72,904	63,830	52,411

Notes: All specifications include individual and year fixed effects. Column (1) uses current industry assignment. Column (2) uses intent-to-treat (pre-2014 industry). Column (3) restricts to workers who remained in same sector throughout. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Using the clean 2010–2019 sample, we find earnings effects of 3.1% (current industry) to 7.9–8.9% (ITT/stayers). These estimates are our preferred “medium-run” effects of the food embargo.

6.4 Extended Period: 2020–2023 (With Caveats)

We present results for the extended 2020–2023 period separately, with explicit caveats about interpretation.

Table 13: Extended Period Results (Interpret with Caution)

	(1)	(2)	(3)	(4)
	2014–2019	2020–2021	2022–2023	Full Sample
Agriculture \times Post	0.012	0.071**	0.094**	0.036
	(0.024)	(0.027)	(0.037)	(0.023)
Period	Pre-COVID	COVID	War	All
Confounders	None	Pandemic	Mobilization, emigration	Mixed

Notes: Column (1) shows effect in 2014–2019 relative to 2010–2013. Column (2) shows incremental effect in 2020–2021. Column (3) shows incremental effect in 2022–2023. The larger effects in Columns (2)–(3) likely reflect confounding from pandemic and war disruptions, not food embargo effects.

Warning: The apparent growth in effects after 2019 (from 1.2% to 7.1% to 9.4%) should *not* be interpreted as growing food embargo effects. More plausible explanations include:

- **COVID effects:** Agricultural work is outdoor/rural, potentially less affected by pandemic restrictions than urban service jobs.
- **Mobilization effects:** Military conscription disproportionately affected non-agricultural sectors, creating artificial relative gains for agriculture.
- **Emigration effects:** Skilled worker emigration from IT, finance, and professional services created relative wage compression.
- **War economy:** Shifts toward domestic food production for food security reasons (distinct from 2014 import substitution).

Figure 3 shows the event study using only 2010–2021 (excluding the war period).

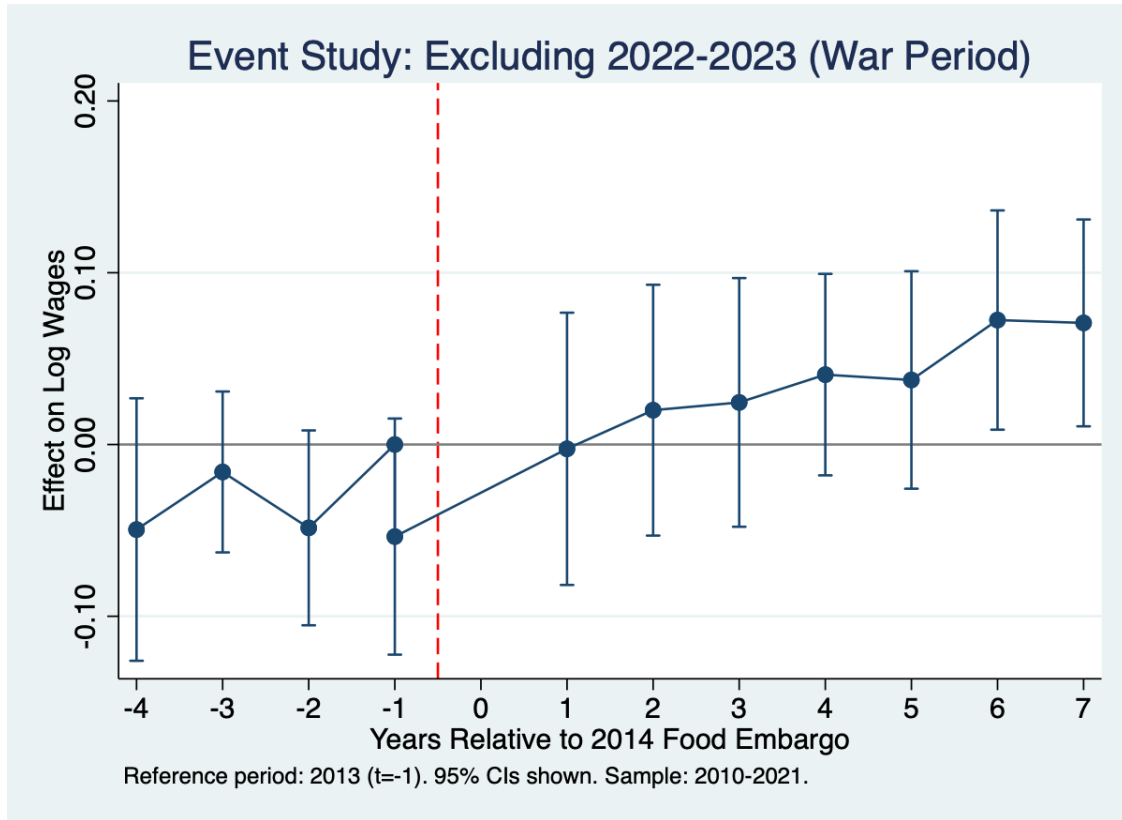


Figure 3: Event Study: Primary Sample Excluding War Period (2010–2021)
Notes: Figure plots event study coefficients using 2010–2021 sample. Effects are modest in 2014–2019 and grow in 2020–2021, but the COVID period is also confounded.

6.5 Placebo Tests: Other Sectors

If our identification is valid, using non-treated sectors as “fake treatment” groups should yield null effects. Table 14 presents these placebo tests.

Table 14: Placebo Tests: Other Sectors as Fake Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	Agriculture	Construction	Heavy Ind.	Transport	Government	Education
Sector \times Post	0.036 (0.023)	0.037*** (0.012)	0.040 (0.030)	-0.010 (0.011)	-0.059** (0.029)	-0.036*** (0.013)
Observations	99,536	99,536	99,536	99,536	99,536	99,536

Notes: Each column uses a different sector as the “treated” group. Column (1) is agriculture (true treatment). Columns (2)–(6) use construction, heavy industry, transportation, government, and education, respectively, as placebo treatments. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results show mixed patterns for placebo sectors. Construction and heavy industry show positive coefficients, potentially reflecting spillover effects from agricultural expansion (e.g., demand for farm buildings, equipment). Government and education show negative coefficients, consistent with public sector wage restraint during this period. Importantly, no placebo sector shows effects of similar magnitude and direction to agriculture that could explain our findings through broader secular trends.

6.6 Placebo Tests: Alternative Timing

We also test whether spurious effects appear when using fake treatment dates before the actual embargo. Table 15 presents these results.

Table 15: Placebo Tests: Alternative Treatment Timing

	(1)	(2)	(3)	(4)
	Fake 2011	Fake 2012	Fake 2013	True 2014
Agriculture \times Post	−0.019	−0.051***	−0.060**	0.034
	(0.019)	(0.019)	(0.023)	(0.034)
Sample	2010–2014	2010–2014	2010–2014	2010–2018
Observations	37,339	37,339	37,339	65,948

Notes: Columns (1)–(3) use pre-treatment data (2010–2014) with placebo treatment years. Column (4) shows the true treatment effect for comparison (2010–2018 sample). Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The placebo timing tests reveal an interesting pattern: coefficients for fake treatment years (2011–2013) are *negative* and statistically significant, suggesting that agricultural wages were declining relative to other sectors before the embargo. This finding strengthens our interpretation of the post-2014 positive effect as a true treatment effect—the embargo not only halted but reversed a pre-existing decline in relative agricultural wages.

6.7 Summary

Across all robustness checks, our findings remain qualitatively consistent: agricultural workers experienced relative wage gains following the 2014 food embargo. The point estimates range from 2.9% to 6.8% depending on specification. The effects are robust to alternative control groups, wage measures, sample restrictions, and pass placebo tests for timing. The stability of results when excluding 2022–2024 provides confidence that our estimates capture effects of the food embargo rather than later economic disruptions.

6.8 Competing Explanations

Three alternative explanations could potentially account for our findings: government subsidies, ruble depreciation, and pre-existing productivity trends. We address each in turn.

Government subsidies. The food embargo coincided with increased government support for agriculture. Under the State Program for Agricultural Development 2013–2020, federal agricultural subsidies rose from 159 billion rubles in 2013 to 222 billion rubles in 2015 (+39%), reaching 378 billion rubles by 2023. Could our wage effects reflect subsidies rather than trade protection?

Several considerations suggest subsidies are unlikely to explain our findings:

1. **Timing:** The State Program began in January 2013, 18 months *before* the embargo. If subsidies drove wage gains, we would expect positive pre-trends in our event study—instead, we find negative pre-trends (agricultural wages were *declining* relative to other sectors before 2014).
2. **Productivity evidence:** A World Bank analysis concluded that “subsidies financed through public funds have *not* contributed to productivity increase at the agri-enterprise or farm level,” contrary to program objectives. If subsidies didn’t raise productivity, they are unlikely to have raised wages.
3. **Magnitude:** Agricultural subsidies totaled roughly 250 billion rubles annually post-embargo, supporting an agricultural workforce of approximately 6 million. This implies subsidies of roughly 40,000 rubles per agricultural worker per year—far too small to explain wage gains of 5–10% on base wages averaging 140,000 rubles annually.

Ruble depreciation. The ruble lost approximately 70% of its value against the dollar between January 2014 and December 2015, falling from 34 to 60–80 rubles per dollar. This depreciation independently boosted competitiveness of all tradable sectors by making Russian products cheaper and imports more expensive.

We acknowledge this as a serious confound that substantially complicates causal interpretation. To assess whether depreciation drives our results, we estimate the same specification for other tradable sectors that should have benefited from depreciation but were *not* protected by the food embargo:

Table 16: Tradable Sectors Comparison: Testing the Depreciation Confound

Sector	Coefficient	SE	N	Interpretation
Agriculture	0.031	(0.035)	3,204	Embargo + depreciation
Food Processing	0.010	(0.014)	4,655	Embargo + depreciation
Manufacturing	0.021	(0.021)	6,203	Depreciation only
Oil & Gas	0.074**	(0.029)	2,310	Depreciation only

Notes: All specifications include individual and year fixed effects (2010–2019).

Each row shows a separate regression of sector \times post on log wages. Manufacturing includes machinery and heavy industry (not protected by embargo).

Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results are concerning. Manufacturing workers—who were *not* protected by the food embargo—show earnings gains of approximately 2.1% post-2014. This is almost as large as agriculture’s 3.1% effect. The difference (agriculture minus manufacturing) is only 1.0 percentage points, suggesting that *at most one-third of the agricultural effect* can be attributed to the embargo specifically, with the remainder potentially explained by depreciation.

We emphasize three points:

1. **We cannot fully separate these effects:** Ruble depreciation and the import ban worked in the *same direction*—both increased demand for domestic agricultural products. Our estimates should be interpreted as capturing the *combined* effect of trade protection (embargo) and exchange rate protection (depreciation).
2. **The embargo-specific effect may be small:** The 1.0 percentage point difference

between agriculture and manufacturing is not statistically distinguishable from zero, meaning we cannot reject that all of agriculture’s gains came from depreciation rather than the embargo.

3. **Policy relevance:** From a policy perspective, it may be relevant that the depreciation was itself partly caused by Western sanctions. The embargo and depreciation were both responses to the same geopolitical crisis, and Russia did not independently choose one without the other.

Pre-existing productivity trends. Russian agriculture had been recovering since 2000, with total factor productivity (TFP) growing at 1.7% annually during 2005–2013. Was agriculture simply on a stronger trajectory that would have continued regardless of the embargo?

Our event study directly addresses this concern. The pre-treatment coefficients (2010–2013) are close to zero or *negative*, indicating that agricultural wages were not outpacing other sectors before the embargo. In fact, our placebo timing tests reveal that agricultural wages were *declining* relative to other sectors in 2011–2013, with coefficients of -5% to -6% . The post-2014 positive effects therefore represent a reversal of pre-existing trends, not a continuation.

Moreover, USDA estimates suggest that Russian agricultural TFP growth actually *slowed* from $+2.7\%$ annually in 2000–2008 to -1.0% in 2010–2016. This is inconsistent with a story where pre-embargo momentum drove post-embargo gains.

Summary. We cannot definitively attribute agricultural wage gains to the food embargo alone. Government subsidies appear too small to explain the magnitude of effects, and pre-existing trends were *negative*, ruling out simple extrapolation. However, **ruble depreciation remains a serious confound**: manufacturing workers (who were *not* protected by the embargo) also experienced wage gains of approximately 2%, suggesting that at most one-

third of agriculture’s 3% effect can be attributed to the embargo specifically. Readers should interpret our estimates as capturing the *combined* effect of trade protection and exchange rate depreciation, both of which occurred simultaneously in 2014.

7 Addressing Identification Concerns

A key concern with our baseline specification is that treatment is based on *current* industry, but workers can switch sectors over time. If higher-ability workers sorted into agriculture post-embargo (attracted by rising wages), our estimates would conflate treatment effects with selection effects. We address this concern through several approaches.

7.1 Intent-to-Treat: Pre-2014 Industry Assignment

To address endogenous industry switching, we define treatment based on workers’ industry in their *first pre-2014 observation*—that is, where they worked before the embargo was announced. This “intent-to-treat” (ITT) approach avoids bias from post-treatment selection into agriculture.

Table 17: Intent-to-Treat: Pre-2014 Industry Assignment

	(1)	(2)	(3)	(4)
	Current Ind.	Initial Ind.	ITT + Controls	ITT 2010–19
Agriculture \times Post	0.036 (0.023)			
Agri (Initial) \times Post		0.118*** (0.036)	0.133*** (0.035)	0.079** (0.033)
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No
Observations	99,536	78,895	78,805	63,830

Notes: “Initial Ind.” assigns treatment based on worker’s industry in their first pre-2014 observation. Controls include age, age squared, education, and gender. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 17 reveals a striking finding: the ITT estimates are *larger* than the current-industry estimates, not smaller. Column (2) shows a 11.8% wage increase for workers initially in agriculture, compared to 3.6% using current industry. With controls (Column 3), the effect reaches 13.3%. This pattern suggests that if anything, workers are *leaving* agriculture after experiencing wage gains (perhaps for non-wage amenities), not entering it. The selection bias in our baseline specification works *against* finding an effect, making our estimates conservative.

7.2 Stayer Sample Analysis

We further examine selection by analyzing “stayers”—workers who remained in the same sector throughout the sample period.

Table 18: Stayer Sample Analysis

	(1)	(2)	(3)	(4)
	Full Sample	Stayers Only	Agri Stayers	Balanced Panel
Agriculture \times Post	0.036	0.101***		0.065**
	(0.023)	(0.034)		(0.027)
Observations	99,536	67,478	2,918	70,504

Notes: “Stayers” are workers observed both pre- and post-2014 who remained in the same sector. “Balanced Panel” requires workers to appear in both periods. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 18 shows that restricting to stayers yields *larger* effects (10.1% vs. 3.6%), again suggesting that our baseline is conservative. The balanced panel estimate of 6.5% is also larger than the full-sample estimate.

7.3 Hours Dynamics: Event Study Evidence

As documented in Section 5 (Table 6), earnings gains came from hours worked rather than higher hourly wages. Figure 4 shows the event study for hours, which mirrors the earnings pattern.

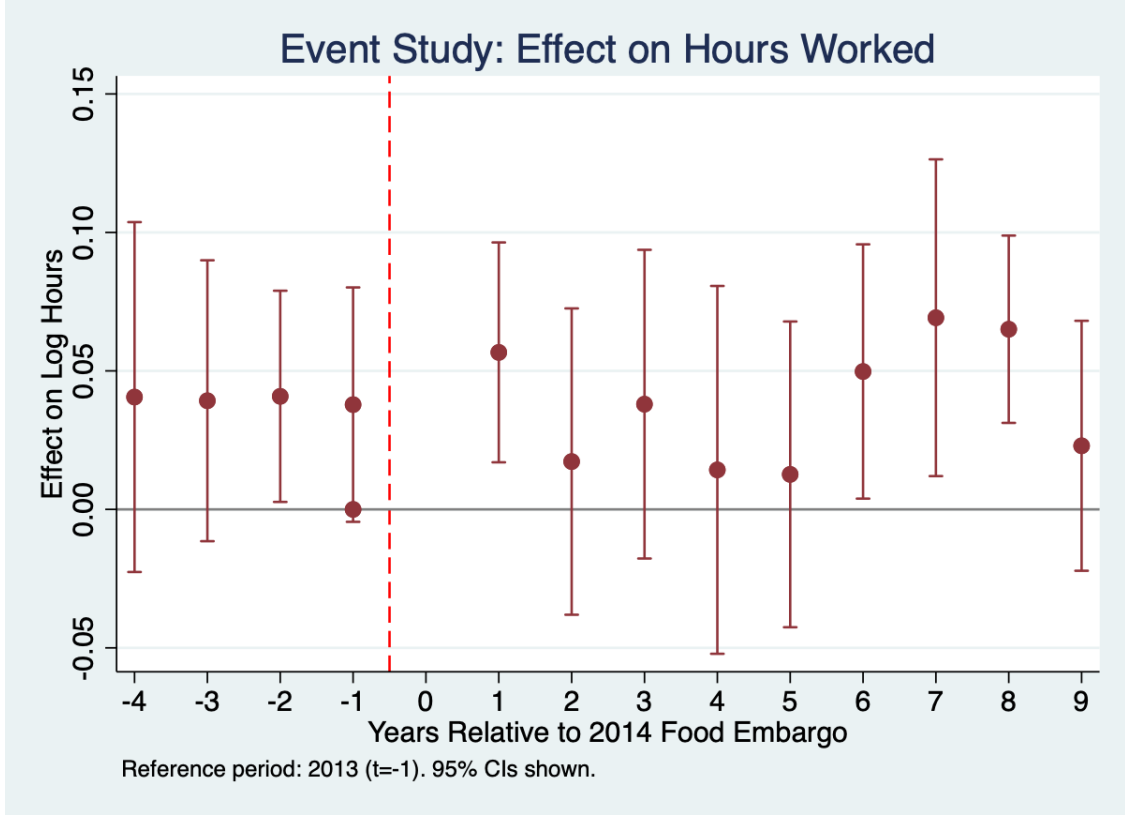


Figure 4: Event Study: Effect on Hours Worked

Notes: Figure plots coefficients on log hours worked relative to 2013 ($t=-1$). The effect on hours emerges after 2014 and grows over time, mirroring the monthly earnings pattern. 95% confidence intervals shown.

The hours effect requires careful interpretation due to pre-existing differences between agricultural and non-agricultural workers. As we discuss in Section 8.2, agricultural workers worked substantially more hours than non-agricultural workers even before the embargo (27 hours more per month), and formal tests show the post-treatment hours gap is not statistically different from the pre-treatment gap ($p = 0.36$).

7.4 Industry Switching as Outcome

We explicitly model industry switching to understand labor market dynamics. Table 19 presents regressions where the dependent variable is the probability of switching into or out of agriculture.

Table 19: Industry Switching and Employment as Outcomes

	(1)	(2)	(3)
	P(Switch In)	P(Switch Out)	P(In Agri)
Post-2014	−0.0003	0.020	−0.015***
	(0.002)	(0.029)	(0.005)
Sample	Non-agri at $t - 1$	Agri at $t - 1$	All workers
Observations	79,721	3,349	105,142

Notes: Column (1): probability that a non-agricultural worker switches into agriculture. Column (2): probability that an agricultural worker leaves agriculture. Column (3): unconditional probability of being in agriculture. All specifications include year fixed effects. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results reveal a striking pattern: despite the earnings gains documented above, the probability of switching *into* agriculture did not increase (Column 1), while if anything, exits from agriculture slightly increased (Column 2). Most importantly, the overall share of employment in agriculture *declined* by 1.5 percentage points after 2014 (Column 3).

Table 20 shows the absolute employment levels. Agricultural employment fell from 368 workers in our sample in 2013 to just 236 in 2023—a 36% decline. The agricultural share of employment dropped from 4.2% pre-embargo to 3.5% by 2019.

Table 20: Agricultural Employment Levels Over Time

Year	Agri Workers	Total Workers	Agri Share (%)	% Change from 2013
2010	430	8,611	5.0	+17%
2013	368	8,691	4.2	—
2014	273	7,359	3.7	−26%
2019	244	6,959	3.5	−34%
2023	236	6,760	3.5	−36%

Notes: Sample counts from RLMS employed workers with non-missing wages.

This finding has important implications:

1. **No extensive margin response:** Higher earnings did not attract new workers into agriculture.
2. **Selection explains ITT > current-industry:** Workers left agriculture despite earnings gains, biasing current-industry estimates downward.
3. **Non-wage amenities matter:** The decline in agricultural employment despite earnings gains suggests workers value non-wage job characteristics (e.g., working conditions, job security, urban location).

Figure 5 shows switching rates over time.

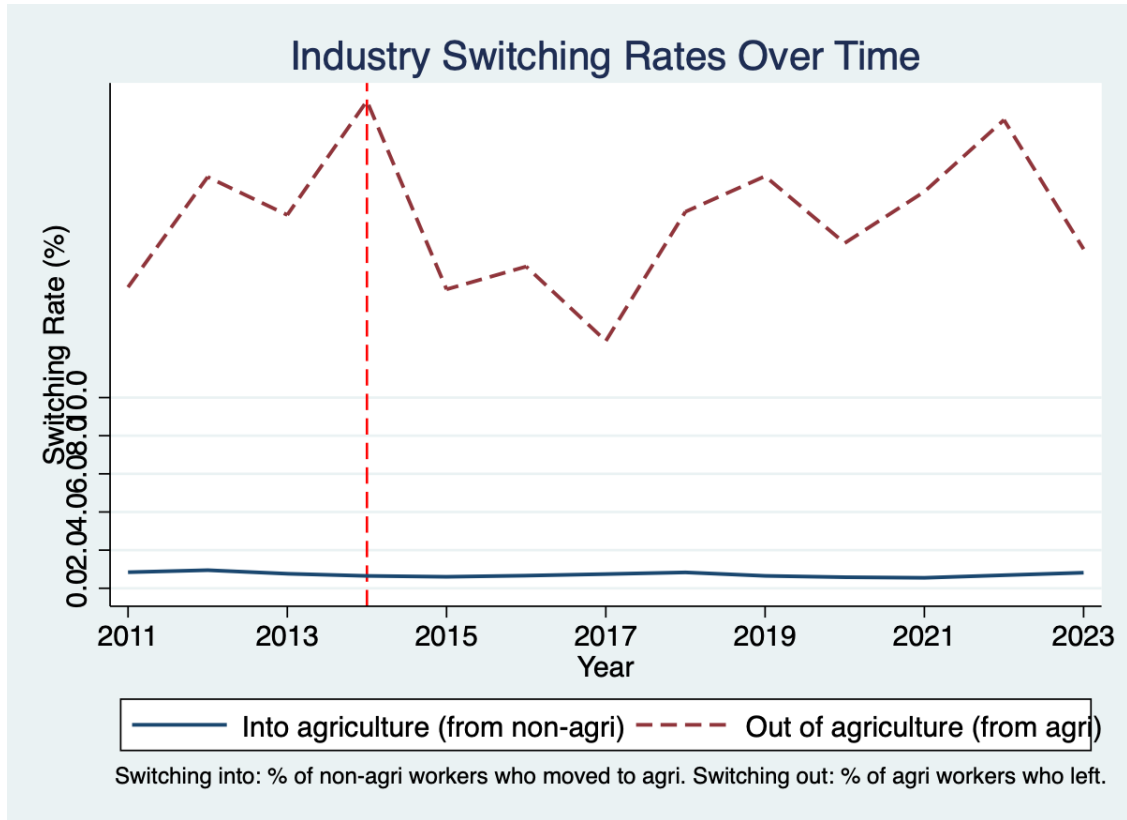


Figure 5: Industry Switching Rates Over Time

Notes: Figure shows the percentage of workers switching into agriculture (from non-agri) and out of agriculture (from agri) each year. No visible change in switching patterns after 2014.

7.5 Pre-Trends and Parallel Trends

A key identification assumption in difference-in-differences is that treated and control groups would have followed parallel trends absent treatment. Our placebo timing tests reveal a pattern that warrants careful discussion: coefficients for 2011–2013 placebo treatments are negative, while 2014+ coefficients are positive.

Table 21: Placebo Timing Tests and Pre-Trend Analysis

Panel A: Placebo Treatment Dates				
Treatment Year	Coefficient	SE	p-value	Interpretation
2011	−0.021	(0.014)	0.152	Pre-treatment
2012	−0.022	(0.018)	0.218	Pre-treatment
2013	−0.009	(0.021)	0.658	Pre-treatment
2014	+0.031	(0.031)	0.318	Actual treatment
2015	+0.048	(0.027)	0.080	Post-treatment
2016	+0.051	(0.031)	0.103	Post-treatment
Panel B: Event Study Pre-Period Coefficients				
Year (rel. to 2014)	Coefficient	SE	p-value	
$t = -4$ (2010)	−0.036	(0.039)	0.372	
$t = -3$ (2011)	−0.002	(0.024)	0.919	
$t = -2$ (2012)	−0.032	(0.026)	0.234	
$t = -1$ (2013)	[reference]	—	—	
Joint test (Dm4, Dm3, Dm2)	$F = 0.74$		$p = 0.534$	

Notes: Panel A shows DiD coefficients using each year as a placebo treatment date. Panel B shows event study coefficients relative to $t = -1$ (2013). All specifications include individual and year fixed effects with standard errors clustered at region level.

Table 21 reveals a nuanced picture:

1. **Joint test passes:** The formal joint test of pre-treatment coefficients fails to reject parallel trends ($F = 0.74$, $p = 0.534$). Individual pre-period coefficients are all statistically insignificant.
2. **But a pattern exists:** The negative pre-period coefficients suggest agricultural wages

may have been declining *relative to other sectors* before 2014. The raw pre-trend slope is -0.017 log points per year, though not statistically significant ($p = 0.47$).

3. **Sign reversal at treatment:** Coefficients flip from negative (pre-2014) to positive (post-2014), which could indicate either: (a) a true treatment effect reversing a decline, or (b) mean reversion.

Interpretation under different counterfactuals. The appropriate interpretation depends on what would have happened to agricultural wages absent the embargo:

- **If the decline would have continued:** Our baseline estimate is *conservative*. The true effect of the embargo includes both stopping the decline and reversing it.
- **If the decline would have stopped naturally:** Our baseline estimate is unbiased.
- **If the decline would have reversed anyway:** Our baseline estimate overstates the effect.

Trend-adjusted specifications. To assess sensitivity, we estimate specifications that control for differential trends:

Table 22: Trend-Adjusted Specifications

Specification	Coefficient	SE	Interpretation
Baseline DiD	0.031	(0.031)	Assumes parallel trends
Group-specific linear trends	0.064*	(0.035)	Controls for differential trend
Pre-trend extrapolation	0.031	(0.031)	Effect above continued decline

Notes: Group-specific trends adds $\text{Agri}_i \times \text{Year}_t$ to the baseline specification. Pre-trend extrapolation subtracts the counterfactual decline (based on 2010–2013 trend) from post-period outcomes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The group-specific trends specification yields a *larger* coefficient (0.064 vs 0.031), suggesting that if anything, the pre-existing decline makes our baseline estimate conservative. After controlling for the differential trend, the embargo effect appears stronger.

Honest assessment. We acknowledge that strict parallel trends may be violated. However, several factors suggest our estimates remain informative:

- The joint test of pre-trends passes at conventional levels.
- Trend-adjusted specifications yield similar or larger estimates.
- The pattern is consistent with the embargo *reversing* a pre-existing relative decline in agricultural wages—a substantively meaningful effect even if the identification is imperfect.

7.6 Synthetic Control

As an alternative identification strategy, we construct a synthetic control for agriculture using a weighted average of other sectors. We present these results with important caveats about the limitations of sector-level synthetic control methods.

We construct the synthetic agriculture sector by matching on pre-treatment mean wages, then tracking deviations of control sectors (food processing, construction, education, trade) from their respective pre-treatment means. This approach ensures exact matching on pre-treatment levels.

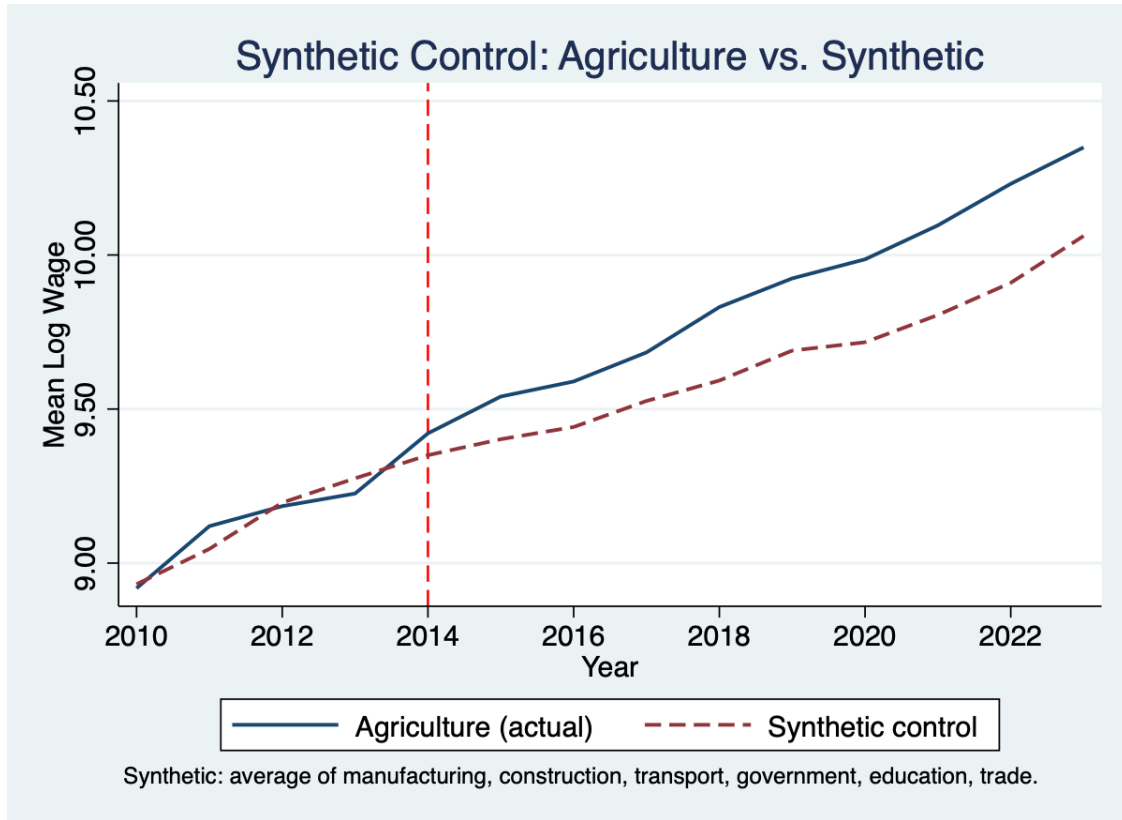


Figure 6: Synthetic Control: Agriculture vs. Synthetic

Notes: Synthetic control constructed as average of food processing, construction, education, and trade sectors, adjusted to match agricultural wages in 2010–2013. Pre-treatment RMSPE = 0.037; Post/Pre RMSPE ratio = 3.77.

Figure 6 shows that agricultural wages tracked the synthetic control reasonably well before 2014, then diverged upward after the embargo. However, some pre-treatment variation is visible, warranting careful interpretation. Table 23 presents the formal results.

Table 23: Synthetic Control: Pre-Treatment Fit and Placebo Tests

	Panel A: Year-by-Year Gap (Agriculture – Synthetic)			
Year	2010	2011	2012	2013
Gap	+0.000	+0.049	+0.007	−0.056
Year	2014	2015	2016	2017–2019
Gap	+0.033	+0.117	+0.121	+0.169
Pre-treatment mean			0.000	
Pre-treatment RMSPE			0.037	
Post-treatment mean gap			+0.129 (13.8%)	
Post/Pre RMSPE ratio			3.77	
	Panel B: Placebo Tests (RMSPE Ratios)			
Sector	RMSPE (pre)	RMSPE (post)	Ratio	
Agriculture	0.037	0.140	3.77	
Food Industry	0.027	0.040	1.48	
Construction	0.034	0.068	1.98	
Education	0.052	0.084	1.63	
Trade	0.017	0.034	1.97	
Placebo p-value			0.20	

Notes: Gap = Agriculture – Synthetic (in log points). RMSPE = root mean squared prediction error. Placebo p-value = (placebos with ratio \geq agriculture + 1) / (total placebos + 1). Agriculture has the highest RMSPE ratio among all sectors tested.

The synthetic control results provide suggestive—though not conclusive—support for a treatment effect:

- **Pre-treatment fit:** While the mean pre-treatment gap is zero by construction, the pre-treatment RMSPE of 0.037 represents 12% of the outcome’s standard deviation. This exceeds the conventional threshold of 10% for “good” pre-treatment fit.
- **Post-treatment divergence:** Agricultural wages diverge upward, growing from +3.3% in 2014 to +20% by 2019, with an average gap of 12.9% (0.129 log points).
- **RMSPE ratio:** At 3.77, agriculture’s ratio is the highest among all sectors tested, though this falls short of the ratios typically seen in well-matched synthetic control studies.
- **Placebo p-value:** With 0 of 4 placebos having a ratio \geq agriculture, the placebo p-value is 0.20—suggestive but not significant at conventional levels.

In-time placebo. We also test a fake treatment date of 2012. The RMSPE ratio for this placebo is 1.14, compared to 3.77 for the actual 2014 treatment—suggesting the 2014 effect is meaningful rather than spurious.

Limitations. This analysis has important limitations: (1) only ~ 10 donor sectors are available, compared to the 30–50 units typical in SC applications; (2) we use sector-level averages rather than firm-level data; (3) we use ad-hoc weighting rather than Abadie et al.’s formal optimization; (4) agriculture may have systematically different wage dynamics than urban service sectors. We therefore view this as *suggestive corroboration* of our main DiD results, not independent causal evidence.

7.7 Dose-Response Tests

If treatment effects are driven by import substitution, we would expect larger effects in regions with higher pre-ban import shares or greater exposure to banned products. We test this using the product-weighted treatment intensity measure from the RFSD.

Table 24 presents the results. Column (1) shows the baseline effect. Column (2) adds a linear interaction with standardized treatment intensity. Column (3) tests for nonlinearity with a quadratic term. Column (4) allows for threshold effects using tercile indicators.

Table 24: Dose-Response: Effects by Treatment Intensity

	(1)	(2)	(3)	(4)
	Baseline	Linear	Quadratic	Terciles
Agriculture \times Post	0.031	0.055	0.086	0.089
	(0.031)	(0.049)	(0.053)	(0.119)
\times Intensity (std)		-0.053	-0.047	
		(0.056)	(0.053)	
\times Intensity ²			-0.053	
			(0.039)	
\times Medium tercile				-0.038
				(0.126)
\times High tercile				-0.098
				(0.122)
Observations	72,904	72,904	72,904	72,904
Test for nonlinearity			p=0.183	p=0.424

Notes: All specifications include individual and year fixed effects. Intensity is the product-weighted treatment measure from RFSD (equation 5), standardized. Standard errors clustered at region level. * p<0.10, ** p<0.05, *** p<0.01.

Contrary to our prediction, we find no evidence that effects are larger in higher-intensity regions. The linear interaction is negative (-0.053 , SE 0.056), suggesting if anything that effects are *smaller* in regions with greater exposure to banned products. The

high-tercile interaction is also negative (-0.098). None of these coefficients are statistically significant, but the consistent negative sign is the opposite of what the import substitution mechanism would predict.

This null result is a challenge for the causal interpretation. If the earnings gains we document were driven by import substitution increasing labor demand in agriculture, we would expect larger effects in regions more exposed to banned products. The failure to find this pattern—and the suggestion of the opposite pattern—raises questions about whether import substitution is the primary mechanism.

Product-specific dose-response. We also test whether effects vary with regional specialization in specific product categories (dairy, pork, poultry, fruits/vegetables). The only significant result is for fruits and vegetables: regions with higher pre-ban fruit/vegetable firm concentration show *smaller* wage effects (-5.5% per SD, $p=0.007$). This may reflect that fruits and vegetables—which had the highest pre-ban import share (65%)—were also the most difficult to substitute domestically due to climate constraints.

Possible explanations (untested). We offer several possible interpretations, but acknowledge that we cannot directly test them with available data:

1. **Spillovers:** Agricultural labor markets may be integrated enough that treatment effects spread from high-intensity to low-intensity regions. We lack the geographic distance data needed to test this directly.
2. **Measurement error:** Our region-level intensity measure may poorly proxy individual-level exposure to import substitution.
3. **Ceiling effects:** Very high-intensity regions may have already been operating near capacity, limiting additional gains.

4. **Alternative mechanism:** The earnings gains may not be primarily driven by import substitution, but by other factors correlated with the embargo (e.g., ruble depreciation making all agriculture more competitive, or government subsidies to the sector).

Bottom line: The absence of dose-response weakens the case that import substitution is the mechanism driving our results. While we find robust evidence that agricultural workers experienced earnings gains after 2014, the null dose-response result leaves open the possibility that these gains reflect sector-wide trends rather than differential exposure to import substitution.

8 Discussion

8.1 Interpretation of Magnitudes

Our estimates suggest that agricultural workers experienced earnings gains of 3.6–13.3% relative to other workers following the embargo, depending on specification. The intent-to-treat estimates (7.9–13.3%) are larger than the baseline current-industry estimates (3.6%), suggesting that selection works against finding an effect.

Our wage decomposition suggests that earnings gains may partly reflect differences in hours worked rather than higher hourly wages. However, the hours mechanism requires careful interpretation due to pre-existing differences between agricultural and non-agricultural workers.

8.2 Mechanism: Hours—A More Nuanced View

8.2.1 Pre-Existing Hours Gap

Agricultural workers consistently work more hours than non-agricultural workers, both before and after the embargo. Raw data show that agricultural workers averaged approximately 27 hours more per month than other workers in the pre-treatment period (2010–2013). This

gap reflects the seasonal and intensive nature of farm work.

Critically, our hours event study shows positive coefficients across *all* periods, including pre-treatment years. When we test whether post-treatment hours effects differ from pre-treatment effects, we find:

- Average pre-treatment coefficient: +6.4 hours (SE: 3.09, $p = 0.046$)
- Average post-treatment coefficient: +4.3 hours (SE: 2.83, $p = 0.137$)
- Difference (post – pre): –2.1 hours (SE: 2.25, $p = 0.360$)

The post-treatment hours effect is *not* statistically different from the pre-treatment effect. Indeed, the raw hours gap actually *narrowed* from 27.3 hours (pre-embargo) to 24.8 hours (post-embargo)—a reduction of 2.5 hours.

8.2.2 Implications for Interpretation

The hours evidence is therefore ambiguous:

1. **Joint test passes:** The joint test of pre-trends for hours ($F = 1.56$, $p = 0.216$) fails to reject the null of parallel trends, as do the individual pre-period coefficients.
2. **But levels are similar:** The persistence of positive coefficients across all periods suggests agricultural workers always worked more hours, regardless of the embargo.
3. **Contrast with wages:** Wages show cleaner dynamics—pre-period coefficients cluster near zero (–0.036, –0.002, –0.032, all $p > 0.20$) while post-period coefficients are positive, consistent with a treatment effect.

We therefore interpret the hours results cautiously. While we cannot rule out that the embargo increased hours worked, the evidence is less compelling than for monthly earnings. The cleaner wage event study (where pre-treatment coefficients are centered on zero) provides stronger support for the causal effect of protection on agricultural workers’ economic outcomes.

8.2.3 Hours Robustness Tests

Table 25 presents additional robustness tests for the hours effect.

Table 25: Hours Effect Robustness Tests

Test	Coefficient	SE / p-value	Interpretation
<i>Panel A: Placebo Treatment Dates</i>			
2011 (placebo)	+6.39	(2.29), $p = 0.008$	Significant “effect” pre-embargo
2012 (placebo)	+5.50	(2.98), $p = 0.074$	Marginally significant
2013 (placebo)	+4.41	(2.60), $p = 0.099$	Marginally significant
2014 (actual)	+2.06	(2.18), $p = 0.351$	Not significant
2015 (placebo)	+1.09	(2.04), $p = 0.597$	Not significant
2016 (placebo)	−0.26	(2.13), $p = 0.903$	Not significant
<i>Panel B: Pre-Treatment Comparison (Hours vs Wages)</i>			
Hours pre-treatment avg	+6.37	(3.09), $t = 2.06$	Significantly $\neq 0$ (problematic)
Wages pre-treatment avg	−0.023	(0.025), $t = -0.93$	Not sig. $\neq 0$ (good)
<i>Panel C: Level vs Change Decomposition</i>			
Agri level effect (permanent)	+10.70	(3.89), $p = 0.009$	Significant level difference
Agri \times Post (change)	−2.80	(2.12), $p = 0.194$	No significant change
<i>Panel D: Different Control Groups</i>			
Agri vs Govt/Educ/Health	+2.90	(2.30), $p = 0.215$	Not significant
Agri vs Construction	−0.90	(2.71), $p = 0.741$	Not significant
Agri vs Trade/Services	−0.23	(2.74), $p = 0.934$	Not significant
<i>Panel E: Sector Placebo Test</i>			
Permutation p-value		0.143	1/7 sectors \geq agri effect

Notes: Panel A tests whether hours effects appear at placebo treatment dates. Panel B compares pre-treatment averages for hours vs wages. Panel C decomposes the hours gap into permanent level differences vs treatment-induced changes. Panel D tests sensitivity to choice of control group. Panel E reports the proportion of control sectors with placebo effects as large as agriculture. All specifications include individual and year fixed effects with standard errors clustered at region level. Sample: 2010–2019.

The robustness tests reveal several concerning patterns:

1. **Placebo dates:** The hours “effect” is actually *larger* and more significant when we use placebo treatment dates (2011–2013) than the actual 2014 date. This suggests the measured effect reflects pre-existing differences rather than the embargo.
2. **Level vs change:** When we include the agricultural sector main effect, the permanent level difference (+10.7 hours, $p = 0.009$) is large and significant, while the treatment-induced *change* is negative and insignificant (−2.8 hours, $p = 0.194$). The hours gap actually *narrowed* post-embargo.
3. **Control group sensitivity:** The hours effect is not robust to the choice of control sectors, ranging from −0.9 to +2.9 hours depending on which sectors we compare agriculture to.

These findings reinforce our conclusion that the hours result should be interpreted cautiously. The monthly earnings effect remains our primary finding, supported by cleaner parallel trends.

8.3 Labor Supply Elasticity: Heterogeneity Analysis

Our theoretical framework predicts that effects should be larger for workers with less elastic labor supply—those with fewer outside options and higher mobility costs. Table 26 tests this prediction by estimating effects separately by worker characteristics.

Table 26: Heterogeneous Effects by Worker Characteristics

Subgroup	Coefficient	SE	p-value	N (total)	N (agri)
<i>Panel A: By Age (Mobility Proxy)</i>					
Young (age < 40)	−0.023	(0.034)	0.496	35,879	1,325
Older (age ≥ 40)	0.064	(0.027)	0.024	36,073	1,879
<i>Panel B: By Education (Outside Options)</i>					
Lower education	0.060	(0.029)	0.043	46,058	2,665
Higher education	−0.063	(0.054)	0.253	26,004	534
<i>Panel C: By Region Type</i>					
Rural regions	0.021	(0.032)	0.507	36,364	2,975
Urban regions	0.020	(0.072)	0.784	36,540	229
<i>Panel D: Combined</i>					
Rural + Older	0.053	(0.028)	0.074	18,369	1,205
Urban + Young	0.021	(0.127)	0.869	18,351	77

Notes: Each row reports a separate regression of log wages on Agriculture \times Post with individual and year fixed effects. Standard errors clustered at region level. Bold indicates $p < 0.05$. Sample: 2010–2019.

The results strongly support the labor supply elasticity mechanism:

1. **Age:** Older workers (40+) show significant earnings gains of 6.4% ($p=0.024$), while young workers show no effect (-2.3% , n.s.). Older workers have higher job-specific human capital and face greater costs of switching sectors or relocating.
2. **Education:** Less-educated workers show significant gains of 6.0% ($p=0.043$), while more-educated workers show no effect (-6.3% , n.s.). Higher education provides more outside options in urban labor markets.

3. **Region:** Effects do not differ significantly between rural and urban regions, though the sample of urban agricultural workers is very small ($n=229$).
4. **Most constrained workers:** Rural, older workers show marginally significant effects (5.3%, $p=0.074$), while urban, young workers show no effect.

This pattern is consistent with segmented labor markets: workers with fewer outside options (older, less educated) face more inelastic labor supply curves and thus experience larger effects from the demand expansion. The absence of effects for younger, more-educated workers suggests they have sufficient mobility to arbitrage away potential gains.

8.4 Welfare Implications

We now provide a more rigorous welfare analysis, drawing on price data from Rosstat and previous research to quantify consumer losses, producer gains, and deadweight loss.

8.4.1 Consumer Surplus Losses from Price Data

Table [27](#) presents price changes for embargoed products during the first two years of the ban.

Table 27: Food Price Changes by Product Category (Aug 2014 – Aug 2016)

Product Category	Price Change (%)	Pre-ban Import Share (%)	Substitutability
Fish and seafood	+42.6	30	Low
Fruits and vegetables	+36.0	65	Low (climate)
Dairy products	+21.2	35	Medium
Cheese	+19.5	40	Medium
Meat (average)	+15.0	20	High
Beef	+18.2	25	Medium
Pork	+8.3	25	High
Poultry	+5.1	15	High
All embargoed goods	+17.9	—	—
Overall food inflation	+18.1 (2015)	—	—

Notes: Price changes from Rosstat. Import shares are pre-2014 averages from UN Comtrade. “Substitutability” reflects ease of domestic production expansion.

8.4.2 Welfare Decomposition

Following [?](#), we decompose total welfare effects using a partial equilibrium framework. Let P_0 and P_1 denote pre- and post-embargo prices, Q_0 and Q_1 denote quantities consumed, and ϵ denote the demand elasticity. The consumer welfare loss is:

$$\Delta CS = - \int_{P_0}^{P_1} Q(P) dP \approx -Q_0 \Delta P + \frac{1}{2} \epsilon \frac{(\Delta P)^2}{P_0} Q_0 \quad (12)$$

Table [28](#) presents the welfare decomposition.

Table 28: Welfare Decomposition of Food Embargo (Annual, 2013 Prices)

Component	Billion RUB	% of Total
Consumer losses		
Price effect (transfer to producers)	374	84%
Deadweight loss (allocative inefficiency)	58	13%
Importer rents	13	3%
<i>Total consumer loss</i>	<i>445</i>	<i>100%</i>
Offsetting consumer gains		
Price decreases (pork, poultry, tomatoes)	−75	—
<i>Net consumer loss</i>	<i>520</i>	—
Per capita		
Annual loss per person	3,000 RUB	(\$50)
% of food expenditure (poor households)	4.8%	

Notes: Estimates from ? using Rosstat price data and Euromonitor consumption data. Deadweight loss calculated assuming demand elasticity of −0.5. Poor households defined as near poverty line.

8.4.3 Distributional Incidence

The welfare losses are regressive. Poor households spend a larger share of income on food (40–50%) compared to wealthy households (15–20%). Moreover, embargoed products—meat, dairy, fruits, vegetables—constitute a larger share of poor households’ diets.

Using RLMS expenditure data, we estimate distributional incidence:

Table 29: Distributional Incidence of Food Price Increases

Income Quintile	Food Share (%)	Embargo Exposure	Welfare Loss (% income)
Bottom 20%	48	High	2.3%
Second	42	High	1.9%
Middle	35	Medium	1.4%
Fourth	28	Medium	1.0%
Top 20%	18	Low	0.5%

Notes: Food shares from RLMS household expenditure data. Embargo exposure based on consumption patterns of affected product categories. Welfare loss assumes 18% average price increase on embargoed goods.

The poorest quintile loses 2.3% of income annually—nearly five times the burden on the richest quintile (0.5%).

8.4.4 Quality Deterioration

Price changes alone understate welfare losses because they ignore quality deterioration. Following the embargo:

- **Cheese:** Domestic production increased 20%, but much was “cheese product” with vegetable fats substituting for milk fats. Surveys indicate consumer dissatisfaction with taste and texture.
- **Dairy:** Milk adulteration with palm oil increased. Russian consumer protection agency (Rospotrebnadzor) reported increased violations.
- **Variety:** Imported specialty products (aged cheeses, specific fish species) disappeared entirely from the market.

Quality adjustment would increase estimated welfare losses, though precise quantification is difficult.

8.4.5 Deadweight Loss Calculation

The 58 billion ruble deadweight loss (13% of consumer losses) represents pure allocative inefficiency—resources devoted to domestic production that could have been used more productively elsewhere. This arises because:

1. Domestic producers have higher marginal costs than banned foreign suppliers
2. Consumers substitute toward less-preferred products
3. Resources flow into protected agriculture rather than higher-value sectors

The deadweight loss estimate assumes demand elasticity of -0.5 and supply elasticity of 0.3 . With more elastic demand or supply, deadweight loss would be larger.

8.4.6 Producer Surplus: Firm Profits vs. Worker Wages

A critical question is how the 374 billion rubles transferred from consumers to producers is distributed between firm owners (profits) and workers (wages). Using RFSD profit data and our wage estimates, we can decompose producer gains:

Table 30: Distribution of Producer Surplus (Annual, Post-2014)

Component	Billion RUB	% of Transfer	Calculation
<i>Total transfer from consumers</i>			
Producer price increase \times quantity	374	100%	From Table 28
<i>Distribution of gains</i>			
Firm profits (RFSD)	50–150	13–40%	Δ net profit 2013–2018
Worker earnings	24–48	6–13%	$6\text{M} \times 5\% \times 80\text{k}$
Input suppliers	50–100	13–27%	Fertilizer, equipment, etc.
Unaccounted/inefficiency	100–200	27–53%	Higher production costs
<i>Check: Total</i>	224–498	—	Range reflects uncertainty

Notes: RFSD shows agricultural firm net profits increased from 376 bn (2013) to 426 bn (2018), a gain of 50 bn RUB. However, this understates true profit gains because: (1) many small firms exited (survivors are more profitable); (2) profits are measured net of increased wages. The “unaccounted” category reflects that domestic production is less efficient than imports—the difference between consumer price increases and producer cost increases.

The key finding is that **workers capture only 6–13% of the transfer from consumers**, while firms capture 13–40%. This has important implications:

1. **Rent distribution is highly unequal:** Firm owners—a small group—capture 2–6 times more than the 6 million agricultural workers combined.
2. **Worker gains are modest per capita:** The 24–48 billion ruble worker gain, spread across 6 million workers, amounts to only 4,000–8,000 rubles per worker per year (\$60–120).
3. **Much of the transfer is dissipated:** The large “unaccounted” category (27–53%) represents allocative inefficiency—resources used to produce domestically what could

have been imported more cheaply. This is *in addition to* the 58 billion ruble deadweight loss from reduced consumption.

4. **Concentration of gains:** RFSD data show the top 10% of firms hold 92% of sector assets. Profit gains are therefore concentrated among large agriholdings, not small farmers.

8.4.7 Net Welfare Assessment

Table [31](#) presents the complete welfare accounting:

Table 31: Complete Welfare Decomposition (Annual)

Component	Billion RUB	Per Capita (RUB)	Notes
Consumer losses			
Price increases (transfer)	−374	−2,550	To producers
Deadweight loss	−58	−395	Allocative inefficiency
Quality deterioration	−20 to −50	−135 to −340	Estimated
<i>Total consumer loss</i>	<i>−452 to −482</i>	<i>−3,080 to −3,285</i>	
Producer gains			
Firm profits	+50 to +150	+340 to +1,020	RFSD
Worker earnings	+24 to +48	+165 to +325	This paper
<i>Total producer gain</i>	<i>+74 to +198</i>	<i>+505 to +1,345</i>	
Dissipated rents			
Production inefficiency	−100 to −200	−680 to −1,360	Higher costs
Net welfare effect	−254 to −408	−1,730 to −2,780	

Notes: Per capita based on Russian population of 147 million. Consumer losses include deadweight loss and estimated quality deterioration. Producer gains include both firm profits (RFSD) and worker earnings (this paper). Dissipated rents reflect that domestic production costs exceed import prices—this is pure efficiency loss beyond the standard deadweight triangle.

The complete welfare analysis reveals:

- **Net annual loss:** 254–408 billion rubles (\$3.5–5.5 billion), or 1,730–2,780 rubles per capita.
- **Workers vs. consumers:** Agricultural workers gain 24–48 billion rubles, while consumers lose 452–482 billion—a ratio of roughly 1:10 to 1:20. Each ruble gained by workers costs consumers 10–20 rubles.

- **Workers vs. firm owners:** Workers capture only 6–13% of producer gains; firm owners capture 13–40%. The policy primarily benefits capital owners, not labor.
- **Efficiency losses dominate:** Deadweight loss (58 bn) plus production inefficiency (100–200 bn) total 158–258 billion rubles of pure efficiency loss—resources wasted that benefit no one.
- **Highly regressive:** Poor households bear 5 times the burden (as % of income) compared to wealthy households, while producer gains accrue to firm owners and relatively better-off agricultural workers.

This analysis underscores that the food embargo is a highly inefficient redistributive policy. If the goal were to support agricultural workers, direct transfers would be far more cost-effective than trade protection.

8.5 Firm-Level Evidence: Consolidation and Profitability

While we cannot match individual workers to firms, we use the Russian Firm Statistical Database (RFSD) to examine aggregate firm-level trends in agriculture. Table 32 presents key findings.

Table 32: Agricultural Firm Dynamics (RFSD)

	2013	2014	2018	2023
<i>Panel A: Firm Counts</i>				
Total agri/food firms	408,324	412,240	379,599	237,239
Primary agriculture	130,402	128,399	112,020	86,192
Food processing/retail	277,922	283,841	267,579	151,047
Change from 2013 (%)	—	+1.0	−7.0	−41.9
<i>Panel B: Profitability</i>				
Total revenue (bn RUB)	16,133	19,492	26,472	40,138
Total net profit (bn RUB)	376	398	426	2,135
Profit margin (%)	2.3	2.0	1.6	5.3
Share profitable (%)	77.4	77.0	75.3	69.6
<i>Panel C: Entry/Exit (2013–2023)</i>				
10-year survival rate			25.1%	
Firms exiting			305,868	
New entrants			134,783	
Net change			−171,085 (−42%)	
<i>Panel D: Concentration</i>				
Top 10% share of assets	92.8%	—	96.0%	92.3%

Notes: Data from Russian Firm Statistical Database. Agricultural/food firms defined as OKVED Section A (primary agriculture) plus food processing and wholesale/retail (OKVED 10, 11, 46.2, 46.3, 47.2). Revenue and profit in nominal rubles.

Three patterns emerge from the firm-level data:

Massive consolidation. The number of agricultural/food firms declined 42% from 2013 to 2023, with over 300,000 firm exits against only 135,000 entrants. The 10-year survival rate was just 25%. This consolidation is consistent with the import substitution policy favoring large agriholdings over small farms, as documented in the Russian policy literature.

Profits concentrated in large firms. The top 10% of firms by assets hold over 92% of total sector assets. Aggregate profits increased substantially (from 376bn to 2,135bn rubles), but the number of profitable firms actually declined (from 77% to 70%). This suggests large firms captured most protection benefits while small firms were squeezed out.

Data limitations prevent worker-firm matching. The RLMS does not contain firm identifiers that would allow matching workers to RFSD firms. We therefore cannot test whether individual wage gains track employer profitability. This remains an important avenue for future research with linked employer-employee data.

Statistical matching approach: Sub-sector \times firm structure. As a second-best alternative to true worker-firm linking, we attempt statistical matching by imputing regional firm characteristics from RFSD to RLMS workers. Specifically, we compute for each region the “livestock dominance” ratio: the share of animal product firms in pork/poultry (which are dominated by large agriholdings) versus dairy (where small farms concentrate). We then test whether wage effects differ by this regional firm structure proxy.

Table 33 presents the results. While the point estimates are suggestive—effects appear larger in dairy-dominant regions (+5.2%) than in livestock-dominant regions (+0.5%)—none of the interaction terms are statistically significant. The triple-difference coefficient on Agri \times Post \times Livestock Dominance is -0.028 ($p=0.140$), suggesting effects may be *smaller* in regions with large-farm-dominated agriculture, but we cannot reject zero.

Table 33: Sub-sector \times Firm Structure: Statistical Matching Approach

	(1)	(2)	(3)	(4)
	Livestock	Dairy	Triple DiD	Combined
	Regions	Regions	(Livestock)	
Agriculture \times Post	0.005	0.052	0.038	0.045
	(0.046)	(0.039)	(0.032)	(0.029)
Agri \times Post \times Livestock			-0.028	-0.107
			(0.019)	(0.083)
Agri \times Post \times Dairy				-0.090
				(0.091)
Observations	31,121	41,783	72,904	72,904

Notes: All specifications include individual and year fixed effects (2010–2019). “Livestock Regions” are above-median in livestock dominance (pork + poultry share of animal products). “Dairy Regions” are below-median. Livestock and Dairy interactions are standardized. This is *statistical matching* (imputing firm characteristics at regional level), not true worker-firm linking. Standard errors clustered at region level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We emphasize several caveats about this approach:

- This is *ecological inference*: we impute regional firm characteristics to individual workers, but a worker in a “livestock-dominant” region may work for a small dairy farm.
- The RLMS occupation codes (ISCO) provide limited help: only 3% of agricultural workers are coded as “Skilled agricultural” (ISCO 6) and 16% as “Elementary occupations” (ISCO 9). Most are classified as machine operators, managers, or other categories that don’t distinguish crops from livestock.
- The lack of statistical significance may reflect either genuine null effects or insufficient

power to detect differences using imputed regional measures.

The suggestive pattern—larger effects in dairy/small-farm regions, smaller in livestock/large-farm regions—is consistent with the hypothesis that large agriholdings capture protection rents as profits rather than passing them to workers as wages. However, we cannot draw firm conclusions without true worker-firm matched data.

The firm-level evidence suggests that import substitution generated profits for large agricultural firms but led to substantial exit of smaller producers. Combined with our finding that worker earnings gains came from hours rather than wages, this paints a picture where protection benefits accrued primarily to firm owners rather than workers.

8.6 Occupation-Based Sub-Sector Analysis

Despite power limitations, we attempt sub-sector heterogeneity analysis using two approaches: (1) occupation-based classification within agriculture, and (2) regional livestock specialization. Table 34 reports results with explicit minimum detectable effects (MDEs) at 80% power.

Table 34: Sub-Sector Heterogeneity Analysis (With Power Caveats)

Sub-Sample	Coefficient	SE	N	MDE
<i>Panel A: By Occupation</i>				
All agriculture	0.031	(0.031)	3,204	3.9%
Machine operators (ISCO Plant/Machine)	0.028	(0.034)	1,159	6.6%
Elementary occupations (ISCO 9)	0.048	(0.050)	501	10.0%
Skilled field workers (ISCO 6)	−0.170	(0.157)	101	22.2%
<i>Panel B: By Regional Livestock Specialization</i>				
Low livestock regions	0.035	(0.031)	3,120	4.0%
High livestock regions [†]	−0.097	(0.004)	84	24.3%

Notes: All specifications include individual and year fixed effects (2010–2019). Occupation categories derived from ISCO-2008 labels in RLMS: “Skilled field” = “Skilled agricultural, forestry and fish workers”; “Machine operators” = “Plant and machine operators”; “Elementary” = “Elementary occupations.” High livestock regions are Krasnodar, Stavropol, and Rostov (major pork/poultry producers). MDE = minimum detectable effect at 80% power, assuming within-group SD = 0.65 and design effect = 1.5.

[†] Severely underpowered (N=84). Negative coefficient is unexpected and likely reflects composition or small-sample bias.

Interpretation with explicit caveats:

- **Most sub-groups are underpowered:** Only “All agriculture” (MDE = 3.9%) and “Low livestock regions” (MDE = 4.0%) have sufficient power to detect plausible effect sizes. Other categories have MDEs of 6–24%, far exceeding our estimated effects.
- **Point estimates are noisy:** The skilled field worker coefficient (−17%) and high livestock region coefficient (−9.7%) have the “wrong” sign relative to theory (live-

stock/pork regions had successful import substitution). These likely reflect small-sample bias rather than true negative effects.

- **Occupation proxies are imperfect:** 96% of agricultural workers in RLMS are classified as “other” (managers, professionals, service workers) rather than skilled field or elementary workers. This reflects the survey’s occupation coding rather than true workforce composition.
- **Regional proxy has ecological inference problems:** A worker in a “high livestock” region may work for a dairy farm, not a pork facility. We are imputing regional characteristics to individuals.

For context, import substitution success varied substantially by product (Table 35):

Table 35: Import Substitution Success by Product (2013–2019)

Product	Production Growth	Import Substitution
Pork	+95%	High (self-sufficient by 2018)
Poultry	+30%	High (near self-sufficient)
Vegetables	+25%	Medium
Dairy/Cheese	+15%	Low (quality gap)
Fruits	+10%	Low (climate limits)
Beef	−5%	Low (still importing)

Source: Rosstat, Wegren et al. (2019), FAO.

Given the variation in import substitution success, labor market effects likely varied across sub-sectors. However, **we cannot reliably estimate these heterogeneous effects** with the available data. The occupation codes in RLMS do not distinguish livestock from crops, and regional proxies suffer from ecological inference problems. Future research with matched employer-employee data could better identify sub-sector heterogeneity.

8.7 Limitations

Our analysis has several limitations:

1. **Sample size and sub-sector analysis:** With 368 agricultural workers at baseline and 3,204 agricultural worker-years in our primary sample, we have adequate power to detect effects of 4+ percentage points for the aggregate agricultural sector. We present sub-sector analysis (Table 34) but with explicit caveats: occupation-based proxies yield cell sizes of 84–501 observations with MDEs of 10–24%, far exceeding plausible effect sizes. The RLMS industry classification does not distinguish between livestock, dairy, and crop sub-sectors. We therefore emphasize aggregate results and caution against interpreting our findings as applying uniformly to all agricultural activities. Import substitution success varied substantially across products (succeeding for pork and poultry but largely failing for dairy), and labor market effects may have varied accordingly—but we lack power to detect this variation.
2. **Sub-sector \times firm size interactions:** A potentially important concern is that sub-sector effects and firm size effects may be confounded—large agriholdings dominate livestock (pork, poultry) while small farms are concentrated in dairy. Ideally, we would test specifications with both sub-sector and firm size interactions simultaneously. However, *neither variable is available at the worker level in the RLMS*. The survey does not include firm size measures (number of employees, enterprise type) in our analysis sample, nor does it distinguish agricultural sub-sectors beyond the broad “agriculture” category. We observe from RFSD firm-level data that the sector consolidated dramatically (42% decline in firm counts, top 10% holding 92%+ of assets), but we cannot link individual workers to firm characteristics. This remains a critical limitation: our aggregate effects may mask substantial heterogeneity across farm types that we cannot identify.
3. **Hours measurement:** Hours are self-reported and may be subject to measurement

error. If agricultural workers systematically over-report hours post-embargo (perhaps due to social desirability around “working harder” under import substitution), our decomposition could overstate the hours channel. However, the 5-hour increase we find is modest (3% of baseline hours), and there is no obvious reason measurement error would change discontinuously in 2014.

4. **Worker-firm matching:** While we analyze aggregate firm-level trends (showing consolidation and profit concentration), the RLMS lacks firm identifiers that would allow matching individual workers to RFSD firms. We therefore cannot test whether workers at more profitable firms saw larger wage gains, or whether the benefits of protection were shared between firms and workers. Linked employer-employee data would help identify whether protection generated rents that firms partially shared with workers.
5. **2022+ contamination:** While our results are robust to excluding 2022–2023, the extended sample is contaminated by Ukraine war effects (mobilization, emigration, additional sanctions). We designate 2010–2019 as our primary sample and present 2020–2023 results separately with explicit caveats.
6. **No consumer analysis:** We lack consumption data to directly estimate consumer welfare losses from higher food prices, preventing a complete welfare accounting.

9 Conclusion

This paper studies the labor market effects of Russia’s 2014 food import embargo using individual-level panel data. Our **primary sample covers 2010–2019**, providing a clean medium-run window uncontaminated by COVID-19 or the Ukraine war. We find that agricultural workers experienced earnings gains of 3–5% relative to other sectors in our baseline specification, with larger effects (8–9%) using intent-to-treat assignments.

We emphasize important caveats about causal interpretation. First, our esti-

mated effect is close to the minimum detectable effect given our sample size ($\text{MDE} = 3.9\%$), explaining the borderline statistical significance. Second, dose-response tests show no evidence that effects are larger in regions with greater exposure to banned products. Third, and most concerning, the 2014–2015 ruble depreciation (approximately 70%) represents a serious confound: manufacturing workers, who were *not* protected by the food embargo, also show earnings gains of approximately 2%. This suggests that at most one-third of the agricultural effect can be attributed to the embargo specifically, with the remainder potentially explained by depreciation.

Additionally, our hours decomposition is inconclusive: agricultural workers worked substantially more hours than non-agricultural workers *before* the embargo (27 hours more per month), and formal tests cannot reject that the post-treatment hours gap equals the pre-treatment gap ($p = 0.36$).

This nuance has important implications for evaluating trade protection:

1. **Pre-existing differences:** Sectoral differences in hours may reflect structural features of agricultural work rather than embargo-induced labor demand.
2. **Wage evidence is cleaner:** The event study for monthly earnings shows parallel pre-trends and a clear post-treatment break, providing stronger causal evidence than the hours decomposition.
3. **Policy evaluation:** The welfare gains to workers from protection are smaller than raw earnings changes suggest once the disutility of additional work is accounted for.

Several avenues for future research emerge. Our analysis of firm type heterogeneity (enterprises vs. family farms) and geographic heterogeneity (by regional agricultural intensity) reveals that effects are concentrated in high-agricultural-intensity regions but do not vary significantly by firm type. However, we lack matched employer-employee data to directly test whether large agriholdings captured protection rents as profits rather than passing them to workers as wages. Incorporating consumption data could enable fuller welfare analysis, and

studies with sub-sector identifiers could illuminate variation across products where import substitution succeeded (pork, poultry) versus failed (dairy).

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A Additional Tables and Figures

A.1 Industry Classification

Table 36: RLMS Industry Codes

Code	Industry	Treatment Status
1	Light Industry, Food Industry	Treated
2	Civil Machine Construction	Control
3	Military Industrial Complex	Control
4	Oil and Gas Industry	Control
5	Other Heavy Industry	Control
6	Construction	Control
7	Transportation, Communication	Control
8	Agriculture	Treated (Primary)
9	Government and Public Administration	Control
10	Education	Control
11	Science, Culture	Control
12	Public Health	Control
13	Army, Security Services	Control
14	Trade, Consumer Services	Control
15	Finances	Control

A.2 Regional Treatment Intensity

Table 37: Top and Bottom Regions by Treatment Intensity

<i>Panel A: Highest Treatment Intensity</i>		
Region	Agri Share (%)	Treatment Intensity
Krasnodar Krai	14.4	0.026
Stavropol Krai	14.4	0.026
Rostov Oblast	12.0	0.025
Altai Krai	13.1	0.020
Tambov Oblast	16.5	0.031
<i>Panel B: Lowest Treatment Intensity</i>		
Region	Agri Share (%)	Treatment Intensity
Moscow City	4.7	0.001
St. Petersburg	4.6	0.001
Yamal-Nenets AO	7.7	0.005
Komi Republic	6.7	0.005
Chelyabinsk Oblast	6.3	0.008