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Does the Capacity Reduction Policy Affect Income and Household Expenditure? Evidence from China

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Abstract

This thesis investigates whether China's capacity reduction policy affected workers' income and household expenditure. The analysis uses seven waves of China Family Panel Studies data from 2010 to 2022 and estimates difference-in-differences and event-study models for individual income, wage income, family income, and family expenditure. The income estimates are generally small, imprecise, and sensitive to treatment definitions and matching. The clearest pattern appears in household expenditure. In the controlled baseline event-study, the estimates imply expenditure reductions of approximately 15–28 percent between 2014 and 2020, relative to the 2010 treatment-control difference, with the largest decline occurring in 2016. The average fixed-effects DID estimate is also negative but is not statistically significant, so the expenditure result is interpreted mainly as dynamic evidence. Alternative treatment and control groups, matched samples, policy-timing specifications, and outlier checks produce a broadly similar pattern. Overall, the results indicate that industrial restructuring may affect household welfare through spending responses to perceived income and employment risk, even when changes in measured income are weak.

Keywords: Capacity reduction policy; supply-side structural reform; difference-in-differences; household welfare; China Family Panel Studies (CFPS)

Table of Contents

1. Introduction 1
2. Background 4
    2.1 China's Growth Model and Industrial Overcapacity 4
    2.2 Evolution and Implementation of the Capacity Reduction Policy 6
    2.3 Labour-Market and Household Welfare Implications 7
3. Method 9
    3.1 Empirical Strategy 9
    3.2 Treatment and Control Group Definitions 10
    3.3 Policy Timing and Outcome Variables 13
    3.4 Baseline Specifications 14
    3.5 Control Variables and Identification Assumptions 16
    3.6 Statistical Inference and Interpretation of Coefficients 18
    3.7 Additional Robustness and Sensitivity Analyses 19
4. Data 20
    4.1 Data Source 20
    4.2 Sample Construction 21
    4.3 Variables and Descriptive Statistics 22
    4.4 Data Quality and Measurement 26
5. Results 27
    5.1 Worker-Level Income Outcomes 27
    5.2 Household Income and Expenditure Outcomes 30
    5.3 Interpretation and Mechanism Discussion 33
6. Robustness 35
    6.1 Overview of Robustness Analyses 35
    6.2 Alternative Control Group Definitions 36
    6.3 Alternative Treatment Group Definitions 39
    6.4 Matching-Based Difference-in-Differences Analysis 42
    6.5 Sensitivity to Alternative Policy Timing 46
    6.6 Sensitivity to Outlier Treatment 49
    6.7 Summary of Robustness Evidence 50
7. Policy Implications 52
8. Conclusion 55
References 58
Appendix. 65

1. Introduction

Over the past two decades, China's growth model has been closely linked to industrialisation, urbanisation, and investment-led expansion, with upstream heavy industries playing a central role. While this model supported sustained output growth, it also generated persistent structural imbalances, particularly resource misallocation and excess capacity in heavy industrial sectors. Misallocation lowers aggregate productivity when capital and labour remain tied to relatively inefficient firms and sectors instead of moving toward more productive activities (Hsieh & Klenow, 2009; Song et al., 2011). In China, these problems were reinforced by local government incentives, continued support for state-linked firms, and weak market exit mechanisms, which slowed restructuring and prolonged inefficient production (Li & Zhou, 2005; Brandt et al., 2012). As growth slowed and profitability weakened in many heavy industries after the global financial crisis, overcapacity became a more visible macroeconomic and policy concern.

Against this background, China introduced a broader supply-side structural reform agenda in which capacity reduction became one of the central policy instruments. The reform process was institutionalised in 2013 and implemented more intensively around 2015–2016. It combined administrative measures, including the closure of obsolete or non-compliant capacity and tighter approval standards, with financial and organisational measures related to restructuring, consolidation, debt resolution, and worker resettlement. The policy was also part of a wider effort to improve resource allocation, reduce financial risks, and promote industrial upgrading in sectors characterised by low efficiency, high leverage, and high pollution intensity (Caballero et al., 2008; Shen & Chen, 2017; Shi et al., 2018). At the aggregate level, reforms that induce exit and reallocation may improve efficiency when resources move from less productive to more productive activities. However, industrial restructuring may also impose concentrated adjustment costs on particular workers, households, and localities, even when it is justified on efficiency, financial stability, or environmental grounds.

Research on structural shocks and worker displacement shows that labour-market losses can be persistent when affected workers possess industry-specific human capital and face barriers to reallocation. Displacement may therefore generate long-lasting earnings declines rather than only temporary income fluctuations (Jacobson et al., 1993; Couch & Placzek, 2010; Schmieder et al., 2023; Lachowska et al., 2020). Sector-specific shocks can also spread beyond directly exposed workers through local labour markets and local demand, creating broader welfare costs within affected communities (Moretti, 2010; Autor et al., 2013; Dix-Carneiro & Kovak, 2017; Notowidigdo, 2020). These mechanisms are relevant to China's capacity reduction policy because targeted industries such as steel, coal, cement, and related heavy sectors are often regionally concentrated and closely connected to local employment systems.

Worker income, however, is only one component of household welfare. Studies of consumption insurance and household adjustment show that households may respond to adverse income shocks by using savings, reallocating labour supply, relying on transfers, or changing the timing and composition of consumption (Cochrane, 1991; Blundell et al., 2008; Heathcote et al., 2014; Blundell et al., 2016). Household labour supply can also adjust when a primary earner faces a labour-market shock, weakening the direct transmission from individual earnings losses to total household resources (Stephens, 2002; Fadlon & Nielsen, 2019). Consumption commitments, liquidity constraints, and incomplete insurance further affect how income shocks are translated into household expenditure and welfare (Gruber, 1997; Chetty, 2008; Chetty & Szeidl, 2007).

Household expenditure may also respond to expected income risk, perceived job insecurity, and policy-induced uncertainty, rather than only to realised income losses. A policy that signals industrial downsizing can affect household behaviour before large changes in observed income appear in the data. Uncertainty may reduce consumption and investment by increasing the value of waiting and encouraging households to preserve liquidity (Bloom, 2009; Carroll, 1997). Policy-related uncertainty may also shape household and firm behaviour by changing expectations

about future economic conditions (Baker et al., 2016). These arguments suggest that the welfare effects of capacity reduction should be evaluated through both income-related outcomes and household expenditure, rather than through a mechanical link from worker income to household spending.

Micro-level causal evidence on China's capacity reduction reform remains limited. Existing studies have mainly assessed restructuring using firm-level, sector-level, or macro indicators such as productivity, output, profitability, overcapacity reduction, and environmental performance. These outcomes are important for evaluating aggregate and sectoral performance, but they do not directly show how policy-induced industrial adjustment affected workers and households over time. In particular, less is known about whether exposed workers experienced income changes, whether these changes were transmitted to household income, and whether households adjusted expenditure in response to income risk and uncertainty. This gap is important because capacity reduction may generate concentrated labour-market risk while also inducing precautionary and coping responses at the household level (Brandt et al., 2012; Shen & Chen, 2017).

This thesis examines whether China's capacity reduction policy affected workers in exposed industries and how household income and expenditure adjusted to capacity-reduction-related exposure. Using seven waves of nationally representative panel data from the China Family Panel Studies (CFPS) covering 2010–2022, the analysis defines workers in policy-covered industries and related occupations as the treatment group and workers in non-covered sectors as the control group. A difference-in-differences design and event-study specifications are used to estimate average and dynamic effects on four outcomes: individual income, wage income, family income, and family expenditure. This framework allows the analysis to assess both worker-level income responses and household-level adjustment to labour-market exposure, income risk, and policy-induced uncertainty.

The thesis contributes to the literature in three ways. First, it provides micro-level evidence on the labour-market and household-welfare consequences of China's capacity reduction policy, complementing existing studies that mainly

examine firm-level, sector-level, or aggregate outcomes. Second, by analysing individual income, wage income, family income, and family expenditure within the same empirical framework, it distinguishes between measured income effects and household expenditure adjustment. Third, the empirical results show that the strength of the evidence differs across outcomes. Income-related outcomes are weaker and more sensitive to specification choices, whereas household expenditure shows the most consistent evidence of adjustment. This pattern suggests that the welfare effects of supply-side structural reform may operate not only through realised income changes, but also through household responses to income risk, employment uncertainty, and precautionary motives.

This distinction is important for evaluating structural reform. Policies that promote productivity growth, financial risk control, environmental improvement, and industrial upgrading may still create concentrated adjustment pressures for affected workers, households, and communities. This tension is especially relevant in China's transition toward higher-quality growth, where the benefits of restructuring may be distributed differently from its costs. Evaluating capacity reduction therefore requires attention not only to aggregate efficiency gains, but also to the distribution of adjustment costs and the social sustainability of the transition.

The remainder of the thesis is organised as follows. Section 2 provides the institutional background and discusses the main channels through which capacity reduction may affect workers and households. Section 3 presents the empirical strategy. Section 4 describes the data, sample construction, and descriptive statistics. Section 5 reports the baseline results. Section 6 presents robustness checks. Section 7 discusses policy implications, and Section 8 concludes.

2. Background

2.1 China's Growth Model and Industrial Overcapacity

China's rapid industrialisation and urbanisation in the early 2000s were supported by an investment-driven growth model that relied heavily on infrastructure construction, real estate development, and upstream heavy industries. This growth

pattern helped sustain high output expansion, but it also directed large amounts of capital toward traditional industrial sectors and gradually intensified structural imbalances. A key issue was that distortions in factor allocation allowed less efficient firms and sectors to retain or attract resources that could otherwise have been used more productively. This type of misallocation can reduce aggregate productivity when capital and labour do not move toward more efficient producers (Hsieh & Klenow, 2009; Song et al., 2011). Evidence from Chinese manufacturing also shows that productivity growth has been closely connected to resource reallocation across firms and sectors (Brandt et al., 2012).

Local government incentives further reinforced this investment pattern. Under China's decentralised governance system and performance-based promotion framework, local officials had strong incentives to support industrial projects that could raise GDP, employment, and fiscal revenue in the short run (Li & Zhou, 2005). These incentives encouraged continued investment in high-output and capital-intensive sectors, including steel, coal, cement, and other upstream industries. At the same time, access to bank credit, policy support for state-owned and locally important enterprises, and weak market exit mechanisms reduced the pressure on inefficient firms to leave the market. Over time, excess capacity became embedded in many traditional industrial sectors rather than appearing only as a temporary cyclical problem.

These sectoral and institutional conditions meant that, by the early 2010s, overcapacity had become a more visible macroeconomic and structural concern. Falling prices, declining profitability, rising leverage, and inventory accumulation in heavy industries exposed the limits of the earlier growth model. Some firms continued to operate not because they were competitive, but because refinancing, local support, or implicit guarantees postponed restructuring and exit. This pattern is consistent with the literature on zombie firms, which shows that continued support for inefficient firms can distort competition and worsen resource misallocation (Caballero et al., 2008). In China, zombie firms have also been linked directly to persistent overcapacity in manufacturing sectors (Shen & Chen, 2017).

Capacity reduction should therefore be understood as part of China's broader shift from high-speed growth toward higher-quality development. The policy was intended to address inefficiency, financial risk, and industrial upgrading at the firm and sectoral levels. At the same time, reducing capacity in industries such as steel, coal, cement, and related heavy sectors could create adjustment pressures for workers and households connected to those industries. This background helps explain why a policy designed to improve aggregate efficiency may also have important distributional and household-welfare consequences.

2.2 Evolution and Implementation of the Capacity Reduction Policy

China's capacity reduction policy was not introduced as a one-off intervention. It developed through a sequence of policy documents and implementation measures. An important institutional starting point was the State Council's Guiding Opinions on Resolving the Contradiction of Severe Overcapacity, issued in October 2013. The document identified several sectors with severe overcapacity, including steel, cement, electrolytic aluminium, flat glass, and shipbuilding. It also set out the basic policy direction by calling for stricter control over new capacity, the closure of illegal and obsolete capacity, and the promotion of mergers, restructuring, and industrial reorganisation.

Capacity reduction became more central to national economic policy in late 2015 and early 2016, when supply-side structural reform was elevated within the reform agenda and "cutting overcapacity" was listed as one of the five major tasks of structural reform (State Council of the People's Republic of China, 2016a; Xinhua News Agency, 2015). The 2016 policy agenda placed particular emphasis on steel and coal, two sectors with large excess capacity, high leverage, and close links to local employment. Policy measures included tighter control of new capacity, elimination of outdated capacity, mergers and restructuring, debt resolution, bankruptcy procedures, and support for worker resettlement (State Council of the People's Republic of China, 2016a). Sector-specific State Council documents on steel and coal then translated these broad objectives into more concrete implementation arrangements (State

Council of the People's Republic of China, 2016b, 2016c). In this sense, 2013 marks the beginning of a systematic policy framework, while the period around 2016 represents a phase of stronger and more coordinated enforcement.

The implementation of capacity reduction combined administrative controls with financial and organisational measures. Administrative tools included stricter approval standards, restrictions on new investment, closure of non-compliant and obsolete facilities, and production limits in heavily polluting industries (State Council of the People's Republic of China, 2013, 2016b, 2016c). These measures were accompanied by support for mergers and restructuring, worker resettlement and retraining, and tighter financing conditions for inefficient firms (State Council of the People's Republic of China, 2016a, 2016b, 2016c). The intensity of implementation differed across industries and regions. It was generally stronger in coal, steel, and other resource-intensive sectors, and in areas where heavy-industry concentration, environmental pressure, and local overcapacity problems were more severe (Shen & Chen, 2017; Shi et al., 2018).

This thesis therefore treats capacity reduction as an evolving policy process rather than as a single policy shock occurring at one date. The 2013 State Council document established the institutional framework, the years around 2016 marked stronger implementation, and the later period reflected continued governance under the broader objectives of industrial restructuring, financial risk control, and environmental improvement. This timing matters for the interpretation of household responses. Workers and households connected to affected sectors may have adjusted not only after realised job losses or income changes, but also in response to policy signals, expected restructuring, and perceived employment risk. Capacity reduction may therefore have affected household behaviour through uncertainty and precautionary adjustment before its full labour-market effects were observed in measured income outcomes.

2.3 Labour-Market and Household Welfare Implications

The link between capacity reduction and household welfare is likely to operate

first through the labour market. Plant closures, output restrictions, tighter capacity controls, and industrial restructuring can change firms' production decisions and reduce labour demand in targeted sectors. For affected workers, these changes may appear as layoffs, underemployment, reduced overtime, lower bonuses, wage compression, or reassignment to less favourable positions. Evidence from the broader literature on worker displacement and local labour-market shocks shows that major industrial changes can generate substantial and persistent earnings losses for exposed workers and communities (Jacobson et al., 1993; Autor et al., 2013). In the setting of China's capacity reduction policy, the most immediate effects are therefore likely to be observed in individual income and wage income among workers connected to policy-affected industries.

The household consequences of capacity reduction may also arise before large realised income losses are observed. A policy that signals industrial downsizing can change expectations about future employment, wage stability, and sectoral prospects. Workers and their families may respond to this perceived risk by adjusting consumption even if current earnings have not yet fallen substantially. This precautionary channel is important because uncertainty can reduce consumption and investment by increasing the value of waiting and encouraging households to preserve liquidity (Bloom, 2009; Carroll, 1997). Household expenditure may therefore respond to capacity-reduction exposure even when contemporaneous measured income shows only weak or statistically insignificant changes.

Worker-level shocks do not necessarily translate mechanically into household-level outcomes. A decline, or perceived risk of decline, in one worker's earnings may be partly absorbed within the household through savings, transfers, changes in labour supply by other members, or adjustments in the timing and composition of spending. Households may reduce discretionary expenditure, postpone durable purchases, or smooth consumption over time rather than immediately cutting all categories of spending. Studies of consumption insurance and household adjustment suggest that expenditure often responds less than proportionally to income shocks, particularly when households have access to internal coping mechanisms or

external support networks (Cochrane, 1991; Blundell et al., 2008; Heathcote et al., 2014; Blundell et al., 2016). In China, these responses may also be shaped by sector-specific skills, migration constraints, family-based support, and precautionary saving motives.

These mechanisms motivate the empirical focus of this thesis. Existing research has examined China's restructuring process mainly through macroeconomic indicators, industrial policy, and firm-level performance, while less is known about how industrial downsizing affects workers and their families at the micro level (Brandt et al., 2012; Shen & Chen, 2017). Using panel data from the China Family Panel Studies (CFPS) for 2010–2022, this study examines whether workers exposed to capacity-reduction-related sectors experienced changes in income, whether these changes were transmitted to household income, and whether household expenditure adjusted in response to labour-market risk and policy-induced uncertainty. The empirical analysis uses difference-in-differences and event-study specifications to estimate effects on four outcomes: individual income, wage income, family income, and family expenditure. The next section develops the empirical strategy used to identify these effects.

3. Method

3.1 Empirical Strategy

This study estimates the income and household-expenditure effects associated with China's capacity reduction policy using a difference-in-differences (DID) framework combined with an event-study specification. The identification strategy exploits variation along two dimensions: policy timing and heterogeneous exposure across industries and occupations. Specifically, workers classified as exposed to capacity-reduction-related industries or occupations are compared with workers in control groups before and after the policy period. Under the parallel-trends assumption, the DID framework estimates the average change in outcomes for the treated group relative to the corresponding change for the control group (Imbens & Wooldridge, 2009; Angrist & Pischke, 2010; Callaway & Sant'Anna, 2021; Roth et al.,

2023).

The baseline specification uses the main treatment group and the industrial-related control group. The main treatment group is chosen because it captures both direct exposure to policy-targeted sectors and closely related heavy-industrial occupations, while avoiding an overly narrow or overly broad treatment definition. The industrial-related control group is used because it compares treated workers with workers in economically related but non-treated industrial sectors. The detailed construction of the treatment and control groups is described in Section 3.2 and summarised in Table 1.

The event-study specification is used to examine how treatment-control differences evolve across CFPS survey waves. Using 2010 as the omitted base year, the event-study estimates show whether the gap between treated and control workers changes in each survey year relative to the baseline. The pre-policy coefficient for 2012 provides evidence on pre-policy comparability, while the post-policy coefficients describe the timing and persistence of treatment effects. Because the CFPS is biennial and the analysis observes only two pre-policy waves, the event-study evidence on pre-policy comparability is interpreted cautiously.

The analysis is conducted at both the individual and household levels. At the individual level, the main outcomes are individual income and wage income. At the household level, the main outcomes are family income and family expenditure. Applying a common empirical framework across these outcomes makes it possible to examine whether policy-related exposure is associated not only with worker-level income changes, but also with household-level income and expenditure adjustment. In the baseline specification, all income and expenditure outcomes are expressed in logarithmic form.

3.2 Treatment and Control Group Definitions

The treatment and control groups are constructed using workers' fixed industry and occupation classifications. Treatment status is assigned using industry and/or occupation criteria, whereas the control groups are constructed from non-treated

industry categories. Rather than relying only on a broad distinction between policy-covered and non-covered industries, the treatment definitions use a nested classification of industries and occupations related to China's capacity reduction policy. The classification follows the policy coverage described in official documents, which emphasises coal, steel, and other heavy-industrial sectors affected by severe overcapacity. It also allows exposure to extend beyond directly targeted industries to closely connected heavy-industrial occupations, where policy effects may be transmitted through production-network linkages (State Council of the People's Republic of China, 2013, 2016b, 2016c; Liu, 2019).

Table 1 summarises the treatment and control group definitions. The treatment definitions are organised into three nested categories. The strict treatment group, or Block A, includes workers employed in the mining industry, identified by industry code 2, as well as workers in specified mining- and extraction-related occupations irrespective of their reported industry. This group captures the most direct form of exposure to capacity-reduction-related sectors. The main treatment group adds Block B, which consists of selected heavy-industrial occupations within manufacturing, identified by industry code 3. These occupations cover metal smelting, steelmaking and metal rolling; shipbuilding, cement, glass and related materials; machinery processing; and equipment maintenance and assembly. This is the baseline treatment definition because it captures both direct exposure and closely related heavy-industrial production activities while avoiding an overly narrow definition. The broad treatment group further adds Block C, which includes broader production-related occupations within manufacturing. This definition captures a wider range of potentially exposed workers, but may also dilute the intensity of policy exposure.

Table 1. Summary of Treatment and Control-Group Definitions

Panel A. Treatment-group definitions		
Treatment-group definition	**Industry criterion**	**Coverage**
Strict treatment group (A)	Industry code 2, or specified mining- and extraction-related occupations with no industry restriction	Workers employed in the mining industry or in mining- and extraction-related occupations.
Main treatment group (A + B)	Block A, plus selected occupations within industry code 3	Strict-group coverage plus selected occupations in metal smelting, steelmaking and metal rolling; shipbuilding, cement, glass and related materials; machinery processing; and equipment maintenance and assembly
Broad treatment group (A + B + C)	Blocks A and B, plus broader production-related occupations within industry code 3	Main-group coverage plus additional workers in broader production-related occupations
Panel B. Control-group definitions		
Control-group definition	**Industry criterion**	**Included sectors**
Industrial-related control group	Industry codes 4, 5, and 6	Production and supply of electricity, gas and water; construction; transport, storage and postal services.
Supply-chain-related control group	Industry codes 4, 5, 6, and 11	Production and supply of electricity, gas and water; construction; transport, storage and postal services; real estate.
Pre-trend-selected control group	Industry codes 6, 9, and 15	Transport, storage and postal services; accommodation and catering; resident services and other services.

Notes: Treatment-group definitions are nested: Strict = Block A; Main = Blocks A + B; Broad = Blocks A + B + C. Treatment status is assigned using industry and/or occupation criteria, whereas the control groups are constructed from non-treated industry categories. The industry codes correspond to the cleaned CFPS industry classification: 2 = mining; 3 = manufacturing; 4 = production and supply of electricity, gas and water; 5 = construction; 6 = transport, storage and postal services; 9 = accommodation and catering; 11 = real estate; and 15 = resident services and other services. Individuals classified as treated under the corresponding treatment-group definition are excluded from the control group. Complete occupation codes and detailed construction rules are reported in Appendix Table A1.

The baseline analysis uses the main treatment group. The strict and broad treatment groups are retained for robustness checks. Comparing these definitions with the main specification helps assess whether the estimates depend on a narrower measure of direct exposure or on a broader measure that includes less intensively exposed workers.

The control groups are constructed from alternative sets of non-treated industry categories using economic and empirical criteria. The industrial-related control group includes workers in production and supply of electricity, gas and water, construction, and transport, storage and postal services. The supply-chain-related control group adds real estate to the industrial-related control group, reflecting potential linkages between heavy industry, construction, infrastructure, and property-related demand. The pre-trend-selected control group includes workers in sectors whose pre-policy outcome patterns are closer to those of the treated group. The industrial-related control group is used as the baseline control group, while the supply-chain-related and pre-trend-selected control groups are used as alternatives in the robustness analysis.

Throughout the analysis, individuals classified as treated under the corresponding treatment-group definition are excluded from the control group. Detailed industry criteria and occupation codes used to construct the treatment groups, together with the industry composition of the control groups, are reported in Appendix Table A1.

3.3 Policy Timing and Outcome Variables

China's capacity reduction policy evolved over time rather than being implemented as a single one-time intervention. The policy was institutionalised in 2013, while implementation became more intensive around 2015 and 2016 as cutting overcapacity became a central task of supply-side structural reform (State Council of the People's Republic of China, 2013, 2016a; Xinhua News Agency, 2015). Because the CFPS is conducted every two years, the 2014 wave is the first survey wave after the initial policy framework in which early policy responses could appear. The baseline specification therefore defines 2010 and 2012 as the pre-policy period and 2014, 2016, 2018, 2020, and 2022 as the post-policy period. The baseline post-policy indicator is defined as:

$$Post_t = \begin{cases} 1, & \text{if } t \geq 2014 \\ 0, & \text{otherwise} \end{cases} \tag{1}$$

This timing choice treats the 2013 policy framework as the starting point of systematic intervention and avoids classifying the 2014 survey wave as part of the

untreated pre-policy period. At the same time, implementation became stronger around 2016, especially in steel and coal. For this reason, an alternative timing definition that sets the post-policy period from 2016 onward is examined as a robustness check.

The outcome variables are selected to capture both worker-level labour-market responses and household-level welfare adjustment. At the individual level, the analysis uses individual income and wage income. At the household level, it uses family income and family expenditure. This distinction is important because a policy-related labour-market shock may affect a worker's own earnings without being transmitted fully to total household resources. Household expenditure may also adjust through precautionary behaviour, savings, transfers, or changes in spending composition even when measured income changes are limited.

All monetary variables are deflated to constant 2010 prices before estimation. In the baseline regressions, the income and expenditure outcomes are expressed in logarithmic form to reduce the influence of skewness and to allow the estimated coefficients to be interpreted approximately as proportional changes. The four baseline dependent variables are therefore log individual income, log wage income, log family income, and log family expenditure. Because logarithmic variables are defined only for positive monetary values, observations with zero or missing values for the relevant outcome are excluded from the corresponding outcome-specific estimation sample.

3.4 Baseline Specifications

The empirical analysis proceeds in three steps. First, pooled DID models are estimated with and without controls. The main pooled DID specification is:

$$Y_{it} = \alpha + \beta_1 \, Treat_{it} + \beta_2 \, Post_t + \beta_3 \, (Treat_i \times Post_t) + \gamma \, X_{it} + \varepsilon_{it} \qquad (2)$$

where Y_{it} denotes the outcome variable for unit i in year t, $Treat_i$ is an indicator equal to one if unit i is classified as treated based on the fixed treatment-group definition, $Post_t$ is the post-policy indicator defined in Equation (1). X_{it} is a vector of time-varying control variables. The coefficient of interest is β_3, which captures the

post-policy change in outcomes associated with treatment exposure, relative to the corresponding change for the control group. A corresponding specification without controls is also estimated.

Second, fixed-effects DID models are estimated using the panel structure of the data. The baseline fixed-effects specification is:

$$Y_{it} = \mu_i + \lambda_t + \beta_3 \left(Treat_i \times Post_t \right) + \gamma X_{it} + \varepsilon_{it} \tag{3}$$

where μ_i denotes unit fixed effects and λ_t denotes year fixed effects. For individual-level regressions, μ_i represents individual fixed effects; for household-level regressions, μ_i represents household fixed effects. In these models, the main effect of $Treat_i$ is absorbed by the unit fixed effects, while the main effect of $Post_t$ is absorbed by the year fixed effects. Therefore, the coefficient of interest is β_3 , which captures the additional post-policy change in outcomes for the treated group relative to the control group, after accounting for unit fixed effects, year fixed effects, and observed controls. A corresponding fixed-effects DID specification without controls is also estimated.

Third, an event-study specification is estimated to examine the dynamic effects of the policy. Using 2010 as the omitted base year, fixed treatment-group status is interacted with year dummies:

$$Y_{it} = \mu_i + \sum_{k \neq 2010} \theta_k Year_k + \sum_{k \neq 2010} \delta_k (Treat_i \times Year_k) + \gamma X_{it} + \varepsilon_{it} \tag{4}$$

where $Year_k$ denotes year dummy variables for 2012, 2014, 2016, 2018, 2020, and 2022, with 2010 omitted as the base year. The interaction coefficients δ_k capture the treated-control difference in year k, relative to the corresponding difference in 2010. The event-study model therefore provides a more flexible account of the timing and persistence of estimated treatment effects and is widely used to assess dynamic treatment effects and pre-policy patterns in DID settings (Jacobson et al., 1993; Roth, 2022; Sun & Abraham, 2021). A corresponding event-study specification without controls is also estimated.

Taken together, the pooled DID models provide a simple estimate of average post-policy differences, the fixed-effects DID models control for time-invariant

unobserved heterogeneity and common year shocks, and the event-study specification allows the treatment-control difference to vary across survey years.

3.5 Control Variables and Identification Assumptions

The with-controls specifications include a set of observable covariates chosen to account for systematic differences in human capital, demographic structure, and household composition that may affect income and expenditure trajectories independently of the policy shock. The role of these controls is not to create identification, which continues to rely on the DID design and the parallel-trends assumption, but rather to improve comparability between treated and control units and to reduce residual variation associated with observable characteristics.

For individual-level outcomes, the control set in the pooled specifications includes gender, age, age squared, marital status, years of education, and urban residence. These variables capture key differences in labour-market attachment, human capital, life-cycle earnings profiles, and local economic conditions. Years of education proxies for human capital, age and age squared capture the life-cycle profile of earnings, and urban residence accounts for differences in labour-market opportunities and local economic conditions. Marital status may also reflect household constraints and labour-supply decisions. More broadly, the labour-income literature shows that workers' earnings and adjustment to labour-demand shocks are shaped by both individual characteristics and local labour-market conditions (Jacobson et al., 1993; Notowidigdo, 2020). In the individual fixed-effects specifications, time-invariant characteristics such as gender are absorbed by individual fixed effects. The remaining controls therefore include variables that may vary over time, such as age, age squared, marital status, and, where applicable, years of education and urban residence.

For household-level outcomes, the controls include household representative age, household representative age squared, household representative years of education, indicators for whether the household representative is female and married, urban residence, and family size. These variables account for observable differences in

household socioeconomic position, demographic structure, and the scope for within-household adjustment. Household representative characteristics proxy for household socioeconomic position and potential earning capacity, while family size captures differences in resource needs and the possibility of risk-sharing within the household. The inclusion of these controls is consistent with the broader literature showing that the transmission of earnings shocks to household outcomes depends not only on income changes at the individual level, but also on household composition, labour-supply responses, partial insurance, and consumption smoothing (Cochrane, 1991; Blundell et al., 2008; Heathcote et al., 2014; Fadlon & Nielsen, 2019).

The main identifying assumption is parallel trends: in the absence of the capacity reduction policy, outcomes for treated and control units would have followed similar trajectories over time. The event-study specification provides an empirical check on this assumption by examining whether treated and control units displayed similar outcome dynamics before the post-policy period. In this study, the 2012 treatment-year interaction is the only pre-policy interaction coefficient relative to the omitted 2010 base year. The event-study evidence therefore provides a useful but limited check on pre-policy comparability, rather than a definitive test of the parallel-trends assumption.

Several identification concerns remain. First, workers classified as treated and workers in the control groups may have followed different outcome trajectories even before the policy period, especially because capacity-reduction-related industries and occupations differ from other sectors in worker composition, local labour-market conditions, and exposure to broader industrial change. Second, post-policy changes may partly reflect other contemporaneous shocks, such as macroeconomic slowdown, industrial restructuring, environmental regulation, or local economic conditions, rather than the capacity reduction policy alone. Third, treatment status is defined using a fixed treatment-group classification based on workers' industry and occupation information. This fixed exposure definition avoids mechanically redefining treatment status after the policy period. However, workers initially classified as treated may still move across jobs, sectors, or occupations after the policy.

As a result, the estimated worker-level income effects should be interpreted as effects associated with initial treatment exposure, rather than as effects of remaining continuously employed in treated sectors.

The empirical design addresses these concerns in several ways. Unit fixed effects absorb time-invariant differences between treated and control units, while year fixed effects capture common macroeconomic shocks affecting all units in a given survey year. Observable controls further account for differences in demographic characteristics, human capital, urban residence, and household composition. The event-study specification is used to examine the evolution of treatment-control differences over time, with the 2012 interaction coefficient providing limited evidence on pre-policy comparability. The study also implements matching-based DID as a supplementary robustness analysis to improve comparability between treated and control units on pre-policy observable characteristics and outcome patterns.

Overall, the empirical design combines treatment and control-group definitions based on policy-related industry and occupation exposure with unit fixed effects, year fixed effects, observable controls, event-study checks, and matching-based robustness analysis. These features strengthen the comparability of treated and control units and help separate policy-related exposure from time-invariant differences and common macroeconomic shocks. At the same time, because treatment status is fixed using the corresponding treatment-group definition, the estimates are best interpreted as effects associated with initial exposure to capacity-reduction-related sectors and occupations. The results are therefore interpreted together with the event-study patterns and robustness checks rather than relying on a single specification.

3.6 Statistical Inference and Interpretation of Coefficients

In panel DID settings, repeated observations for the same household may generate correlated errors over time. If this serial correlation is ignored, standard errors may be underestimated, leading to overstated statistical significance (Bertrand et al., 2004; Cameron & Miller, 2015). For this reason, all regressions report household-clustered robust standard errors. The same clustering rule is applied to both

individual- and household-level specifications, because individuals and households observed within the same household may share unobserved shocks and correlated outcome trajectories across survey waves.

Since the main dependent variables are expressed in logarithmic form, the coefficients on the DID interaction term and the event-study interaction terms can be read approximately as percentage changes. To report the estimates more precisely, the coefficients are converted into percentage effects using the semi-log transformation:

$$\%\triangle Y = 100 \times (e^{\beta} - 1) \tag{5}$$

following the semi-log interpretation in Halvorsen and Palmquist (1980).

3.7 Additional Robustness and Sensitivity Analyses

Several supplementary analyses are conducted to assess whether the baseline findings are sensitive to alternative group definitions, policy-timing assumptions, and sample construction choices. These checks focus on the main sources of uncertainty in the empirical design: how policy exposure is defined, which workers are used as controls, whether the post-policy period begins in 2014 or 2016, and whether the results are affected by outlier treatment.

The first robustness exercise keeps the baseline treatment definition fixed and varies the control group across the industrial-related, supply-chain-related, and pre-trend-selected control groups. This comparison examines whether the estimated effects depend on using a particular set of non-treated workers as the benchmark. The second exercise holds the industrial-related control group fixed and varies the treatment definition across the main, strict, and broad treatment groups. This allows the analysis to assess whether the results are stronger under a narrower definition of direct exposure or weaker when the treatment group is expanded to include more broadly exposed workers.

The analysis also includes a matching-based DID specification as a supplementary check on treated-control comparability. This exercise does not replace the baseline DID specification. Instead, it examines whether the main patterns remain similar when treated units are compared with more closely matched control units

based on pre-policy observable characteristics and outcome patterns. In addition, an alternative timing specification defines the post-policy period as beginning in 2016, corresponding to the more intensive implementation phase of the capacity reduction policy. Finally, the robustness analysis compares the baseline trimmed sample with an untrimmed sample to assess whether the results are sensitive to outlier treatment.

These exercises are interpreted as complementary sensitivity checks rather than alternative primary identification strategies. Their purpose is to evaluate whether the baseline findings are robust to plausible changes in control-group choice, treatment-group definition, policy timing, and sample construction. The detailed specifications and results are presented in Section 6.

4. Data

4.1 Data Source

This study uses data from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey administered by the Institute of Social Science Survey (ISSS) at Peking University. The CFPS follows individuals and households over time and contains information that is central to this analysis, including demographic characteristics, education, urban residence, employment, industry and occupation, income, wages, household income, and household expenditure. These features make the CFPS suitable for linking worker-level exposure to capacity-reduction-related sectors with both individual income outcomes and household-level welfare outcomes.

The analysis uses seven biennial survey waves: 2010, 2012, 2014, 2016, 2018, 2020, and 2022. This period covers the years before the institutional start of the capacity reduction policy, the early phase of policy implementation, and the later years in which implementation became more established. The panel structure of the CFPS allows the analysis to compare outcome trajectories across treated and control groups over time and to account for time-invariant individual or household characteristics in the fixed-effects specifications.

4.2 Sample Construction

The empirical analysis is based on adult workers observed in the 2010–2022 CFPS waves. The sample is constructed separately for individual-level and household-level outcomes. At the individual level, workers are classified into treatment and control groups using the fixed treatment-group definition based on their industry and occupation classification. At the household level, a household is classified as treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition.

The baseline sample uses the main treatment group and the industrial-related control group. The main treatment group includes workers in the mining industry or specified mining- and extraction-related occupations, together with selected heavy-industrial occupations within manufacturing. The industrial-related control group consists of workers in related industrial sectors—electricity, gas and water supply, construction, and transport, storage and postal services—after excluding individuals classified as treated under the baseline treatment definition. The same exclusion rule is applied to alternative control groups: individuals classified as treated under a given treatment definition are excluded from the corresponding control group. Alternative treatment definitions and alternative control groups are used only in the robustness analysis.

The baseline post-policy period is defined as 2014–2022, while 2010–2012 is treated as the pre-policy period. Observations with missing values in the relevant outcome variable, treatment or control-group classification, or core covariates used in the with-controls specifications are excluded from the corresponding estimation sample. Because the baseline dependent variables are expressed in logarithmic form, observations with zero or missing values for the relevant monetary outcome are excluded from the outcome-specific log sample. As a result, the number of observations differs across the individual income, wage income, and household outcome samples. In particular, the wage-income sample is smaller than the individual-income sample because log wage income is defined only for individuals

with positive and non-missing wage income, whereas individual income may include broader income sources.

Monetary outcomes are trimmed within each survey year to reduce the influence of extreme values. The trimming rule removes observations outside the year-specific lower and upper 1 percent of the relevant monetary outcome distribution. After applying the sample restrictions, log transformation requirements, and trimming rule, the baseline estimation samples contain 13,119 person-year observations for log individual income, 12,607 person-year observations for log wage income, and 10,784 household-year observations for the household-level outcomes. These are the samples used for the descriptive statistics reported in Tables 2 and 3.

4.3 Variables and Descriptive Statistics

The analysis uses four outcome variables to capture both worker-level and household-level responses. Individual income and wage income measure labour-market outcomes at the worker level, while family income and family expenditure capture broader household economic outcomes. The regression analysis uses logarithmic versions of these variables, whereas the descriptive statistics also report the corresponding level variables to show the raw patterns across survey waves. All monetary variables are measured in thousands of CNY and deflated to constant 2010 prices. Logarithmic variables are calculated using positive monetary values only.

Table 2 presents descriptive statistics for the main outcome variables by survey year. The table is based on the baseline sample, consisting of the main treatment group and the industrial-related control group, and is organised by outcome-specific estimation samples. Panel A reports the sample used for log individual income, Panel B reports the sample used for log wage income, and Panel C reports the household-level sample used for log family income and log family expenditure. This structure reflects the fact that the number of observations differs across outcomes, particularly because log wage income is observed only for individuals with positive wage income.

Table 2. Descriptive Statistics for Outcome Variables by Survey Year: Main Treatment and Industrial-Related Control Groups

		Pre-policy period			Post-policy period			
Variable	**Group**	**2010**	**2012**	**2014**	**2016**	**2018**	**2020**	**2022**
Panel A. Individual-level outcomes: log individual income sample (N = 13,119)								
Individual income	Main treatment	17.91	25.18	27.84	27.15	34.45	39.44	43.62
	Industrial-related control	21.42	20.50	26.96	26.86	33.58	36.20	41.83
Log individual income	Main treatment	2.70	3.03	3.12	3.08	3.34	3.49	3.57
	Industrial-related control	2.83	2.67	3.04	3.00	3.27	3.34	3.49
Observations (Panel A)	Main treatment	635	632	518	175	596	494	506
	Industrial-related control	1,388	2,023	1,114	503	1,593	1,464	1,478
Panel B. Individual-level outcomes: log wage income sample (N = 12,607)								
Wage income	Main treatment	17.91	24.92	26.92	16.63	33.01	37.46	40.82
	Industrial-related control	21.42	20.24	25.84	15.61	31.72	34.17	39.37
Log wage income	Main treatment	2.70	3.01	3.07	2.59	3.26	3.39	3.47
	Industrial-related control	2.83	2.65	2.98	2.46	3.20	3.26	3.40
Observations (Panel B)	Main treatment	635	632	515	46	596	492	503
	Industrial-related control	1,388	2,022	1,110	153	1,589	1,454	1,472
Panel C. Household-level outcomes: household outcome sample (N = 10,784)								
Family income	Main treatment	34.82	46.91	54.50	57.17	77.67	85.05	83.70
	Industrial-related control	39.62	43.89	54.98	62.11	77.24	82.34	86.50
Log family income	Main treatment	3.34	3.64	3.82	3.89	4.19	4.26	4.28
	Industrial-related control	3.44	3.50	3.79	3.92	4.13	4.18	4.27
Family expenditure	Main treatment	32.38	39.71	50.46	47.03	61.73	61.58	65.49
	Industrial-related control	35.09	36.47	52.60	61.00	65.51	63.38	70.46
Log family expenditure	Main treatment	3.25	3.44	3.71	3.61	3.88	3.88	3.92
	Industrial-related control	3.33	3.34	3.68	3.80	3.89	3.87	3.96
Observations (Panel C)	Main treatment	499	491	459	178	535	437	432
	Industrial-related control	1,042	1,488	923	536	1,390	1,206	1,168

Note. This table reports mean values of the outcome variables by survey year for the outcome-specific baseline estimation samples. The baseline groups consist of the main treatment group and the industrial-related control group. The pre-policy period includes the 2010 and 2012 survey waves, while the post-policy period includes the 2014, 2016, 2018, 2020, and 2022 survey waves. Panel A corresponds to the estimation sample for log individual income, Panel B corresponds to the estimation sample for log wage income, and Panel C corresponds to the household-level estimation sample for log family income and log family expenditure. Monetary variables in levels are measured in thousands of CNY and deflated to constant 2010 prices. Within each panel, level and logarithmic means are calculated using the same estimation sample. Because logarithmic outcomes are defined only for positive monetary values, the number of observations may differ across panels. In particular, the log wage-income sample excludes observations with zero wage income.

The descriptive patterns in Table 2 show that income and expenditure generally increase over the sample period in both the treatment and control groups. At the same time, the raw means vary across survey waves and across outcome-specific samples. The wage-income sample is smaller than the individual-income sample, especially in

2016, because the log wage-income specification excludes individuals without positive wage income. At the household level, family income and family expenditure also increase over the full period, although the relative differences between treatment and control groups are not constant across years. These descriptive patterns provide useful background, but they are not interpreted causally. The causal analysis is based on the DID and event-study estimates reported in the following sections.

Table 3 reports descriptive statistics for the control variables used in the regression analysis. For the individual-level models, the controls include years of education, marital status, gender, urban residence, age, and age squared. For the household-level models, the controls include the household representative's years of education, marital status, gender, age, and age squared, together with urban residence and family size. The table reports these variables separately for the main treatment group and the industrial-related control group before and after the policy period.

Table 3. Descriptive Statistics for Control Variables by Policy Period: Main Treatment and Industrial-Related Control Groups

	Main treatment group		Industrial-related control group	
Variable	**Pre-policy**	**Post-policy**	**Pre-policy**	**Post-policy**
Panel A. Individual-level controls: log individual income sample (N = 13,119)				
Years of education	8.39	9.65	8.02	9.19
Married	0.84	0.56	0.88	0.59
Female	0.14	0.17	0.14	0.14
Urban	0.56	0.61	0.51	0.54
Age	39.08	40.05	40.58	40.93
Age squared	1649.65	1735.52	1765.33	1808.31
Observations (Panel A)	1,267	2,289	3,411	6,152
Panel B. Individual-level controls: log wage income sample (N = 12,607)				
Years of education	8.39	9.69	8.02	9.23
Married	0.84	0.60	0.88	0.62
Female	0.14	0.17	0.14	0.14
Urban	0.56	0.62	0.51	0.54
Age	39.08	40.32	40.57	41.10
Age squared	1649.65	1756.28	1765.17	1821.01
Observations (Panel B)	1,267	2,152	3,410	5,778
Panel C. Household-level controls: household outcome sample (N = 10,784)				
Household representative years of education	7.52	8.85	7.24	8.31
Married household representative	0.93	0.89	0.93	0.88
Female household representative	0.37	0.47	0.38	0.45
Urban	0.54	0.59	0.51	0.53
Household representative age	45.57	45.04	45.94	45.66
Household representative age squared	2191.31	2168.06	2223.67	2228.34
Family size	3.88	3.85	4.17	4.18
Observations (Panel C)	990	2,041	2,530	5,223

Note. This table reports mean values of the control variables by policy period for the outcome-specific baseline estimation samples. The baseline groups consist of the main treatment group and the industrial-related control group. The pre-policy period includes the 2010 and 2012 survey waves, while the post-policy period includes the 2014, 2016, 2018, 2020, and 2022 survey waves. Panel A corresponds to the estimation sample for log individual income, Panel B corresponds to the estimation sample for log wage income, and Panel C corresponds to the household-level estimation sample for log family income and log family expenditure. Female, Married, and Urban are binary indicator variables equal to one for female individuals or household representatives, married individuals or household representatives, and urban households, respectively.

The control-variable statistics show observable differences between the main treatment group and the industrial-related control group. These differences are not themselves evidence of policy effects, but they are relevant for the empirical design because they indicate why controlled specifications and fixed-effects models are

useful. The regression analysis therefore combines observable controls with unit fixed effects, year fixed effects, and event-study checks to compare changes in outcomes over time rather than relying only on raw group differences.

4.4 Data Quality and Measurement

The CFPS provides the longitudinal individual- and household-level information required for this study, but several measurement issues need to be considered when interpreting the results. Income and wage variables in household surveys may be affected by reporting error, non-response, and year-to-year volatility. These issues are especially relevant for worker-level income measures, which can change because of employment status, hours worked, bonuses, temporary work, or movement across jobs and sectors. For this reason, the results are interpreted using both average DID estimates and event-study patterns rather than relying only on raw income changes in a single survey wave. More generally, these measurement issues can complicate the interpretation of observed income changes over time (Deaton, 1997).

To improve sample consistency, observations with missing values in the relevant outcome variable, treatment or control-group classification, or core covariates are excluded from the corresponding estimation sample. Because the baseline regressions use logarithmic outcomes, observations with zero or missing values for the relevant monetary outcome are also excluded from the outcome-specific log sample. This restriction is particularly important for wage income because the log wage-income sample includes only individuals with positive and non-missing wage income.

The smaller wage-income sample in 2016 should therefore be interpreted in light of these outcome-specific restrictions. The log wage-income specification requires positive wage income as well as non-missing industry, occupation, and control-variable information. These requirements reduce the number of usable observations in 2016, which may make the wage-income estimates for that year less precise. For this reason, the 2016 wage-income results are interpreted cautiously in the results section.

Extreme values in monetary variables are addressed through two-sided 1%

trimming within each survey year. This procedure removes observations in the year-specific lower and upper tails of the relevant monetary outcome distribution, reducing the influence of unusually large or small reported income and expenditure values while preserving the main structure of the panel sample. The robustness analysis also reports estimates using the untrimmed sample to assess whether the main findings are sensitive to this treatment of extreme values.

5. Results

5.1 Worker-Level Income Outcomes

Table 4 reports the baseline estimates for log individual income and log wage income using the main treatment group and the industrial-related control group. Worker-level income is an important outcome because labour-market adjustment is one of the main channels through which industrial restructuring may affect individuals. Previous evidence on displaced workers shows that sectoral and employment shocks can generate substantial and persistent earnings losses (Jacobson et al., 1993). In the present baseline specification, however, the estimated worker-level income effects are relatively weak and imprecisely estimated.

Table 4. Baseline Estimates of the Effects of the Capacity Reduction Policy on Individual Income and Wage Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	Controls	No controls	Controls	No controls	Controls
Panel A. Log Individual Total Income						
	-0.05	-0.05	-0.04	-0.04		
Treatment x Post	(0.03)	(0.03)	(0.05)	(0.05)		
					0.16*	0.16*
Treatment x Year 2012					(0.08)	(0.08)
					-0.05	-0.05
Treatment x Year 2014					(0.09)	(0.09)
					0.05	0.03
Treatment x Year 2016					(0.16)	(0.16)
					-0.00	0.00
Treatment x Year 2018					(0.09)	(0.09)
					0.02	0.01
Treatment x Year 2020					(0.09)	(0.09)
					-0.01	-0.02
Treatment x Year 2022					(0.09)	(0.09)
Observations	13,119	13,119	13,119	13,119	13,119	13,119
Panel B. Log Individual Wage Income						
	-0.06*	-0.06*	-0.09	-0.09		
Treatment x Post	(0.03)	(0.03)	(0.06)	(0.06)		
					0.14	0.13
Treatment x Year 2012					(0.09)	(0.08)
					-0.07	-0.08
Treatment x Year 2014					(0.09)	(0.09)
					0.05	0.02
Treatment x Year 2016					(0.31)	(0.31)
					-0.05	-0.05
Treatment x Year 2018					(0.10)	(0.09)
					-0.01	-0.02
Treatment x Year 2020					(0.10)	(0.10)
					-0.10	-0.10
Treatment x Year 2022					(0.10)	(0.10)
Observations	12,607	12,607	12,607	12,607	12,607	12,607

Note. This table reports baseline estimates for individual-level income outcomes using the main treatment group and the industrial-related control group. Post-policy equals one for 2014, 2016, 2018, 2020, and 2022; the omitted event-study base year is 2010. Columns (1)–(2) report pooled DID estimates, columns (3)–(4) report individual fixed-effects DID estimates, and columns (5)–(6) report individual fixed-effects event-study estimates. Controls are included in columns (2), (4), and (6). Household-clustered standard errors are reported in parentheses. Full outputs are reported in Appendix Tables B1–B2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For log individual income, the coefficient on *Treatment* × *Post*, corresponding to β_3 in the DID specification, is -0.05 in the pooled DID models both with and without controls. In the individual fixed-effects DID specifications, the coefficient is -0.04 and remains statistically insignificant. This suggests that, after accounting for time-invariant individual heterogeneity and common year effects, there is no precisely estimated average decline in individual income under the baseline specification.

The event-study estimates also do not show a clear post-policy decline in log individual income. The pre-policy interaction coefficient for 2012 is positive and statistically significant at the 10 percent level, indicating some pre-policy difference between the treatment and control groups. The post-policy interaction coefficients are small and statistically insignificant across all post-policy years. This pattern suggests that the individual-income estimates should be interpreted cautiously. It is also consistent with recent DID guidance emphasising that pre-trend evidence from event-study specifications should be interpreted carefully, especially when the number of pre-policy periods is limited (Roth, 2022).

For log wage income, the baseline estimates point in a more negative direction, but the evidence remains imprecise. The pooled DID coefficient on $Treat_i \times Post_t$ is -0.06 and is statistically significant at the 10 percent level. After individual fixed effects are introduced, the coefficient becomes -0.09 but is no longer statistically significant. In semi-log terms, the fixed-effects estimate implies an approximate decline of 8.6 percent, although the lack of statistical significance means that this estimate should be interpreted cautiously. The event-study estimates also provide limited evidence of a sustained wage-income decline. Several post-policy coefficients are negative, especially in later years, but none is statistically significant. The 2016 estimate is particularly imprecise, which is consistent with the smaller wage-income estimation sample discussed in the data section.

Overall, the baseline worker-level results do not show strong evidence of a large or persistent decline in individual income or wage income. The estimated coefficients are generally small, statistically insignificant after individual fixed effects are included, and do not display a consistent negative event-study pattern. This does not

imply that workers exposed to capacity-reduction-related sectors were unaffected. Previous research shows that earnings losses after displacement or labour-market shocks may operate through employment gaps, wage declines, and movement across firms or jobs (Schmieder et al., 2023; Lachowska et al., 2020). In the present baseline specification, however, these forms of labour-market adjustment are not clearly reflected in observed individual income or wage income. This may reflect income volatility, outcome-specific sample restrictions, or post-policy job and sectoral mobility among workers initially classified as treated. The analysis therefore next examines household-level income and expenditure outcomes to assess whether adjustment is more visible at the household level.

5.2 Household Income and Expenditure Outcomes

Table 5 presents the baseline estimates for log household income and log household expenditure using the main treatment group and the industrial-related control group. The household-level results differ from the worker-level income results in an important way. Household income shows little evidence of a clear average response, whereas household expenditure displays a more visible negative dynamic pattern after the policy period.

Table 5. Baseline Estimates for Household Income and Expenditure

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Panel A. Log Household Total Income						
	0.01	0.00	0.02	0.02		
Treatment x Post	(0.03)	(0.03)	(0.04)	(0.04)		
					0.05	0.06
Treatment x Year 2012					(0.09)	(0.09)
					-0.07	-0.07
Treatment x Year 2014					(0.08)	(0.08)
					-0.08	-0.06
Treatment x Year 2016					(0.11)	(0.10)
					-0.01	0.00
Treatment x Year 2018					(0.08)	(0.08)
					0.04	0.04
Treatment x Year 2020					(0.09)	(0.09)
					0.05	0.05
Treatment x Year 2022					(0.09)	(0.08)
Observations	10,784	10,784	10,784	10,784	10,784	10,784
Panel B. Log Household Total Expenditure						
	-0.04	-0.06**	-0.05	-0.05		
Treatment x Post	(0.03)	(0.03)	(0.04)	(0.04)		
					-0.11	-0.10
Treatment x Year 2012					(0.09)	(0.08)
					-0.18**	-0.18**
Treatment x Year 2014					(0.09)	(0.09)
					-0.35***	-0.33***
Treatment x Year 2016					(0.12)	(0.12)
					-0.17*	-0.16*
Treatment x Year 2018					(0.09)	(0.09)
					-0.18*	-0.18**
Treatment x Year 2020					(0.09)	(0.09)
					-0.12	-0.12
Treatment x Year 2022					(0.10)	(0.10)
Observations	10,784	10,784	10,784	10,784	10,784	10,784

Note. This table reports baseline estimates for household-level outcomes using the main treatment group and the industrial-related control group. A household is classified as treated when at least one household member is treated under the corresponding individual-level definition. Post-policy equals one for 2014, 2016, 2018, 2020, and 2022; the omitted event-study base year is 2010. Columns (1)–(2) report pooled DID estimates, columns (3)–(4) report household fixed-effects DID estimates, and columns (5)–(6) report household fixed-effects event-study estimates. Controls are included in columns (2), (4), and (6). Household-clustered standard errors are reported in parentheses. Full outputs are reported in Appendix Tables B3–B4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For log household income, the baseline estimates are small and statistically insignificant. In the pooled DID specifications, the coefficient on *Treatment* × *Post* is 0.01 without controls and 0.00 with controls. After household fixed effects are introduced, the coefficient is 0.02 in both the specification without controls and the specification with controls. None of these estimates is statistically significant, indicating that the baseline specification does not provide evidence of a clear average decline in household income.

The event-study estimates for household income lead to a similar interpretation. The 2012 interaction coefficient is positive but statistically insignificant, providing no strong evidence of pre-policy divergence in household income. The post-policy coefficients are negative in 2014 and 2016, close to zero in 2018, and positive in 2020 and 2022, but none is statistically significant. Taken together, these results indicate that household income does not display a consistent negative response under the baseline specification.

The household expenditure results provide clearer dynamic evidence. In the pooled DID specification with controls, the coefficient on *Treatment* × *Post* is -0.06 and statistically significant at the 5 percent level, corresponding to an approximate decline of 5.8 percent. However, after household fixed effects are included, the coefficient becomes -0.05 and is no longer statistically significant. This indicates that the average post-policy expenditure effect is negative, but not precisely estimated in the preferred fixed-effects specification.

The event-study estimates provide clearer evidence of dynamic expenditure adjustment. In the controlled event-study specification, the pre-policy coefficient for 2012 is negative but statistically insignificant, while the post-policy coefficients are consistently negative. The decline is statistically significant from 2014 to 2020 and is largest in 2016, when the coefficient implies an approximate expenditure decline of 28 percent relative to the omitted 2010 treatment-control difference. By 2022, the coefficient remains negative but is no longer statistically significant, suggesting that the expenditure response weakens in the later period.

Overall, the household-level results suggest that the clearest baseline evidence

appears in household expenditure rather than household income. The household income estimates are small and statistically insignificant, while household expenditure shows a negative dynamic pattern concentrated around the main implementation period and the following survey waves. This pattern is consistent with precautionary household adjustment in response to policy-related uncertainty or expected income risk. This interpretation is related to the literature on buffer-stock saving and consumption smoothing, which shows that households may adjust expenditure in response to income risk and incomplete insurance (Carroll, 1997; Blundell et al., 2008). However, because the average fixed-effects DID estimate for expenditure is not statistically significant, the expenditure result should be interpreted primarily as dynamic evidence rather than as a strong average post-policy effect.

5.3 Interpretation and Mechanism Discussion

The baseline results indicate that the consequences of capacity-reduction-related exposure are more visible in household expenditure than in measured income. Worker-level income and wage-income estimates are relatively weak, and household income does not show a clear average decline. Household expenditure, by contrast, displays a more consistent negative dynamic pattern, especially around the main implementation period. This contrast suggests that the welfare effects of industrial restructuring may not be fully captured by contemporaneous income measures. They may also appear through changes in household spending behaviour, which is consistent with the broader household-finance literature showing that welfare effects of shocks can operate through consumption and expenditure margins as well as through measured income (Chetty & Szeidl, 2007).

One plausible interpretation is precautionary household expenditure adjustment. Capacity reduction may have increased perceived employment or income risk among workers connected to affected sectors, even when realised household income did not immediately decline. In this setting, households may reduce expenditure in order to preserve liquidity or prepare for future uncertainty. This interpretation is consistent with evidence that uncertainty can affect economic behaviour before realised

outcomes fully adjust (Bloom, 2009), and with buffer-stock saving models in which households adjust saving and spending when facing income risk (Carroll, 1997).

The timing of the expenditure response also supports a cautious precautionary interpretation. The negative expenditure coefficients are largest around 2014 and 2016, when the policy moved from institutionalisation toward more intensive implementation, and the response weakens by 2022. This pattern is consistent with an adjustment response concentrated in the earlier and middle phases of the policy process, before households and local labour markets gradually adapted. The absence of a clear contemporaneous decline in household income suggests that the expenditure response may reflect expected income risk and uncertainty rather than only a mechanical response to observed income losses. This interpretation is also related to consumption-smoothing and partial-insurance models, in which household expenditure can respond to income risk and imperfect insurance (Blundell et al., 2008; Heathcote et al., 2014).

The weaker income results may partly reflect measurement issues in household survey data. Reported individual and household income can be affected by recall and reporting error, non-response, and substantial year-to-year volatility associated with changes in employment status, working hours, bonuses, temporary work, and other income sources. Such reporting error and transitory variation may add noise to observed income changes, reduce statistical precision, and, depending on the structure of the measurement error, attenuate the estimated income effects (Deaton, 1997). This is only one of several possible explanations for the statistically insignificant income results and should not be interpreted as evidence that measurement error necessarily accounts for them. Other relevant factors include outcome-specific sample restrictions and post-policy mobility across jobs, sectors, and occupations. Because treatment status is fixed under the corresponding treatment-group definition, the worker-level estimates capture effects associated with initial exposure rather than the effects of remaining continuously employed in treated sectors. Labour-market adjustment may therefore not be fully reflected in observed income, even if exposed workers experienced employment instability or job transitions. Previous evidence on displaced

workers also shows that earnings losses may operate through employment gaps, job transitions, and movement into lower-paying employment (Couch & Placzek, 2010; Lachowska et al., 2020).

These findings should therefore be interpreted as evidence consistent with precautionary expenditure adjustment, rather than as a direct test of the underlying mechanism. The analysis does not directly observe household expectations, perceived job insecurity, savings motives, or detailed expenditure categories. Nevertheless, the contrast between weak income estimates and clearer expenditure dynamics suggests that household expenditure is an important margin through which the welfare effects of industrial restructuring may be observed. Recent evidence on household expenditure responses to labour-market shocks also supports the view that expenditure can be a sensitive margin of household adjustment when employment or income risk changes (Ganong & Noel, 2019; Fagereng et al., 2024).

6. Robustness

6.1 Overview of Robustness Analyses

This section evaluates the stability of the baseline findings under alternative control-group choices, treatment definitions, matching-based specifications, policy timing assumptions, and outlier treatments. These robustness exercises are not intended to replace the baseline specification. Instead, they are used to assess whether the main patterns reported in Section 5, especially the dynamic decline in household expenditure, are sensitive to reasonable changes in research design and sample construction.

The analysis considers five sets of robustness and sensitivity checks. First, the baseline treatment definition is held fixed while the control group is varied across the industrial-related, supply-chain-related, and pre-trend-selected control groups. This assesses whether the results depend on a specific comparison group. Second, the industrial-related control group is held fixed while the treatment definition is varied across the main, strict, and broad treatment groups. This examines whether the results are sensitive to narrower or broader definitions of exposure to

capacity-reduction-related industries and occupations. Third, the study implements a matching-based DID analysis to improve the comparability of treated and control units on pre-policy observable characteristics and outcome patterns. This exercise is used as a supplementary check rather than as a replacement for the baseline fixed-effects DID specification. Fourth, the analysis uses an alternative policy timing definition in which the post-policy period begins in 2016, reflecting the more intensive implementation phase of the capacity reduction policy. Fifth, the study examines whether the results are sensitive to the treatment of extreme values by comparing the baseline trimmed sample with the untrimmed sample.

These robustness checks help distinguish results that are stable across alternative specifications from results that depend more heavily on particular modelling choices. They are especially useful for assessing whether the household-expenditure pattern documented in Section 5 is robust to changes in the control group, treatment definition, matching procedure, policy timing, and outlier treatment. The following sections report the main robustness results, while the full regression outputs are provided in the Appendix.

6.2 Alternative Control Group Definitions

Table 6 keeps the main treatment definition fixed and varies the control group across the industrial-related, supply-chain-related, and pre-trend-selected control groups. The purpose is to assess whether the baseline estimates depend on the particular set of non-treated workers used as the comparison benchmark. The industrial-related control group corresponds to the baseline specification, while the supply-chain-related and pre-trend-selected control groups provide alternative benchmarks based on economic linkage and pre-policy outcome patterns. This check is important because control-group construction is central to the credibility of non-experimental evaluation designs, especially when treated and untreated units may differ in observable characteristics and pre-policy outcome trajectories (Heckman et al., 1998; Abadie, 2005).

Table 6. Sensitivity to Alternative Control Group Definitions

Panel A. Individual-Level Outcomes

	Log Individual Income			Log Wage Income		
Control group	**Industrial**	**Supply-chain**	**Pre-trend**	**Industrial**	**Supply-chain**	**Pre-trend**
Fixed-effects DID estimates with controls						
Treatment x Post	-0.04	-0.03	0.04	-0.09	-0.09	0.03
	(0.05)	(0.05)	(0.07)	(0.06)	(0.06)	(0.07)
Fixed-effects event-study estimates with controls						
Treatment x Year 2012	0.16*	0.15*	0.09	0.13	0.13	0.07
	(0.08)	(0.08)	(0.10)	(0.08)	(0.08)	(0.10)
Treatment x Year 2014	-0.05	-0.06	0.02	-0.08	-0.09	0.02
	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)	(0.11)
Treatment x Year 2016	0.03	0.02	0.06	0.02	0.03	0.26
	(0.16)	(0.15)	(0.16)	(0.31)	(0.30)	(0.30)
Treatment x Year 2018	0.00	-0.00	0.07	-0.05	-0.05	0.03
	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)	(0.11)
Treatment x Year 2020	0.01	0.01	0.07	-0.02	-0.04	0.03
	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.12)
Treatment x Year 2022	-0.02	-0.02	0.04	-0.10	-0.12	-0.05
	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.12)
Observations	13,119	14,159	10,973	12,607	13,578	10,389

Panel B. Household-Level Outcomes

	Log Family Income			Log Family Expenditure		
Control group	**Industrial**	**Supply-chain**	**Pre-trend**	**Industrial**	**Supply-chain**	**Pre-trend**
Fixed-effects DID estimates with controls						
Treatment x Post	0.02	0.02	-0.02	-0.05	-0.05	-0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Fixed-effects event-study estimates with controls						
Treatment x Year 2012	0.06	0.05	-0.06	-0.10	-0.09	-0.11
	(0.09)	(0.09)	(0.10)	(0.08)	(0.08)	(0.09)
Treatment x Year 2014	-0.07	-0.07	-0.12	-0.18**	-0.19**	-0.23**
	(0.08)	(0.08)	(0.09)	(0.09)	(0.08)	(0.10)
Treatment x Year 2016	-0.06	-0.06	-0.10	-0.33***	-0.30***	-0.26**
	(0.10)	(0.10)	(0.11)	(0.12)	(0.12)	(0.13)
Treatment x Year 2018	0.00	-0.00	-0.13	-0.16*	-0.15*	-0.10
	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.10)
Treatment x Year 2020	0.04	0.03	-0.11	-0.18**	-0.16*	-0.20*
	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)	(0.10)
Treatment x Year 2022	0.05	0.04	-0.08	-0.12	-0.11	-0.06
	(0.08)	(0.08)	(0.09)	(0.10)	(0.10)	(0.11)
Observations	10,784	11,557	9,037	10,784	11,557	9,037

Note. Industrial, Supply-chain, and Pre-trend denote the industrial-related, supply-chain-related, and pre-trend-selected control groups. Estimates are from fixed-effects DID and fixed-effects event-study models with controls. Standard errors are household-clustered and reported in parentheses. Full outputs are reported in Appendix Tables B1–B4 and C1–C8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A reports the results for individual-level outcomes. The worker-level income estimates remain weak across alternative control groups. For log individual income, the fixed-effects DID coefficient on Treatment × Post is small and statistically insignificant under all three control-group definitions. The event-study estimates also do not show a consistent negative post-policy pattern. For log wage income, the fixed-effects DID estimates are negative under the industrial-related and supply-chain-related control groups, but they are not statistically significant; the estimate becomes positive under the pre-trend-selected control group. These results confirm that the baseline treatment definition does not generate robust evidence of worker-level income effects across alternative comparison samples.

Panel B reports the household-level results. For log family income, the estimated average effects remain small and statistically insignificant under all three control-group definitions. The event-study coefficients are also mixed and statistically insignificant. This is consistent with the baseline result that household income does not show a clear response to capacity-reduction-related exposure.

The results for log family expenditure are more consistent across control groups. The fixed-effects DID coefficients are negative under all three control-group definitions, although they are not statistically significant. The event-study estimates, however, show a clearer negative post-policy pattern. Under the industrial-related and supply-chain-related control groups, the expenditure coefficients are negative and statistically significant in 2014, 2016, 2018, and 2020. Under the pre-trend-selected control group, the post-policy coefficients also remain negative, with statistically significant estimates in 2014, 2016, and 2020. The 2012 pre-policy coefficient is negative but statistically insignificant across all three control groups.

Overall, Table 6 supports the main interpretation from the baseline results. The income estimates remain weak across alternative control groups, whereas household expenditure continues to show a negative dynamic pattern after the policy period. The magnitude and statistical significance of the expenditure estimates vary across comparison samples, but the negative post-policy pattern does not depend only on the baseline industrial-related control group. This suggests that the expenditure response

is not simply an artefact of a single control-group definition, but reflects a broader household-level adjustment pattern that remains visible under alternative comparison samples.

6.3 Alternative Treatment Group Definitions

Table 7 examines whether the results change when the treatment definition is varied while the industrial-related control group is kept fixed. The main treatment group is the baseline definition. The strict treatment group captures workers with more direct exposure to capacity-reduction-related industries and occupations, while the broad treatment group includes a wider set of manufacturing-related workers whose exposure may be more indirect. This check is useful because estimated policy effects may depend on how exposure is assigned. If the treatment group is defined too narrowly, the estimates may miss some affected workers; if it is defined too broadly, the estimates may be diluted by including workers with weaker exposure to the policy shock (Autor et al., 2013; Topalova, 2010).

Table 7. Sensitivity to Alternative Treatment Group Definitions

Panel A. Individual-Level Outcomes						
	Log Individual Income			**Log Wage Income**		
Treatment definition	**Main**	**Strict**	**Broad**	**Main**	**Strict**	**Broad**
Fixed-effects DID estimates with controls						
Treatment x Post	-0.04	-0.14*	-0.02	-0.09	-0.17**	-0.06
	(0.05)	(0.08)	(0.05)	(0.06)	(0.08)	(0.05)
Fixed-effects event-study estimates with controls						
Treatment x Year 2012	0.16*	0.25**	0.14*	0.13	0.22*	0.12
	(0.08)	(0.11)	(0.08)	(0.08)	(0.11)	(0.08)
Treatment x Year 2014	-0.05	-0.12	-0.06	-0.08	-0.17	-0.10
	(0.09)	(0.13)	(0.08)	(0.09)	(0.13)	(0.09)
Treatment x Year 2016	0.03	0.01	0.05	0.02	0.10	0.11
	(0.16)	(0.24)	(0.14)	(0.31)	(0.38)	(0.28)
Treatment x Year 2018	0.00	-0.15	0.02	-0.05	-0.17	-0.00
	(0.09)	(0.13)	(0.08)	(0.09)	(0.13)	(0.09)
Treatment x Year 2020	0.01	-0.08	0.02	-0.02	-0.11	0.01
	(0.09)	(0.14)	(0.09)	(0.10)	(0.14)	(0.09)
Treatment x Year 2022	-0.02	-0.04	-0.01	-0.10	-0.09	-0.08
	(0.09)	(0.13)	(0.09)	(0.10)	(0.14)	(0.09)
Observations	13,119	10,740	14,140	12,607	10,335	13,573
Panel B. Household-Level Outcomes						
	Log Family Income			**Log Family Expenditure**		
Treatment definition	**Main**	**Strict**	**Broad**	**Main**	**Strict**	**Broad**
Fixed-effects DID estimates with controls						
Treatment x Post	0.02	0.00	0.04	-0.05	-0.15**	-0.05
	(0.04)	(0.07)	(0.04)	(0.04)	(0.07)	(0.04)
Fixed-effects event-study estimates with controls						
Treatment x Year 2012	0.06	0.05	0.06	-0.10	-0.06	-0.07
	(0.09)	(0.14)	(0.08)	(0.08)	(0.12)	(0.08)
Treatment x Year 2014	-0.07	-0.10	-0.02	-0.18**	-0.26**	-0.16**
	(0.08)	(0.13)	(0.08)	(0.09)	(0.12)	(0.08)
Treatment x Year 2016	-0.06	-0.14	-0.03	-0.33***	-0.54***	-0.35***
	(0.10)	(0.16)	(0.10)	(0.12)	(0.18)	(0.11)
Treatment x Year 2018	0.00	-0.06	0.03	-0.16*	-0.26*	-0.13
	(0.08)	(0.12)	(0.07)	(0.09)	(0.14)	(0.08)
Treatment x Year 2020	0.04	0.02	0.07	-0.18**	-0.33**	-0.17*
	(0.09)	(0.13)	(0.08)	(0.09)	(0.15)	(0.09)
Treatment x Year 2022	0.05	0.08	0.06	-0.12	-0.16	-0.12
	(0.08)	(0.13)	(0.08)	(0.10)	(0.16)	(0.09)
Observations	10,784	8,945	11,523	10,784	8,945	11,523

Note. Main, Strict, and Broad denote the three treatment-group definitions; the industrial-related control group is held fixed. Estimates are from fixed-effects DID and fixed-effects event-study models with controls. Standard errors are household-clustered and reported in parentheses. Full outputs are reported in Appendix Tables B1–B4 and D1–D8. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A reports the individual-level results. For log individual income, the fixed-effects DID coefficient on *Treatment* × *Post* is negative but statistically insignificant under the main and broad treatment definitions. Under the strict treatment definition, the coefficient is larger in magnitude and statistically significant at the 10 percent level. A similar pattern appears for log wage income. The fixed-effects DID coefficient is negative but insignificant under the main and broad treatment definitions, whereas the strict treatment definition produces a larger negative estimate that is statistically significant at the 5 percent level. These results suggest that worker-level income effects are more visible when treatment is defined around workers with more direct exposure to capacity-reduction-related sectors and occupations.

The event-study estimates for individual-level outcomes are less clear. Under the strict treatment definition, the 2012 pre-policy coefficient is positive and statistically significant for both log individual income and log wage income, indicating some pre-policy difference between the strict treatment group and the control group. Post-policy coefficients are often negative, especially for wage income, but they are not consistently statistically significant. Under the broad treatment definition, the post-policy coefficients are generally smaller and less systematic. This pattern suggests that narrower exposure definitions may produce larger estimated worker-level effects, but the significant 2012 coefficients under the strict definition mean that these estimates should be interpreted cautiously. More broadly, the weaker estimates under the broad definition are consistent with the possibility that wider exposure measures dilute estimated effects by combining more directly exposed workers with more weakly exposed workers (Griliches & Hausman, 1986).

Panel B reports the household-level results. For log family income, the average fixed-effects DID estimates remain small and statistically insignificant under the main, strict, and broad treatment definitions. The event-study coefficients are also mixed and do not show a consistent negative response. This reinforces the baseline result that household income does not display a clear response to capacity-reduction-related exposure.

The household expenditure estimates show a clearer relationship with treatment intensity. The fixed-effects DID coefficient is negative under all three treatment definitions and is statistically significant under the strict treatment definition. The event-study estimates also show a negative post-policy pattern across all three definitions. The largest and most persistent expenditure declines appear under the strict treatment group. Under this definition, the expenditure coefficients are negative and statistically significant in 2014, 2016, 2018, and 2020, with the largest decline in 2016. Under the main and broad treatment definitions, expenditure coefficients are also negative in most post-policy years, although their magnitudes and statistical significance are generally smaller.

Overall, Table 7 indicates that estimated effects are stronger when treatment is defined more narrowly and more directly. The strict treatment definition produces larger negative estimates, especially for log wage income and log family expenditure, while the broad treatment definition generally weakens the estimated effects. This pattern is consistent with an exposure-intensity interpretation: workers and households more directly connected to capacity-reduction-related sectors appear to show larger adjustments, whereas broader treatment definitions dilute the estimates by including more weakly exposed workers. At the same time, the significant pre-policy coefficients under the strict definition mean that these stronger estimates should be read alongside the event-study evidence. Overall, the results continue to support the baseline conclusion that household expenditure is the clearest and most consistent outcome margin.

6.4 Matching-Based Difference-in-Differences Analysis

Table 8 reports covariate balance before and after propensity score matching. Matching is implemented separately for the individual-level and household-level samples, using the main treatment definition and the industrial-related control group. The aim is to construct matched samples in which treated and control units are more comparable in terms of pre-policy observable characteristics and pre-policy outcome patterns before re-estimating the DID models. This approach follows the logic of

propensity score methods, which use observed pre-treatment characteristics to improve comparability in non-experimental settings (Rosenbaum & Rubin, 1983; Abadie & Imbens, 2016).

Table 8. Covariate Balance Before and After Propensity Score Matching

Panel A. Individual-Level Matching Sample						
	Before Matching			**After Matching**		
Covariate	**Treated**	**Control**	**Std. Diff. (%)**	**Treated**	**Control**	**Std. Diff. (%)**
Pre-policy mean log individual income	2.94	2.96	-3.03	2.95	2.94	1.43
Change in log individual income, 2010-2012	0.42	0.29	17.72	0.40	0.40	-0.74
Years of education	8.53	8.93	-10.67	8.54	8.22	8.19
Age	40.12	40.32	-1.97	40.23	40.07	1.59
Age squared	1713.54	1723.13	-1.19	1719.89	1713.20	0.81
Married	0.85	0.90	-16.77	0.87	0.88	-3.44
Female	0.11	0.13	-5.49	0.11	0.09	8.58
Urban	0.59	0.63	-8.57	0.59	0.57	5.18
Number of units	239	506		235	235	
Panel B. Household-Level Matching Sample						
	Before Matching			**After Matching**		
Covariate	**Treated**	**Control**	**Std. Diff. (%)**	**Treated**	**Control**	**Std. Diff. (%)**
Pre-policy mean log family income	3.43	3.53	-16.26	3.44	3.44	-0.55
Change in log family income, 2010-2012	0.32	0.32	-0.06	0.32	0.35	-3.96
Pre-policy mean log family expenditure	3.38	3.39	-1.55	3.39	3.39	-0.17
Change in log family expenditure, 2010-2012	0.15	0.25	-12.74	0.16	0.18	-2.62
Household representative age	46.14	45.67	5.16	46.09	45.88	2.34
Household representative age squared	2229.70	2183.92	5.18	2224.71	2190.65	3.91
Household representative years of education	7.55	8.03	-12.61	7.56	7.88	-8.56
Married household representative	0.93	0.94	-2.62	0.94	0.93	2.57
Female household representative	0.34	0.39	-14.38	0.34	0.36	-4.65
Urban	0.54	0.61	-13.82	0.55	0.54	2.21
Family size	3.75	4.01	-17.56	3.76	3.59	14.03
Number of units	183	367		182	182	

Note. This table reports covariate balance before and after 1:1 nearest-neighbour propensity score matching without replacement, using a logit propensity score and common support. Matching is conducted separately for the individual- and household-level samples within the baseline framework. The caliper is 0.05 for individual-level matching and 0.03 for household-level matching. Treatment status is anchored in the most recent observed pre-policy wave. Individual matching uses pre-policy log individual income, its 2010–2012 change, and demographic controls; household matching uses pre-policy log family income and expenditure, their 2010–2012 changes, and household controls. Standardised differences are reported in percentage points. The number of units reports eligible units before matching and successfully matched units after matching.

The balance statistics show that matching improves comparability on several important dimensions, although it does not eliminate all differences. In the individual-level sample, the standardised difference in pre-policy mean log individual income changes from -3.03 percent before matching to 1.43 percent after matching, while the standardised difference in the pre-policy change in log individual income falls from 17.72 percent to -0.74 percent. Several demographic variables also become more balanced after matching. In the household-level sample, matching improves balance in pre-policy mean log family income, pre-policy mean log family expenditure, and the pre-policy change in log family expenditure. Some imbalance remains, particularly for family size and years of education. For this reason, the matched analysis is interpreted as a supplementary robustness check rather than as a replacement for the baseline fixed-effects specification.

Table 9 compares the baseline estimates with the estimates obtained from the matched samples. For individual-level outcomes, the matched estimates are more negative than the baseline estimates. For log individual income, the fixed-effects DID coefficient on *Treatment* × *Post* changes from -0.04 in the baseline sample to -0.14 in the matched sample and becomes statistically significant at the 10 percent level. For log wage income, the corresponding coefficient changes from -0.09 to -0.14 and is also statistically significant at the 10 percent level. The event-study estimates in the matched sample are generally more negative in later years, but they remain imprecisely estimated. These results suggest that worker-level income effects become more visible in the matched sample, although the smaller matched sample means that this evidence should be interpreted as supplementary. This interpretation is consistent with the broader evaluation literature, which treats matching-based estimates as useful checks on covariate comparability rather than as a complete substitute for research-design assumptions (Smith & Todd, 2005).

Table 9. Comparison of Baseline and Propensity-Score-Matched Estimates

Panel A. Individual-Level Outcomes				
	Log Individual Income		**Log Wage Income**	
Sample	**Baseline**	**Matched**	**Baseline**	**Matched**
Fixed-effects DID estimates with controls				
Treatment x Post (≥2014)	-0.04	-0.14*	-0.09	-0.14*
	(0.05)	(0.07)	(0.06)	(0.07)
Fixed-effects event-study estimates with controls				
Treatment x 2012	0.18**	0.00	0.16**	-0.01
	(0.07)	(0.08)	(0.08)	(0.08)
Treatment x 2014	-0.03	-0.11	-0.06	-0.16
	(0.08)	(0.10)	(0.08)	(0.10)
Treatment x 2016	0.06	-0.17	0.05	-0.44
	(0.14)	(0.29)	(0.31)	(0.34)
Treatment x 2018	0.03	-0.10	-0.03	-0.06
	(0.07)	(0.10)	(0.08)	(0.10)
Treatment x 2020	0.04	-0.14	0.01	-0.14
	(0.08)	(0.15)	(0.08)	(0.15)
Treatment x 2022	0.01	-0.27**	-0.08	-0.24
	(0.07)	(0.13)	(0.09)	(0.15)
Observations	13,119	1,763	12,607	1,726
Panel B. Household-Level Outcomes				
	Log Family Income		**Log Family Expenditure**	
Sample	**Baseline**	**Matched**	**Baseline**	**Matched**
Fixed-effects DID estimates with controls				
Treatment x Post (≥2014)	0.02	-0.13*	-0.05	-0.21***
	(0.04)	(0.07)	(0.04)	(0.07)
Fixed-effects event-study estimates with controls				
Treatment x 2012	0.10	-0.03	0.01	-0.02
	(0.07)	(0.10)	(0.07)	(0.09)
Treatment x 2014	-0.02	-0.14	-0.06	-0.20*
	(0.06)	(0.10)	(0.07)	(0.11)
Treatment x 2016	-0.00	-0.24*	-0.20*	-0.19
	(0.09)	(0.13)	(0.10)	(0.12)
Treatment x 2018	0.05	-0.09	-0.03	-0.29**
	(0.05)	(0.10)	(0.07)	(0.11)
Treatment x 2020	0.09	-0.11	-0.05	-0.28**
	(0.06)	(0.11)	(0.07)	(0.13)
Treatment x 2022	0.10*	-0.16	0.01	-0.11
	(0.06)	(0.12)	(0.08)	(0.14)
Observations	10,784	1,567	10,784	1,567

Note. This table compares baseline and propensity-score-matched estimates. Estimates are from fixed-effects DID and fixed-effects event-study models with controls. Standard errors are household-clustered and reported in parentheses. Full outputs are reported in Appendix Tables E1–E4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For household-level outcomes, the matched estimates also become more negative. For log family income, the fixed-effects DID coefficient changes from 0.02 in the baseline sample to -0.13 in the matched sample and is statistically significant at the 10 percent level. This provides some evidence that household income may decline once treated and control households are made more comparable on pre-policy characteristics and outcome patterns. However, the event-study estimates for family income remain mixed, suggesting that this result is less stable dynamically than the expenditure result.

The strongest matched-sample evidence appears for log family expenditure. The fixed-effects DID coefficient changes from -0.05 in the baseline sample to -0.21 in the matched sample and is statistically significant at the 1 percent level. The matched event-study estimates are also negative in the post-policy period, with statistically significant declines in 2014, 2018, and 2020. Although the exact timing differs somewhat from the baseline event-study pattern, the matched results continue to support the main finding that household expenditure declines following capacity-reduction-related exposure.

Overall, the matching-based DID results provide additional support for the baseline interpretation, especially for household expenditure. The negative expenditure pattern remains visible after improving balance on pre-policy observable characteristics and outcome changes, suggesting that the baseline expenditure result is not driven only by observable pre-policy differences between treated and control units. The matched estimates also show more negative coefficients for worker-level income and household income, but these results are less dynamically consistent than the expenditure results and should be interpreted as supplementary. Taken together, the matching analysis reinforces the view that household expenditure is the clearest household-level margin of adjustment after imposing a more comparable matched sample.

6.5 Sensitivity to Alternative Policy Timing

Table 10 examines whether the baseline fixed-effects DID estimates are sensitive

to an alternative definition of the post-policy period. In the baseline specification, the post-policy indicator equals one from 2014 onward. This timing reflects the view that capacity reduction may have started to affect expectations and household behaviour after the policy was institutionalised, even before the more intensive implementation phase. As a sensitivity check, the alternative specification defines the post-policy period as beginning in 2016, while retaining the 2014 wave in the estimation sample but coding it as outside the post-intensive-implementation period. Examining alternative timing definitions is useful in quasi-experimental settings because estimated treatment effects may depend on how the onset of policy exposure is operationalised, especially when implementation is gradual rather than immediate (Meyer, 1995).

Table 10. Sensitivity to Alternative Policy Timing

Outcome	Log Individual Income	Log Wage Income	Log Family Income	Log Family Expenditure
Panel A. Baseline Timing: Post >= 2014				
Treatment x Post	-0.04	-0.09	0.02	-0.05
	(0.05)	(0.06)	(0.04)	(0.04)
Panel B. Alternative Timing: Post >= 2016				
Treatment x Post	0.00	-0.05	0.06	-0.04
	(0.05)	(0.06)	(0.04)	(0.05)
Observations	13,119	12,607	10,784	10,784

Note. Panel A defines Post as one from 2014 onward; Panel B defines Post as one from 2016 onward, with 2014 retained in the sample. Estimates are from fixed-effects DID models with controls and year fixed effects. Standard errors are household-clustered and reported in parentheses. Additional results are reported in Appendix Tables F1–F2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The average fixed-effects DID estimates become weaker under the alternative timing definition. For log individual income, the coefficient changes from -0.04 under the baseline timing to 0.00 when the post period begins in 2016. For log wage income, the coefficient changes from -0.09 to -0.05. For log family income, the estimate becomes slightly more positive, increasing from 0.02 to 0.06. For log family expenditure, the coefficient remains negative but changes only slightly from -0.05 to -0.04. None of these average effects is statistically significant under either timing definition.

These results indicate that the average fixed-effects DID estimates are sensitive to how the early policy period is classified. Moving the start of the post-policy period from 2014 to 2016 does not make the average effects stronger. Instead, the coefficients generally become smaller in magnitude or move closer to zero. This pattern suggests that part of the adjustment may have occurred during the early stage of the policy process, before or around the more intensive implementation phase. The alternative timing specification therefore does not overturn the baseline timing choice, but it does show that the policy response is better interpreted as a dynamic adjustment process than as a constant average effect over the entire post-policy period. This is consistent with policy-evaluation settings in which behavioural and economic responses may unfold gradually after policy adoption (Wolfers, 2006).

Appendix Tables F1 and F2 provide event-study results under the alternative timing specification. For individual-level outcomes, the coefficients remain mixed and do not show a clear post-2016 pattern. Household income also does not display a consistent negative response. In contrast, log family expenditure continues to show a negative coefficient in 2016, while the later coefficients are smaller and no longer statistically significant. This pattern is consistent with the baseline evidence that the household-expenditure response is concentrated around the main implementation period rather than evenly distributed across all later years.

Overall, the alternative timing exercise supports a dynamic interpretation of the baseline findings. Shifting the post-policy period to 2016 weakens the average DID estimates, and income-related outcomes continue to show no clear response. However, the event-study evidence still points to a negative household-expenditure response around 2016. The timing results therefore suggest that household expenditure adjustment is most visible around the early implementation window, rather than as a stable average effect across the full post-policy period. This reinforces the conclusion that household expenditure is the outcome most closely associated with capacity-reduction-related exposure.

6.6 Sensitivity to Outlier Treatment

Table 11 examines whether the baseline fixed-effects DID estimates are affected by the treatment of extreme monetary outcomes. In the baseline analysis, monetary outcomes are trimmed within each survey year by excluding observations below the 1st percentile or above the 99th percentile. This restriction is used because income and expenditure variables are right-skewed and can be influenced by unusually large or small reported values. Sensitivity to this type of sample treatment is therefore relevant when analysing monetary outcomes (Cowell & Victoria-Feser, 1996). As a check, the models are re-estimated using untrimmed log outcomes, retaining all positive monetary observations before logarithmic transformation.

Table 11. Sensitivity to Outlier Treatment

Outcome	Log Individual Income	Log Wage Income	Log Family Income	Log Family Expenditure
Panel A. Baseline: Year-Specific 1%-99% Trimming				
Treatment x Post	-0.04	-0.09	0.02	-0.05
	(0.05)	(0.06)	(0.04)	(0.04)
Observations	13,119	12,607	10,784	10,784
Panel B. Untrimmed Log Outcomes				
Treatment x Post	-0.03	-0.08	0.02	-0.04
	(0.05)	(0.06)	(0.04)	(0.05)
Observations	13,335	12,821	11,096	11,097

Note. Panel A uses the baseline year-specific 1%–99% trimmed sample; Panel B uses untrimmed log outcomes. Estimates are from fixed-effects DID models with controls and year fixed effects. Standard errors are household-clustered and reported in parentheses. Additional estimates are reported in Appendix Tables G1–G2. * p < 0.10, ** p < 0.05, *** p < 0.01.

The average fixed-effects DID estimates are very similar across the trimmed and untrimmed samples. For log individual income, the coefficient on *Treatment* × *Post* changes from -0.04 in the baseline trimmed sample to -0.03 in the untrimmed sample. For log wage income, the estimate changes from -0.09 to -0.08. The log family income estimate remains unchanged at 0.02, while the log family expenditure estimate remains negative and changes only slightly from -0.05 to -0.04. None of these average estimates is statistically significant in either sample.

These results indicate that the average baseline estimates are not materially

driven by the exclusion of extreme monetary observations. Retaining all positive monetary values increases the number of observations, but it does not meaningfully change the signs, magnitudes, or statistical significance of the fixed-effects DID estimates. This provides some reassurance that the baseline trimming rule does not mechanically generate the main average results. More generally, this check is useful because estimates based on income and expenditure data may be sensitive to data-cleaning and sample-construction choices when outcome variables contain extreme values (Bollinger & Chandra, 2005).

Appendix Tables G1 and G2 report additional event-study estimates using untrimmed log outcomes. For individual-level outcomes, the coefficients remain similar to the baseline pattern and do not show a clear negative post-policy response. For log family income, the coefficients also remain mixed and statistically insignificant. For log family expenditure, the post-policy coefficients remain negative in all post-policy years, with statistically significant declines in 2016 and 2020. Although the level of statistical significance differs somewhat from the trimmed baseline event-study estimates, the direction of the expenditure response remains consistent.

Overall, the outlier-treatment sensitivity analysis supports the robustness of the baseline interpretation. The main average DID estimates change very little when the trimming rule is removed. Income-related outcomes remain weak and statistically insignificant, while the household-expenditure event-study estimates continue to show a negative post-policy pattern. This suggests that neither the weak income results nor the core expenditure pattern is simply an artefact of excluding extreme monetary observations. Household expenditure therefore remains the most robust outcome margin across alternative sample treatments, even though the statistical significance of individual event-study coefficients varies somewhat between the trimmed and untrimmed samples.

6.7 Summary of Robustness Evidence

The robustness analyses show a clear difference across outcome groups.

Household expenditure is the outcome for which the evidence is most persistent, while the income-related outcomes remain weaker and more sensitive to specification choices. Reporting this sensitivity is important because robustness checks help distinguish results that are stable across reasonable empirical alternatives from results that depend more strongly on modelling assumptions (Leamer, 1983). In this study, worker-level income and wage-income estimates become more negative in some specifications, especially under the strict treatment definition and in the matched sample. However, these income effects are not stable across alternative control groups, treatment definitions, timing assumptions, and event-study specifications. Household income also remains weak and does not display a consistent negative response.

The alternative control-group and treatment-definition checks provide two useful pieces of evidence. First, the expenditure pattern is not confined to the baseline industrial-related control group. When the supply-chain-related and pre-trend-selected control groups are used, household expenditure continues to show negative post-policy coefficients, although the magnitude and statistical significance vary across comparison samples. Second, the alternative treatment-definition results suggest an exposure-intensity pattern. The strict treatment group produces larger negative estimates, especially for log wage income and log family expenditure, while the broad treatment definition generally attenuates the estimates. This suggests that more directly exposed workers and households experienced stronger adjustment, whereas broader definitions dilute the estimates by including workers with weaker exposure.

The matching-based DID results also support the main interpretation, but they should be read as supplementary evidence. Matching improves balance on pre-policy observable characteristics and outcome changes, and the household-expenditure estimates remain negative in the matched sample. The matched estimates for worker-level income and household income also become more negative, but they are less dynamically consistent than the expenditure results. This pattern suggests that matching strengthens the evidence of household expenditure adjustment, while the income-related results remain more sensitive to sample construction and specification

choices.

The timing and outlier-treatment checks point to a similar conclusion. When the post-policy period is redefined as beginning in 2016, the average DID estimates become weaker, suggesting that the policy response is not well described as a constant average effect over the full post-policy period. The event-study evidence instead points to a household-expenditure response concentrated around the main implementation window. Removing the trimming rule also leaves the main average DID estimates largely unchanged, and the household-expenditure event-study estimates continue to show a negative post-policy pattern. These results indicate that the core expenditure finding is not mechanically driven by the exclusion of extreme monetary observations.

Overall, the robustness evidence supports the interpretation that capacity-reduction-related exposure is associated most clearly with household expenditure adjustment rather than with a uniformly strong decline in measured income. The results are not identical across all specifications, and the significance of individual coefficients varies. However, the same broad pattern appears repeatedly: income-related outcomes are weaker and more specification-sensitive, while household expenditure is the most visible and persistent margin of adjustment. In this sense, the robustness checks strengthen the credibility of the main interpretation by showing that the expenditure result is not confined to a single control group, treatment definition, matched sample, timing assumption, or outlier-treatment rule. This use of multiple complementary checks is consistent with applied policy-evaluation practice, where robustness and sensitivity analyses are used to assess the interpretation of empirical findings (Athey & Imbens, 2017).

7. Policy Implications

The findings of this thesis suggest that capacity reduction should be evaluated not only through aggregate objectives such as efficiency improvement, industrial upgrading, and environmental performance, but also through the distribution of adjustment costs across workers and households. The empirical results do not show a

uniformly strong decline in measured income. Individual income, wage income, and household income provide weaker and more specification-sensitive evidence of adverse effects. Household expenditure, however, shows a clearer and more persistent pattern of adjustment. This distinction matters for policy design because welfare losses may appear through household spending behaviour even when realised income losses are not immediately observed. Households connected to affected sectors may reduce expenditure in response to expected income risk, perceived job insecurity, or uncertainty about future labour-market conditions.

One implication is that industrial restructuring should be accompanied by forward-looking labour-market transition support. The income estimates in this thesis are not uniformly robust, but they still indicate that workers in exposed sectors may face wage uncertainty, employment risk, and pressure to move across sectors or occupations. Policy responses should therefore not be limited to compensation after displacement has already occurred. They should also include transition assistance during the restructuring process. Practical measures may include retraining, job-search support, placement services, and targeted re-employment assistance in regions and sectors with high exposure to capacity reduction. This implication is consistent with evidence that workers affected by major industrial or sectoral shocks may experience persistent earnings losses and difficult labour-market transitions (Stevens, 1997; Couch & Placzek, 2010; Schmieder et al., 2023; Walker, 2013). Evidence from the active labour-market policy literature also suggests that such programmes can improve post-shock labour-market outcomes, although their effectiveness depends on programme design, targeting, and local implementation capacity (Card et al., 2010; Card et al., 2018).

A second implication concerns household welfare and income-risk management. The expenditure results suggest that households may adjust their spending even when measured income effects are weak or statistically imprecise. A decline in household expenditure should therefore not be interpreted only as a mechanical consequence of realised income loss. It may also reflect precautionary behaviour when households face uncertainty about employment, wages, or future income. From this perspective,

temporary income support, unemployment protection, and targeted assistance for financially fragile households may help reduce the hidden welfare costs of industrial restructuring. This is consistent with evidence that liquidity constraints and incomplete insurance can make temporary support especially valuable when households face adverse labour-market shocks (Gruber, 1997; Chetty, 2008). Policy design should therefore pay attention not only to whether workers lose jobs or income, but also to whether households reduce consumption, postpone expenditure, or draw down financial buffers in response to uncertainty.

A third implication is that capacity-reduction policy should be monitored using micro-level welfare indicators as well as sectoral and aggregate indicators. Evaluations that focus only on capacity targets, firm restructuring, industrial upgrading, or environmental outcomes may overlook adjustment pressures experienced by workers and households. The evidence in this thesis suggests that reform outcomes should also be assessed by whether affected workers are successfully reallocated and whether households can maintain welfare without large reductions in expenditure. In this sense, household expenditure can serve as an informative margin for detecting adjustment costs that may not be fully visible in income measures alone.

More broadly, the policy lesson is that supply-side structural reform can generate both efficiency gains and distributional pressures. Capacity reduction may help remove inefficient capacity and support longer-term industrial upgrading, but it can also create concentrated risks for workers and households connected to affected industries. Policy responses should therefore combine labour-market transition measures, income-risk management, unemployment and resettlement support, and household-level social protection. Such support is especially important in regions and sectors where exposure to industrial restructuring is concentrated. The results of this thesis suggest that the welfare consequences of capacity reduction are most clearly observed through household expenditure adjustment rather than through a uniformly strong decline in measured income.

8. Conclusion

This thesis studies the effects of China's capacity reduction policy on workers' income and household welfare. The policy was introduced as part of the broader supply-side structural reform agenda and aimed to reduce excess capacity in heavy and upstream industries. Using seven waves of China Family Panel Studies (CFPS) data from 2010 to 2022, the analysis compares workers exposed to capacity-reduction-related industries and occupations with workers in non-treated comparison groups before and after the policy period. The empirical strategy combines difference-in-differences and event-study specifications and examines four outcomes: individual income, wage income, family income, and family expenditure.

The main finding is that the consequences of capacity reduction are more clearly observed in household expenditure than in measured income. Income-related outcomes provide weaker and more specification-sensitive evidence. Individual income and wage income become more negative in some robustness checks, particularly under stricter treatment definitions and in matched samples, but these effects are not consistently strong across all specifications. Household income also does not show a stable negative response. By contrast, household expenditure displays the clearest evidence of adjustment. The expenditure estimates are negative across several specifications, including alternative control groups, alternative treatment definitions, matching-based DID, alternative timing checks, and outlier-treatment sensitivity analyses.

This pattern suggests that the household consequences of capacity reduction should not be evaluated only through realised income losses. Households may reduce expenditure not only because of actual income changes, but also because of expected income risk, perceived employment insecurity, and uncertainty about future labour-market conditions. This interpretation is consistent with evidence that labour-market shocks can create persistent earnings risk and with household consumption models in which uncertainty, precautionary motives, and incomplete insurance shape expenditure responses (Jacobson et al., 1993; Bloom, 2009; Carroll,

1997; Blundell et al., 2008; Heathcote et al., 2014; Schmieder et al., 2023). More broadly, the results indicate that household expenditure is an important welfare margin because it may reflect both realised resource constraints and precautionary responses to risk.

The thesis contributes to the literature in two main ways. First, it provides micro-level evidence on the welfare consequences of China's supply-side structural reform, a policy area that has often been examined through firms, industries, productivity, or aggregate restructuring. The results suggest that capacity reduction was not distributionally neutral for workers and households connected to affected sectors. Second, by jointly examining individual income, wage income, family income, and family expenditure, the thesis highlights an important distinction between labour-market outcomes and household-level welfare adjustment. The household consequences of industrial restructuring may appear not only through observable income losses, but also through expenditure adjustment in response to labour-market risk and uncertainty.

Several limitations should be recognised. First, treatment status is defined using fixed industry and occupation information in survey data. This approach avoids redefining treatment status after the policy period, but it is necessarily coarse. Workers may move across jobs, sectors, or occupations after the policy begins, so the estimates should be interpreted as effects associated with initial exposure rather than as effects of remaining continuously employed in treated sectors. Second, the biennial structure of the CFPS means that only a limited number of pre-policy waves are available, which restricts the ability to assess short-run dynamics and pre-policy comparability in detail. Third, reporting error and year-to-year volatility in survey income measures may reduce the precision of the estimated income effects and, depending on the structure of the measurement error, attenuate their magnitude. These measurement issues are only one of several possible explanations for the statistically insignificant income results. Fourth, the analysis relies on sample restrictions such as positive monetary outcomes for log transformations, observed industry and occupation information, and trimming of extreme monetary values. These restrictions

are necessary for consistent estimation but may affect sample composition. Fifth, the data do not directly observe all underlying mechanisms, such as layoffs, reductions in hours, overtime cuts, migration, savings drawdown, perceived job insecurity, or transitions across firms and sectors. Finally, because income results are weaker and more specification-sensitive than expenditure results, the estimated effects should be interpreted primarily as evidence of household adjustment rather than as proof of a uniform decline in measured income.

These limitations also point to useful directions for future research. Future work could combine household survey data with administrative employment records or firm-level data to distinguish more clearly between employment effects, wage effects, working-hours effects, and mobility responses. Further research could also examine heterogeneity across regions, industries, and worker characteristics, especially where implementation intensity and reallocation opportunities differ. Beyond total income and total expenditure, future studies could investigate detailed expenditure categories, savings behaviour, household financial vulnerability, health, children's education, and intergenerational mobility. These extensions would help clarify how industrial restructuring affects household welfare through both realised income changes and precautionary adjustment.

Overall, the evidence in this thesis suggests that capacity reduction created adjustment pressures for workers and households connected to policy-affected industries. While the reform may have supported broader goals of industrial restructuring, efficiency improvement, and environmental governance, its welfare consequences were not evenly distributed. The most robust evidence concerns household expenditure adjustment, while income effects are weaker and more sensitive to empirical specification. A central implication is therefore that structural reform should be evaluated not only by aggregate or sectoral outcomes, but also by how its adjustment costs are transmitted to workers and households. In this sense, a socially sustainable transition requires not only industrial upgrading, but also credible support for the workers and families most exposed to the costs of adjustment.

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Appendix.

Appendix A. Treatment and Control Group Construction

Appendix Table A1. Detailed Industry and Occupation Criteria for Treatment and Control Groups

Panel A. Treatment-exposure components			
Group component	**Industry criterion**	**Occupation code(s)**	**Included in definition**
A. Mining industry	Industry code 2	—	Strict, Main and Broad
A. Mining- and extraction-related occupations	No industry restriction	20203, 20204, 60103, 60104, 60105, 60106, 60109	Strict, Main and Broad
B. Metal-smelting, steelmaking, and metal-rolling occupations	Industry code 3	60201, 60202, 60203, 60204, 60205, 60206, 60208, 60209, 60219	Main and Broad
B. Shipbuilding, cement, glass and related-material occupations	Industry code 3	60519, 61601, 61700, 61701, 61702, 61703, 61709	Main and Broad
B. Machinery-processing occupations	Industry code 3	60401, 60402, 60403, 60404, 60405, 60406	Main and Broad
B. Equipment-maintenance and assembly occupations	Industry code 3	60601, 60501, 60502, 60503, 60504, 60505, 60506, 60507, 60508, 60509	Main and Broad
C. Broader production-related occupations	Industry code 3	62501, 62903, 63509	Broad only
Panel B. Control-group definitions			
Group definition	**Industry criterion**	**Coverage**	
Industrial-related control group	Industry codes 4, 5, and 6	Production and supply of electricity, gas and water; construction; transport, storage and postal services.	
Supply-chain-related control group	Industry codes 4, 5, 6, and 11	Production and supply of electricity, gas and water; construction; transport, storage and postal services; real estate.	

Pre-trend-selected control group	Industry codes 6, 9, and 15	Transport, storage and postal services; accommodation and catering; resident services and other services.

Notes: Strict treatment group = Block A; Main treatment group = Blocks A + B; Broad treatment group = Blocks A + B + C. The industry codes correspond to the cleaned CFPS industry classification: 2 = mining; 3 = manufacturing; 4 = production and supply of electricity, gas and water; 5 = construction; 6 = transport, storage and postal services; 9 = accommodation and catering; 11 = real estate; and 15 = resident services and other services. The variables ind_clean and occ_clean are constructed from the CFPS industry and occupation classifications. Each control-group definition is applied separately to the three treatment-group definitions. Individuals classified as treated under the corresponding treatment-group definition are excluded from the control group.

Appendix B.

Full Regression Results for the Baseline Specification: Main Treatment Definition and Industrial-Related Control Group

Appendix Table B1. Baseline Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	Controls	No controls	Controls	No controls	Controls
Post (≥2014)	0.54***	0.58***				
	(0.02)	(0.02)				
Treatment	0.13***	0.11***			0.03	0.04
	(0.03)	(0.02)			(0.10)	(0.09)
Treatment x Post	-0.05	-0.05	-0.04	-0.04		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.30***	0.21**	0.25***	0.17*
			(0.04)	(0.10)	(0.05)	(0.10)
Year 2014			0.41***	0.25	0.41***	0.26
			(0.05)	(0.19)	(0.05)	(0.19)
Year 2016			0.44***	0.21	0.41***	0.20
			(0.08)	(0.29)	(0.10)	(0.29)
Year 2018			0.64***	0.33	0.63***	0.32
			(0.05)	(0.38)	(0.05)	(0.38)
Year 2020			0.71***	0.32	0.69***	0.31
			(0.05)	(0.47)	(0.05)	(0.47)
Year 2022			0.86***	0.40	0.85***	0.40
			(0.05)	(0.56)	(0.05)	(0.56)

Treatment x Year 2012					0.16*	0.16*
					(0.08)	(0.08)
Treatment x Year 2014					-0.05	-0.05
					(0.09)	(0.09)
Treatment x Year 2016					0.05	0.03
					(0.16)	(0.16)
Treatment x Year 2018					-0.00	0.00
					(0.09)	(0.09)
Treatment x Year 2020					0.02	0.01
					(0.09)	(0.09)
Treatment x Year 2022					-0.01	-0.02
					(0.09)	(0.09)
Female		-0.41***				
		(0.02)				
Age		0.06***		0.13**		0.13**
		(0.01)		(0.05)		(0.05)
Age squared		-0.00***		-0.00***		-0.00***
		(0.00)		(0.00)		(0.00)
Married		0.26***		-0.00		-0.00
		(0.02)		(0.07)		(0.07)
Years of Education		0.04***		0.02*		0.02*
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.13***		0.04		0.04
		(0.02)		(0.06)		(0.06)
Constant	2.73***	1.26***	2.64***	-0.68	2.63***	-0.67
	(0.02)	(0.11)	(0.03)	(1.67)	(0.04)	(1.66)

Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	13,119	13,119	13,119	13,119	13,119	13,119

Notes: Treatment = main definition; control = industrial-related control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table B2. Baseline Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.48***	0.51***				
	(0.02)	(0.02)				
Treatment	0.13***	0.12***			0.03	0.04
	(0.03)	(0.03)			(0.11)	(0.10)
Treatment x Post	-0.06*	-0.06*	-0.09	-0.09		
	(0.03)	(0.03)	(0.06)	(0.06)		
Year 2012			0.29***	0.17	0.25***	0.13
			(0.04)	(0.12)	(0.05)	(0.12)
Year 2014			0.40***	0.21	0.40***	0.21
			(0.05)	(0.24)	(0.06)	(0.24)
Year 2016			-0.09	-0.40	-0.13	-0.42
			(0.15)	(0.37)	(0.17)	(0.38)
Year 2018			0.59***	0.13	0.57***	0.13
			(0.05)	(0.46)	(0.05)	(0.46)

Year 2020		0.66***	0.09	0.64***	0.08
		(0.05)	(0.57)	(0.05)	(0.57)
Year 2022		0.81***	0.14	0.81***	0.15
		(0.05)	(0.68)	(0.06)	(0.69)
Treatment x Year 2012				0.14	0.13
				(0.09)	(0.08)
Treatment x Year 2014				-0.07	-0.08
				(0.09)	(0.09)
Treatment x Year 2016				0.05	0.02
				(0.31)	(0.31)
Treatment x Year 2018				-0.05	-0.05
				(0.10)	(0.09)
Treatment x Year 2020				-0.01	-0.02
				(0.10)	(0.10)
Treatment x Year 2022				-0.10	-0.10
				(0.10)	(0.10)
Female	-0.40***				
	(0.02)				
Age	0.06***		0.15**		0.15**
	(0.01)		(0.06)		(0.06)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.28***		0.04		0.04
	(0.02)		(0.07)		(0.07)
Years of Education	0.04***		0.02		0.02
	(0.00)		(0.01)		(0.01)

Urban Dummy		0.15***		0.05		0.05
		(0.02)		(0.07)		(0.07)
Constant	2.72***	1.10***	2.62***	-1.23	2.62***	-1.22
	(0.02)	(0.11)	(0.03)	(2.04)	(0.04)	(2.04)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	12,607	12,607	12,607	12,607	12,607	12,607

Notes: Treatment = main definition; control = industrial-related control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive wage income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table B3. Baseline Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.62***	0.57***				
	(0.02)	(0.02)				
Treatment	0.01	0.03			0.07	0.07
	(0.03)	(0.03)			(0.08)	(0.08)
Treatment x Post	0.01	0.00	0.02	0.02		
	(0.03)	(0.03)	(0.04)	(0.04)		
Year 2012			0.32***	0.31***	0.31***	0.30***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2014			0.44***	0.44***	0.48***	0.47***
			(0.04)	(0.04)	(0.05)	(0.05)

Year 2016		0.67***	0.67***	0.70***	0.69***
		(0.05)	(0.05)	(0.07)	(0.06)
Year 2018		0.84***	0.84***	0.86***	0.85***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2020		0.92***	0.91***	0.92***	0.91***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2022		0.99***	0.98***	0.98***	0.97***
		(0.04)	(0.04)	(0.05)	(0.05)
Treatment x Year 2012				0.05	0.06
				(0.09)	(0.09)
Treatment x Year 2014				-0.07	-0.07
				(0.08)	(0.08)
Treatment x Year 2016				-0.08	-0.06
				(0.11)	(0.10)
Treatment x Year 2018				-0.01	0.00
				(0.08)	(0.08)
Treatment x Year 2020				0.04	0.04
				(0.09)	(0.09)
Treatment x Year 2022				0.05	0.05
				(0.09)	(0.08)
HH Rep Female	0.03**				
	(0.01)				
HH Rep Age	0.00		0.01		0.01
	(0.00)		(0.01)		(0.01)
HH Rep Age squared	0.00		-0.00		-0.00
	(0.00)		(0.00)		(0.00)

HH Rep Married		0.12***		0.05		0.05
		(0.03)		(0.06)		(0.06)
HH Rep Years of Education		0.04***		0.00		0.00
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.15***		0.06		0.06
		(0.02)		(0.06)		(0.06)
Family Size		0.10***		0.13***		0.13***
		(0.00)		(0.02)		(0.02)
Constant	3.47***	2.41***	3.29***	2.55***	3.27***	2.53***
	(0.02)	(0.09)	(0.03)	(0.24)	(0.04)	(0.24)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	10,784	10,784	10,784	10,784	10,784	10,784

Notes: Treatment = main definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year⩾2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table B4. Baseline Regression Results for Log Family Expenditure

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.52***	0.48***				
	(0.02)	(0.02)				
Treatment	0.01	0.02			0.17**	0.16**
	(0.03)	(0.03)			(0.08)	(0.08)

Treatment x Post	-0.04	-0.06**	-0.05	-0.05		
	(0.03)	(0.03)	(0.04)	(0.04)		
Year 2012			0.20***	0.21***	0.24***	0.24***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2014			0.44***	0.45***	0.48***	0.49***
			(0.05)	(0.05)	(0.05)	(0.05)
Year 2016			0.64***	0.66***	0.73***	0.75***
			(0.06)	(0.06)	(0.07)	(0.07)
Year 2018			0.66***	0.69***	0.70***	0.73***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2020			0.66***	0.67***	0.70***	0.72***
			(0.05)	(0.05)	(0.05)	(0.05)
Year 2022			0.74***	0.76***	0.76***	0.78***
			(0.05)	(0.05)	(0.06)	(0.06)
Treatment x Year 2012					-0.11	-0.10
					(0.09)	(0.08)
Treatment x Year 2014					-0.18**	-0.18**
					(0.09)	(0.09)
Treatment x Year 2016					-0.35***	-0.33***
					(0.12)	(0.12)
Treatment x Year 2018					-0.17*	-0.16*
					(0.09)	(0.09)
Treatment x Year 2020					-0.18*	-0.18**
					(0.09)	(0.09)
Treatment x Year 2022					-0.12	-0.12
					(0.10)	(0.10)

HH Rep Female		0.04***				
		(0.01)				
HH Rep Age		-0.00		0.01		0.00
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		-0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.17***		0.10		0.10
		(0.03)		(0.08)		(0.08)
HH Rep Years of Education		0.04***		0.01**		0.01**
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.24***		0.03		0.04
		(0.02)		(0.07)		(0.07)
Family Size		0.09***		0.10***		0.10***
		(0.00)		(0.02)		(0.02)
Constant	3.33***	2.59***	3.23***	2.63***	3.17***	2.58***
	(0.01)	(0.10)	(0.03)	(0.26)	(0.04)	(0.26)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	10,784	10,784	10,784	10,784	10,784	10,784

Notes: Treatment = main definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household expenditure values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix C.

Full Regression Results for Alternative Control Group Definitions under the Main Treatment Definition

Supply-Chain-Related Control Group

Appendix Table C1. Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	Controls	No controls	Controls	No controls	Controls
Post (≥2014)	0.52***	0.57***				
	(0.02)	(0.02)				
Treatment	0.12***	0.10***			0.05	0.05
	(0.03)	(0.02)			(0.10)	(0.09)
Treatment x Post	-0.03	-0.04	-0.03	-0.03		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.30***	0.19*	0.26***	0.15
			(0.04)	(0.10)	(0.05)	(0.10)
Year 2014			0.41***	0.20	0.42***	0.21
			(0.05)	(0.19)	(0.05)	(0.19)
Year 2016			0.45***	0.13	0.43***	0.12
			(0.07)	(0.29)	(0.09)	(0.29)
Year 2018			0.64***	0.24	0.63***	0.24
			(0.05)	(0.37)	(0.05)	(0.37)

Year 2020		0.71***	0.20	0.70***	0.20
		(0.05)	(0.46)	(0.05)	(0.46)
Year 2022		0.87***	0.26	0.86***	0.27
		(0.05)	(0.55)	(0.05)	(0.55)
Treatment x Year 2012				0.15*	0.15*
				(0.08)	(0.08)
Treatment x Year 2014				-0.05	-0.06
				(0.09)	(0.09)
Treatment x Year 2016				0.04	0.02
				(0.15)	(0.15)
Treatment x Year 2018				-0.00	-0.00
				(0.09)	(0.09)
Treatment x Year 2020				0.02	0.01
				(0.09)	(0.09)
Treatment x Year 2022				-0.01	-0.02
				(0.09)	(0.09)
Female	-0.40***				
	(0.02)				
Age	0.05***		0.15***		0.15***
	(0.00)		(0.05)		(0.05)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.25***		-0.02		-0.02
	(0.02)		(0.07)		(0.07)
Years of Education	0.04***		0.02*		0.02*
	(0.00)		(0.01)		(0.01)

Urban Dummy		0.13***		0.04		0.04
		(0.02)		(0.06)		(0.06)
Constant	2.74***	1.31***	2.63***	-1.27	2.62***	-1.25
	(0.02)	(0.10)	(0.03)	(1.65)	(0.04)	(1.64)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	14,159	14,159	14,159	14,159	14,159	14,159

Note. Treatment = main definition; control = supply-chain-related control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C2. Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.46***	0.49***				
	(0.02)	(0.02)				
Treatment	0.13***	0.11***			0.04	0.06
	(0.03)	(0.03)			(0.10)	(0.10)
Treatment x Post	-0.04	-0.04	-0.09	-0.09		
	(0.03)	(0.03)	(0.06)	(0.06)		
Year 2012			0.29***	0.16	0.25***	0.13
			(0.04)	(0.12)	(0.05)	(0.12)
Year 2014			0.41***	0.18	0.41***	0.19
			(0.05)	(0.24)	(0.05)	(0.24)

Year 2016		-0.11	-0.45	-0.14	-0.47
		(0.14)	(0.36)	(0.16)	(0.37)
Year 2018		0.58***	0.09	0.57***	0.09
		(0.05)	(0.46)	(0.05)	(0.46)
Year 2020		0.67***	0.06	0.65***	0.06
		(0.05)	(0.57)	(0.05)	(0.57)
Year 2022		0.83***	0.09	0.83***	0.11
		(0.05)	(0.68)	(0.06)	(0.69)
Treatment x Year 2012				0.13	0.13
				(0.09)	(0.08)
Treatment x Year 2014				-0.08	-0.09
				(0.09)	(0.09)
Treatment x Year 2016				0.05	0.03
				(0.30)	(0.30)
Treatment x Year 2018				-0.05	-0.05
				(0.10)	(0.09)
Treatment x Year 2020				-0.03	-0.04
				(0.10)	(0.10)
Treatment x Year 2022				-0.11	-0.12
				(0.10)	(0.10)
Female	-0.39***				
	(0.02)				
Age	0.06***		0.16***		0.16***
	(0.01)		(0.06)		(0.06)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)

Married		0.26***		0.02		0.02
		(0.02)		(0.07)		(0.07)
Years of Education		0.04***		0.01		0.01
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.14***		0.06		0.06
		(0.02)		(0.07)		(0.07)
Constant	2.73***	1.14***	2.61***	-1.60	2.60***	-1.58
	(0.02)	(0.11)	(0.03)	(2.03)	(0.04)	(2.04)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	13,578	13,578	13,578	13,578	13,578	13,578

Note. Treatment = main definition; control = supply-chain-related control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive wage income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C3. Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.62***	0.57***				
	(0.02)	(0.02)				
Treatment	-0.00	0.02			0.07	0.07
	(0.03)	(0.03)			(0.07)	(0.07)
Treatment x Post	0.01	-0.00	0.02	0.02		
	(0.03)	(0.03)	(0.04)	(0.04)		

Year 2012		0.31***	0.31***	0.31***	0.30***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2014		0.44***	0.44***	0.48***	0.47***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2016		0.67***	0.67***	0.70***	0.70***
		(0.05)	(0.05)	(0.06)	(0.06)
Year 2018		0.85***	0.85***	0.86***	0.86***
		(0.04)	(0.04)	(0.04)	(0.04)
Year 2020		0.92***	0.91***	0.92***	0.91***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2022		0.99***	0.99***	0.99***	0.98***
		(0.04)	(0.04)	(0.05)	(0.05)
Treatment x Year 2012				0.04	0.05
				(0.09)	(0.09)
Treatment x Year 2014				-0.08	-0.07
				(0.08)	(0.08)
Treatment x Year 2016				-0.08	-0.06
				(0.10)	(0.10)
Treatment x Year 2018				-0.02	-0.00
				(0.08)	(0.08)
Treatment x Year 2020				0.04	0.03
				(0.09)	(0.08)
Treatment x Year 2022				0.05	0.04
				(0.08)	(0.08)
HH Rep Female	0.03**				
	(0.01)				

HH Rep Age		0.00		0.01		0.01
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.13***		0.05		0.05
		(0.02)		(0.06)		(0.06)
HH Rep Years of Education		0.04***		0.00		0.00
		(0.00)		(0.00)		(0.00)
Urban Dummy		0.16***		0.07		0.07
		(0.01)		(0.06)		(0.06)
Family Size		0.10***		0.13***		0.13***
		(0.00)		(0.01)		(0.01)
Constant	3.49***	2.40***	3.30***	2.59***	3.28***	2.57***
	(0.02)	(0.09)	(0.03)	(0.22)	(0.04)	(0.22)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	11,557	11,557	11,557	11,557	11,557	11,557

Note. Treatment = main definition at the household level; control = supply-chain-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C4. Regression Results for Log Family Expenditure

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**

Post (≥2014)	0.52***	0.47***				
	(0.02)	(0.02)				
Treatment	-0.00	0.01			0.15**	0.15**
	(0.03)	(0.03)			(0.08)	(0.07)
Treatment x Post	-0.04	-0.06**	-0.05	-0.05		
	(0.03)	(0.03)	(0.04)	(0.04)		
Year 2012			0.20***	0.21***	0.24***	0.24***
			(0.04)	(0.04)	(0.04)	(0.04)
Year 2014			0.45***	0.46***	0.50***	0.51***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2016			0.62***	0.65***	0.70***	0.72***
			(0.06)	(0.06)	(0.07)	(0.07)
Year 2018			0.65***	0.69***	0.69***	0.72***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2020			0.64***	0.66***	0.68***	0.69***
			(0.04)	(0.05)	(0.05)	(0.05)
Year 2022			0.73***	0.75***	0.75***	0.77***
			(0.05)	(0.05)	(0.06)	(0.06)
Treatment x Year 2012					-0.10	-0.09
					(0.08)	(0.08)
Treatment x Year 2014					-0.19**	-0.19**
					(0.09)	(0.08)
Treatment x Year 2016					-0.32***	-0.30***
					(0.12)	(0.12)
Treatment x Year 2018					-0.16*	-0.15*
					(0.09)	(0.09)

Treatment x Year 2020					-0.16*	-0.16*
					(0.09)	(0.09)
Treatment x Year 2022					-0.11	-0.11
					(0.10)	(0.10)
HH Rep Female		0.04***				
		(0.01)				
HH Rep Age		-0.00		0.01		0.01
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		-0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.17***		0.10		0.10
		(0.03)		(0.07)		(0.07)
HH Rep Years of Education		0.04***		0.02***		0.02***
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.25***		0.04		0.05
		(0.02)		(0.06)		(0.06)
Family Size		0.09***		0.11***		0.11***
		(0.00)		(0.02)		(0.02)
Constant	3.35***	2.59***	3.24***	2.60***	3.19***	2.55***
	(0.01)	(0.09)	(0.03)	(0.25)	(0.04)	(0.25)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	11,557	11,557	11,557	11,557	11,557	11,557

Note. Treatment = main definition at the household level; control = supply-chain-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household expenditure

values only. * p<0.10, ** p<0.05, *** p<0.01.

Pre-Trend Comparable Control Group

Appendix Table C5. Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.35***	0.44***				
	(0.02)	(0.02)				
Treatment	0.15***	0.04			0.04	0.05
	(0.03)	(0.03)			(0.12)	(0.12)
Treatment x Post	0.14***	0.10***	0.02	0.04		
	(0.03)	(0.03)	(0.07)	(0.07)		
Year 2012			0.35***	0.24	0.31***	0.20
			(0.05)	(0.16)	(0.07)	(0.16)
Year 2014			0.37***	0.11	0.36***	0.11
			(0.06)	(0.31)	(0.08)	(0.31)
Year 2016			0.42***	0.05	0.41***	0.03
			(0.09)	(0.47)	(0.10)	(0.47)
Year 2018			0.64***	0.19	0.63***	0.16
			(0.06)	(0.60)	(0.07)	(0.60)
Year 2020			0.70***	0.13	0.69***	0.11
			(0.06)	(0.75)	(0.08)	(0.75)

Year 2022		0.85***	0.17	0.85***	0.15
		(0.07)	(0.90)	(0.08)	(0.89)
Treatment x Year 2012				0.10	0.09
				(0.10)	(0.10)
Treatment x Year 2014				0.02	0.02
				(0.11)	(0.10)
Treatment x Year 2016				0.05	0.06
				(0.17)	(0.16)
Treatment x Year 2018				0.05	0.07
				(0.11)	(0.10)
Treatment x Year 2020				0.04	0.07
				(0.11)	(0.11)
Treatment x Year 2022				0.01	0.04
				(0.11)	(0.11)
Female	-0.48***				
	(0.02)				
Age	0.07***		0.15*		0.15*
	(0.01)		(0.08)		(0.08)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.25***		-0.02		-0.03
	(0.02)		(0.07)		(0.07)
Years of Education	0.04***		0.01		0.01
	(0.00)		(0.02)		(0.02)
Urban Dummy	0.07***		0.08		0.08
	(0.02)		(0.08)		(0.08)

Constant	2.72***	1.23***	2.56***	-1.14	2.55***	-1.20
	(0.02)	(0.10)	(0.03)	(2.51)	(0.05)	(2.50)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	10,973	10,973	10,973	10,973	10,973	10,973

Note. Treatment = main definition; control = pre-trend matched control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C6. Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.29***	0.36***				
	(0.02)	(0.02)				
Treatment	0.15***	0.04			0.09	0.09
	(0.03)	(0.03)			(0.12)	(0.12)
Treatment x Post	0.13***	0.09***	0.01	0.03		
	(0.04)	(0.03)	(0.07)	(0.07)		
Year 2012			0.34***	0.27	0.30***	0.23
			(0.05)	(0.24)	(0.07)	(0.24)
Year 2014			0.34***	0.16	0.34***	0.16
			(0.07)	(0.49)	(0.08)	(0.48)
Year 2016			-0.23	-0.48	-0.28	-0.54
			(0.16)	(0.73)	(0.19)	(0.71)

Year 2018		0.58***	0.30	0.58***	0.28
		(0.07)	(0.96)	(0.08)	(0.93)
Year 2020		0.66***	0.31	0.66***	0.29
		(0.07)	(1.20)	(0.08)	(1.17)
Year 2022		0.81***	0.39	0.84***	0.39
		(0.07)	(1.44)	(0.08)	(1.40)
Treatment x Year 2012				0.08	0.07
				(0.10)	(0.10)
Treatment x Year 2014				0.02	0.02
				(0.11)	(0.11)
Treatment x Year 2016				0.28	0.26
				(0.31)	(0.30)
Treatment x Year 2018				0.02	0.03
				(0.11)	(0.11)
Treatment x Year 2020				-0.00	0.03
				(0.12)	(0.12)
Treatment x Year 2022				-0.08	-0.05
				(0.12)	(0.12)
Female	-0.48***				
	(0.02)				
Age	0.07***		0.12		0.12
	(0.01)		(0.12)		(0.12)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.26***		-0.04		-0.04
	(0.02)		(0.08)		(0.08)

Years of Education		0.04***		0.00		0.00
		(0.00)		(0.02)		(0.02)
Urban Dummy		0.08***		0.07		0.08
		(0.02)		(0.09)		(0.09)
Constant	2.71***	1.03***	2.55***	-0.26	2.52***	-0.34
	(0.02)	(0.11)	(0.03)	(4.02)	(0.06)	(3.91)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	10,389	10,389	10,389	10,389	10,389	10,389

Note. Treatment = main definition; control = pre-trend matched control. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive wage income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C7. Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.57***	0.56***				
	(0.02)	(0.02)				
Treatment	-0.04	0.01			0.12	0.12
	(0.03)	(0.03)			(0.08)	(0.08)
Treatment x Post	0.06*	0.02	-0.02	-0.02		
	(0.03)	(0.03)	(0.05)	(0.04)		
Year 2012			0.37***	0.36***	0.39***	0.38***
			(0.05)	(0.05)	(0.06)	(0.06)

Year 2014		0.49***	0.48***	0.53***	0.52***
		(0.05)	(0.05)	(0.06)	(0.06)
Year 2016		0.70***	0.69***	0.73***	0.72***
		(0.05)	(0.05)	(0.06)	(0.06)
Year 2018		0.89***	0.90***	0.93***	0.94***
		(0.05)	(0.05)	(0.05)	(0.05)
Year 2020		0.97***	0.97***	1.00***	1.00***
		(0.05)	(0.05)	(0.06)	(0.06)
Year 2022		1.03***	1.04***	1.05***	1.06***
		(0.05)	(0.05)	(0.06)	(0.06)
Treatment x Year 2012				-0.07	-0.06
				(0.11)	(0.10)
Treatment x Year 2014				-0.12	-0.12
				(0.09)	(0.09)
Treatment x Year 2016				-0.10	-0.10
				(0.11)	(0.11)
Treatment x Year 2018				-0.13	-0.13
				(0.09)	(0.09)
Treatment x Year 2020				-0.10	-0.11
				(0.10)	(0.09)
Treatment x Year 2022				-0.07	-0.08
				(0.09)	(0.09)
HH Rep Female	-0.01				
	(0.02)				
HH Rep Age	-0.01*		0.00		0.00
	(0.00)		(0.01)		(0.01)

HH Rep Age squared		0.00***		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.18***		0.06		0.07
		(0.03)		(0.07)		(0.07)
HH Rep Years of Education		0.04***		0.00		0.00
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.13***		-0.01		-0.01
		(0.02)		(0.07)		(0.07)
Family Size		0.10***		0.11***		0.11***
		(0.00)		(0.02)		(0.02)
Constant	3.52***	2.64***	3.30***	2.75***	3.25***	2.71***
	(0.02)	(0.09)	(0.03)	(0.27)	(0.04)	(0.27)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	9,037	9,037	9,037	9,037	9,037	9,037

Note. Treatment = main definition at the household level; control = pre-trend matched control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year≥2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table C8. Regression Results for Log Family Expenditure

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.48***	0.47***				
	(0.02)	(0.02)				

Treatment	-0.07**	-0.02			0.13	0.12
	(0.03)	(0.03)			(0.09)	(0.08)
Treatment x Post	-0.00	-0.05	-0.06	-0.07		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.24***	0.24***	0.29***	0.28***
			(0.05)	(0.04)	(0.06)	(0.06)
Year 2014			0.48***	0.48***	0.55***	0.55***
			(0.06)	(0.05)	(0.07)	(0.07)
Year 2016			0.62***	0.64***	0.69***	0.69***
			(0.06)	(0.06)	(0.07)	(0.07)
Year 2018			0.66***	0.68***	0.68***	0.69***
			(0.05)	(0.05)	(0.06)	(0.06)
Year 2020			0.68***	0.70***	0.73***	0.74***
			(0.06)	(0.06)	(0.07)	(0.07)
Year 2022			0.70***	0.73***	0.70***	0.72***
			(0.06)	(0.06)	(0.07)	(0.07)
Treatment x Year 2012					-0.12	-0.11
					(0.10)	(0.09)
Treatment x Year 2014					-0.23**	-0.23**
					(0.10)	(0.10)
Treatment x Year 2016					-0.27**	-0.26**
					(0.13)	(0.13)
Treatment x Year 2018					-0.11	-0.10
					(0.10)	(0.10)
Treatment x Year 2020					-0.20*	-0.20*
					(0.11)	(0.10)

Treatment x Year 2022					-0.05	-0.06
					(0.11)	(0.11)
HH Rep Female		0.04***				
		(0.02)				
HH Rep Age		-0.01		-0.01		-0.01
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		0.00		0.00		0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.23***		0.11		0.11
		(0.03)		(0.09)		(0.09)
HH Rep Years of Education		0.04***		0.02**		0.02**
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.22***		0.03		0.04
		(0.02)		(0.09)		(0.09)
Family Size		0.08***		0.10***		0.09***
		(0.01)		(0.02)		(0.02)
Constant	3.42***	2.68***	3.27***	3.00***	3.22***	2.96***
	(0.02)	(0.10)	(0.03)	(0.31)	(0.04)	(0.31)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	9,037	9,037	9,037	9,037	9,037	9,037

Note. Treatment = main definition at the household level; control = pre-trend matched control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year⩾2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household expenditure values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix D.

Full Regression Results for Alternative Treatment Group Definitions under the Industrial-Related Control Group Strategy

Strict Treatment Definition

Appendix Table D1. Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	Controls	No controls	Controls	No controls	Controls
Post (≥2014)	0.54***	0.58***				
	(0.02)	(0.02)				
Treatment	0.16***	0.15***			0.11	0.10
	(0.04)	(0.03)			(0.14)	(0.14)
Treatment x Post	-0.08*	-0.11**	-0.13	-0.14*		
	(0.05)	(0.04)	(0.08)	(0.08)		
Year 2012			0.29***	0.25**	0.25***	0.21*
			(0.04)	(0.11)	(0.05)	(0.11)
Year 2014			0.41***	0.32	0.41***	0.33
			(0.05)	(0.21)	(0.06)	(0.21)
Year 2016			0.42***	0.30	0.41***	0.31
			(0.09)	(0.31)	(0.10)	(0.31)
Year 2018			0.63***	0.46	0.63***	0.49
			(0.05)	(0.41)	(0.05)	(0.41)

Year 2020		0.71***	0.49	0.69***	0.52
		(0.05)	(0.51)	(0.05)	(0.51)
Year 2022		0.86***	0.62	0.85***	0.65
		(0.05)	(0.61)	(0.05)	(0.61)
Treatment x Year 2012				0.23**	0.25**
				(0.12)	(0.11)
Treatment x Year 2014				-0.14	-0.12
				(0.13)	(0.13)
Treatment x Year 2016				0.01	0.01
				(0.25)	(0.24)
Treatment x Year 2018				-0.16	-0.15
				(0.13)	(0.13)
Treatment x Year 2020				-0.07	-0.08
				(0.14)	(0.14)
Treatment x Year 2022				-0.04	-0.04
				(0.13)	(0.13)
Female	-0.43***				
	(0.03)				
Age	0.05***		0.11*		0.10*
	(0.01)		(0.06)		(0.06)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.25***		-0.02		-0.02
	(0.02)		(0.09)		(0.09)
Years of Education	0.04***		0.03*		0.03*
	(0.00)		(0.01)		(0.01)

Urban Dummy		0.13***		-0.00		0.00
		(0.02)		(0.07)		(0.07)
Constant	2.73***	1.29***	2.63***	-0.00	2.62***	0.13
	(0.02)	(0.12)	(0.03)	(1.84)	(0.04)	(1.84)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	10,740	10,740	10,740	10,740	10,740	10,740

Note. Treatment = strict definition; control = industrial-related control. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D2. Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.48***	0.51***				
	(0.02)	(0.02)				
Treatment	0.16***	0.16***			0.11	0.12
	(0.04)	(0.03)			(0.15)	(0.15)
Treatment x Post	-0.07	-0.11**	-0.16*	-0.17**		
	(0.05)	(0.05)	(0.09)	(0.08)		
Year 2012			0.28***	0.20	0.25***	0.17
			(0.05)	(0.13)	(0.05)	(0.13)
Year 2014			0.39***	0.29	0.39***	0.31
			(0.05)	(0.27)	(0.06)	(0.27)

Year 2016		-0.10	-0.26	-0.13	-0.27
		(0.17)	(0.42)	(0.18)	(0.43)
Year 2018		0.57***	0.27	0.57***	0.31
		(0.05)	(0.51)	(0.05)	(0.51)
Year 2020		0.65***	0.27	0.64***	0.31
		(0.05)	(0.63)	(0.05)	(0.64)
Year 2022		0.83***	0.38	0.81***	0.42
		(0.05)	(0.75)	(0.06)	(0.77)
Treatment x Year 2012				0.21*	0.22*
				(0.12)	(0.11)
Treatment x Year 2014				-0.18	-0.17
				(0.13)	(0.13)
Treatment x Year 2016				0.11	0.10
				(0.39)	(0.38)
Treatment x Year 2018				-0.17	-0.17
				(0.14)	(0.13)
Treatment x Year 2020				-0.10	-0.11
				(0.14)	(0.14)
Treatment x Year 2022				-0.08	-0.09
				(0.14)	(0.14)
Female	-0.42***				
	(0.03)				
Age	0.06***		0.12*		0.12*
	(0.01)		(0.07)		(0.07)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)

Married		0.27***		0.05		0.05
		(0.02)		(0.08)		(0.08)
Years of Education		0.04***		0.02		0.02
		(0.00)		(0.01)		(0.02)
Urban Dummy		0.15***		-0.00		0.00
		(0.02)		(0.07)		(0.07)
Constant	2.72***	1.14***	2.61***	-0.59	2.60***	-0.43
	(0.02)	(0.12)	(0.03)	(2.29)	(0.04)	(2.32)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	10,335	10,335	10,335	10,335	10,335	10,335

Note. Treatment = strict definition; control = industrial-related control. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive wage income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D3. Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.62***	0.57***				
	(0.02)	(0.02)				
Treatment	-0.11***	-0.05			0.12	0.11
	(0.04)	(0.04)			(0.13)	(0.13)
Treatment x Post	0.02	-0.03	-0.01	0.00		
	(0.05)	(0.04)	(0.07)	(0.07)		

Year 2012		0.32***	0.32***	0.32***	0.31***
		(0.05)	(0.05)	(0.05)	(0.05)
Year 2014		0.45***	0.44***	0.47***	0.46***
		(0.05)	(0.05)	(0.05)	(0.05)
Year 2016		0.68***	0.67***	0.70***	0.69***
		(0.06)	(0.06)	(0.07)	(0.07)
Year 2018		0.85***	0.85***	0.86***	0.86***
		(0.05)	(0.05)	(0.05)	(0.05)
Year 2020		0.92***	0.91***	0.92***	0.91***
		(0.05)	(0.05)	(0.05)	(0.05)
Year 2022		0.99***	0.98***	0.98***	0.98***
		(0.05)	(0.05)	(0.05)	(0.05)
Treatment x Year 2012				0.02	0.05
				(0.14)	(0.14)
Treatment x Year 2014				-0.11	-0.10
				(0.13)	(0.13)
Treatment x Year 2016				-0.18	-0.14
				(0.16)	(0.16)
Treatment x Year 2018				-0.10	-0.06
				(0.12)	(0.12)
Treatment x Year 2020				-0.01	0.02
				(0.14)	(0.13)
Treatment x Year 2022				0.06	0.08
				(0.13)	(0.13)
HH Rep Female	0.04***				
	(0.02)				

HH Rep Age		0.00		0.00		0.00
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.11***		0.05		0.06
		(0.03)		(0.07)		(0.07)
HH Rep Years of Education		0.04***		0.00		0.00
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.15***		0.08		0.09
		(0.02)		(0.06)		(0.06)
Family Size		0.10***		0.12***		0.12***
		(0.00)		(0.02)		(0.02)
Constant	3.48***	2.37***	3.28***	2.61***	3.27***	2.60***
	(0.02)	(0.10)	(0.03)	(0.27)	(0.04)	(0.28)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	8,945	8,945	8,945	8,945	8,945	8,945

Note. Treatment = strict definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D4. Regression Results for Log Family Expenditure

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**

Post (≥2014)	0.51***	0.47***				
	(0.02)	(0.02)				
Treatment	-0.04	0.01			0.24*	0.23*
	(0.03)	(0.03)			(0.13)	(0.13)
Treatment x Post	-0.05	-0.11**	-0.16**	-0.15**		
	(0.05)	(0.04)	(0.07)	(0.07)		
Year 2012			0.23***	0.24***	0.25***	0.25***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2014			0.46***	0.47***	0.49***	0.50***
			(0.05)	(0.05)	(0.06)	(0.06)
Year 2016			0.66***	0.69***	0.72***	0.74***
			(0.07)	(0.07)	(0.07)	(0.07)
Year 2018			0.67***	0.70***	0.69***	0.72***
			(0.05)	(0.05)	(0.05)	(0.05)
Year 2020			0.67***	0.69***	0.70***	0.72***
			(0.05)	(0.05)	(0.06)	(0.06)
Year 2022			0.75***	0.78***	0.77***	0.79***
			(0.06)	(0.06)	(0.06)	(0.06)
Treatment x Year 2012					-0.08	-0.06
					(0.12)	(0.12)
Treatment x Year 2014					-0.27**	-0.26**
					(0.12)	(0.12)
Treatment x Year 2016					-0.58***	-0.54***
					(0.18)	(0.18)
Treatment x Year 2018					-0.29**	-0.26*
					(0.14)	(0.14)

Treatment x Year 2020					-0.36**	-0.33**
					(0.15)	(0.15)
Treatment x Year 2022					-0.19	-0.16
					(0.17)	(0.16)
HH Rep Female		0.03*				
		(0.02)				
HH Rep Age		-0.00		0.01		0.01
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		-0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.16***		0.16		0.16*
		(0.03)		(0.10)		(0.10)
HH Rep Years of Education		0.04***		0.01		0.01
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.26***		0.03		0.04
		(0.02)		(0.08)		(0.08)
Family Size		0.09***		0.10***		0.10***
		(0.01)		(0.02)		(0.02)
Constant	3.34***	2.63***	3.20***	2.53***	3.17***	2.50***
	(0.01)	(0.11)	(0.03)	(0.31)	(0.04)	(0.31)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	8,945	8,945	8,945	8,945	8,945	8,945

Note. Treatment = strict definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household expenditure

values only. * p<0.10, ** p<0.05, *** p<0.01.

Broad Treatment Definition

Appendix Table D5. Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.54***	0.58***				
	(0.02)	(0.02)				
Treatment	0.10***	0.11***			0.03	0.05
	(0.03)	(0.02)			(0.09)	(0.08)
Treatment x Post	-0.08**	-0.05*	-0.02	-0.02		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.29***	0.22**	0.25***	0.18*
			(0.04)	(0.10)	(0.05)	(0.10)
Year 2014			0.39***	0.25	0.40***	0.27
			(0.05)	(0.19)	(0.05)	(0.19)
Year 2016			0.44***	0.24	0.42***	0.22
			(0.08)	(0.28)	(0.10)	(0.28)
Year 2018			0.64***	0.38	0.62***	0.37
			(0.05)	(0.37)	(0.05)	(0.37)
Year 2020			0.71***	0.38	0.69***	0.37
			(0.05)	(0.46)	(0.05)	(0.46)

Year 2022		0.86***	0.46	0.85***	0.47
		(0.05)	(0.55)	(0.05)	(0.55)
Treatment x Year 2012				0.14*	0.14*
				(0.08)	(0.08)
Treatment x Year 2014				-0.05	-0.06
				(0.09)	(0.08)
Treatment x Year 2016				0.06	0.05
				(0.15)	(0.14)
Treatment x Year 2018				0.03	0.02
				(0.09)	(0.08)
Treatment x Year 2020				0.03	0.02
				(0.09)	(0.09)
Treatment x Year 2022				0.01	-0.01
				(0.09)	(0.09)
Female	-0.41***				
	(0.02)				
Age	0.06***		0.13***		0.13***
	(0.01)		(0.05)		(0.05)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.26***		-0.02		-0.02
	(0.02)		(0.07)		(0.07)
Years of Education	0.04***		0.02*		0.02*
	(0.00)		(0.01)		(0.01)
Urban Dummy	0.12***		0.06		0.06
	(0.02)		(0.06)		(0.06)

Constant	2.73***	1.29***	2.63***	-0.55	2.62***	-0.56
	(0.02)	(0.10)	(0.03)	(1.62)	(0.04)	(1.62)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	14,140	14,140	14,140	14,140	14,140	14,140

Note. Treatment = broad definition; control = industrial-related control. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D6. Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.48***	0.51***				
	(0.02)	(0.02)				
Treatment	0.11***	0.11***			0.01	0.03
	(0.03)	(0.02)			(0.09)	(0.09)
Treatment x Post	-0.08**	-0.06**	-0.06	-0.06		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.29***	0.17	0.25***	0.13
			(0.04)	(0.11)	(0.05)	(0.12)
Year 2014			0.38***	0.18	0.39***	0.19
			(0.05)	(0.23)	(0.06)	(0.24)
Year 2016			-0.06	-0.37	-0.12	-0.43
			(0.14)	(0.36)	(0.17)	(0.37)

Year 2018		0.59***	0.16	0.57***	0.13
		(0.05)	(0.44)	(0.05)	(0.45)
Year 2020		0.67***	0.13	0.64***	0.09
		(0.05)	(0.55)	(0.05)	(0.56)
Year 2022		0.81***	0.17	0.81***	0.16
		(0.05)	(0.66)	(0.06)	(0.66)
Treatment x Year 2012				0.12	0.12
				(0.08)	(0.08)
Treatment x Year 2014				-0.09	-0.10
				(0.09)	(0.09)
Treatment x Year 2016				0.13	0.11
				(0.28)	(0.28)
Treatment x Year 2018				0.00	-0.00
				(0.09)	(0.09)
Treatment x Year 2020				0.03	0.01
				(0.09)	(0.09)
Treatment x Year 2022				-0.06	-0.08
				(0.09)	(0.09)
Female	-0.40***				
	(0.02)				
Age	0.06***		0.15**		0.15***
	(0.01)		(0.06)		(0.06)
Age squared	-0.00***		-0.00***		-0.00***
	(0.00)		(0.00)		(0.00)
Married	0.28***		0.02		0.02
	(0.02)		(0.07)		(0.07)

Years of Education		0.04***		0.01		0.01
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.14***		0.06		0.06
		(0.02)		(0.06)		(0.06)
Constant	2.72***	1.11***	2.62***	-1.27	2.62***	-1.31
	(0.02)	(0.11)	(0.03)	(1.97)	(0.04)	(1.97)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	13,573	13,573	13,573	13,573	13,573	13,573

Note. Treatment = broad definition; control = industrial-related control. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive wage income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D7. Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.61***	0.57***				
	(0.02)	(0.02)				
Treatment	0.02	0.03			0.05	0.04
	(0.03)	(0.03)			(0.07)	(0.07)
Treatment x Post	0.01	0.00	0.05	0.04		
	(0.03)	(0.03)	(0.04)	(0.04)		
Year 2012			0.31***	0.31***	0.30***	0.29***
			(0.04)	(0.04)	(0.05)	(0.05)

Year 2014		0.44***	0.44***	0.47***	0.46***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2016		0.66***	0.66***	0.69***	0.69***
		(0.05)	(0.05)	(0.06)	(0.06)
Year 2018		0.84***	0.84***	0.85***	0.85***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2020		0.92***	0.91***	0.91***	0.90***
		(0.04)	(0.04)	(0.05)	(0.05)
Year 2022		0.99***	0.98***	0.98***	0.97***
		(0.04)	(0.04)	(0.05)	(0.05)
Treatment x Year 2012				0.05	0.06
				(0.09)	(0.08)
Treatment x Year 2014				-0.02	-0.02
				(0.08)	(0.08)
Treatment x Year 2016				-0.06	-0.03
				(0.10)	(0.10)
Treatment x Year 2018				0.03	0.03
				(0.08)	(0.07)
Treatment x Year 2020				0.07	0.07
				(0.08)	(0.08)
Treatment x Year 2022				0.07	0.06
				(0.08)	(0.08)
HH Rep Female	0.02*				
	(0.01)				
HH Rep Age	0.00		0.01		0.01
	(0.00)		(0.01)		(0.01)

HH Rep Age squared		0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.13***		0.05		0.05
		(0.02)		(0.06)		(0.06)
HH Rep Years of Education		0.04***		0.00		0.00
		(0.00)		(0.00)		(0.00)
Urban Dummy		0.15***		0.08		0.08
		(0.01)		(0.06)		(0.06)
Family Size		0.10***		0.13***		0.13***
		(0.00)		(0.01)		(0.01)
Constant	3.47***	2.39***	3.29***	2.54***	3.28***	2.53***
	(0.02)	(0.09)	(0.03)	(0.23)	(0.04)	(0.23)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	11,523	11,523	11,523	11,523	11,523	11,523

Note. Treatment = broad definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table D8. Regression Results for Log Family Expenditure

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (≥2014)	0.52***	0.48***				
	(0.02)	(0.02)				

Treatment	0.03	0.04			0.16**	0.16**
	(0.03)	(0.02)			(0.07)	(0.07)
Treatment x Post	-0.05*	-0.07**	-0.04	-0.05		
	(0.03)	(0.03)	(0.04)	(0.04)		
Year 2012			0.21***	0.21***	0.24***	0.24***
			(0.04)	(0.04)	(0.05)	(0.04)
Year 2014			0.43***	0.44***	0.47***	0.48***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2016			0.61***	0.64***	0.73***	0.75***
			(0.06)	(0.06)	(0.07)	(0.07)
Year 2018			0.66***	0.69***	0.69***	0.72***
			(0.04)	(0.04)	(0.05)	(0.05)
Year 2020			0.65***	0.67***	0.70***	0.72***
			(0.05)	(0.05)	(0.05)	(0.05)
Year 2022			0.74***	0.76***	0.76***	0.79***
			(0.05)	(0.05)	(0.06)	(0.06)
Treatment x Year 2012					-0.08	-0.07
					(0.08)	(0.08)
Treatment x Year 2014					-0.16*	-0.16**
					(0.08)	(0.08)
Treatment x Year 2016					-0.38***	-0.35***
					(0.11)	(0.11)
Treatment x Year 2018					-0.13	-0.13
					(0.08)	(0.08)
Treatment x Year 2020					-0.16*	-0.17*
					(0.09)	(0.09)

Treatment x Year 2022					-0.11	-0.12
					(0.09)	(0.09)
HH Rep Female		0.03**				
		(0.01)				
HH Rep Age		0.00		0.00		0.00
		(0.00)		(0.01)		(0.01)
HH Rep Age squared		-0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.17***		0.08		0.09
		(0.03)		(0.08)		(0.08)
HH Rep Years of Education		0.04***		0.01**		0.01**
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.24***		0.06		0.06
		(0.02)		(0.06)		(0.06)
Family Size		0.09***		0.10***		0.10***
		(0.00)		(0.02)		(0.02)
Constant	3.33***	2.54***	3.23***	2.71***	3.17***	2.66***
	(0.01)	(0.09)	(0.03)	(0.25)	(0.04)	(0.25)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	11,523	11,523	11,523	11,523	11,523	11,523

Note. Treatment = broad definition at the household level; control = industrial-related control. A household is treated if at least one household member is classified as treated under the corresponding fixed individual-level treatment definition. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include household fixed effects. HH rep female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive household expenditure values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix E.

Full Regression Results for the Matching-Based Analysis: Main Treatment Definition and Industrial-Related Control Group

Appendix Table E1. Matching-Based Regression Results for Log Individual Income

	Pooled DID		Panel DID FE		Event-study FE	
	No controls	Controls	No controls	Controls	No controls	Controls
Post (≥2014)	0.54***	0.58***				
	(0.02)	(0.02)				
Treatment	0.10***	0.11***			0.03	0.05
	(0.03)	(0.02)			(0.09)	(0.08)
Treatment x Post	-0.08**	-0.05*	-0.02	-0.02		
	(0.03)	(0.03)	(0.05)	(0.05)		
Year 2012			0.29***	0.22**	0.25***	0.18*
			(0.04)	(0.10)	(0.05)	(0.10)
Year 2014			0.39***	0.25	0.40***	0.27
			(0.05)	(0.19)	(0.05)	(0.19)
Year 2016			0.44***	0.24	0.42***	0.22
			(0.08)	(0.28)	(0.10)	(0.28)
Year 2018			0.64***	0.38	0.62***	0.37
			(0.05)	(0.37)	(0.05)	(0.37)
Year 2020			0.71***	0.38	0.69***	0.37
			(0.05)	(0.46)	(0.05)	(0.46)
Year 2022			0.86***	0.46	0.85***	0.47
			(0.05)	(0.55)	(0.05)	(0.55)

Treatment x Year 2012					0.14*	0.14*
					(0.08)	(0.08)
Treatment x Year 2014					-0.05	-0.06
					(0.09)	(0.08)
Treatment x Year 2016					0.06	0.05
					(0.15)	(0.14)
Treatment x Year 2018					0.03	0.02
					(0.09)	(0.08)
Treatment x Year 2020					0.03	0.02
					(0.09)	(0.09)
Treatment x Year 2022					0.01	-0.01
					(0.09)	(0.09)
Female		-0.41***				
		(0.02)				
Age		0.06***		0.13***		0.13***
		(0.01)		(0.05)		(0.05)
Age squared		-0.00***		-0.00***		-0.00***
		(0.00)		(0.00)		(0.00)
Married		0.26***		-0.02		-0.02
		(0.02)		(0.07)		(0.07)
Years of Education		0.04***		0.02*		0.02*
		(0.00)		(0.01)		(0.01)
Urban Dummy		0.12***		0.06		0.06
		(0.02)		(0.06)		(0.06)
Constant	2.73***	1.29***	2.63***	-0.55	2.62***	-0.56
	(0.02)	(0.10)	(0.03)	(1.62)	(0.04)	(1.62)

Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	14,140	14,140	14,140	14,140	14,140	14,140

Note. Treatment = broad definition; control = industrial-related control. Base year = 2010. Post = 1(year ≥ 2014). Standard errors clustered at the household level (fid) are in parentheses. Columns (3)-(6) include individual fixed effects. Female is not included in FE specifications because it is time-invariant. All columns use the same estimation sample. The log outcome is defined for positive income values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table E2. Matching-Based Regression Results for Log Wage Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (>=2014)	0.38***	0.46***				
	(0.05)	(0.05)				
Treatment	0.00	-0.00				
	(0.05)	(0.05)				
Treatment x Post	-0.08	-0.09	-0.14*	-0.14*		
	(0.07)	(0.07)	(0.07)	(0.07)		
Year 2012			0.39***	0.47**	0.40***	0.49**
			(0.04)	(0.21)	(0.06)	(0.21)
Year 2014			0.46***	0.67	0.47***	0.70*
			(0.06)	(0.42)	(0.08)	(0.41)
Year 2016			-0.00	0.29	0.18	0.50
			(0.17)	(0.58)	(0.31)	(0.54)
Year 2018			0.60***	0.92	0.56***	0.94
			(0.06)	(0.84)	(0.08)	(0.82)

Year 2020		0.59***	0.98	0.59***	1.06
		(0.08)	(1.06)	(0.09)	(1.04)
Year 2022		0.70***	1.20	0.75***	1.35
		(0.08)	(1.28)	(0.10)	(1.27)
Treatment x Year 2012				-0.01	-0.01
				(0.08)	(0.08)
Treatment x Year 2014				-0.16	-0.16
				(0.10)	(0.10)
Treatment x Year 2016				-0.48	-0.44
				(0.34)	(0.34)
Treatment x Year 2018				-0.06	-0.06
				(0.10)	(0.10)
Treatment x Year 2020				-0.14	-0.14
				(0.15)	(0.15)
Treatment x Year 2022				-0.23	-0.24
				(0.15)	(0.15)
Female	-0.22**				
	(0.09)				
Age	0.05***		0.03		0.02
	(0.01)		(0.11)		(0.10)
Age squared	-0.00***		-0.00**		-0.00**
	(0.00)		(0.00)		(0.00)
Married	0.24***		0.06		0.06
	(0.05)		(0.10)		(0.10)
Years of Education	0.03***		0.02		0.02
	(0.01)		(0.02)		(0.02)

Urban Dummy		0.07		0.04		0.04
		(0.05)		(0.11)		(0.11)
Constant	2.94***	1.55***	2.77***	2.80	2.77***	3.11
	(0.04)	(0.28)	(0.03)	(4.07)	(0.03)	(3.98)
Individual FE	No	No	Yes	Yes	Yes	Yes
Observations	1,726	1,726	1,726	1,726	1,726	1,726

Note. Treatment uses the main treatment definition and the control group is the industrial-related control group. Results are estimated on the propensity-score-matched individual sample. Matching uses 1:1 nearest-neighbour matching without replacement, a logit propensity score, common support, and a caliper of 0.05. Treatment status is fixed using the latest observed pre-policy classification (2012, or 2010 where 2012 is unavailable). Consequently, the standalone Treatment indicator is time-invariant and is absorbed by individual fixed effects in columns (3)-(6). The base year is 2010 and Post = 1(year>=2014). Standard errors clustered at the household level (fid) are reported in parentheses. Female is not included in FE specifications because it is time-invariant. All columns use the same outcome-specific estimation sample. Log outcomes are defined for positive monetary values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table E3. Matching-Based Regression Results for Log Family Income

	Pooled DID		**Panel DID FE**		**Event-study FE**	
	No controls	**Controls**	**No controls**	**Controls**	**No controls**	**Controls**
Post (>=2014)	0.65***	0.58***				
	(0.05)	(0.05)				
Treatment	-0.00	-0.02				
	(0.06)	(0.06)				
Treatment x Post	-0.04	-0.07	-0.13*	-0.13*		
	(0.07)	(0.06)	(0.07)	(0.07)		
Year 2012			0.34***	0.33***	0.35***	0.35***
			(0.05)	(0.05)	(0.07)	(0.07)

Year 2014		0.57***	0.56***	0.58***	0.57***
		(0.07)	(0.07)	(0.08)	(0.08)
Year 2016		0.76***	0.76***	0.82***	0.82***
		(0.08)	(0.08)	(0.10)	(0.10)
Year 2018		0.87***	0.90***	0.84***	0.88***
		(0.06)	(0.06)	(0.08)	(0.08)
Year 2020		1.03***	1.05***	1.03***	1.04***
		(0.07)	(0.07)	(0.08)	(0.08)
Year 2022		1.09***	1.16***	1.10***	1.17***
		(0.08)	(0.08)	(0.09)	(0.09)
Treatment x Year 2012				-0.03	-0.03
				(0.10)	(0.10)
Treatment x Year 2014				-0.15	-0.14
				(0.11)	(0.10)
Treatment x Year 2016				-0.25*	-0.24*
				(0.13)	(0.13)
Treatment x Year 2018				-0.08	-0.09
				(0.10)	(0.10)
Treatment x Year 2020				-0.13	-0.11
				(0.12)	(0.11)
Treatment x Year 2022				-0.15	-0.16
				(0.13)	(0.12)
HH Rep Female	0.07*				
	(0.04)				
HH Rep Age	-0.02**		-0.01		-0.01
	(0.01)		(0.01)		(0.01)

HH Rep Age squared		0.00**		0.00		0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.15*		0.01		0.01
		(0.09)		(0.11)		(0.11)
HH Rep Years of Education		0.03***		-0.00		-0.00
		(0.01)		(0.01)		(0.01)
Urban Dummy		0.19***		-0.11		-0.11
		(0.05)		(0.14)		(0.14)
Family Size		0.12***		0.14***		0.14***
		(0.02)		(0.03)		(0.03)
Constant	3.44***	2.90***	3.29***	3.10***	3.29***	3.12***
	(0.04)	(0.30)	(0.03)	(0.32)	(0.03)	(0.32)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	1,567	1,567	1,567	1,567	1,567	1,567

Note. Treatment uses the main treatment definition and the control group is the industrial-related control group. Results are estimated on the propensity-score-matched household sample. Matching uses 1:1 nearest-neighbour matching without replacement, a logit propensity score, common support, and a caliper of 0.03. Treatment status is fixed using the latest observed pre-policy classification (2012, or 2010 where 2012 is unavailable). Consequently, the standalone Treatment indicator is time-invariant and is absorbed by household fixed effects in columns (3)-(6). The base year is 2010 and Post = 1(year>=2014). Standard errors clustered at the household level (fid) are reported in parentheses. HH Rep Female is not included in FE specifications. Some residual imbalance remains in family size after matching; family size is retained as a control in controlled household-level regressions. All columns use the same outcome-specific estimation sample. Log outcomes are defined for positive monetary values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table E4. Matching-Based Regression Results for Log Family Expenditure

	Pooled DID	**Panel DID FE**	**Event-study FE**

	No controls	Controls	No controls	Controls	No controls	Controls
Post (>=2014)	0.51***	0.49***				
	(0.05)	(0.05)				
Treatment	-0.00	-0.01				
	(0.06)	(0.06)				
Treatment x Post	-0.09	-0.13**	-0.20***	-0.21***		
	(0.07)	(0.07)	(0.07)	(0.07)		
Year 2012			0.17***	0.18***	0.18***	0.18***
			(0.04)	(0.04)	(0.06)	(0.06)
Year 2014			0.51***	0.52***	0.51***	0.52***
			(0.07)	(0.07)	(0.08)	(0.08)
Year 2016			0.66***	0.68***	0.66***	0.67***
			(0.07)	(0.07)	(0.09)	(0.09)
Year 2018			0.66***	0.70***	0.69***	0.74***
			(0.07)	(0.07)	(0.09)	(0.08)
Year 2020			0.72***	0.75***	0.76***	0.79***
			(0.07)	(0.07)	(0.09)	(0.09)
Year 2022			0.70***	0.77***	0.64***	0.72***
			(0.08)	(0.08)	(0.11)	(0.10)
Treatment x Year 2012					-0.02	-0.02
					(0.09)	(0.09)
Treatment x Year 2014					-0.19*	-0.20*
					(0.11)	(0.11)
Treatment x Year 2016					-0.19	-0.19
					(0.12)	(0.12)

Treatment x Year 2018					-0.27**	-0.29**
					(0.12)	(0.11)
Treatment x Year 2020					-0.28**	-0.28**
					(0.13)	(0.13)
Treatment x Year 2022					-0.09	-0.11
					(0.15)	(0.14)
HH Rep Female		0.05				
		(0.04)				
HH Rep Age		-0.01		0.00		0.00
		(0.01)		(0.02)		(0.02)
HH Rep Age squared		0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)
HH Rep Married		0.11		0.03		0.03
		(0.09)		(0.13)		(0.13)
HH Rep Years of Education		0.03***		0.00		0.00
		(0.01)		(0.01)		(0.01)
Urban Dummy		0.24***		0.04		0.04
		(0.06)		(0.12)		(0.12)
Family Size		0.11***		0.11***		0.11***
		(0.02)		(0.03)		(0.03)
Constant	3.39***	2.78***	3.31***	2.98***	3.31***	2.97***
	(0.04)	(0.34)	(0.03)	(0.43)	(0.03)	(0.43)
Household FE	No	No	Yes	Yes	Yes	Yes
Observations	1,567	1,567	1,567	1,567	1,567	1,567

Note. Treatment uses the main treatment definition and the control group is the industrial-related control group. Results are estimated on the propensity-score-matched household sample. Matching uses 1:1 nearest-neighbour matching without replacement, a logit propensity score, common

support, and a caliper of 0.03. Treatment status is fixed using the latest observed pre-policy classification (2012, or 2010 where 2012 is unavailable). Consequently, the standalone Treatment indicator is time-invariant and is absorbed by household fixed effects in columns (3)-(6). The base year is 2010 and Post = 1(year>=2014). Standard errors clustered at the household level (fid) are reported in parentheses. HH Rep Female is not included in FE specifications. Some residual imbalance remains in family size after matching; family size is retained as a control in controlled household-level regressions. All columns use the same outcome-specific estimation sample. Log outcomes are defined for positive monetary values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix F.

Additional Estimates under the Alternative Policy Timing Specification

Appendix Table F1. Alternative Policy Timing: Individual-Level Outcomes

Variable	Log Individual Income		Log Wage Income	
	FE-DID	Event Study	FE-DID	Event Study
Treatment x Post (Post >=2016)	0.00		-0.05	
	(0.05)		(0.06)	
Event-study coefficients				
Treatment x Year 2012		0.18**		0.16**
		(0.07)		(0.08)
Treatment x Year 2014		-0.03		-0.06
		(0.08)		(0.08)
Treatment x Year 2016		0.06		0.05
		(0.14)		(0.31)
Treatment x Year 2018		0.03		-0.03
		(0.07)		(0.08)
Treatment x Year 2020		0.04		0.01
		(0.08)		(0.08)
Treatment x Year 2022		0.01		-0.08
		(0.07)		(0.09)
Fixed effects	Individual FE	Individual FE	Individual FE	Individual FE
Controls	Yes	Yes	Yes	Yes
Clustered SE	Household (fid)	Household (fid)	Household (fid)	Household (fid)

Observations	13,119	13,119	12,607	12,607

Note. Treatment uses the main treatment definition and the control group is the industrial-related control group. The alternative timing specification defines Post as one for the 2016, 2018, 2020, and 2022 survey waves; the 2014 wave is retained in the estimation sample but coded as outside the post-intensive-implementation period. Both FE-DID and event-study specifications include controls, individual fixed effects, and year fixed effects. The omitted event-study base year is 2010. Standard errors clustered at the household level (fid) are reported in parentheses. Female is excluded from the fixed-effects specifications because it is time-invariant. Log outcomes are defined for positive monetary values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table F2. Alternative Policy Timing: Household-Level Outcomes

Variable	Log Family Income		Log Family Expenditure	
	FE-DID	**Event Study**	**FE-DID**	**Event Study**
Treatment x Post (Post >=2016)	0.06		-0.04	
	(0.04)		(0.05)	
Event-study coefficients				
Treatment x Year 2012		0.10		0.01
		(0.07)		(0.07)
Treatment x Year 2014		-0.02		-0.06
		(0.06)		(0.07)
Treatment x Year 2016		-0.00		-0.20*
		(0.09)		(0.10)
Treatment x Year 2018		0.05		-0.03
		(0.05)		(0.07)
Treatment x Year 2020		0.09		-0.05
		(0.06)		(0.07)

Treatment x Year 2022		0.10*		0.01
		(0.06)		(0.08)
Fixed effects	Household FE	Household FE	Household FE	Household FE
Controls	Yes	Yes	Yes	Yes
Clustered SE	Household (fid)	Household (fid)	Household (fid)	Household (fid)
Observations	10,784	10,784	10,784	10,784

Note. Treatment uses the main treatment definition and the control group is the industrial-related control group. The alternative timing specification defines Post as one for the 2016, 2018, 2020, and 2022 survey waves; the 2014 wave is retained in the estimation sample but coded as outside the post-intensive-implementation period. Both FE-DID and event-study specifications include controls, household fixed effects, and year fixed effects. The omitted event-study base year is 2010. Standard errors clustered at the household level (fid) are reported in parentheses. HH Rep Female is excluded from the fixed-effects specifications. Log outcomes are defined for positive monetary values only. * p<0.10, ** p<0.05, *** p<0.01.

Appendix G.

Additional Estimates Using Untrimmed Log Outcomes

Appendix Table G1. Untrimmed Log Outcomes: Individual-Level Estimates

Variable	Log Individual Income		Log Wage Income	
	FE-DID	Event Study	FE-DID	Event Study
Treatment x Post	-0.03		-0.08	
	(0.05)		(0.06)	
Event-study coefficients				
Treatment x Year 2012		0.14*		0.11
		(0.08)		(0.08)
Treatment x Year 2014		-0.05		-0.08
		(0.09)		(0.09)
Treatment x Year 2016		-0.01		-0.08
		(0.15)		(0.30)
Treatment x Year 2018		-0.00		-0.05
		(0.09)		(0.09)
Treatment x Year 2020		0.02		-0.01
		(0.09)		(0.10)
Treatment x Year 2022		-0.01		-0.09
		(0.09)		(0.10)
Fixed effects	Individual FE	Individual FE	Individual FE	Individual FE
Controls	Yes	Yes	Yes	Yes
Clustered SE	Household (fid)	Household (fid)	Household (fid)	Household (fid)

Observations	13,335	13,335	12,821	12,821

Note. This table reports estimates using untrimmed log outcomes under the baseline treatment-control framework. Treatment uses the main treatment definition, and the control group is the industrial-related control group. Monetary outcomes retain all positive values prior to logarithmic transformation rather than applying the baseline year-specific 1%-99% trimming procedure. Post equals one for the 2014, 2016, 2018, 2020, and 2022 survey waves. Both FE-DID and event-study specifications include individual fixed effects, year fixed effects, and controls. The omitted event-study base year is 2010. Standard errors clustered at the household level (fid) are reported in parentheses. Female is excluded from the fixed-effects regressions because it is time-invariant. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table G2. Untrimmed Log Outcomes: Household-Level Estimates

Variable	Log Family Income		Log Family Expenditure	
	FE-DID	**Event Study**	**FE-DID**	**Event Study**
Treatment x Post	0.02		-0.04	
	(0.04)		(0.05)	
Event-study coefficients				
Treatment x Year 2012		0.04		-0.09
		(0.09)		(0.09)
Treatment x Year 2014		-0.11		-0.12
		(0.09)		(0.09)
Treatment x Year 2016		-0.10		-0.25**
		(0.11)		(0.13)
Treatment x Year 2018		-0.02		-0.15
		(0.08)		(0.09)

Treatment x Year 2020		0.01		-0.17*
		(0.09)		(0.10)
Treatment x Year 2022		-0.00		-0.12
		(0.09)		(0.10)
Fixed effects	Household FE	Household FE	Household FE	Household FE
Controls	Yes	Yes	Yes	Yes
Clustered SE	Household (fid)	Household (fid)	Household (fid)	Household (fid)
Observations	11,096	11,096	11,097	11,097

Note. This table reports estimates using untrimmed log outcomes under the baseline treatment-control framework. Treatment uses the main treatment definition, and the control group is the industrial-related control group. Monetary outcomes retain all positive values prior to logarithmic transformation rather than applying the baseline year-specific 1%-99% trimming procedure. Post equals one for the 2014, 2016, 2018, 2020, and 2022 survey waves. Both FE-DID and event-study specifications include household fixed effects, year fixed effects, and controls. The omitted event-study base year is 2010. Standard errors clustered at the household level (fid) are reported in parentheses. HH Rep Female is excluded from the fixed-effects regressions because it is time-invariant. * p<0.10, ** p<0.05, *** p<0.01.