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【要旨】

本稿は、2010 年代に日本で実施された一連の所得税改革が、等価世帯可処分所得の分布および所得税制の再分配機能に与えた影響を検証する。分析には、日本家計パネル調査（JHPS/KHPS）の 2009-2021 年調査の個票データを用いる。所得税額および社会保険料をマイクロシミュレーションにより推計し、等価世帯可処分所得を構築したうえで、加重差の差法（weighted DID）、条件付き分位点 DID、および無条件分位点 DID を用いて、平均的効果と分布上の異質的効果を推定する。さらに、イベントスタディ分析を通じて、平行トレンド仮定の妥当性を補足的に検証する。 分析の結果、所得税改革の再分配効果は、各改革の制度設計および対象となる所得階層に応じて体系的に異なることが明らかとなった。2011 年および 2018 年の給付拡充・税負担軽減型の改革は、主として低・中所得層の等価世帯可処分所得を増加させた。一方、2013 年、2016 年、2017 年および 2020 年に実施された所得控除の縮小・上限設定を伴う改革は、高所得層の可処分所得を低下させた。2014 年の上場株式等の配当・譲渡所得に対する軽減税率の廃止は、中・高所得層を中心に可処分所得を増加させたものの、所得分布全体に与える影響は限定的であった。特に、基礎控除を引き上げる一方で給与所得控除および公的年金等控除を見直した 2020 年改革は、高所得層の可処分所得を低下させるとともに、低所得層では小幅な増加をもたらし、所得分布を圧縮する効果を示した。以上より、2010 年代の日本の所得税改革は、控除縮小と給付拡充の組合せを通じて、所得税制の累進性を強化したことが示唆される。

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Quantile Analysis of Redistributive Effects of Personal Income Tax Reforms: Evidence from Japan in the 2010s*

Takero Doi†, Jeonyong Park‡

Abstract

This paper examines how a sequence of personal income tax reforms implemented in Japan during the 2010s affected the distribution of equivalent household disposable income and the redistributive role of the tax system. Using microdata from the 2009–2021 waves of the Japan Household Panel Survey (JHPS/KHPS), we analyze personal income taxes and social security contributions to construct equivalent household disposable income by a microsimulation and estimate average and distributional effects using weighted difference-in-differences, together with conditional and unconditional quantile difference-in-differences. We complement these estimates with an event-study analysis to assess the plausibility of the parallel-trends assumption.

We find that redistributive effects vary systematically with policy design and the targeted income range. Benefit expansions and tax-relief reforms in 2011 and 2018 increased equivalent household disposable income among lower- and middle-income households, whereas reforms that reduced or capped deductions in 2013, 2016, 2017, and 2020 lowered disposable income at the top. The 2014 abolition of the reduced tax rate on dividends and capital gains increased disposable income mainly among middle- and upper-income groups and did not materially alter the overall distribution. The 2020 reform—which raised the Basic Exemption while reducing the Deduction for Salary Income and the Deduction for Public and Other Pensions—reduced disposable income at the top and modestly increased it at the bottom, compressing the income distribution. Overall, the combination of deduction cuts and benefit expansions strengthened the progressivity of Japan's personal income tax system.

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Declarations

The authors declare that they have no actual or potential conflicts of interest related to this research. The present study was conducted without the necessity of obtaining ethical approval.

1. Introduction

During the 2010s, Japan implemented a series of personal income tax reforms motivated by multiple policy considerations. These included an effort under the Democratic Party of Japan (DPJ) administration to shift from deduction-based relief toward benefit-type measures, subsequent restrictions on deductions for high-income taxpayers, revisions to exemptions intended to encourage female labor force participation amid the rise of dual-earner households, and the redesign of deductions that are not tied to income source in response to the expansion of non-regular employment and greater labor market mobility. Whether the resulting institutional changes were fully consistent with these stated motivations is, of course, an open question.

Against this background, the Government Tax Commission's Interim Report on the Tax System in Light of Structural Changes in the Economy and Society (November 2016) articulated three broad directions for personal income tax reform: (i) constructing a tax system that is neutral with respect to individuals' choices of work style; (ii) restoring the income-redistribution function by revisiting deduction-based design, under which tax relief tends to be larger for higher-income taxpayers; and (iii) restructuring deductions so that they better accommodate diverse working arrangements.

Accordingly, the reforms implemented during the 2010s explicitly treated the restoration of the income-redistribution function as a central policy objective. Nevertheless, systematic empirical evidence on whether—and to what extent—this sequence of reforms altered the income distribution, and the redistributive capacity of the tax system remains limited.

This paper provides a quantitative assessment of how Japan's personal income tax reforms during the 2010s affected household disposable income across the income distribution and examines their implications for redistribution and income inequality in an integrated framework.[1] Although some reforms were not primarily designed to enhance redistribution, they may have had nontrivial distributional consequences; we aim to capture these effects as well.

Our focus is therefore not only on average effects—changes in mean income—but on two complementary distributional dimensions. First, we examine how policy effects

[1] Doi (2023) also analyzes the income redistribution effects of Japan's personal income tax reforms in the 2010s, using changes in the Gini coefficient as the main metric. In contrast, we employ quantile regression methods to investigate heterogeneity in the effects across different parts of the income distribution.

vary across segments of the income distribution. Second, we assess how the structure of the income distribution itself changes in response to the reforms. To this end, we combine conditional quantile regression (CQR) and unconditional quantile regression (UQR). CQR traces reform effects at different points of the conditional distribution of equivalent household disposable income, while UQR captures shifts in the unconditional distribution. This complementary use of CQR and UQR allows for a more nuanced assessment of how the redistributive role of Japan's personal income tax system evolved over the 2010s reforms.

2. Background

2.1. Personal Income Tax Reforms in Japan in the 2010s

The personal income tax reforms in the 2010s analyzed in this paper are summarized in Table 1. For each reform, we define the "policy year" based on the income year to which the revised rules applied (i.e., the implementation year in terms of income accrual), rather than the year of legislative enactment. An important exception concerns the 2010 legislative changes associated with the introduction of the Child Allowance (Kodomo Teate): the reduction in the Exemption for Specified Dependents (from 630,000 yen to 380,000 yen per dependent aged 16–18) and the abolition of the Exemption for Dependents up to age 15. Because these provisions applied to income earned from 2011 onward, we treat them as part of the 2011 tax reform in the empirical analysis. Throughout the paper, the years reported in the tables therefore refer to the implementation (income-earning) year used in the analysis, not the statutory year of introduction.

Table 1. Personal income tax reforms in Japan in the 2010s

Year	Reforms
2011	Introducing Child Allowance (Kodomo Teate): paid to children from 0 years old until the age of finishing junior high school without income limitation Abolishing income exemption for dependents up to 15 years old (380 thousand yen per dependent) and the additional part of income exemption for the specified dependents between 16 and 18 years old (250 thousand yen per dependent)
2013	Setting the maximum for Deduction for Salary Income (salary earning over 15 million yen: deduction upper limit 2.45 million yen) Introducing the special personal income tax for reconstruction (2.1% of income tax), the special inhabitant tax on individuals for reconstruction (1000 yen per person) Abolishing 10% reduction of the tax amount of the inhabitant tax on individuals on retirement incomes
2014	Abolishing reduced tax rate in capital gains taxation (Increasing tax rate from 10% to 20%)
2015	Increasing the maximum tax rate (to 45% at over 40 million yen)
2016	Reducing the maximum of Deduction for Salary Income (Decreasing the deduction upper limit to 2.3 million yen for over 12 million yen of salary earning)
2017	Reducing the maximum of Deduction for Salary Income (Decreasing the deduction upper limit to 2.2 million yen for over 10 million yen of salary earning)
2018	Revision of Exemption for Spouses and Special Exemption for Spouses
2020	Revising of Basic Exemption , Deduction for Salary Income, and Deduction for Public and Other Pensions Establishing of Income Adjustment Deduction Revising Exemption for Widows or Widowers Introducing Exemption for Single Parents

Notes: Years refer to the implementation (income-earning) year used in the analysis, not the statutory year of introduction.
Source: Authors' compilation.

2.2. Related Literature

A substantial empirical literature—particularly for countries outside Japan—uses quantile regression methods to study the distributional effects of fiscal policies, including taxes and transfers. How these studies address endogeneity varies with the policy setting. In particular, the nature and severity of endogeneity concerns, and the associated identification strategies, differ across domains such as tax reforms, government spending, social benefits, minimum wages, and monetary policy.

Personal income tax reforms are typically enacted through institutional decisions by governments and legislatures and are often modeled as policy shocks that are largely independent of households' or taxpayers' contemporaneous behavior. Accordingly, many studies treat tax reforms as exogenous shocks and include policy indicators directly in regression specifications without instrumental variables (e.g., Kleven and Schultz, 2014; Mertens and Olea, 2018; Saez, 2010; Zidar, 2019). By contrast, in contexts where policy choices plausibly respond endogenously to macroeconomic conditions—such as government spending (Auerbach and Gorodnichenko, 2012; Blanchard and Perotti, 2002), minimum wages (Card and Krueger, 1994), and monetary policy (Romer and Romer, 2004)—the identification of exogenous shocks and the use of instrumental variables are widely viewed as essential.

In analyzing Japan's personal income tax reforms during the 2010s, this paper employs a quantile difference-in-differences (QR-DID) approach that combines quantile regression with a difference-in-differences design to identify heterogeneous effects across the income distribution. A closely related application is Yokoyama (2018), who studies Japan's 2004 reform of the Exemption for Spouses and examines its effects on the distribution of married women's working hours using a QR-DID framework.

A growing domestic literature evaluates Japan's 2010s personal income tax reforms. Using microdata from the Japan Household Panel Survey (JHPS/KHPS) and a microsimulation model, Doi (2021, 2023) quantifies the effects of the 2013 introduction of a cap on the Deduction for Salary Income, the 2014 abolition of the reduced tax rate on capital income (i.e., restoration of the statutory rate), the 2015 increase in the top marginal tax rate, the 2016 and 2017 reductions in the maximum Deduction for Salary Income, the 2018 revision of the Exemption for Spouses and Special Exemption for Spouses, and the 2020 reform combining a higher Basic Exemption with revised deductions, focusing on inequality measures such as the Gini coefficient. Ohno et al. (2021, 2022, 2025) use the National Survey of Family Income and Expenditure (NSFIE) and tax return data, together with Burman-type tax recalculation and contribution decomposition methods, to evaluate the relief and redistributive effects of various deductions. Hisanaga (2022) also uses JHPS/KHPS data to study distributional impacts while explicitly accounting for heterogeneity in income and wealth. Bessho (2018) analyzes the 2010 introduction of the Child Allowance and the 2011 abolition of the Exemption for Dependents up to age 15 using a discrete-choice labor supply model that endogenizes behavioral responses and evaluates effects on after-tax income and welfare.

Most of these studies, however, emphasize distributional summaries—such as tax burdens by income group or changes in the Gini coefficient—or rely on static evaluations based on tax recalculation and decomposition. Relatively little work simultaneously identifies causal effects at different quantiles (τ) of the income distribution and evaluates changes in households' relative positions in that distribution (i.e., changes in the redistribution structure itself). This paper addresses this gap by jointly applying four econometric models within a common design—Weighted Ordinary Least Squares Difference-in-Differences (W-OLS-DID), Two-Step Weighted Fixed Effects Difference-in-Differences (W-2SFE-DID), Weighted Conditional Quantile Regression Difference-in-Differences (W-CQR-DID), and Weighted Unconditional

Quantile Regression Difference-in-Differences (W-UQR-DID)—and by examining both average and distributional effects.

The combined use of mean-based models (OLS or fixed effects) with CQR and UQR to study distributional policy impacts is well established. Firpo, Fortin, and Lemieux (2009) develop the UQR framework and emphasize that OLS captures mean effects, whereas UQR characterizes effects on the distribution more broadly; using both provides a richer picture of policy impacts. Bitler et al. (2006), in their analysis of U.S. welfare reform (PRWORA), first estimate average treatment effects using OLS and then use CQR to examine effects at different quantiles, documenting substantial heterogeneity. They present OLS and CQR estimates side by side and treat OLS specifications without instrumental variables as their baseline, consistent with the approach in this paper. Chernozhukov *et al.* (2013) similarly combine OLS and UQR to study the distributional effects of minimum wage policies.

In the regression framework used in this study, the dependent variable is the logarithm of equivalent household disposable income, and the key explanatory variables include indicators for whether an observation is in the post-reform period (reform = 1) or the pre-reform period (reform = 0). This general strategy—using income-related outcomes (taxable income, disposable income, or their logarithms) and including tax rates or policy indicators as key regressors—is standard in empirical public finance (Blundell *et al.*, 1998; Feldstein, 1995; Gruber and Saez, 2002; Heim, 2009; Piketty *et al.*, 2014; Saez, 2010).

For example, Blundell et al. (1998) examine the effects of U.K. tax reforms on labor supply and income, using disposable income (and its logarithm) and hours worked as outcomes and including tax rates, reform indicators, and household characteristics as regressors. Feldstein (1995) studies the 1986 U.S. tax reform using log taxable income as the outcome and estimates DID and fixed-effects models with marginal and average tax rates and a reform indicator. Gruber and Saez (2002) estimate the elasticity of taxable income with respect to tax rates using DID and fixed-effects specifications and conduct robustness checks using instrumental variables. Heim (2009) evaluates reforms for married couples using pre-tax income and its logarithm. Piketty et al. (2014) analyze the relationship between changes in top tax rates and taxable income using data on high-income groups across several countries, and Saez (2010) exploits kinks in the tax schedule, combining regression discontinuity and DID approaches to study behavioral responses.

Building on this literature, our models use income measures as outcomes and aim to identify the causal effects of tax reforms on disposable income. We focus on equivalent household disposable income as the key outcome for evaluating redistributive impacts. Although Japan's personal income tax is levied at the individual level, institutional features can generate within-household behavioral responses. For instance, revisions to the Exemption for Spouses and Special Exemption for Spouses directly affect the primary earner's tax liability but may also change the spouse's labor supply, thereby affecting total household disposable income. For this reason, even under individual-based taxation, household-level analysis is natural. Consistent with this perspective, prior work such as Atkinson (1983) and Osberg (2017) evaluates policy effects at the household level, noting that many institutional rules depend on family composition. In this paper, we treat the main earner as the reference individual and attach their characteristics to equivalized household income, following standard international practice.

Accounting for joint consumption and economies of scale within households also motivates the use of equivalence-scale–adjusted income. As a result, equivalent household disposable income has become the standard outcome measure in redistribution analysis in international organizations and empirical research. The OECD (2013), for example, notes that equivalized household disposable income is appropriate for comparing economic well-being across households with different compositions and is consistent with common inequality indices such as the Gini and Atkinson indices. Buhmann *et al.* (1988) and Immervoll *et al.* (2005) also provide evidence that using equivalent household disposable income is important for capturing redistributive effects of taxes, social security, and cash transfers. For these reasons, we use equivalent household disposable income as the dependent variable when assessing the substantive consequences of Japan's 2010s personal income tax reforms.

International comparative research further emphasizes that institutional design shapes both the magnitude and the pattern of redistribution. In the United States, the Earned Income Tax Credit (EITC) is often found to be particularly effective in supporting lower- and middle-income households, whereas in many EU countries cash transfers play a larger role (Avram *et al.*, 2014; Immervoll *et al.*, 2005; Nichols and Rothstein, 2016). In contrast, increases in top marginal tax rates and tighter deduction caps tend to have pronounced effects at the top of the distribution, potentially mediated by behavioral responses among high-income taxpayers (Browne and Phillips, 2017; Saez *et al.*, 2012).

Against this background, the present paper uses quantile regression methods to provide new evidence on the distributional effects of Japan's personal income tax reforms in the 2010s and to situate Japan's experience within the broader international literature.

3. Empirical Strategy

This study evaluate the redistributive effects of Japan's personal income tax reforms in the 2010s using microdata from the Japan Household Panel Survey (JHPS/KHPS) for survey years 2009–2021. The analysis focuses on reforms implemented between 2011 and 2020 and excludes the 2022–2023 survey waves, which may already reflect reforms introduced after 2021. This restriction helps delineate the policy environment and supports cleaner identification of reform-specific redistribution effects.

Our empirical framework differs from the now-standard "staggered-adoption" difference-in-differences (DID) design. The reforms studied here are a sequence of distinct tax changes implemented in 2011, 2013–2018, and 2020. For each reform, we define treatment and control groups based on institutional eligibility criteria and use a limited panel of calendar years centered on the relevant reform year. Following Autor (2003), we first estimate event-study specifications with relative-year indicators to examine pre-trends and assess the plausibility of the parallel trends assumption. Conditional on the absence of systematic pre-trends, we then estimate W-OLS-DID, W-2SFE-DID, W-CQR-DID, and W-UQR-DID separately for each reform to quantify both average and distributional effects. Because the same household may be treated in one reform and serve as a control in another, our setting is structurally different from canonical staggered-adoption designs in which a single treatment indicator turns on at different times across groups and remains on thereafter.

The empirical analysis proceeds in three steps. First, we construct the outcome variable via microsimulation: building on Doi (2021, 2023), we simulate personal income taxes, local inhabitant taxes, and social security premiums (medical care, long-term care, public pensions, and employment insurance) and then construct equivalent household disposable income. Second, because DID identification hinges on parallel trends, we conduct pre-trend tests using event-study specifications (Autor, 2003) to assess whether treatment and control groups exhibit differential trends prior to reform implementation. Third, we estimate the main effects of each reform: W-OLS-DID and W-2SFE-DID quantify average (mean) effects; W-CQR-DID documents heterogeneity across the conditional income distribution; and W-UQR-DID captures effects on unconditional quantiles—i.e., changes in households' positions in the overall distribution and, therefore, changes in the redistribution structure itself. Together, these methods provide a multifaceted assessment of the micro-level impacts of Japan's 2010s personal income tax reforms.

3.1. Microsimulation of Equivalent Household Disposable Income

To assess reform effects on household well-being, we use equivalent household disposable income as the outcome variable. We construct it by microsimulating personal income taxes, local inhabitant taxes, and social security premiums using JHPS/KHPS microdata and then computing disposable income at the household level. The microsimulation procedure closely follows Doi (2021, 2023), which we summarize briefly here.

The JHPS/KHPS collects rich information on all household members, including demographic characteristics (year of birth, gender, education, employment status, relationship to the household head, etc.) and detailed income components (wage and salary income, self-employment income, rental income, capital income, public transfers, and others), regardless of whether members live together or apart. Using this information, we classify income items into comprehensive and separate taxation categories under the Japanese Income Tax Act and apply relevant deductions—including the Deduction for Salary Income, Deduction for Public and Other Pensions, Basic Exemption, Exemption for Dependents, Exemption for Spouses, Exemption for Medical Expenses, Exemption for Social Insurance Premiums, and the special deduction for blue tax returns—to compute taxable income.

Reported incomes in the JHPS/KHPS refer to earnings in the calendar year preceding the survey. Following Doi (2021, 2023), we treat local inhabitant taxes and some social security premiums that are legally assessed on the prior year's income but paid in the subsequent year on an accrual basis, as if they were imposed in the income year itself. We then compute tax liabilities, social security premiums, and disposable income on this accrual basis.

Deductions related to family composition are imputed by reconstructing dependency relationships using household member characteristics. The Exemption for Medical Expenses is computed from reported out-of-pocket medical expenditures, and the Exemption for Social Insurance Premiums is based on the simulated contributions described below. Retirement income is constructed by applying the statutory retirement income deduction to reported retirement benefit receipts and then applying the relevant tax schedule using inferred years of service; years of service are imputed using age- and gender-specific averages from the Basic Survey on Wage Structure published by the Ministry of Health, Labour and Welfare. For interest income, dividends, and capital gains, we use reported balances of savings and securities together with information on interest rates and dividend yields to allocate capital income into interest and dividends and apply the corresponding tax rates. Social security premiums are simulated by inferring scheme eligibility from employment status, income, legal form of business, and firm size and then applying scheme-specific contribution rules from official sources. For region-specific premiums—especially National Health Insurance and the Medical Care System for the Advanced Elderly—we use municipality-of-residence information to replicate local contribution formulas.

We also follow Doi (2023) in imputing items not fully observed in the survey, including mortgage loan tax credits, the adjustment deduction in local inhabitant taxation, and unreported Child Allowance receipts. We then aggregate after-tax, after-contribution disposable incomes across household members and equivalize household disposable income by household size. This equivalent household disposable income measure is used as the dependent variable in the subsequent analysis.

Households that include members with plausibly positive income reported as missing are excluded, because comprehensive and internally consistent disposable income cannot be constructed for such households.

3.2. Pre-trend Tests

To assess the validity of the parallel trends assumption underlying the Difference-in-Differences (DID) estimators, we implement pre-trend tests based on an event-study specification following Autor (2003). Let t index calendar years ($t = 2009, \cdots, 2021$), and let T_i denote the first year in which household i is exposed to a given reform. Define event time as $k = t - T_i$. Taking the year immediately before the reform ($k = -1$) as the reference period, we specify for each reform a lower bound L for the pre-reform period and an upper bound H for the post-reform period, and introduce event-time dummies $D_{it}^{(k)}$ for all $k \in \{L, \dots, H\}$ with $k \neq -1$. The baseline specification is

$$Y_{it} = \alpha + \sum_{k=L, k\neq -1}^{H} \beta_k D_{it}^{(k)} + \gamma_i + \delta_t + \varepsilon_{it}$$

where Y_{it} is the logarithm of equivalent household disposable income, γ_i is a household fixed effect, δ_t is a year fixed effect, and ε_{it} is an error term. This equation provides a common backbone for all estimators, with structural differences across models as follows: OLS-DID corresponds to a linear specification with year fixed effects only; Two-Step FE-DID adds household fixed effects; CQR-DID implements Conditional Quantile Regression in the sense of Koenker and Bassett (1978); and UQR-DID implements Unconditional Quantile Regression using the recentered influence function (RIF) as in Firpo, Fortin, and Lemieux (2009). In all cases, population restoration weights are incorporated in the objective function, and standard errors are clustered at the household level.

The coefficients β_k measure, for each event time k, the difference in outcomes between the treatment and control groups relative to the reference period $k = -1$. The pre-trend analysis evaluates the parallel trends assumption by testing whether all pre-reform event-time coefficients are jointly equal to zero, i.e., whether $\beta_k = 0$ for all $k < 0$. We conduct joint F-tests of the null hypothesis $H_0 : \beta_k = 0$ for all $k < 0$ and assess the parallel trends assumption based on the resulting p-values. For CQR-DID and UQR-DID, the event-study specification is estimated separately for a set of representative quantiles $(\tau = 0.1, \cdots, 0.9)$, and the dynamics of the pre-reform coefficients are examined in detail at each quantile.

The length of the pre-reform window in the event-study specification is not determined mechanically by calendar years alone, but is chosen with reference to the "margin of impact" of each reform—namely, the tax base, income range, and family composition directly affected by the policy—as well as the composition of the treatment and control groups defined in this paper. For each reform year T, we consider the three years from $T - 3$ to $T - 1$ as the pre-reform period and include them in the event window, and we use at least two pre-reform event times $(k = -3, -2)$ to test for pre-trends.[2]

3.3. Empirical Analysis

We estimate four models using the 2009–2021 JHPS/KHPS panel: (1) W-OLS-DID, (2) W-2SFE-DID, (3) W-CQR-DID, and (4) W-UQR-DID.

Because we exclude households with missing income information (roughly 10–20 percent of the sample), and because the JHPS/KHPS has relatively limited sample

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[2] Using two pre-reform coefficients $(k = -3, -2)$ allows us to check not only for level differences but also for approximate trend differences between treatment and control groups. In addition, because Japan implemented multiple tax reforms in close succession during the 2010s, it is difficult to construct a long pre-reform period over which the relevant "margin of impact" can be treated as institutionally unchanged. For the 2014–2018 reforms in particular, restricting attention to the three years $T - 3$ to $T - 1$ in which the specific margin affected by the reform is stable, and using two of these years for the pre-trend test, strikes a balance between minimizing contamination from other reforms and retaining sufficient information to evaluate pre-trends.

mass at the very top of the income distribution, we apply population restoration weights adjusted using the Population Census to strengthen representativeness—particularly in the upper tail. Following Doi (2017, 2021), we construct adjustment factors from the 2005, 2010, 2015, and 2020 Population Census tabulations (with linear interpolation for intermediate years) using residence area (urban versus rural), age group of the household head, and gender, and we use these factors to reweight the JHPS/KHPS sample.

3.3.1. Definition of Treatment and Control Groups

The validity of the DID design in this paper hinges on a careful definition of treatment and control groups for each tax reform. We adopt the following principles:

- Treatment group: households for which the reform directly changes tax liability or deduction amounts (i.e., households directly affected by the tax increase or tax relief).
- Control group: households not directly affected by the reform but comparable to treated households in income level and household composition, especially in the presence and structure of dependents.

This design is intended to make the parallel trends assumption more credible—namely, that absent the reform, treated and control households would have followed similar income trajectories.

For the 2011 reforms—(a) the abolition of the Exemption for Young Dependents and (b) the reduction in the Exemption for Specified Dependents—we treat these components separately and report estimates for each. For reforms implemented in 2013, 2015–2018, and 2020, treatment/control definitions also incorporate an equivalent household disposable income threshold. Specifically, for each reform year, we set the threshold equal to the minimum equivalent household disposable income observed among treated households and impose this lower bound symmetrically when defining the control group. This ensures that, in reforms where grouping depends on income, control households remain unaffected by the institutional change while still being comparable in income level and household structure.

Although Japan's personal income tax is assessed at the individual level, we measure redistribution using equivalent household disposable income. Accordingly, we classify a household as treated if it contains at least one individual whose tax liability is directly affected by the reform and treat as candidate controls households containing only individuals clearly unaffected by the reform. We then use the income threshold to prevent large discrepancies in equivalent household disposable income between treated and control households. Detailed definitions are summarized in Table 2.

Table 2. Definitions of treatment and control groups

Year	Treatment group	Control group
2011 (a)	Households with dependents aged 0–15	Households with dependents aged 19–64, or no dependents
2011 (b)	Households with dependents aged 16–18	Households with dependents aged 19–64, or no dependents
2013	Households with equivalent household disposable income ≥ ~1.57 million yen and salary income of the main earner > 15 million yen	Households with equivalent household disposable income ≥ ~1.57 million yen and non-salary income of the main earner > 15 million yen
2014	Households with annual financial income > 100,000 yen	Households with annual financial income ≤ 100,000 yen (including losses)
2015	Households with equivalent household disposable income ≥ ~11.79 million yen and taxable income of the main earner > 40 million yen	Households with equivalent household disposable income ≥ ~11.79 million yen and no household member with taxable income > 40 million yen
2016	Households with equivalent household disposable income ≥ ~0.36 million yen and salary income of the main earner > 12 million yen	Households with equivalent household disposable income ≥ ~0.36 million yen and non-salary income of the main earner > 12 million yen
2017	Households with equivalent household disposable income ≥ ~0.36 million yen and salary income of the main earner > 10 million yen	Households with equivalent household disposable income ≥ ~0.36 million yen and non-salary income of the main earner > 10 million yen
2018	Households with equivalent household disposable income ≥ ~33,000 yen, primary earner's income ≤ 11.2 million yen, and spouse's income 1.03–1.50 million yen	Households with equivalent household disposable income ≥ ~33,000 yen, primary earner's income ≤ 11.2 million yen, and spouse's income ≤ 1.03 million yen
2020	Households with equivalent household disposable income ≥ ~0.36 million yen and salary income of the main earner > 8.5 million yen	Households with equivalent household disposable income ≥ ~0.36 million yen and non-salary income of the main earner > 8.5 million yen

Notes: "Equivalent household disposable income" follows the microsimulation definition in the text. Where a threshold is shown (e.g., ~1.57 million yen), it equals the minimum value observed among treated observations and is applied symmetrically to define the control group. "Financial income" includes interest, dividends, and realized capital gains. "Non-salary income" refers to income sources other than wages/salaries for the main earner.
Source: Authors' compilation.

3.3.2. Theoretical and Empirical Justification for Population Weights

(1) Weighted Ordinary Least Squares Difference-in-Differences

Applying survey design weights in weighted least squares is appropriate when the sampling design is informative for the outcome and is often important for obtaining population-representative estimates (Angrist and Pischke, 2008; Solon *et al.*, 2015; Wooldridge, 2001). Related empirical applications include Erten and Keskin (2018) and Shin and Solon (2011). Our baseline weighted OLS DID specification is

$$\ln Y_{it} = \beta_0 + \beta_1 Reform_t + \beta_2 Treat_i + \beta_3 (Reform_t \times Treat_i) + \gamma' X_{it} + \varepsilon_{it} ,$$

estimated by weighted least squares using population restoration weights w_i, with standard errors clustered at the household level. Here, $\ln Y_{it}$ is log equivalent household disposable income; Reform_t is a post-reform indicator; Treat_i is a treatment-group indicator; X_{it} is a vector of covariates (age and age squared of the main earner, gender, education, industry, region, and year dummies); and β_3 is the DID effect of interest

(2) Two-Step Weighted Fixed Effects Difference-in-Differences

To account for unobserved time-invariant heterogeneity at the household level, we also estimate a Two-Step Weighted Fixed Effects DID (W-2SFE-DID) model based on the Frisch–Waugh–Lovell theorem. Fixed-effects models can be estimated either by including individual dummies or by applying a within transformation, but these approaches are strictly equivalent only in the unweighted case. When population weights are used, the within transformation must be based on weighted, rather than unweighted, individual means. Implementing the FWL two-step procedure explicitly allows us to apply the same weights consistently in both stages.

In the first stage, we partial out individual fixed effects from the dependent variable and key regressors:

$$r_{it}^{Y} = \ln Y_{it} - \hat{\alpha}_i^{Y} \,,$$
$$r_{it}^{Reform} = Reform_t - \hat{\alpha}_i^{Reform} \,,$$
$$r_{it}^{Treat} = Treat_i - \hat{\alpha}_i^{Treat} \,,$$
$$r_{it}^{DID} = (Reform_t \times Treat_i) - \hat{\alpha}_i^{DID} \,,$$

where each $\hat{\alpha}_i$ is the (weighted) individual-specific mean corresponding to the fixed effect for household i. Importantly, the interaction term $(\mathrm{Reform}_t \times \mathrm{Treat}_i)$ must itself be residualized; because the within operator $M(\cdot)$ is linear but not multiplicative, we generally have $M(xz) \neq (Mx)(Mz)$.

In the second stage, we regress the residualized dependent variable on the residualized regressors:

$$r_{it}^{Y} = \beta_1 r_{it}^{Reform} + \beta_2 r_{it}^{Treat} + \beta_3 r_{it}^{DID} + \gamma' r_{it}^{X} + e_{it} \,,$$

again weighted by w_i, where r_{it}^{X} is the vector of residualized covariates and e_{it} is the second-stage error term. Because the regression is run on residuals, the constant term is theoretically zero, and we estimate the model without an intercept. When year fixed effects δ_t are included, the reform dummy $\mathrm{Reform}_t$is absorbed by the year dummies, implying $r_{it}^{\mathrm{Reform}} \equiv 0$; in that case, β_1 is not identified and r_{it}^{Reform} is omitted from the second-stage regression. This two-step implementation clarifies how the treatment effect is identified after controlling for individual fixed effects while preserving the population-representative nature of the weighted estimator.

(3) Weighted Conditional Quantile Regression Difference-in-Differences

Conditional Quantile Regression (CQR), as developed by Koenker and Bassett (1978), estimates quantiles of the conditional distribution of the outcome given covariates. It is well established that incorporating design weights yields consistent estimators for conditional quantiles when the sampling design is informative (Buchinsky, 1994). Koenker (2005) shows that weighted quantile regression can be formulated as a linear programming problem and provides a theoretical foundation for treating it as a weighted minimization problem. In this paper, we follow that framework but use exogenous survey design weights—constructed as described in Section 3.3—rather than endogenous weights based on error structure or efficiency considerations.

Erten and Keskin (2018) also employ weighted CQR with survey design weights in a policy evaluation context. We adopt a similar setup to assess heterogeneity in policy effects across the conditional income distribution. The CQR-DID specification is

$$Q_\tau(\ln Y_{it} \mid Reform_t, Treat_i, X_{it}) = \beta_0^\tau + \beta_1^\tau Reform_t + \beta_2^\tau Treat_i + \beta_3^\tau (Reform_t \cdot Treat_i) + \gamma^{\tau'} X_{it} + u_{it}^\tau ,$$

where $Q_\tau(\cdot)$ denotes the τ-th conditional quantile of $\ln Y_{it}$ for household i in year t; τ indexes points of the conditional distribution (for example, $\tau = 0.2$ for the lower part, $\tau = 0.5$ for the median, and $\tau = 0.8$ for the upper part of the distribution); and u_{it}^τ is an error term for quantile τ. The coefficient β_3^τ measures the DID effect at quantile τ.

To account for the nonlinearity of quantile regression and the potential asymmetry of the error distribution, we compute standard errors using a cluster bootstrap at the household level. As noted by Koenker and Bassett (1978), standard errors based solely on asymptotic quantile regression theory can be unstable in small samples or in the tails of the distribution. By using bootstrap methods, we obtain more robust standard errors and confidence intervals that better reflect the finite-sample variability of the quantile estimates. This enhances the statistical reliability of our CQR-DID results.

(4) Weighted Unconditional Quantile Regression Difference-in-Differences

Unconditional Quantile Regression (UQR) as proposed by Firpo, Fortin, and Lemieux (2009) uses the Recentered Influence Function (RIF) to estimate unconditional quantile treatment effects within a linear regression framework. Because the RIF regression is linear, survey design weights can be incorporated in a natural way, and population-weighted RIF regressions are widely used in applied work (e.g., Havnes and Mogstad, 2015).

Our UQR-DID specification is

$$RIF(\ln Y_{it}, q_\tau) = \beta_0^\tau + \beta_1^\tau Reform_t + \beta_2^\tau Treat_i + \beta_3^\tau (Reform_t \cdot Treat_i) + \gamma^{\tau'} X_{it} + \eta_{it}^\tau ,$$

where $\mathrm{RIF}(\ln Y_{it}, q_\tau)$ is the recentered influence function of the τ-th unconditional quantile q_τ of $\ln Y_{it}$; q_τ is the τ-th quantile of the unconditional distribution of $\ln Y_{it}$; and η_{it}^τ is an error term. As in the CQR case, β_3^τ is the DID coefficient of interest, now interpreted as the effect of the reform on the τ-th unconditional quantile of $\ln Y_{it}$.

In principle, standard errors for UQR can also be obtained via bootstrap methods, since the RIF regression is implemented as a standard linear model. In practice, however, our implementation relies on the rifhdreg command in Stata, which does not support automatic bootstrap resampling within the weighted RIF regression framework. We therefore use the analytical variance estimation provided by rifhdreg, which computes standard errors under the theoretically consistent asymptotic variance formula derived by Firpo, Fortin, and Lemieux (2009). This approach ensures methodological consistency with the original UQR framework while accommodating the practical constraints of our estimation environment.

4. Results

4.1. Pre-trend test results

We begin by reporting pre-trend tests based on event-study specifications following Autor (2003) to assess the plausibility of the parallel trends assumption underlying our Difference-in-Differences (DID) estimators. Let t index calendar years and let T_i denote the year in which household i is first exposed to a given tax reform; event time is defined as $k = t - T_i$. Taking the year immediately before implementation ($k = -1$) as the reference period, we conduct joint F-tests of whether the coefficients on the pre-reform event-time indicators $D_{it}^{(k)}$ for $k = -2, -3$ are jointly equal to zero, and we assess statistical significance using the associated p-values.

For each reform year, we estimate the event-study specification using W-OLS-DID, W-2SFE-DID, W-CQR-DID (for quantiles $\tau = 0.1, \dots, 0.9$), and W-UQR-DID (for $\tau = 0.1, \dots, 0.9$). Throughout, we treat $p < 0.05$ as the primary threshold for statistical significance and $p < 0.10$ as a secondary, suggestive threshold.

For the 2011 reform—covering (a) the Exemption for Young Dependents and (b) the Exemption for Specified Dependents—the W-2SFE-DID estimates exhibit statistically significant pre-trends at the 5 percent level for both components. By contrast, under W-OLS-DID the pre-trend is not statistically significant for the Exemption for Young Dependents and is only marginally significant at the 10 percent level for the

Exemption for Specified Dependents. Across quantiles, the W-CQR-DID and W-UQR-DID estimates show no systematic evidence of statistically significant pre-trends.

For the 2013 introduction of the cap on the Deduction for Salary Income, we detect a statistically significant pre-trend at the 5 percent level only for W-CQR-DID at $\tau = 0.8$. Under W-OLS-DID, W-2SFE-DID, and most W-UQR-DID specifications ($\tau \geq 0.3$), the pre-reform coefficients are not statistically significant, and parallel trends appears broadly supported. For the lower quantiles of W-UQR-DID ($\tau = 0.1, 0.2$), the joint F-test is not available (N/A) due to insufficient observations in some event-time cells, so the evidence for these quantiles is inherently limited.

For the 2014 abolition of the reduced tax rate on capital income, neither W-OLS-DID nor W-2SFE-DID indicates statistically significant pre-trends, even at the 10 percent level, suggesting no clear pre-trend in average outcomes. However, W-CQR-DID reveals statistically significant pre-trends at the 5 percent level for $\tau = 0.6$ and $\tau = 0.7$, pointing to some imbalance in pre-reform dynamics in the upper-middle part of the conditional distribution. For other quantiles—and for W-UQR-DID—the pre-reform coefficients are not statistically significant.

For the 2015 increase in the top marginal tax rate, both W-OLS-DID and W-2SFE-DID are marginally significant at the 10 percent level, indicating weak evidence of pre-trends in average outcomes. By contrast, most quantiles in W-CQR-DID and W-UQR-DID do not exhibit statistically significant pre-trends, and we do not observe a systematic imbalance across the distribution. That said, for 2015, joint F-tests for W-CQR-DID at $\tau = 0.1, 0.2, 0.9$ and for some W-UQR-DID quantiles are N/A, again reflecting sparse cells in the event-time window; results for those quantiles should therefore be interpreted with caution.

For the 2016 reduction in the maximum Deduction for Salary Income, W-CQR-DID at $\tau = 0.7$ is marginally significant at the 10 percent level, suggesting a weak pre-trend at that quantile. In contrast, W-OLS-DID, W-2SFE-DID, and all other W-CQR-DID and W-UQR-DID quantiles show no statistically significant pre-trends. For the 2017 further reduction, W-CQR-DID indicates a statistically significant pre-trend at the 1 percent level for $\tau = 0.9$ and a marginally significant pre-trend at the 10 percent level for $\tau = 0.3$, implying nontrivial pre-reform dynamics at selected conditional quantiles, particularly in the upper tail. In contrast, W-OLS-DID, W-2SFE-DID, and W-UQR-DID do not show statistically significant pre-trends.

For the 2018 expansion of the Exemption for Spouses and the Special Exemption for Spouses, the joint F-tests are not statistically significant for W-OLS-DID, W-2SFE-DID, or W-CQR-DID at any quantile, providing strong support for parallel trends across the conditional distribution. Under W-UQR-DID, the pre-trend at $\tau = 0.6$ is marginally significant at the 10 percent level, suggesting a weak potential imbalance around the middle of the unconditional distribution, but no statistically significant pre-trends are detected at other quantiles. Finally, for the 2020 reform—combining an increase in the Basic Exemption with reductions in the Deduction for Salary Income and the Deduction for Public and Other Pensions and the introduction of the Income Adjustment Deduction—none of the W-OLS-DID, W-2SFE-DID, W-CQR-DID, or W-UQR-DID specifications shows statistically significant pre-trends.

In sum, for the 2013, 2018, and 2020 reforms, we find no statistically significant pre-trends in the majority of specifications, and the parallel trends assumption appears broadly plausible. For the 2011 reform (5 percent significance in W-2SFE-DID and 10 percent in W-OLS-DID for the Exemption for Specified Dependents), the 2014 reform (selected W-CQR-DID quantiles), the 2015 reform (marginal significance in W-OLS-DID and W-2SFE-DID and several N/A quantiles), the 2016 and 2017 reforms (marginal or significant pre-trends at selected W-CQR-DID quantiles, particularly in the upper tail), and the 2018 reform (a marginal pre-trend at one W-UQR-DID quantile), we find limited evidence of pre-reform imbalances. Nevertheless, because statistically significant pre-trends are absent for most reform years, estimators, and quantiles, we conclude that the DID identifying assumptions are, on balance, reasonably supported.

Full results are reported in Table 3. In the table, we summarize the pre-trend assessment using the following symbols: "○" denotes no statistically significant pre-trend ($p > 0.10$); "△" indicates suggestive evidence of a weak pre-trend ($0.05 < p \leq 0.10$); and "×" indicates a statistically significant pre-trend ($p \leq 0.05$). Quantiles for which the joint F-test is N/A—typically due to insufficient observations in some event-time cells—are interpreted as providing only limited information.

Table 3. Joint F-tests for pre-trends

(p-values by reform year, estimation method, and quantile)

Year	Model	Pre-Trend Joint F-Test	p-value	Pre-Trend Assessment
2011 (a)	Weighted OLS-DID	F(2, 5919)=0.97	0.3808	○
	Two-Step Weighted FE-DID	F(2, 5718)=3.21	0.0404	×
	Weighted CQR-DID (τ=0.1)	F(2, 37777)=1.16	0.3126	○
	Weighted CQR-DID (τ=0.2)	F(2, 37777)=1.35	0.2585	○
	Weighted CQR-DID (τ=0.3)	F(2, 37777)=0.23	0.7927	○
	Weighted CQR-DID (τ=0.4)	F(2, 37777)=0.22	0.7990	○
	Weighted CQR-DID (τ=0.5)	F(2, 37777)=0.04	0.9649	○
	Weighted CQR-DID (τ=0.6)	F(2, 37777)=0.04	0.9601	○
	Weighted CQR-DID (τ=0.7)	F(2, 37777)=0.23	0.7945	○
	Weighted CQR-DID (τ=0.8)	F(2, 37777)=0.09	0.9153	○
	Weighted CQR-DID (τ=0.9)	F(2, 37777)=0.53	0.5906	○
	Weighted UQR-DID (τ=0.1)	F(2, 5919)=0.15	0.8585	○
	Weighted UQR-DID (τ=0.2)	F(2, 5919)=0.27	0.7632	○
	Weighted UQR-DID (τ=0.3)	F(2, 5919)=0.51	0.6011	○
	Weighted UQR-DID (τ=0.4)	F(2, 5919)=0.89	0.4120	○
	Weighted UQR-DID (τ=0.5)	F(2, 5919)=0.60	0.5475	○
	Weighted UQR-DID (τ=0.6)	F(2, 5919)=0.90	0.4060	○
	Weighted UQR-DID (τ=0.7)	F(2, 5919)=0.18	0.8362	○
	Weighted UQR-DID (τ=0.8)	F(2, 5919)=0.34	0.7139	○
	Weighted UQR-DID (τ=0.9)	F(2, 5919)=0.28	0.7584	○
2011 (b)	Weighted OLS-DID	F(2, 5095)=2.74	0.0644	△
	Two-Step Weighted FE-DID	F(2, 4917)=3.91	0.0201	×
	Weighted CQR-DID (τ=0.1)	F(2, 31998)=0.60	0.5481	○
	Weighted CQR-DID (τ=0.2)	F(2, 31998)=0.98	0.3770	○
	Weighted CQR-DID (τ=0.3)	F(2, 31998)=0.76	0.4692	○
	Weighted CQR-DID (τ=0.4)	F(2, 31998)=1.02	0.3614	○
	Weighted CQR-DID (τ=0.5)	F(2, 31998)=0.30	0.7378	○
	Weighted CQR-DID (τ=0.6)	F(2, 31998)=0.70	0.4990	○
	Weighted CQR-DID (τ=0.7)	F(2, 31998)=0.25	0.7813	○
	Weighted CQR-DID (τ=0.8)	F(2, 31998)=0.65	0.5204	○
	Weighted CQR-DID (τ=0.9)	F(2, 31998)=1.85	0.1569	○
	Weighted UQR-DID (τ=0.1)	F(2, 5095)=0.59	0.5553	○
	Weighted UQR-DID (τ=0.2)	F(2, 5095)=0.77	0.4647	○
	Weighted UQR-DID (τ=0.3)	F(2, 5095)=0.42	0.6571	○
	Weighted UQR-DID (τ=0.4)	F(2, 5095)=0.79	0.4522	○
	Weighted UQR-DID (τ=0.5)	F(2, 5095)=0.45	0.6368	○
	Weighted UQR-DID (τ=0.6)	F(2, 5095)=0.26	0.7691	○
	Weighted UQR-DID (τ=0.7)	F(2, 5095)=0.09	0.9176	○
	Weighted UQR-DID (τ=0.8)	F(2, 5095)=0.01	0.9913	○
	Weighted UQR-DID (τ=0.9)	F(2, 5095)=0.22	0.8036	○

2013	Weighted OLS-DID	F(2, 112)=0.51	0.6018	○
	Two-Step Weighted FE-DID	F(2, 112)=0.25	0.7826	○
	Weighted CQR-DID (τ=0.1)	F(2, 786)=0.00	0.9982	○
	Weighted CQR-DID (τ=0.2)	F(2, 786)=0.01	0.9884	○
	Weighted CQR-DID (τ=0.3)	F(2, 787)=0.02	0.9769	○
	Weighted CQR-DID (τ=0.4)	F(2, 787)=0.09	0.9112	○
	Weighted CQR-DID (τ=0.5)	F(2, 786)=0.37	0.6890	○
	Weighted CQR-DID (τ=0.6)	F(2, 786)=0.10	0.9055	○
	Weighted CQR-DID (τ=0.7)	F(2, 786)=0.29	0.7447	○
	Weighted CQR-DID (τ=0.8)	F(2, 787)=4.43	0.0122	×
	Weighted CQR-DID (τ=0.9)	F(1, 789)=0.01	0.9250	○
	Weighted UQR-DID (τ=0.1)	N/A	N/A	×
	Weighted UQR-DID (τ=0.2)	N/A	N/A	×
	Weighted UQR-DID (τ=0.3)	F(1, 112)=0.96	0.3304	○
	Weighted UQR-DID (τ=0.4)	F(1, 112)=0.96	0.3304	○
	Weighted UQR-DID (τ=0.5)	F(1, 112)=0.96	0.3304	○
	Weighted UQR-DID (τ=0.6)	F(2, 112)=0.48	0.6214	○
	Weighted UQR-DID (τ=0.7)	F(2, 112)=0.48	0.6214	○
	Weighted UQR-DID (τ=0.8)	F(2, 112)=0.48	0.6214	○
	Weighted UQR-DID (τ=0.9)	F(2, 112)=0.48	0.6214	○
2014	Weighted OLS-DID	F(2, 5274)=2.00	0.1359	○
	Two-Step Weighted FE-DID	F(2, 5220)=2.30	0.1003	○
	Weighted CQR-DID (τ=0.1)	F(2, 36299)=0.20	0.8194	○
	Weighted CQR-DID (τ=0.2)	F(2, 36299)=1.03	0.3580	○
	Weighted CQR-DID (τ=0.3)	F(2, 36299)=2.25	0.1056	○
	Weighted CQR-DID (τ=0.4)	F(2, 36299)=1.10	0.3316	○
	Weighted CQR-DID (τ=0.5)	F(2, 36299)=1.40	0.2462	○
	Weighted CQR-DID (τ=0.6)	F(2, 36299)=3.64	0.0263	×
	Weighted CQR-DID (τ=0.7)	F(2, 36299)=3.31	0.0365	×
	Weighted CQR-DID (τ=0.8)	F(2, 36299)=1.23	0.2930	○
	Weighted CQR-DID (τ=0.9)	F(2, 36299)=0.43	0.6529	○
	Weighted UQR-DID (τ=0.1)	F(2, 5274)=1.87	0.1544	○
	Weighted UQR-DID (τ=0.2)	F(2, 5274)=1.98	0.1377	○
	Weighted UQR-DID (τ=0.3)	F(2, 5274)=1.68	0.1867	○
	Weighted UQR-DID (τ=0.4)	F(2, 5274)=1.52	0.2182	○
	Weighted UQR-DID (τ=0.5)	F(2, 5274)=1.15	0.3178	○
	Weighted UQR-DID (τ=0.6)	F(2, 5274)=0.85	0.4258	○
	Weighted UQR-DID (τ=0.7)	F(2, 5274)=1.01	0.3633	○
	Weighted UQR-DID (τ=0.8)	F(2, 5274)=0.55	0.5759	○
	Weighted UQR-DID (τ=0.9)	F(2, 5274)=0.44	0.6472	○
2015	Weighted OLS-DID	F(2, 59)=3.13	0.0509	△
	Two-Step Weighted FE-DID	F(2, 59)=2.68	0.0767	△
	Weighted CQR-DID (τ=0.1)	N/A	N/A	×
	Weighted CQR-DID (τ=0.2)	N/A	N/A	×
	Weighted CQR-DID (τ=0.3)	F(2, 429)=0.00	0.9969	○
	Weighted CQR-DID (τ=0.4)	F(2, 428)=0.02	0.9800	○
	Weighted CQR-DID (τ=0.5)	F(1, 428)=0.00	0.9892	○
	Weighted CQR-DID (τ=0.6)	F(1, 428)=0.00	0.9694	○
	Weighted CQR-DID (τ=0.7)	F(1, 429)=0.01	0.9306	○
	Weighted CQR-DID (τ=0.8)	F(1, 432)=0.40	0.5298	○
	Weighted CQR-DID (τ=0.9)	N/A	N/A	×
	Weighted UQR-DID (τ=0.1)	N/A	N/A	×
	Weighted UQR-DID (τ=0.2)	F(1, 59)=0.00	0.9999	○
	Weighted UQR-DID (τ=0.3)	F(1, 59)=0.00	0.9999	○
	Weighted UQR-DID (τ=0.4)	F(2, 59)=0.00	1	○
	Weighted UQR-DID (τ=0.5)	F(2, 59)=0.00	1	○
	Weighted UQR-DID (τ=0.6)	N/A	N/A	×
	Weighted UQR-DID (τ=0.7)	N/A	N/A	×
	Weighted UQR-DID (τ=0.8)	F(2, 59)=0.48	0.6220	○
	Weighted UQR-DID (τ=0.9)	F(1, 59)=0.96	0.3319	○

2016	Weighted OLS-DID	F(2, 209)=0.49	0.6142	○
	Two-Step Weighted FE-DID	F(2, 209)=0.02	0.9787	○
	Weighted CQR-DID (τ=0.1)	F(2, 1502)=0.15	0.8643	○
	Weighted CQR-DID (τ=0.2)	F(2, 1502)=0.34	0.7124	○
	Weighted CQR-DID (τ=0.3)	F(2, 1502)=0.48	0.6160	○
	Weighted CQR-DID (τ=0.4)	F(2, 1502)=0.55	0.5784	○
	Weighted CQR-DID (τ=0.5)	F(2, 1502)=0.03	0.9749	○
	Weighted CQR-DID (τ=0.6)	F(2, 1502)=0.82	0.4393	○
	Weighted CQR-DID (τ=0.7)	F(2, 1502)=2.90	0.0555	△
	Weighted CQR-DID (τ=0.8)	F(2, 1502)=0.86	0.4217	○
	Weighted CQR-DID (τ=0.9)	F(2, 1502)=0.56	0.5731	○
	Weighted UQR-DID (τ=0.1)	F(2, 209)=0.00	1.0000	○
	Weighted UQR-DID (τ=0.2)	F(1, 209)=1.61	0.2065	○
	Weighted UQR-DID (τ=0.3)	F(2, 209)=0.80	0.4494	○
	Weighted UQR-DID (τ=0.4)	F(2, 209)=1.30	0.2749	○
	Weighted UQR-DID (τ=0.5)	F(1, 209)=0.04	0.8377	○
	Weighted UQR-DID (τ=0.6)	F(1, 209)=0.04	0.8377	○
	Weighted UQR-DID (τ=0.7)	F(2, 209)=0.02	0.9792	○
	Weighted UQR-DID (τ=0.8)	F(2, 209)=0.53	0.5912	○
	Weighted UQR-DID (τ=0.9)	F(2, 209)=0.53	0.5912	○
2017	Weighted OLS-DID	F(2, 376)=1.15	0.3186	○
	Two-Step Weighted FE-DID	F(2, 376)=0.33	0.7191	○
	Weighted CQR-DID (τ=0.1)	F(2, 2681)=0.10	0.9045	○
	Weighted CQR-DID (τ=0.2)	F(2, 2681)=1.04	0.3540	○
	Weighted CQR-DID (τ=0.3)	F(2, 2680)=2.86	0.0576	△
	Weighted CQR-DID (τ=0.4)	F(2, 2680)=0.13	0.8744	○
	Weighted CQR-DID (τ=0.5)	F(2, 2680)=0.02	0.9796	○
	Weighted CQR-DID (τ=0.6)	F(2, 2680)=0.98	0.3765	○
	Weighted CQR-DID (τ=0.7)	F(2, 2680)=0.64	0.5289	○
	Weighted CQR-DID (τ=0.8)	F(2, 2680)=1.21	0.2978	○
	Weighted CQR-DID (τ=0.9)	F(2, 2681)=10.14	0.0000	×
	Weighted UQR-DID (τ=0.1)	F(1, 376)=0.99	0.3205	○
	Weighted UQR-DID (τ=0.2)	F(1, 376)=0.99	0.3205	○
	Weighted UQR-DID (τ=0.3)	F(1, 376)=0.99	0.3205	○
	Weighted UQR-DID (τ=0.4)	F(2, 376)=0.99	0.3736	○
	Weighted UQR-DID (τ=0.5)	F(2, 376)=1.45	0.2358	○
	Weighted UQR-DID (τ=0.6)	F(2, 376)=1.45	0.2358	○
	Weighted UQR-DID (τ=0.7)	F(2, 376)=1.45	0.2358	○
	Weighted UQR-DID (τ=0.8)	F(2, 376)=1.45	0.2358	○
	Weighted UQR-DID (τ=0.9)	F(2, 376)=1.45	0.2358	○
2018	Weighted OLS-DID	F(2, 3897)=0.03	0.9735	○
	Two-Step Weighted FE-DID	F(2, 3651)=0.12	0.8878	○
	Weighted CQR-DID (τ=0.1)	F(2, 23113)=0.04	0.9608	○
	Weighted CQR-DID (τ=0.2)	F(2, 23113)=0.00	0.9984	○
	Weighted CQR-DID (τ=0.3)	F(2, 23113)=0.15	0.8610	○
	Weighted CQR-DID (τ=0.4)	F(2, 23113)=0.01	0.9852	○
	Weighted CQR-DID (τ=0.5)	F(2, 23113)=0.09	0.9130	○
	Weighted CQR-DID (τ=0.6)	F(2, 23113)=0.75	0.4725	○
	Weighted CQR-DID (τ=0.7)	F(2, 23113)=0.27	0.7652	○
	Weighted CQR-DID (τ=0.8)	F(2, 23113)=0.03	0.9660	○
	Weighted CQR-DID (τ=0.9)	F(2, 23113)=0.41	0.6645	○
	Weighted UQR-DID (τ=0.1)	F(2, 3897)=1.36	0.2568	○
	Weighted UQR-DID (τ=0.2)	F(2, 3897)=1.28	0.2789	○
	Weighted UQR-DID (τ=0.3)	F(2, 3897)=0.17	0.8406	○
	Weighted UQR-DID (τ=0.4)	F(2, 3897)=1.36	0.2561	○
	Weighted UQR-DID (τ=0.5)	F(2, 3897)=1.83	0.1602	○
	Weighted UQR-DID (τ=0.6)	F(2, 3897)=2.46	0.0853	△
	Weighted UQR-DID (τ=0.7)	F(2, 3897)=1.53	0.2159	○
	Weighted UQR-DID (τ=0.8)	F(2, 3897)=1.08	0.3383	○
	Weighted UQR-DID (τ=0.9)	F(2, 3897)=1.08	0.3383	○

2020	Weighted OLS-DID	F(2, 644)=2.20	0.1114	○
	Two-Step Weighted FE-DID	F(2, 642)=1.74	0.1769	○
	Weighted CQR-DID (τ=0.1)	F(2, 3502)=0.10	0.9084	○
	Weighted CQR-DID (τ=0.2)	F(2, 3502)=0.18	0.8364	○
	Weighted CQR-DID (τ=0.3)	F(2, 3502)=0.09	0.9143	○
	Weighted CQR-DID (τ=0.4)	F(2, 3502)=0.10	0.9054	○
	Weighted CQR-DID (τ=0.5)	F(2, 3502)=0.11	0.8961	○
	Weighted CQR-DID (τ=0.6)	F(2, 3502)=1.44	0.2376	○
	Weighted CQR-DID (τ=0.7)	F(2, 3502)=1.33	0.2659	○
	Weighted CQR-DID (τ=0.8)	F(2, 3502)=1.52	0.2192	○
	Weighted CQR-DID (τ=0.9)	F(2, 3502)=1.26	0.2833	○
	Weighted UQR-DID (τ=0.1)	F(1, 644)=0.99	0.3189	○
	Weighted UQR-DID (τ=0.2)	F(2, 644)=1.49	0.2272	○
	Weighted UQR-DID (τ=0.3)	F(2, 644)=0.20	0.8181	○
	Weighted UQR-DID (τ=0.4)	F(2, 644)=0.50	0.6094	○
	Weighted UQR-DID (τ=0.5)	F(2, 644)=0.50	0.6094	○
	Weighted UQR-DID (τ=0.6)	F(2, 644)=0.61	0.5424	○
	Weighted UQR-DID (τ=0.7)	F(2, 644)=0.61	0.5424	○
	Weighted UQR-DID (τ=0.8)	F(2, 644)=0.61	0.5424	○
	Weighted UQR-DID (τ=0.9)	F(2, 644)=1.56	0.2111	○

Notes: Symbols for the assessment of pre-trends are as follows: "○" indicates no statistically significant pre-trend ($p > 0.10$); "△" indicates weak evidence of a pre-trend ($0.05 < p \leq 0.10$); and "×" indicates a statistically significant pre-trend ($p \leq 0.05$). For the 2013 reform, the joint F-test for CQR at $\tau = 0.8$ could not be computed because the interaction terms between the treatment dummy and the event-time dummies were dropped by the estimation routine; hence, the pre-trend assumption at this quantile cannot be statistically assessed.
Source: Authors' calculations based on JHPS/KHPS microdata.

4.2. Estimation results for each tax reform

4.2.1. 2011 reform

The 2011 personal income tax reform abolished the Exemption for Young Dependents (0–15 years old) and used the fiscal space to raise the Child Allowance to 15,000 yen per month, while at the same time reducing the Exemption for Specified Dependents (16–18 years old) from 630,000 yen to 380,000 yen. The former change aimed to secure financing for the Child Allowance by converting a tax deduction into a non-taxable cash benefit, whereas the latter was designed to finance the de facto free provision of upper secondary education by reducing deductions rather than expanding cash benefits.

We use the 2009–2012 waves of the JHPS/KHPS for this analysis, treating the 2009–2011 waves as pre-reform and the 2012 wave as post-reform. Our identification strategy defines the treatment group as (i) households with at least one dependent aged 0–15 (those affected by the abolishment of the Exemption for Young Dependents) and (ii) households with at least one dependent aged 16–18 (those affected by the reduction in the Exemption for Specified Dependents). The control group consists of households with dependents only in the 19–64 age range, as well as households without any dependents. We estimate W-OLS-DID and W-2SFE-DID, and to study distributional impacts we additionally estimate W-CQR-DID and W-UQR-DID for quantiles $\tau = 0.1, \cdots, 0.9$.

(a) Abolition of the Exemption for Young Dependents and expansion of the Child Allowance

We first consider average effects. As reported in Table 4, the W-OLS-DID estimate is 0.082 (standard error 0.022), statistically significant at the 1 percent level, corresponding to an increase of approximately 8.5 percent in equivalent household disposable income. The W-2SFE-DID estimate is 0.069 (standard error 0.020), also significant at the 1 percent level and implying an increase of about 7.1 percent. The sign and magnitude of the coefficients are consistent across specifications with and without household fixed effects, suggesting that the shift from a tax deduction to a cash transfer via the Child Allowance significantly raised average equivalent household disposable income.

Turning to distributional effects, the W-CQR-DID results indicate consistently positive and statistically significant effects from the lower to the middle part of the distribution. Specifically, we obtain 0.169 at $\tau = 0.1$ (1 percent significance, about 18.4 percent), 0.069 at $\tau = 0.2$ (5 percent, about 7.1 percent), 0.060 at $\tau = 0.3$ (5 percent, about 6.2 percent), 0.067 at $\tau = 0.4$ (1 percent, about 6.9 percent), 0.077 at $\tau = 0.5$ (1 percent, about 8.0 percent), and 0.051 at $\tau = 0.6$ (5 percent, about 5.2 percent). These results show statistically significant income gains for households in the lower and middle parts of the treated income distribution. By contrast, the estimates for the upper quantiles ($\tau = 0.7, 0.8, 0.9$) are not statistically significant, implying that the benefits of the reform were concentrated among lower- and middle-income treated households.[3] This pattern is consistent with the policy design: replacing a tax deduction with a cash transfer tends to raise disposable income proportionally more for lower-income households.

The W-UQR-DID results show positive and statistically significant effects over a wide range of unconditional quantiles, from $\tau = 0.2$ to $\tau = 0.8$. For instance, the

[3] In this subsection, "quantiles" and the terms "lower," "middle," and "upper" parts of the distribution refer to the income distribution estimated from the weighted sample consisting of treatment and control households used for each tax reform, rather than to the income distribution of the full JHPS sample.

estimates are 1.002 at $\tau = 0.3$ (5 percent significance), 0.288 at $\tau = 0.5$ (10 percent), and 0.628 at $\tau = 0.7$ (10 percent). These findings suggest a broad upward shift of the unconditional distribution of equivalent household disposable income in the combined sample of treated and control households. At the same time, the estimates at $\tau = 0.1$ and $\tau = 0.9$ are not statistically significant, indicating limited structural change at the very bottom and very top of the distribution. Because UQR coefficients measure effects on the unconditional quantiles themselves, they are not directly comparable in magnitude to the conditional quantile coefficients from CQR; interpretation should focus on the sign, statistical significance, and the range of affected quantiles (Firpo, Fortin, and Lemieux, 2009).

From an identification perspective, the event-study pre-trend tests indicate no statistically significant pre-trends for the main W-OLS-DID and W-CQR-DID/W-UQR-DID quantiles, whereas W-2SFE-DID shows a statistically significant pre-trend at the 5 percent level. Accordingly, the main conclusions regarding average effects should be based primarily on W-OLS-DID and the quantile regression results, with the W-2SFE-DID estimates treated as complementary and interpreted with caution. In addition, comparisons of pre-tax income show that the mean income difference between the treatment and control groups—on the order of 1.6–2.0 million yen—was broadly stable before and after the reform. While this level difference exists, its trend is limited, which supports the plausibility of the DID identifying assumptions.

Overall, the abolishment of the Exemption for Young Dependents and the expansion of the Child Allowance raised average equivalent household disposable income by about 7–9 percent and generated relatively larger gains for households in the lower and middle parts of the treated income distribution. In other words, the shift from a deduction-based to a benefit-based design strengthened income redistribution primarily in favor of low- and middle-income households, while leaving effects at the top of the distribution relatively modest.

(b) Reduction in the Exemption for Specified Dependents (ages 16–18)

We now turn to the reduction of the Exemption for Specified Dependents. As shown in Table 4, the W-OLS-DID estimate for the average effect is 0.117 (standard error 0.029), statistically significant at the 1 percent level and corresponding to an increase of about 12.4 percent in equivalent household disposable income. The W-2SFE-DID estimate is 0.103 (standard error 0.024), also significant at the 1 percent level, implying an increase of roughly 10.9 percent. The sign and magnitude are consistent across specifications, indicating that the reduction in the Exemption for Specified

Dependents raised average equivalent household disposable income in the treatment group.

The W-CQR-DID results show positive and statistically significant effects over a broad range of quantiles, from $\tau = 0.1$ to $\tau = 0.8$. For example, we obtain 0.126 at $\tau = 0.1$ (5 percent significance, about 13 percent), 0.101 at $\tau = 0.4$ (1 percent, about 11 percent), and 0.098 at $\tau = 0.7$ (1 percent, about 10 percent). The effect is thus sizable and significant from the lower to the upper-middle part of the distribution, while the estimate for the top decile ($\tau = 0.9$) is not statistically significant. This pattern again suggests that the reform mainly raised equivalent household disposable income for households in the lower to upper-middle range of the treated income distribution, with no clear effect at the very top.

The W-UQR-DID estimates are positive and statistically significant (at the 5–10 percent levels) for the middle to upper quantiles, $\tau = 0.3$ to $\tau = 0.9$, indicating an upward shift of the unconditional income distribution from the middle towards the top. By contrast, the estimate at $\tau = 0.1$ is not statistically significant, pointing to limited structural change at the very bottom.

Regarding identification, the event-study pre-trend tests reveal statistically significant pre-trends for W-2SFE-DID at the 5 percent level and for W-OLS-DID at the 10 percent level, whereas W-CQR-DID and W-UQR-DID show no systematic evidence of pre-trends across quantiles. This calls for some caution in interpreting the average effects, while suggesting that concerns about pre-reform dynamics are more limited for the quantile regression results. Comparisons of pre-tax incomes indicate that the treatment group consistently had higher income levels than the control group even before the reform, and that this gap widened somewhat after the reform. This persistent level difference can be viewed as reflecting differences in life-cycle stage. While DID estimation differences out these level effects, changes in group composition may affect the fixed-effects estimates in W-2SFE-DID, a possibility that aligns with the pre-trend patterns observed in the event-study analysis.

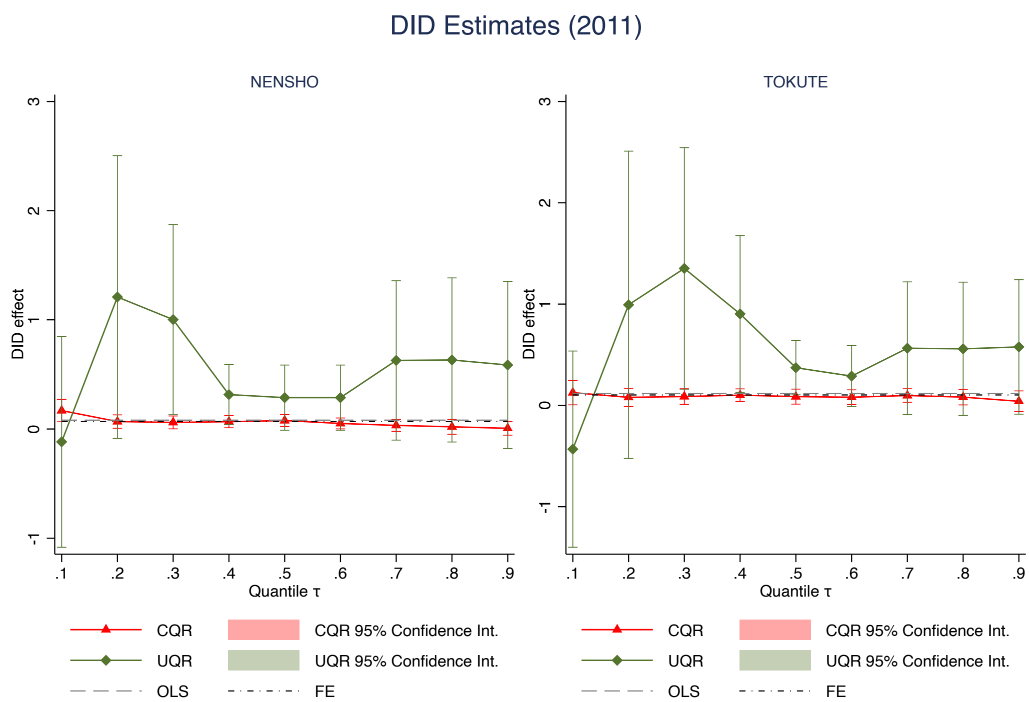
Taken together, the reduction in the Exemption for Specified Dependents—though it might superficially be regarded as a policy that lowers disposable income—turns out empirically to have positive effects on both the mean and the distribution of equivalent household disposable income. Given the evidence of pre-trends in the average-effect specifications, however, it is more appropriate to place greater weight on the CQR and UQR findings when discussing the distributional impact of this reform.

Table 4. Income redistribution effects of the 2011 tax reform

	W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
(a)	0.082***	0.069***	τ=0.1	0.169***	τ=0.1	-0.117
			τ=0.2	0.069**	τ=0.2	1.209*
			τ=0.3	0.060**	τ=0.3	1.002**
			τ=0.4	0.067**	τ=0.4	0.315**
			τ=0.5	0.077***	τ=0.5	0.288*
			τ=0.6	0.051**	τ=0.6	0.288*
			τ=0.7	0.03	τ=0.7	0.628*
			τ=0.8	0.020	τ=0.8	0.633*
			τ=0.9	0.007	τ=0.9	0.586
(b)	0.117***	0.103***	τ=0.1	0.126**	τ=0.1	-0.432
			τ=0.2	0.079*	τ=0.2	0.993
			τ=0.3	0.087**	τ=0.3	1.353**
			τ=0.4	0.101***	τ=0.4	0.903**
			τ=0.5	0.087**	τ=0.5	0.372***
			τ=0.6	0.081**	τ=0.6	0.289*
			τ=0.7	0.098***	τ=0.7	0.565*
			τ=0.8	0.082**	τ=0.8	0.558*
			τ=0.9	0.041	τ=0.9	0.577*

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 1. Income redistribution effects of the 2011 tax reform



Source: Authors' calculations based on JHPS/KHPS microdata.

Table 5. Mean pre-tax income of treatment and control groups, 2011 reform

	Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
				Unweighted	Weighted
(a)	Treatment	Pre	2008	625	638
			2009	599	616
			2010	619	643
		Post	2011	636	659
	Control	Pre	2008	450	481
			2009	435	458
			2010	429	448
		Post	2011	396	427
(b)	Treatment	Pre	2008	682	718
			2009	684	703
			2010	695	719
		Post	2011	669	712
	Control	Pre	2008	450	481
			2009	435	458
			2010	429	448
		Post	2011	396	427

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.2. 2013 reform

The 2013 tax reform consisted of three main elements: (1) introducing a cap of 2.45 million yen on the Deduction for Salary Income, thereby limiting the deduction for high-income employees with salary earnings above 15 million yen; (2) imposing a 2.1 percent Special Income Tax for Reconstruction on personal income tax liabilities for 25 years; and (3) adding 1,000 yen per year to the per-capita component of the local inhabitant tax. In this paper, we treat the marginal increase in the tax burden due to the cap on the Deduction for Salary Income as the primary exogenous policy shock.

We use the 2012–2014 JHPS/KHPS panel data, treating the 2012 and 2013 waves as pre-reform and the 2014 wave as post-reform. The treatment group consists of households whose equivalent household disposable income is at least about 1.57 million yen and whose main earner has salary income above 15 million yen. The control group consists of households with equivalent household disposable income at or above the same threshold whose main earner has non-salary income exceeding 15 million yen. We set the threshold of approximately 1.57 million yen equal to the lowest equivalent household disposable income observed among treated households and apply this threshold symmetrically to define the control group. This design is intended to select, as controls, households that are not directly affected by the reform but are comparable to treated households in terms of income level and household structure (especially the presence and composition of dependents).

As reported in Table 6, the W-OLS-DID estimate of the average effect is −0.580 (standard error 0.102), statistically significant at the 1 percent level, corresponding to a reduction of about 44.0 percent in equivalent household disposable income. The W-2SFE-DID estimate is −0.534 (standard error 0.129), also significant at the 1 percent level and implying a decrease of roughly 41.4 percent. The two estimates are consistent in sign and magnitude, and the introduction of household fixed effects confirms the robustness of the results. These findings indicate that the cap on the Deduction for Salary Income significantly reduced disposable income among high-income employee households.

Turning to distributional effects, the W-CQR-DID results show consistently negative and statistically significant effects across a wide range of quantiles, from $\tau = 0.1$ to $\tau = 0.8$. For representative quantiles, the estimates are −0.669 at $\tau = 0.2$ (1 percent significance, about −48.8 percent), −0.659 at $\tau = 0.5$ (1 percent, about −48.3 percent), and −0.547 at $\tau = 0.7$ (1 percent, about −42.1 percent). Thus, within the treated–control subsample, the reform generated substantial negative level effects from the lower to the upper-middle part of the conditional income distribution. At the top decile ($\tau = 0.9$), the coefficient is estimated at −0.329 (about −28.0 percent) but is not statistically significant, suggesting relatively imprecise estimates at the very top.

By contrast, the W-UQR-DID estimates for the unconditional distribution are negative at $\tau = 0.5$–0.9 (e.g., around −30.4 percent near $\tau = 0.5$ and about −43.7 percent at $\tau = 0.8$ and 0.9), but none of these coefficients is statistically significant. We thus do not find clear evidence of a downward shift in the unconditional quantiles of the population income distribution. In other words, relative to the strong level effects in the conditional distribution documented by CQR, the estimated structural changes in households' positions in the unconditional distribution (UQR) appear limited.

On the identification side, the event-study pre-trend tests show no statistically significant pre-trends for most estimators and quantiles, with the exception of W-CQR-DID at $\tau = 0.8$, which is significant at the 5 percent level. In W-UQR-DID, the joint F-tests for the lower quantiles ($\tau = 0.1, 0.2$) are not available (N/A) due to insufficient observations in some event-time cells, so those quantiles carry limited statistical information. Given the isolated significance at $\tau = 0.8$ and the instability of some tests, it is appropriate to interpret the results primarily based on the remaining quantiles and estimators and to treat the outlying quantile with caution. The relatively small sample size (N = 303) also calls for attention to the influence of outliers, particularly in the UQR estimates.

Pre-tax income comparisons further show that, after the reform, the mean income of the control group exceeds that of the treatment group (weighted means of approximately 29.26 million yen for the control group and 19.90 million yen for the treatment group), reversing the pre-reform ranking. Because we apply the same equivalent income threshold to both groups, the control group includes a non-negligible number of very high-income households. This compositional feature is consistent with the pattern that we find large negative average and CQR effects for treated high-income employees, while UQR detects no statistically significant change in relative positions within the overall income distribution.

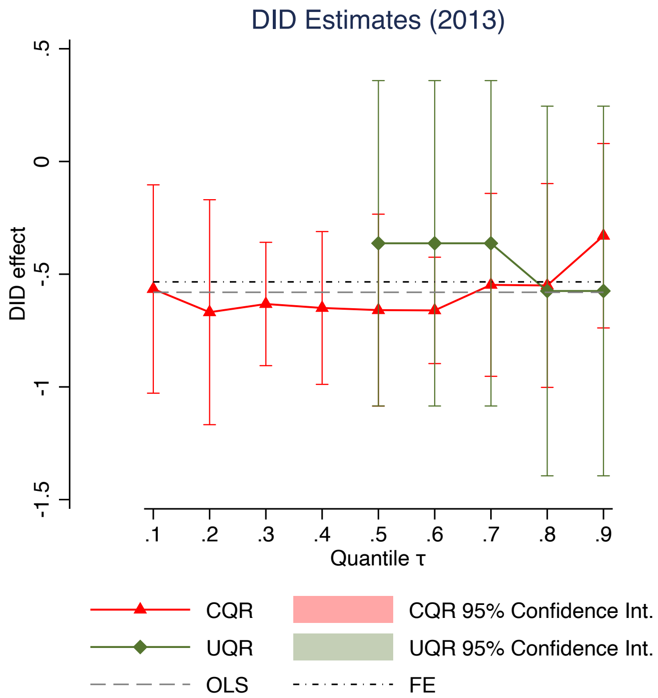
Overall, the introduction of the cap on the Deduction for Salary Income clearly reduced equivalent household disposable income among high-income employee households, both on average and across a wide range of conditional quantiles (τ = 0.1–0.8). At the same time, we find no statistically significant evidence that the policy changed treated households' relative positions in the unconditional income distribution. This suggests that the reform worked as intended by concentrating its impact on the level of disposable income among high-income wage earners, while leaving the overall ranking of households in the income distribution largely unchanged.

Table 6. Income redistribution effects of the 2013 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
-0.580***	-0.534***	τ=0.1	-0.566**	τ=0.1	–
		τ=0.2	-0.669***	τ=0.2	–
		τ=0.3	-0.632***	τ=0.3	–
		τ=0.4	-0.650***	τ=0.4	–
		τ=0.5	-0.659***	τ=0.5	-0.363
		τ=0.6	-0.661***	τ=0.6	-0.363
		τ=0.7	-0.547***	τ=0.7	-0.363
		τ=0.8	-0.550**	τ=0.8	-0.575
		τ=0.9	-0.329	τ=0.9	-0.575

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 2. Income redistribution effects of the 2013 tax reform



Notes: For W-UQR-DID, the τ = 0.1, 0.2, 0.3, and 0.4 quantiles are excluded from the analysis because the sample size is too small to ensure reliable estimates. Source: Authors' calculations based on JHPS/KHPS microdata.

Table 7. Mean pre-tax income of treatment and control groups, 2013 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2012	1779	1789
	Post	2013	1959	1990
Control	Pre	2012	1704	1674
	Post	2013	2664	2926

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.3. 2014 reform

The 10 percent reduced tax rate on dividends and capital gains (7 percent national income tax + 3 percent local inhabitant tax), introduced in 2003 with the aim of stimulating securities markets, was a temporary measure. This preferential treatment expired at the end of 2013, and from January 2014 the statutory 20 percent tax rate (15 percent national income tax + 5 percent inhabitant tax) was reinstated.

For the empirical analysis, we use the 2014 and 2015 waves of the JHPS/KHPS, treating the 2014 wave as pre-reform and the 2015 wave as post-reform. The treatment group consists of households whose annual capital income (interest, dividends, capital gains, etc.) exceeds 100,000 yen, while the control group consists of households whose annual capital income is at most 100,000 yen. By defining the treatment group based on a threshold at which the tax change is likely to be salient, we seek to identify the effective impact of the reform. As in the other sections, we estimate W-OLS-DID and W-2SFE-DID, and, to capture distributional heterogeneity, W-CQR-DID and W-UQR-DID for quantiles $\tau = 0.1, \cdots, 0.9$.

As reported in Table 8, the W-OLS-DID estimate of the average effect is 0.165 (standard error 0.071), statistically significant at the 5 percent level, while the W-2SFE-DID estimate is 0.107 (standard error 0.048), also significant at the 5 percent level. Both coefficients are positive and imply that equivalent household disposable income increased on average by about 17.9 percent and 11.3 percent, respectively, indicating a clear positive average effect.

Turning to distributional impacts, the W-CQR-DID results show statistically significant positive effects at several quantiles in the middle and upper parts of the distribution: 0.229 at $\tau = 0.5$ (5 percent significance, about 25.7 percent), 0.133 at $\tau = 0.6$ (5 percent, about 14.2 percent), and 0.284 at $\tau = 0.9$ (5 percent, about 32.8 percent). No statistically significant effects are detected at other quantiles. Thus, the reform appears to have raised equivalent household disposable income mainly around the middle and upper parts of the conditional distribution, suggesting that its impact was relatively stronger among higher-income or upper-middle households within the treated–control subsample.

By contrast, the W-UQR-DID results show no statistically significant effects at any unconditional quantile $\tau = 0.1, \cdots, 0.9$. Hence, we do not find evidence of statistically significant shifts in the unconditional quantiles of the population income distribution. The positive effects at the upper quantiles observed in W-CQR-DID therefore seem to operate primarily as level effects in the conditional distribution, conditional on covariates, rather than as structural changes in households' relative positions in the unconditional distribution.

Regarding identification, the event-study pre-trend tests for 2014 show no statistically significant pre-trends in W-OLS-DID, indicating no clear pre-reform trend differences in average outcomes. However, W-CQR-DID exhibits statistically significant pre-trends at the 5 percent level at $\tau = 0.6$ and $\tau = 0.7$, suggesting some imbalance in pre-reform dynamics around the upper-middle part of the conditional distribution. In contrast, W-UQR-DID shows no statistically significant pre-trends at any quantile, and W-2SFE-DID is only marginally significant at the 10 percent level, implying that some caution is warranted in interpreting average effects. Comparisons of pre-tax income indicate that, before the reform (2013), the treatment group had higher income than the control group, while after the reform both groups experienced income growth and the gap narrowed. In weighted means, the difference shrank from 7.72 million yen versus 5.22 million yen before the reform to 17.27 million yen versus 16.42 million yen after the reform. This pattern is consistent with the positive average effects and the concentration of positive impacts at middle and upper quantiles documented in the main results.

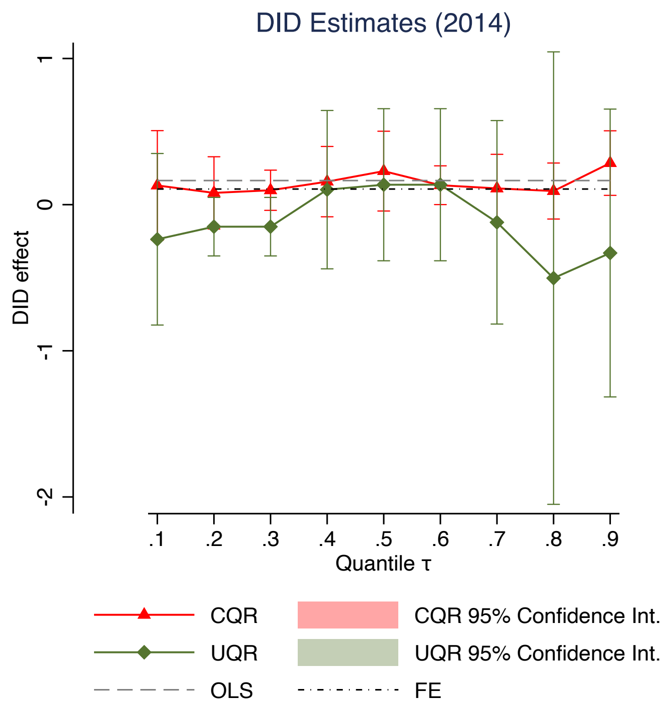
Overall, the 2014 reform is found to have raised equivalent household disposable income on average, with its effects concentrated in the middle and upper parts of the income distribution. At the same time, we do not detect statistically significant structural changes in households' relative positions in the unconditional income distribution; the reform did not generate large shifts in relative rankings or in the overall shape of the distribution. Accordingly, the 2014 reform can be interpreted as a policy that produced limited upward effects for specific income groups, rather than a reform that fundamentally altered the structure of the income distribution.

Table 8. Income redistribution effects of the 2014 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
0.165**	0.107**	τ=0.1	0.131	τ=0.1	-0.237
		τ=0.2	0.081	τ=0.2	-0.151
		τ=0.3	0.099	τ=0.3	-0.151
		τ=0.4	0.157	τ=0.4	0.103
		τ=0.5	0.229*	τ=0.5	0.136
		τ=0.6	0.133**	τ=0.6	0.136
		τ=0.7	0.111	τ=0.7	-0.121
		τ=0.8	0.093	τ=0.8	-0.503
		τ=0.9	0.284**	τ=0.9	-0.331

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 3. Income redistribution effects of the 2014 tax reform



Source: Authors' calculations based on JHPS/KHPS microdata.

Table 9. Mean pre-tax income of treatment and control groups, 2014 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2013	832	772
	Post	2014	1719	1727
Control	Pre	2013	467	522
	Post	2014	1686	1642

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.4. 2015 reform

The 2015 tax reform raised the top marginal personal income tax rate from 40 percent to 45 percent for the first time since 1999. The higher rate applies only to very high-income taxpayers with taxable income above 40 million yen.

For the empirical analysis, we use the 2015–2016 JHPS/KHPS panel data, treating 2015 as the pre-reform year and 2016 as the post-reform year. The treatment group consists of households whose equivalent household disposable income is at least about

11.79 million yen and whose main earner has taxable income above 40 million yen. The control group consists of households with equivalent household disposable income at or above the same threshold but without any member whose taxable income exceeds 40 million yen. This design compares households around a clear policy threshold, differing in whether they are subject to the higher top rate. We estimate W-OLS-DID and W-2SFE-DID, and to capture distributional heterogeneity we also estimate W-CQR-DID for quantiles $\tau = 0.1, \cdots, 0.9$. Because of the very small sample size (N = 104), we do not estimate W-UQR-DID in the main analysis.

As reported in Table 10, the W-OLS-DID estimate of the average effect is 0.071 (standard error 0.251) and the W-2SFE-DID estimate is 0.102 (standard error 0.250); neither is statistically significant at conventional levels.

Focusing on distributional effects, the W-CQR-DID estimates are generally positive across quantiles $\tau = 0.1, \cdots, 0.9$, but none of the coefficients is statistically significant. Thus, within this very small subsample, we do not detect statistically significant effects of the reform at any part of the conditional income distribution.

For W-UQR-DID, estimation fails at all quantiles: the joint F-tests and variance estimates are unstable due to the extremely small sample size, and we are therefore unable to draw conclusions about shifts in the unconditional income distribution.

From an identification perspective, the event-study pre-trend tests show that both W-OLS-DID and W-2SFE-DID are marginally significant at the 10 percent level, suggesting weak evidence of pre-trends in average outcomes. In contrast, most W-CQR-DID and W-UQR-DID quantiles do not exhibit statistically significant pre-trends, indicating limited evidence of systematic pre-reform imbalances across the distribution as a whole. However, joint F-tests for W-CQR-DID at $\tau = 0.1, 0.2, 0.9$ and for some W-UQR-DID quantiles are not available due to sparse observations in some relative-year cells, so the statistical evidence is inherently limited. Thus, some caution is warranted in interpreting average effects, although we do not find strong or pervasive evidence of pronounced pre-trends.

Pre-tax income comparisons further highlight the unusual nature of the sample: before the reform, the treatment group's mean income is extremely high (weighted means of 46.15 million yen for the treatment group versus 23.68 million yen for the control group), while after the reform both groups' mean incomes drop sharply (to 7.10 million yen and 5.24 million yen, respectively). This pattern underscores the small and highly selected nature of the sample at the very top of the distribution.

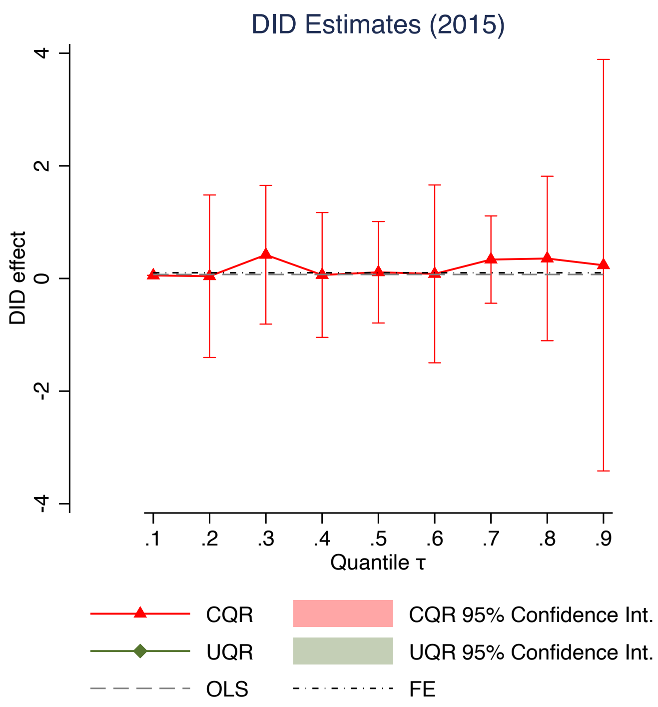
Overall, the 2015 reform targets a very narrow segment of extremely high-income taxpayers, and the small sample size leads to low statistical power and large standard errors. As a result, we do not find statistically significant effects on either average outcomes or the conditional income distribution. This should be interpreted not as evidence that the reform had no effect, but rather as reflecting the limited precision with which we can test the null hypothesis in the available data. From a policy perspective, reliably identifying the short-run impact of higher top marginal tax rates on disposable income would require a larger sample of very high-income households and a longer observation window.

Table 10. Income redistribution effects of the 2015 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
		τ=0.1	0.054	τ=0.1	
		τ=0.2	0.04	τ=0.2	
		τ=0.3	0.421	τ=0.3	
		τ=0.4	0.062	τ=0.4	
0.071	0.102	τ=0.5	0.11	τ=0.5	–
		τ=0.6	0.082	τ=0.6	
		τ=0.7	0.336	τ=0.7	
		τ=0.8	0.355	τ=0.8	
		τ=0.9	0.236	τ=0.9	

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 4. Income redistribution effects of the 2015 tax reform



Notes: W-UQR-DID cannot be estimated because of insufficient sample size. Source: Authors' calculations based on JHPS/KHPS microdata.

Table 11. Mean pre-tax income of treatment and control groups, 2015 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2014	4620	4615
	Post	2015	683	710
Control	Pre	2014	2421	2368
	Post	2015	468	524

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.5. 2016 reform

The January 2016 tax reform reduced the cap on the Deduction for Salary Income—initially introduced in 2013 as 2.45 million yen for salary income above 15 million yen—to 2.30 million yen, and simultaneously lowered the salary threshold at which the cap binds from 15 million yen to 12 million yen.

For the empirical analysis, we use the 2016–2017 JHPS/KHPS panel data, treating 2016 as the pre-reform year and 2017 as the post-reform year. The treatment group consists of households whose equivalent household disposable income is at least about 0.36 million yen and whose main earner's salary income exceeds 12 million yen, while the control group consists of households with equivalent household disposable income at or above the same threshold whose main earner has non-salary income exceeding 12 million yen. We estimate W-OLS-DID, W-2SFE-DID, W-CQR-DID ($\tau = 0.1, \cdots, 0.9$), and W-UQR-DID ($\tau = 0.1, \cdots, 0.9$).

As reported in Table 12, the average-effect estimates show sizable and statistically significant negative impacts. The W-OLS-DID coefficient is −0.540 (standard error 0.103), significant at the 1 percent level, and the W-2SFE-DID coefficient is −0.528 (standard error 0.098), also significant at the 1 percent level. These correspond to average declines in equivalent household disposable income of about 41.7 percent and 41.0 percent, respectively, indicating a substantial reduction in disposable income among high-income employee households.

Turning to distributional effects, the W-CQR-DID results show negative and statistically significant coefficients at all quantiles from $\tau = 0.1$ to $\tau = 0.9$. For example, the estimates are −0.691 at $\tau = 0.2$ (1 percent significance, about −49.9 percent), −0.529 at $\tau = 0.5$ (1 percent, about −41.1 percent), −0.459 at $\tau = 0.8$ (1 percent, about −36.8 percent), and −0.558 at $\tau = 0.9$ (1 percent, about −42.8 percent). These results indicate a broad-based reduction in the level of equivalent household disposable income across the entire high-income subsample, suggesting that the lower cap on the Deduction for Salary Income reduced disposable income for treated households throughout the conditional distribution.

By contrast, the W-UQR-DID results show negative coefficients for $\tau = 0.3$–0.9, but none of them is statistically significant (and $\tau = 0.1, 0.2$ cannot be estimated due to limited sample size). We therefore do not find evidence of statistically significant changes in households' relative positions in the unconditional income distribution. The large negative effects documented by W-CQR-DID are thus best interpreted as conditional level effects given covariates, while structural changes in the unconditional income distribution appear limited.

From an identification standpoint, the event-study pre-trend tests show no statistically significant pre-trends for W-OLS-DID or W-2SFE-DID. For W-CQR-DID, the pre-trend at $\tau = 0.7$ is marginally significant at the 10 percent level, so results for that quantile should be interpreted with some caution; for most other quantiles and for the average effects, concerns about pre-reform dynamics are relatively limited. Comparisons of pre-tax income show that, after the reform, the mean income of the treatment group rises to 16.88 million yen without weights and 21.02 million yen with weights, indicating a strong concentration of high-income households in the treated population. By contrast, the mean income of the control group falls sharply from 25.23 million yen before the reform to 13.30 million yen afterwards, implying an upward re-ranking of the treatment group and a downward shift of the control group. This reallocation is consistent with the broad negative effects in the average and CQR estimates and with the absence of statistically significant changes in the unconditional distribution in the UQR results. In other words, the tightening of the cap on the Deduction for Salary Income reduced conditional income levels among upper-income employees, while leaving the overall structure of the income distribution largely unchanged.

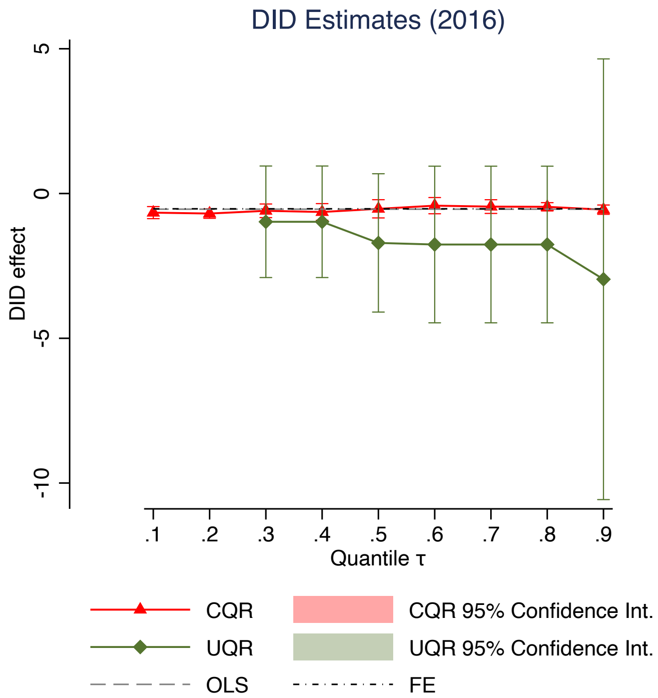
Overall, the 2016 reduction in the maximum Deduction for Salary Income led to a substantial reduction in equivalent household disposable income for high-income employee households, both on average and across the conditional distribution. Distributionally, the negative effects are observed throughout the high-income subsample, consistent with the policy design of concentrating the burden on upper-income groups. At the same time, we do not find statistically significant changes in households' relative positions in the unconditional income distribution.

Table 12. Income redistribution effects of the 2016 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
-0.540***	-0.528***	τ=0.1	-0.660***	τ=0.1	–
		τ=0.2	-0.691***	τ=0.2	–
		τ=0.3	-0.598***	τ=0.3	-0.976
		τ=0.4	-0.637***	τ=0.4	-0.976
		τ=0.5	-0.529***	τ=0.5	-1.706
		τ=0.6	-0.419***	τ=0.6	-1.761
		τ=0.7	-0.451***	τ=0.7	-1.761
		τ=0.8	-0.459***	τ=0.8	-1.761
		τ=0.9	-0.558***	τ=0.9	-2.961

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 5. Income redistribution effects of the 2016 tax reform



Notes: For W-UQR-DID, the τ = 0.1 and 0.2 quantiles are excluded from the analysis because the sample size is too small to ensure reliable estimates. Source: Authors' calculations based on JHPS/KHPS microdata.

Table 13. Mean pre-tax income of treatment and control groups, 2016 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2015	1634	1649
	Post	2016	1688	2102
Control	Pre	2015	2501	2523
	Post	2016	1427	1330

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.6. 2017 reform

The January 2017 tax reform further tightened the cap on the Deduction for Salary Income: the maximum deduction, which had been reduced to 2.30 million yen in the 2016 reform, was lowered again to 2.20 million yen, and the income threshold at which the cap binds was reduced from 12 million yen to 10 million yen.

For the empirical analysis, we use the 2017–2018 JHPS/KHPS panel data, treating 2017 as the pre-reform year and 2018 as the post-reform year. The treatment group consists of households whose equivalent household disposable income is at least about 0.36 million yen and whose main earner has salary income above 10 million yen. The control group consists of households whose equivalent household disposable income is at or above the same threshold and whose main earner has non-salary income above 10 million yen. The dependent variable is the logarithm of equivalent household disposable income. We estimate W-OLS-DID, W-2SFE-DID, W-CQR-DID ($\tau = 0.1, \cdots, 0.9$), and W-UQR-DID ($\tau = 0.1, \cdots, 0.9$).

As reported in Table 14, average-effect estimates show large and precisely estimated negative impacts. The W-OLS-DID coefficient is −0.571 (standard error 0.085), significant at the 1 percent level, and the W-2SFE-DID coefficient is −0.569 (standard error 0.087), also significant at the 1 percent level. These estimates correspond to average declines in equivalent household disposable income of about 43.5 percent and 43.4 percent, respectively, indicating that the further reduction in the cap generated substantial downward pressure on disposable income among high-income employee households.

Turning to distributional effects, the W-CQR-DID results show negative and statistically significant treatment effects at all quantiles $\tau = 0.1, \cdots, 0.9$. For example, we obtain −0.729 at $\tau = 0.1$ (1 percent significance, about −51.8 percent), −0.464 at $\tau = 0.5$ (1 percent, about −37.1 percent), and −0.496 at $\tau = 0.9$ (1 percent, about −39.1 percent). These results indicate a broad-based decline in the level of equivalent household disposable income across the entire conditional distribution of the high-income subsample: the reform had substantial negative effects from the lower to the upper tail of the treated–control distribution.

The W-UQR-DID estimates show negative and statistically significant effects at the median and higher quantiles. In particular, we find significant downward shifts in the unconditional quantiles at $\tau = 0.5, 0.7, 0.8$, and 0.9 (with coefficients of −1.367, −1.872, −2.064, and −2.305, respectively), while estimates for the lowest deciles ($\tau = 0.1, 0.2$) are not statistically significant. These findings suggest that the unconditional income distribution shifts downward from the median upward, in contrast to the 2016 reform, for which UQR estimates were not statistically significant. This pattern is consistent with the idea that lowering the salary threshold to 10 million yen expanded the reach of the policy and generated a downward shift in the distributional position of high-income employee households.

From an identification perspective, the event-study pre-trend tests indicate no statistically significant pre-trends for W-OLS-DID, W-2SFE-DID, or W-UQR-DID. For W-CQR-DID, the coefficient at $\tau = 0.3$ is marginally significant at the 10 percent level, and at $\tau = 0.9$ we find a strongly significant pre-trend at the 1 percent level. The estimates for these quantiles should therefore be interpreted with particular caution, whereas concerns about pre-reform dynamics are relatively limited for most other quantiles and for the average effects.

Pre-tax income comparisons show that, before the reform, the control group had higher mean income than the treatment group (weighted means of 21.82 million yen for the control group versus 13.70 million yen for the treatment group). After the reform, the relationship reverses: the treatment group's mean income rises to 15.66 million yen, while the control group's mean income is 13.77 million yen. This re-ranking reflects the expansion of the policy's coverage as the salary income threshold for the cap was lowered, effectively broadening the range of high-income wage earners classified as treated. The resulting reallocation in the upper part of the distribution is consistent with the large negative average and CQR effects and with the significant downward shifts in unconditional quantiles at and above the median in the UQR results.

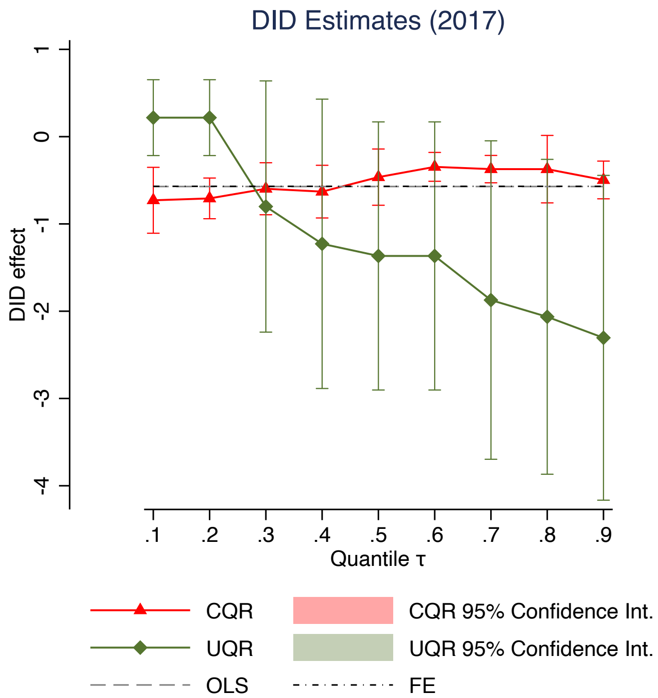
Overall, the 2017 reduction in the maximum Deduction for Salary Income led to large reductions in equivalent household disposable income for high-income employee households, both on average and across the conditional distribution, and also generated statistically significant downward shifts in the unconditional distribution at the median and above. This is consistent with the policy design: by lowering the salary threshold from 12 million to 10 million yen, the reform extended the burden of deduction cuts to a broader segment of the upper income distribution and strengthened the redistributive impact targeted at high-income wage earners. At the same time, results at quantiles with significant pre-trends (particularly $\tau = 0.3$) should be interpreted cautiously.

Table 14. Income redistribution effects of the 2017 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
-0.571***	-0.569***	τ=0.1	-0.729***	τ=0.1	0.218
		τ=0.2	-0.707***	τ=0.2	0.218
		τ=0.3	-0.597***	τ=0.3	-0.801
		τ=0.4	-0.630***	τ=0.4	-1.228
		τ=0.5	-0.464***	τ=0.5	-1.367*
		τ=0.6	-0.346***	τ=0.6	-1.367*
		τ=0.7	-0.372***	τ=0.7	-1.872**
		τ=0.8	-0.372*	τ=0.8	-2.064**
		τ=0.9	-0.496***	τ=0.9	-2.305**

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 6. Income redistribution effects of the 2017 tax reform



Source: Authors' calculations based on JHPS/KHPS microdata.

Table 15. Mean pre-tax income of treatment and control groups, 2017 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2016	1350	1370
	Post	2017	1551	1566
Control	Pre	2016	2099	2182
	Post	2017	1356	1377

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.7. 2018 reform

The 2018 tax reform introduced several changes to the Exemption for Spouses and the Special Exemption for Spouses. First, the upper bound on the spouse's earnings for the full Exemption for Spouses (38,000 yen) was relaxed from 1.03 million yen to 1.5 million yen. Second, the phase-out range for the Special Exemption for Spouses (up to 38,000 yen) was expanded to cover spouse earnings from 1.41 to 2.01 million yen. In addition, a new phase-out with respect to the primary taxpayer's income was introduced: the deduction amount is gradually reduced when the primary taxpayer's total income exceeds 11.2 million yen and is fully eliminated when income exceeds 12.2 million yen. The reform was intended to shift the "earnings wall" outward, mitigating labor supply disincentives for secondary earners, while partly strengthening redistribution by reducing deductions for high-income taxpayers.

We use the 2018–2019 JHPS/KHPS panel data, treating 2018 as the pre-reform year and 2019 as the post-reform year. The treatment group is defined as households with equivalent household disposable income above roughly 33,000 yen, in which the primary earner's annual income does not exceed 11.2 million yen and the spouse's annual income lies between 1.03 and 1.5 million yen. The control group consists of households with equivalent household disposable income above the same threshold, with primary earner income no higher than 11.2 million yen and spouse income at or below 1.03 million yen. This design identifies households directly affected by the expansion of the spousal deduction. We estimate W-OLS-DID, W-2SFE-DID, W-CQR-DID ($\tau = 0.1, \cdots, 0.9$), and W-UQR-DID ($\tau = 0.1, \cdots, 0.9$) using log equivalent household disposable income as the dependent variable.

As reported in Table 16, the W-OLS-DID estimate of the average effect is 0.083 (standard error 0.032), statistically significant at the 5 percent level, implying an average increase in equivalent household disposable income of about 8.6 percent. By contrast, the W-2SFE-DID estimate is 0.039 (standard error 0.028) and is not statistically significant, so the evidence for an average effect is somewhat weaker once household fixed effects are controlled for.

Turning to distributional effects, the W-CQR-DID results show positive effects concentrated at the lower part of the conditional distribution. The estimates are 0.166 at $\tau = 0.1$ (10 percent significance, about 18.0 percent), 0.109 at $\tau = 0.2$ (10 percent, about 11.5 percent), and 0.083 at $\tau = 0.3$ (5 percent, about 8.6 percent), while coefficients at the middle (τ = 0.4–0.6) and upper (τ = 0.7–0.9) quantiles are not statistically significant. These results indicate that the reform predominantly raised disposable income among households located in the lower part of the treated–control conditional income distribution, with effects tapering off toward the middle and top.

The W-UQR-DID estimates, in contrast, show positive and statistically significant effects across a wide range of unconditional quantiles from $\tau = 0.3$ to $\tau = 0.9$ (for example, 1.868 at $\tau = 0.3$, 0.655 at $\tau = 0.4$, 0.621 at $\tau = 0.5$, 0.793 at $\tau = 0.6$, 0.572 at $\tau = 0.7$, 0.514 at $\tau = 0.8$, and 0.629 at $\tau = 0.9$). Because UQR coefficients capture shifts in the unconditional quantiles themselves, they are best interpreted in terms of sign and the range of affected quantiles rather than in terms of their absolute magnitudes. The results suggest that the unconditional distribution of equivalent household disposable income shifts upward from around the 30th percentile through the upper tail. By contrast, the lowest deciles ($\tau = 0.1, 0.2$) are not statistically significant, indicating limited structural change at the very bottom of the distribution.

From an identification perspective, the event-study pre-trend tests detect no statistically significant pre-trends for W-OLS-DID, W-2SFE-DID, or any of the W-CQR-DID quantiles, providing strong support for the parallel trends assumption in these specifications. For W-UQR-DID, the pre-trend at $\tau = 0.6$ is marginally significant at the 10 percent level, suggesting a possible weak pre-trend around the middle of the unconditional distribution; interpretation of that particular quantile should therefore be somewhat cautious. Overall, however, pre-reform differences in trends appear limited, and the DID identifying assumptions seem reasonably satisfied. Comparisons of pre-tax income also show that the treatment group consistently has higher average income than the control group before and after the reform, and that this ranking is stable, with only minor effects of the weights.

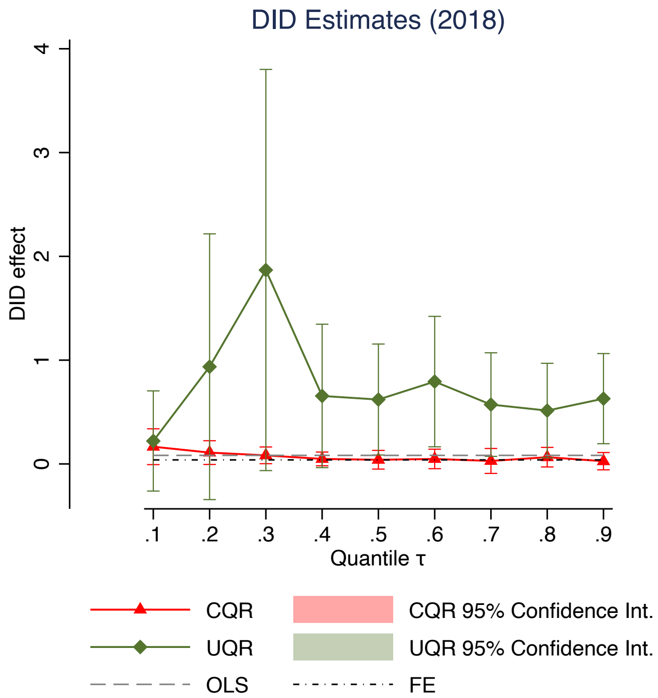
Taken together, the 2018 expansion of spousal-related deductions increased equivalent household disposable income on average (in W-OLS-DID) and had particularly pronounced positive effects at the lower end of the conditional income distribution ($\tau \leq 0.3$). At the same time, UQR results indicate broad upward shifts in the unconditional distribution at and above the median, implying that the policy's effects propagated across a relatively wide range of the targeted population. Combining the CQR and UQR findings, we interpret the reform as generating both level improvements for lower-income treated households and upward shifts in the relative distributional position for households from the middle upward. From a policy design perspective, the results are consistent with the intent of reducing the effective tax burden via more generous spousal deductions, with redistributive gains concentrated in the lower part of the distribution but also extending meaningfully into the middle.

Table 16. Income redistribution effects of the 2018 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
0.083**	0.039	τ=0.1	0.166*	τ=0.1	0.221
		τ=0.2	0.109*	τ=0.2	0.937
		τ=0.3	0.083**	τ=0.3	1.868*
		τ=0.4	0.048	τ=0.4	0.655*
		τ=0.5	0.04	τ=0.5	0.621**
		τ=0.6	0.048	τ=0.6	0.793**
		τ=0.7	0.029	τ=0.7	0.572**
		τ=0.8	0.065	τ=0.8	0.514**
		τ=0.9	0.026	τ=0.9	0.629***

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 7. Income redistribution effects of the 2018 tax reform



Source: Authors' calculations based on JHPS/KHPS microdata.

Table 17. Mean pre-tax income of treatment and control groups, 2018 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2017	484	514
	Post	2018	1283	1285
		2019	463	486
Control	Pre	2017	412	444
	Post	2018	1179	1187
		2019	372	413

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

4.2.8. 2020 reform

The 2020 tax reform reorganized income deductions with the stated aim of making the personal income tax system more neutral with respect to work arrangements. In particular, the Basic Exemption was uniformly increased by 100,000 yen, while the Deduction for Salary Income and the Deduction for Public and Other Pensions were each reduced by the same amount. In addition, a new "Income Adjustment Deduction" of up to 150,000 yen was introduced for taxpayers with annual earnings above 8.5 million yen who have dependent family members or disabled household members.

For the empirical analysis, we use the 2019–2021 JHPS/KHPS panel data, treating the 2019 and 2020 waves as pre-reform and the 2021 wave as post-reform. The treatment group consists of households whose equivalent household disposable income is at least about 0.36 million yen and whose main earner has salary income above 8.5 million yen. The control group consists of households with equivalent household disposable income above the same threshold whose main earner has non-salary income exceeding 8.5 million yen. We estimate W-OLS-DID, W-2SFE-DID, W-CQR-DID ($\tau = 0.1, \cdots, 0.9$), and W-UQR-DID ($\tau = 0.1, \cdots, 0.9$) using log equivalent household disposable income as the dependent variable.

As reported in Table 18, the average-effect estimates show sizable and precisely estimated negative impacts. The W-OLS-DID coefficient is −0.293 (standard error 0.068), significant at the 1 percent level, and the W-2SFE-DID coefficient is −0.3 (standard error 0.060), also significant at the 1 percent level. These estimates correspond to average declines in equivalent household disposable income of about 25.4 percent and 25.9 percent, respectively, indicating that, on net, the reform reduced disposable income among high-income households once all deduction changes are taken into account.

Turning to distributional effects, the W-CQR-DID results show negative and statistically significant coefficients over a wide range of quantiles, from $\tau = 0.2$ to $\tau = 0.9$. Representative estimates include −0.259 at $\tau = 0.5$ (1 percent significance, about −22.8 percent), −0.302 at $\tau = 0.6$ (1 percent, about −26.0 percent), −0.448 at $\tau = 0.8$ (1 percent, about −36.1 percent), and −0.326 at $\tau = 0.9$ (5 percent, about −27.8 percent). The lowest decile ($\tau = 0.1$) is not statistically significant. These results indicate that the reform significantly reduced conditional disposable income in the middle and upper parts of the treated–control distribution, consistent with the view that deduction cuts primarily burdened upper-income households.

The W-UQR-DID results paint a complementary picture for the unconditional distribution. At the lower quantiles $\tau = 0.2$ and $\tau = 0.3$, we find small but statistically significant positive effects (at the 10 percent level), whereas at the top decile $\tau = 0.9$ we find a statistically significant negative effect (also at the 10 percent level). Other quantiles are not statistically significant. Taken together, these patterns suggest a slight upward shift at the lower part of the unconditional distribution and a downward shift at the very top, implying a compression of the income distribution (with a reduction in the mass at the very top).

From an identification standpoint, the event-study pre-trend tests show no statistically significant pre-reform trends in any of the main specifications or quantiles; the parallel trends assumption is therefore broadly supported. This strengthens the credibility of the DID estimates. Comparisons of pre-tax income show that, after the reform, the control group's mean income clearly exceeds that of the treatment group (weighted means of 18.46 million yen for the control group and 12.27 million yen for the treatment group). Before the reform, the ranking between treatment and control groups fluctuated across years: in some years (e.g., 2018) the treatment group had higher income, while in others (e.g., 2019) the control group did. By 2020, however, the ordering between treatment and control groups had clearly reversed. This re-ranking is consistent with the negative CQR effects for the middle and upper parts of the distribution and the UQR pattern of small positive effects at the lower quantiles and negative effects at the top, indicating that the reform compressed the distribution by increasing the burden at the top while slightly easing conditions at the lower end. The influence of survey weights is moderate, and the signs of the main effects are robust.

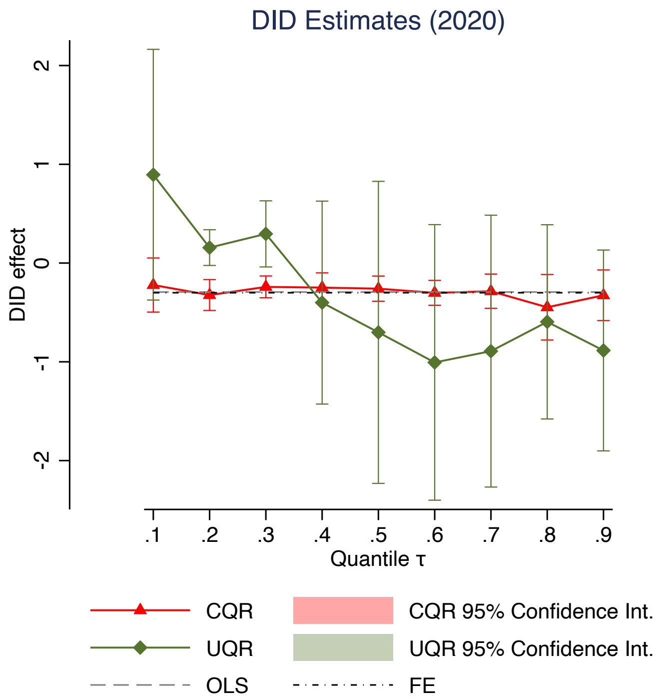
Overall, the 2020 reform combines a tax-cut component—an increase in the Basic Exemption that benefits taxpayers with non-salary and non-pension income—with tax-increase components that reduce the Deduction for Salary Income and the Deduction for Public and Other Pensions at higher incomes. Empirically, we find clear downward effects on average and on the conditional distribution, especially for the middle and upper parts of the income distribution, together with modest upward shifts at lower unconditional quantiles and downward shifts at the very top. These patterns suggest that the deduction cuts for high-income earners more than offset the gains from the higher Basic Exemption at the top, while the Basic Exemption increase partly benefited lower-income taxpayers such as self-employed workers and freelancers. In this sense, the 2020 reform appears to have strengthened income redistribution by compressing the distribution—reducing disposable income at the top while preventing deterioration at the bottom—even though the UQR estimates are relatively noisy and should be interpreted primarily in terms of the sign and the range of significant quantiles rather than in terms of their exact magnitudes.

Table 18. Income redistribution effects of the 2020 tax reform

W-OLS-DID	W-2SFE-DID	W-CQR-DID		W-UQR-DID	
-0.293***	-0.300***	τ=0.1	-0.222	τ=0.1	0.895
		τ=0.2	-0.324***	τ=0.2	0.157*
		τ=0.3	-0.241***	τ=0.3	0.295*
		τ=0.4	-0.248***	τ=0.4	-0.4
		τ=0.5	-0.259***	τ=0.5	-0.702
		τ=0.6	-0.302***	τ=0.6	-1.006
		τ=0.7	-0.285***	τ=0.7	-0.892
		τ=0.8	-0.448***	τ=0.8	-0.595
		τ=0.9	-0.326**	τ=0.9	-0.885*

Notes: * p<0.1, ** p<0.05, *** p<0.01. Source: Authors' calculations based on JHPS/KHPS microdata.

Figure 8. Income redistribution effects of the 2020 tax reform



Source: Authors' calculations based on JHPS/KHPS microdata.

Table 19. Mean pre-tax income of treatment and control groups, 2020 reform

Group	Pre / Post	Income year	Mean pre-tax income (10,000 yen)	
			Unweighted	Weighted
Treatment	Pre	2018	1166	1149
		2019	1179	1186
	Post	2020	1226	1227
Control	Pre	2018	1063	1070
		2019	1299	1209
	Post	2020	1779	1846

Notes: Weighted and unweighted means are reported. Figures are rounded to the nearest unit. Source: Authors' calculations based on JHPS/KHPS microdata.

5. Conclusion

This paper examined how a sequence of personal income tax reforms implemented in Japan during the 2010s affected equivalent household disposable income and the income distribution, focusing on both average and distributional effects. Using microdata from the 2009–2021 waves of the Japan Household Panel Survey (JHPS/KHPS), we analyze personal income taxes, local inhabitant taxes, and social security contributions and constructed equivalent household disposable income by a microsimulation. We then estimated reform-specific effects using Weighted OLS-DID, Two-Step Weighted FE-DID, Weighted Conditional Quantile Regression DID (W-CQR-DID), and Weighted Unconditional Quantile Regression DID (W-UQR-DID), and assessed the parallel trends assumption using event-study specifications.

Our results show that the direction and magnitude of redistribution effects differed markedly across reforms, reflecting differences in stated objectives, targeted income ranges, and the design of deduction and benefit provisions. The 2011 reform (abolition of the Exemption for Young Dependents and reduction of the Exemption for Specified Dependents) increased average equivalent household disposable income, with particularly pronounced gains in the lower and middle parts of the distribution. This pattern is consistent with the view that shifting from deduction-based support to benefit-based support strengthened redistribution toward lower- and middle-income households. By contrast, the 2013, 2016, and 2017 reforms that introduced and then tightened caps on the Deduction for Salary Income targeted high-income employees and generated large negative effects on equivalent household disposable income in both mean outcomes and the conditional distribution. For 2016—and especially for 2017—we find statistically significant declines across a wide range of conditional quantiles; for 2017, we also detect downward shifts at the top of the unconditional distribution. These results indicate that the deduction caps concentrated tax increases on upper-income households and strengthened redistribution at the top of the income distribution.

The 2014 abolition of the reduced tax rate on dividends and capital gains produced positive effects on equivalent household disposable income at some middle and upper conditional quantiles in W-CQR-DID, while W-UQR-DID showed no statistically significant effects at unconditional quantiles. This pattern suggests level effects within the treated subsample but limited evidence of a structural change in the overall income distribution. For the 2015 increase in the top marginal income tax rate, the treated group is extremely narrow, and sample sizes are small; accordingly, we do not detect statistically significant effects in any specification. In contrast, the 2018 expansion of the Exemption for Spouses and the Special Exemption for Spouses yield statistically significant positive effects on both mean outcomes and the distribution: CQR estimates show sizable gains in the lower part of the conditional distribution, while UQR estimates indicate upward shifts at and above the median, consistent with a broad-based improvement for treated households. Finally, the 2020 reform—combining an increase in the Basic Exemption with reductions in the Deduction for Salary Income and the Deduction for Public and Other Pensions and the introduction of the Income Adjustment Deduction—produced clearly negative average and conditional effects concentrated in the middle and upper parts of the distribution, together with small positive effects at lower unconditional quantiles and negative effects at the very top. This pattern is consistent with modest compression of the distribution through reduced disposable income at the top and slight improvement at the bottom.

Taken together, benefit-expanding or tax-relief reforms (2011, 2018) raised disposable income for lower- and middle-income households, whereas reforms that capped or reduced deductions (2013, 2016, 2017, 2020) lowered disposable income primarily in the upper part of the distribution. These findings suggest that, while Japan's personal income tax system continued to emphasize horizontal equity, the sequence of reforms—though not necessarily coordinated as a single package—tended to target specific income ranges and, in aggregate, strengthened the redistributive role of the personal income tax.

The event-study analyses provide additional support for a causal interpretation. For most reforms, the data are broadly consistent with parallel trends, and the DID estimates appear reasonably well identified. In a limited number of cases (2011, 2014, 2016, and 2017), we detect statistically significant pre-trends in some specifications or quantiles; in those instances, we interpret estimates more cautiously and emphasize patterns that are robust across estimators and across the distribution. Moreover, evidence on pre-tax income patterns suggests that the composition of treatment and control groups is broadly stable over time, which further supports the plausibility of the identifying assumptions.

In an international context, Japan's reforms during the 2010s align with a broader trend toward strengthening progressivity at the top of the distribution. Caps on the Deduction for Salary Income and increases in the top marginal tax rate increased tax burdens for upper-income groups. At the same time, our UQR results suggest that structural changes in the overall income distribution were often modest and concentrated in the upper tail for some reforms, in contrast to policies such as refundable tax credits (e.g., the EITC in the United States) that can generate broader gains across the lower and middle parts of the distribution. Although we do not explicitly identify behavioral channels, the concentration of effects in the upper tail is consistent with mechanisms emphasized in the literature, including taxable income elasticities and income shifting among top earners.

Our results point to several policy implications. First, benefit-type instruments and expansions of deductions targeted at lower-income households (as in 2011 and 2018) can strengthen redistribution, particularly toward the bottom of the distribution. Second, caps or cuts to deductions for upper-income groups (2013, 2016, 2017, 2020) enhance progressivity by reducing disposable income at the top, but the incidence can extend into upper-middle income ranges depending on the placement of thresholds and caps. Third, "balancing" reforms that combine tax reductions and tax increases (as in 2020) can compress the distribution by simultaneously reducing disposable income at the top and modestly supporting the bottom.

Overall, Japan's 2010s personal income tax reforms—though not all explicitly motivated by redistribution—altered the redistributive profile of the system through a sequence of changes that combined deduction reductions at the top with targeted benefit expansions and deduction increases. Future research could build on these findings by explicitly identifying behavioral responses (including labor supply adjustments and income shifting) and by using structural or dynamic approaches to evaluate reforms over longer horizons while jointly accounting for behavioral margins and distributional outcomes.

Appendix A. Methodological details

A.0 Notation and setup

Let Y_{it} denote the log of equivalent household disposable income for household i in year t. Let $\boldsymbol{X}_{it}$ be a $K \times 1$ vector of covariates (including year fixed effects, treatment / event-time indicators, and their interactions as specified below), and let $w_i > 0$ denote survey (population-restoration) weights. The quantile index is $\tau \in (0, 1)$.

Define the check (pinball) loss $\rho_\tau(u) = u(\tau - \mathbf{1}\{u < 0\})$ and the score $\psi_\tau(u) = \tau - \mathbf{1}\{u \leq 0\}$. Let F_Y and f_Y be the unconditional cumulative distribution function (CDF) and probability density function (PDF) of Y, and let $q_t = \inf\{y: F_Y(y) \geq \tau\}$ be the τ-th unconditional quantile. For conditional objects, $F_{Y|X}$, $f_{Y|X}$, and $q_t(\boldsymbol{x})$ denote the CDF, PDF, and conditional quantile of $Y|X = \boldsymbol{x}$, respectively.

Unless otherwise noted, inference is clustered at the household level.

A.1 Conditional quantile regression (CQR)

CQR estimates the τ-th conditional quantile function $Q_\tau(Y_{it}|\boldsymbol{X}_{it}) = \boldsymbol{X}_{it}^\top \beta_\tau^C$ by minimizing a weighted sum of check losses (Koenker and Bassett, 1978; Koenker, 2005; Buchinsky, 1994):

$$\hat{\beta}_\tau^C = \arg\min_{\beta} \sum_{i,t} w_i \, \rho_\tau(Y_{it} - \boldsymbol{X}_{it}^\top \beta). \qquad \text{(A.1a)}$$

In our DID/event-study specifications, $\boldsymbol{X}_{it}$ contains: (i) year fixed effects, (ii) treatment indicators and event-time dummies $D_{it}^{(k)}$ for relative year k (with $k = -1$ omitted), and (iii) their interactions, plus demographic controls. The DID effect at quantile τ is read from the relevant interaction coefficient(s) in $\hat{\beta}_\tau^C$. Standard errors for CQR are obtained via household-cluster bootstrap, which is recommended for quantile models due to potential small-sample/tail instabilities in asymptotic formulas (Koenker and Bassett, 1978; Koenker, 2005).

A.2 Unconditional quantile regression (UQR)

UQR evaluates policy effects on the unconditional quantile q_τ by regressing the recentered influence function (RIF) for the quantile on $\boldsymbol{X}$ (Firpo, Fortin, and Lemieux, 2009). The influence function of the quantile is

$$\mathrm{IF}(Y; q_\tau) = \frac{\tau - \mathbf{1}\{Y \leq q_\tau\}}{f_Y(q_\tau)}, \qquad \mathrm{RIF}(Y; q_\tau) = q_\tau + \mathrm{IF}(Y; q_\tau).$$

The UQR estimator solves the weighted least-squares problem

$$\hat{\beta}_\tau^U = \arg\min_{\beta} \sum_{i,t} w_i \, (\mathrm{RIF}(Y_{it}; \hat{q}_\tau) - \boldsymbol{X}_{it}^\top \beta)^2 \ , \tag{A.1b}$$

where $\hat{q}_\tau$ and $\hat{f}_Y(\hat{q}_\tau)$ are obtained in a first step (e.g., empirical quantile and kernel density at $\hat{q}_\tau$). Under standard conditions, coefficients on policy regressors in $\hat{\beta}_\tau^U$ identify unconditional quantile partial effects (UQPEs) at τ (Firpo, Fortin, and Lemieux, 2009). In our application, we report analytic (sandwich) standard errors that account for first-step estimation; cluster-robust versions are used throughout.

Connection between UQPE and CQPE (Proposition 1 in FFL, 2009)

Let $\xi_\tau(\boldsymbol{X})$ denote the conditional quantile level that corresponds to the unconditional τ-quantile, i.e., $\xi_\tau(\boldsymbol{x})$ satisfies $q_\tau = \int q_{\xi_\tau(\boldsymbol{x})} (\boldsymbol{x}) \, dF_X(\boldsymbol{x})$ in Firpo, Fortin, and Lemieux (2009). Then the UQPE at τ can be written as a density-weighted average of CQPEs evaluated at $\xi_\tau(\boldsymbol{X})$:

$$\mathrm{UQPE}(\tau) = \mathbb{E}[w_\tau(\boldsymbol{X}) \cdot \mathrm{CQPE}(\xi_\tau(\boldsymbol{X})|\boldsymbol{X})] , \quad w_\tau(\boldsymbol{X}) \propto \frac{f_{Y|X}(q_t|\boldsymbol{X})}{f_Y(q_t)} \ . \tag{A.2}$$

This clarifies why CQR and UQR need not match numerically: CQR targets conditional quantiles, while UQR aggregates conditional effects into an effect on the unconditional quantile via density-based weighting.

A.3 Event-study DID for CQR and UQR

For pre-trend assessment and dynamic effects, we adopt an event-time basis with $k \in \{L, \cdots, -2, 0, 1, \cdots, H\}$ (excluding $k = -1$) and include year fixed effects.

CQR event-study (estimated by (A.1a)).

$$Q_\tau(Y_{it}|\boldsymbol{X}_{i\tau}) = \alpha_\tau^C + \sum_{k=L, k\neq -1}^{H} \beta_{\tau,k}^C \, D_{it}^{(k)} + \boldsymbol{c}_{it}^\top \boldsymbol{\theta}_\tau^C \ , \tag{A.3a}$$

where $\boldsymbol{c}_{it}$ collects controls and fixed effects. The joint no pre-trend null is $H_0\colon \beta_{\tau,k}^C = 0$ for all $k < 0$.

UQR event-study (estimated by (A.1b)).

$$\mathbb{E}[\mathrm{RIF}(Y_{it}; q_\tau)|\boldsymbol{X}_{it}] = \alpha_\tau^U + \sum_{k=L,k\neq-1}^{H} \beta_{\tau,k}^U D_{it}^{(k)} + \boldsymbol{c}_{it}^\top \boldsymbol{\theta}_\tau^{\mathrm{U}} \quad , \tag{A.3b}$$

The same joint pre-trend test applies to $\{\beta_{\tau,k}^U : k < 0\}$.

A.4 Weighting and inference

We apply design weights w_i directly in the CQR objective (A.1a) and in the UQR least-squares criterion (A.1b), consistent with survey-weight practice for population-representative quantiles and RIF moments (Buchinsky, 1994; Koenker, 2005; Firpo, Fortin, and Lemieux, 2009). For CQR, we use household-cluster bootstrap for standard errors and confidence intervals. For UQR, we report analytic (sandwich) standard errors that account for the first-step estimation of q_τ and $f_Y(q_\tau)$; all specifications are cluster-robust at the household level.

Appendix B. Additional empirical results

Table B1. Descriptive statistics

Variable	Obs	Mean	Std. dev.	Min	Max
Log equivalent household disposable income	77,273	5.724208	0.6076782	-4.62244	9.040685
Age of the primary earner	83,025	55.22338	15.15693	0	109
Age of the primary earner, squared	83,025	3279.351	1669.612	0	11881
Sex of the primary earner	84,222	0.6494265	0.4771524	0	1
Educational attainment of the primary earner	77,223	5.835036	1.430653	0	11
Employment status of the primary earner	65,396	3.505811	2.567307	1	9
Industry of the primary earner	66,675	9.31673	5.012444	0	18
Marital status (dummy) of the primary earner	83,143	0.813923	0.3891711	0	1
Number of tax dependents in the household	77,462	1.438202	1.30893	0	9
reform_2011	27,009	0.2479914	0.4318548	0	1
treated_2011_Nensho	64,638	0.3373093	0.4727951	0	1
did_2011_Nensho	21,297	0.089637	0.2856678	0	1
treated_2011_Tokute	54,819	0.2186103	0.4133074	0	1
did_2011_Tokute	18,491	0.0586772	0.2350259	0	1
reform_2013	18,517	0.3062051	0.4609284	0	1
treated_2013	1,380	0.5731884	0.4947938	0	1
did_2013	339	0.1858407	0.3895531	0	1
reform_2014	10,992	0.4841703	0.4997721	0	1
treated_2014	66,309	0.0351536	0.1841693	0	1
did_2014	10622	0.0174167	0.1308241	0	1
reform_2015	10315	0.4840524	0.4997698	0	1
treated_2015	801	0.0474407	0.2127124	0	1
did_2015	120	0.025	0.1567796	0	1
reform_2016	9619	0.4809232	0.4996619	0	1
treated_2016	2819	0.7701313	0.4208229	0	1
did_2016	420	0.3809524	0.4862001	0	1
reform_2017	8917	0.4812157	0.499675	0	1
treated_2017	5182	0.8249711	0.3800285	0	1
did_2017	752	0.4135638	0.4927999	0	1
reform_2018	10461	0.5898098	0.4918916	0	1
treated_2018	41023	0.133998	0.3406543	0	1
did_2018	6480	0.0726852	0.2596391	0	1
reform_2020	16457	0.2927022	0.4550167	0	1
treated_2020	7915	0.8590019	0.3480416	0	1
did_2020	1921	0.2915148	0.4545784	0	1
Region	84942	4.222787	1.83975	1	9
Year	84942	2014.542	4.434007	2008	2022

Source: Authors' calculations based on JHPS/KHPS microdata.

Table B2. Estimation results from Weighted OLS-DID

(dependent variable: log equivalent household disposable income)

	2011 reform (a)	2011 reform (b)	2013 reform
Reform dummy (post = 1)	-0.101***	-0.088***	0.627***
	(0.022)	(0.022)	(0.107)
Treatment dummy	-0.078***	-0.016	0.161**
	(0.025)	(0.029)	(0.079)
Reform × Treatment (DID effect)	0.082***	0.117***	-0.580***
	(0.022)	(0.029)	(0.102)
Age of the primary earner	0.026***	0.029***	-0.075***
	(0.004)	(0.004)	(0.020)
Age of the primary earner, squared	-0.000***	-0.000***	0.001***
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.008	0.025	-0.014
	(0.021)	(0.025)	(0.054)
Educational attainment of the primary earner	0.055***	0.056***	-0.015
	(0.009)	(0.010)	(0.024)
Industry of the primary earner	0.006**	0.005**	0.008
	(0.002)	(0.003)	(0.005)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	4.743***	4.666***	8.056***
	(0.107)	(0.122)	(0.536)
N	15018	12369	303

	2014 reform	2015 reform	2016 reform
Reform dummy (post = 1)	0.022	0.719***	0.590***
	(0.019)	(0.136)	(0.103)
Treatment dummy	0.375***	0.326	0.037
	(0.055)	(0.231)	(0.083)
Reform × Treatment (DID effect)	0.165**	0.071	-0.540***
	(0.071)	(0.251)	(0.103)
Age of the primary earner	0.034***	0.021	-0.019
	(0.005)	(0.017)	(0.020)
Age of the primary earner, squared	-0.000***	0.000	0.000
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.049	-0.306*	0.008
	(0.031)	(0.163)	(0.061)
Educational attainment of the primary earner	0.048***	0.09	0.019
	(0.011)	(0.059)	(0.018)
Industry of the primary earner	0.006*	-0.007	0.006
	(0.003)	(0.015)	(0.005)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	4.547***	5.338***	6.494***
	(0.138)	(0.630)	(0.575)
N	7988	104	401

	2017 reform	2018 reform	2020 reform
Reform dummy (post = 1)	0.618***	-0.067***	0.378***
	(0.084)	(0.022)	(0.063)
Treatment dummy	0.176**	0.076**	0.005
	(0.086)	(0.033)	(0.071)
Reform × Treatment (DID effect)	-0.571***	0.083**	-0.293***
	(0.085)	(0.032)	(0.068)
Age of the primary earner	0.012	0.024***	0.022**
	(0.020)	(0.007)	(0.010)
Age of the primary earner, squared	0.000	-0.000***	0.000
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.121	0.046	0.137**
	(0.288)	(0.072)	(0.060)
Educational attainment of the primary earner	0.036**	0.045***	0.038***
	(0.018)	(0.009)	(0.014)
Industry of the primary earner	0.006	0.001	-0.004
	(0.004)	(0.003)	(0.003)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	5.504***	4.923***	5.149***
	(0.586)	(0.252)	(0.342)
N	703	4675	1831

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Source: Authors' calculations based on JHPS/KHPS microdata.

Table B3. Estimation results from Two-Step Weighted FE-DID (dependent variable: log equivalent household disposable income)

	2011 reform (a)	2011 reform (b)	2013 reform
Reform × Treatment (DID effect)	0.069***	0.103***	-0.534***
	(0.020)	(0.024)	(0.129)
Age of the primary earner	0.021***	0.019***	0.031
	(0.003)	(0.003)	(0.046)
Age of the primary earner, squared	-0.000***	-0.000***	0.000
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.054***	0.046**	0.13
	(0.019)	(0.021)	(0.101)
Educational attainment of the primary earner	-0.008	-0.003	-0.067
	(0.009)	(0.009)	(0.104)
Industry of the primary earner	-0.004	-0.005	-0.021**
	(0.004)	(0.004)	(0.010)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	0.064***	0.038	0.158
	(0.022)	(0.024)	(0.111)
N	15018	12369	303

	2014 reform	2015 reform	2016 reform
Reform × Treatment (DID effect)	0.107**	0.102	-0.528***
	(0.048)	(0.250)	(0.098)
Age of the primary earner	0.003	-0.004	-0.031*
	(0.004)	(0.030)	(0.016)
Age of the primary earner, squared	0.000	0.000	0.000*
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.012	0.061	-0.036
	(0.030)	(0.151)	(0.087)
Educational attainment of the primary earner	-0.009	0.071	0.012
	(0.007)	(0.102)	(0.034)
Industry of the primary earner	0	-0.026	-0.017**
	(0.003)	(0.024)	(0.008)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	0.004	0.484**	0.192**
	(0.055)	(0.195)	(0.086)
N	7988	104	401

	2017 reform	2018 reform	2020 reform
Reform × Treatment (DID effect)	-0.569***	0.039	-0.300***
	(0.087)	(0.028)	(0.060)
Age of the primary earner	0.034	0.012**	0.015
	(0.021)	(0.005)	(0.009)
Age of the primary earner, squared	-0.000*	-0.000***	0.000
	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.01	-0.029	0.052
	(0.063)	(0.026)	(0.033)
Educational attainment of the primary earner	-0.025	-0.003	-0.007
	(0.043)	(0.011)	(0.013)
Industry of the primary earner	-0.005	0.001	-0.006
	(0.009)	(0.004)	(0.004)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Constant	0.346***	0.091**	0.042
	(0.102)	(0.043)	(0.092)
N	703	4675	1831

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Notes: The treatment dummy is perfectly collinear with individual fixed effects and is therefore omitted from the estimation. The reform dummy is a linear combination of the year fixed effects, and its coefficient depends on the choice of base year; accordingly, it is not interpreted. Source: Authors' calculations based on JHPS/KHPS microdata.

Table B4. Estimation results from Weighted CQR-DID
(dependent variable: log equivalent household disposable income)

2011 reform (a)	Quantile τ 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	-0.179***	-0.083***	-0.087***	-0.104***	-0.109***	-0.093***	-0.085***	-0.069**	-0.071**
	(0.051)	(0.031)	(0.028)	(0.027)	(0.026)	(0.024)	(0.025)	(0.032)	(0.030)
Treatment dummy	0.139***	0.011	-0.047**	-0.078***	-0.117***	-0.156***	-0.183***	-0.196***	-0.214***
	(0.035)	(0.022)	(0.020)	(0.019)	(0.018)	(0.018)	(0.017)	(0.021)	(0.023)
Reform × Treatment (DID effect)	0.169***	0.069**	0.060**	0.067**	0.077***	0.051**	0.033	0.02	0.007
	(0.052)	(0.031)	(0.030)	(0.029)	(0.028)	(0.025)	(0.028)	(0.035)	(0.032)
Age of the primary earner	0.025***	0.031***	0.032***	0.031***	0.030***	0.030***	0.030***	0.024***	0.025***
	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Age of the primary earner, squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.070***	0.023	0.016	0.007	-0.002	0.004	-0.01	-0.021	-0.036**
	(0.027)	(0.017)	(0.016)	(0.015)	(0.014)	(0.014)	(0.013)	(0.016)	(0.018)
Educational attainment of the primary earner	0.060***	0.058***	0.059***	0.056***	0.054***	0.057***	0.047***	0.045***	0.047***
	(0.011)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.007)	(0.007)
Industry of the primary earner	0.008***	0.006***	0.007***	0.007***	0.006***	0.006***	0.006***	0.007***	0.003
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.953***	4.172***	4.337***	4.484***	4.658***	4.805***	4.981***	5.313***	5.567***
	(0.151)	(0.087)	(0.077)	(0.079)	(0.072)	(0.074)	(0.075)	(0.104)	(0.104)
N					15018				

2011 reform (b)	Quantile τ 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	-0.155***	-0.061*	-0.082***	-0.093***	-0.099***	-0.076***	-0.076***	-0.064**	-0.036
	(0.060)	(0.034)	(0.029)	(0.028)	(0.028)	(0.026)	(0.025)	(0.032)	(0.030)
Treatment dummy	0.112***	0.051**	-0.008	-0.021	-0.037*	-0.055***	-0.069***	-0.075***	-0.074***
	(0.038)	(0.025)	(0.021)	(0.021)	(0.020)	(0.021)	(0.019)	(0.023)	(0.023)
Reform × Treatment (DID effect)	0.126**	0.079*	0.087**	0.101***	0.087**	0.081**	0.098***	0.082**	0.041
	(0.062)	(0.046)	(0.039)	(0.031)	(0.038)	(0.037)	(0.034)	(0.040)	(0.052)
Age of the primary earner	0.029***	0.032***	0.033***	0.033***	0.031***	0.031***	0.030***	0.026***	0.029***
	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)
Age of the primary earner, squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.130***	0.054**	0.042**	0.033*	0.018	0.013	-0.002	0.002	-0.022
	(0.032)	(0.023)	(0.019)	(0.017)	(0.018)	(0.017)	(0.016)	(0.019)	(0.022)
Educational attainment of the primary earner	0.067***	0.063***	0.066***	0.062***	0.059***	0.058***	0.050***	0.043***	0.043***
	(0.013)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)	(0.008)
Industry of the primary earner	0.006*	0.002	0.005**	0.005***	0.006***	0.006***	0.007***	0.007***	0.004*
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.856***	4.160***	4.293***	4.376***	4.613***	4.770***	4.971***	5.315***	5.437***
	(0.189)	(0.116)	(0.076)	(0.098)	(0.097)	(0.106)	(0.086)	(0.123)	(0.096)
N					12369				

2013 reform	Quantile τ 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.623***	0.735***	0.726***	0.708***	0.717***	0.669***	0.642***	0.602**	0.367*
	(0.227)	(0.211)	(0.142)	(0.147)	(0.233)	(0.125)	(0.180)	(0.234)	(0.219)
Treatment dummy	0.283*	0.265***	0.226***	0.215***	0.201***	0.205***	0.143	0.057	-0.12
	(0.151)	(0.087)	(0.038)	(0.068)	(0.053)	(0.071)	(0.118)	(0.124)	(0.201)
Reform × Treatment (DID effect)	-0.566**	-0.669***	-0.632***	-0.650***	-0.659***	-0.661***	-0.547***	-0.550**	-0.329
	(0.236)	(0.255)	(0.140)	(0.173)	(0.217)	(0.120)	(0.207)	(0.231)	(0.209)
Age of the primary earner	-0.076***	-0.083	-0.091**	-0.093*	-0.082	-0.069	-0.071	-0.037	-0.044
	(0.013)	(0.109)	(0.046)	(0.054)	(0.111)	(0.082)	(0.050)	(0.044)	(0.063)
Age of the primary earner, squared	0.001***	0.001	0.001**	0.001*	0.001	0.001	0.001	0.001	0.001
	0.000	(0.001)	0.000	(0.001)	(0.001)	(0.001)	(0.001)	0.000	(0.001)
Sex of the primary earner (male = 1)	0.126*	0.082	0.038	0.005	-0.01	-0.034	-0.033	-0.029	0.091
	(0.069)	(0.099)	(0.042)	(0.046)	(0.084)	(0.085)	(0.058)	(0.093)	(0.223)
Educational attainment of the primary earner	-0.039	-0.026	-0.006	-0.003	0.019	0.015	0.019	-0.007	-0.059
	(0.050)	(0.048)	(0.022)	(0.038)	(0.035)	(0.041)	(0.042)	(0.041)	(0.086)
Industry of the primary earner	0.019*	0.01	0.008	0.008	0.006	0.005	0.008	0	0.017
	(0.011)	(0.008)	(0.006)	(0.008)	(0.011)	(0.012)	(0.011)	(0.009)	(0.018)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	7.490***	7.851***	8.151***	8.349***	8.031***	7.834***	7.917***	7.498***	8.133***
	(0.476)	(2.640)	(1.009)	(1.225)	(2.470)	(1.590)	(1.141)	(1.093)	(1.532)
N					303				

2014 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.068	0.042	0.036	-0.015	0.006	0.002	-0.014	-0.007	0.026
	(0.041)	(0.031)	(0.024)	(0.022)	(0.022)	(0.020)	(0.021)	(0.028)	(0.024)
Treatment dummy	0.461***	0.434***	0.425***	0.336***	0.299***	0.359***	0.375***	0.321***	0.183*
	(0.166)	(0.088)	(0.036)	(0.062)	(0.111)	(0.064)	(0.097)	(0.023)	(0.100)
Reform × Treatment (DID effect)	0.131	0.081	0.099	0.157	0.229*	0.133**	0.111	0.093	0.284**
	(0.191)	(0.126)	(0.070)	(0.123)	(0.139)	(0.068)	(0.119)	(0.098)	(0.113)
Age of the primary earner	0.036***	0.039***	0.040***	0.038***	0.039***	0.036***	0.030***	0.029***	0.021***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.002)
Age of the primary earner, squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.087**	-0.041	-0.052**	-0.038*	-0.039*	-0.037**	-0.026	-0.042*	-0.007
	(0.041)	(0.031)	(0.023)	(0.022)	(0.022)	(0.019)	(0.021)	(0.025)	(0.023)
Educational attainment of the primary earner	0.069***	0.056***	0.050***	0.052***	0.053***	0.049***	0.043***	0.042***	0.038***
	(0.014)	(0.011)	(0.009)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Industry of the primary earner	0.004	0.008**	0.007***	0.006**	0.005**	0.005**	0.005**	0.004*	0.005**
	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.867***	3.973***	4.123***	4.311***	4.410***	4.637***	4.914***	5.023***	5.483***
	(0.209)	(0.140)	(0.120)	(0.118)	(0.119)	(0.105)	(0.114)	(0.138)	(0.106)
N					7988				
2015 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.965***	0.873***	0.812***	0.781***	0.544***	0.535***	0.510***	0.494***	0.612***
	(0.153)	(0.123)	(0.119)	(0.164)	(0.174)	(0.145)	(0.072)	(0.072)	(0.072)
Treatment dummy	0.105	0.171	0.285	0.602	0.37	0.21	0.272	0.142	0.1
	(1.050)	(0.659)	(0.556)	(0.513)	(0.444)	(0.808)	(0.235)	(0.700)	(0.263)
Reform × Treatment (DID effect)	0.054	0.04	0.421	0.062	0.11	0.082	0.336	0.355	0.236
	(.)	(0.736)	(0.628)	(0.565)	(0.460)	(0.806)	(0.396)	(0.745)	(1.864)
Age of the primary earner	0.019	0.034	0.032*	0.038	0.02	0.002	0.018	0.027	-0.028
	(0.073)	(0.029)	(0.019)	(0.040)	(0.038)	(0.041)	(0.031)	(0.045)	(0.050)
Age of the primary earner, squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.162	-0.223*	-0.245**	-0.143	-0.186	-0.305*	-0.411***	-0.353***	-0.24
	(0.190)	(0.132)	(0.106)	(0.177)	(0.213)	(0.169)	(0.115)	(0.101)	(0.168)
Educational attainment of the primary earner	0.068	0.033	0.025	0.037	0.051	0.080	0.09	0.087	0.152**
	(0.055)	(0.048)	(0.046)	(0.070)	(0.083)	(0.080)	(0.069)	(0.062)	(0.070)
Industry of the primary earner	-0.018	-0.013	-0.005	0.008	0.008	0.001	0.01	0.010	-0.005
	(0.018)	(0.011)	(0.018)	(0.016)	(0.019)	(0.016)	(0.010)	(0.016)	(0.019)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.118**	5.066***	5.176***	4.866***	5.359***	6.016***	5.775***	5.720***	7.100***
	(1.996)	(0.906)	(0.583)	(1.231)	(1.217)	(1.338)	(1.145)	(1.398)	(1.564)
N					104				
2016 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.727***	0.726***	0.654***	0.725***	0.590***	0.490***	0.456***	0.464***	0.531***
	(0.101)	(0.075)	(0.115)	(0.142)	(0.159)	(0.140)	(0.113)	(0.061)	(0.074)
Treatment dummy	0.229**	0.223***	0.153*	0.103	-0.011	-0.102	-0.171**	-0.107*	-0.044
	(0.090)	(0.069)	(0.089)	(0.131)	(0.137)	(0.119)	(0.086)	(0.056)	(0.053)
Reform × Treatment (DID effect)	-0.660***	-0.691***	-0.598***	-0.637***	-0.529***	-0.419***	-0.451***	-0.459***	-0.558***
	(0.106)	(0.084)	(0.120)	(0.147)	(0.161)	(0.143)	(0.121)	(0.073)	(0.082)
Age of the primary earner	-0.006	0.009	-0.002	0.009	-0.005	-0.042	-0.04	-0.063***	-0.052***
	(0.018)	(0.017)	(0.018)	(0.023)	(0.033)	(0.032)	(0.025)	(0.023)	(0.017)
Age of the primary earner, squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000**	0.001***	0.001***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.02	-0.017	-0.041	-0.086	-0.034	-0.002	0.065	0.081	0.049
	(0.043)	(0.061)	(0.048)	(0.059)	(0.075)	(0.076)	(0.079)	(0.059)	(0.067)
Educational attainment of the primary earner	0.025	0.02	0.036**	0.034	0.025	0.003	0.011	0.023*	0.020
	(0.021)	(0.017)	(0.017)	(0.026)	(0.031)	(0.027)	(0.018)	(0.013)	(0.018)
Industry of the primary earner	0.005	0.008*	0.008**	0.005	0.007	0.007	0.003	0.009*	0.013***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.003)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.437***	5.241***	5.688***	5.476***	6.093***	7.384***	7.403***	8.152***	7.960***
	(0.785)	(0.547)	(0.537)	(0.697)	(0.925)	(0.902)	(0.755)	(0.709)	(0.488)
N					401				

2017 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.814***	0.763***	0.654***	0.660***	0.489***	0.382***	0.425***	0.402**	0.528***
	(0.189)	(0.115)	(0.149)	(0.148)	(0.162)	(0.080)	(0.076)	(0.193)	(0.100)
Treatment dummy	0.529***	0.422***	0.229	0.238*	0.09	-0.008	0.008	-0.052	-0.104
	(0.143)	(0.110)	(0.141)	(0.141)	(0.160)	(0.070)	(0.050)	(0.121)	(0.072)
Reform × Treatment (DID effect)	-0.729***	-0.707***	-0.597***	-0.630***	-0.464***	-0.346***	-0.372***	-0.372*	-0.496***
	(0.193)	(0.119)	(0.152)	(0.154)	(0.164)	(0.084)	(0.080)	(0.197)	(0.110)
Age of the primary earner	0.039***	0.043**	0.039***	0.037	0.019	-0.006	-0.01	-0.049*	-0.083**
	(0.015)	(0.019)	(0.014)	(0.024)	(0.028)	(0.022)	(0.029)	(0.028)	(0.041)
Age of the primary earner, squared	-0.000**	-0.000*	-0.000**	0.000	0.000	0.000	0.000	0.001**	0.001**
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.216	-0.07	0.064	0.099	-0.031	-0.021	-0.405	-0.88	-0.546
	(0.288)	(0.172)	(0.217)	(0.275)	(0.213)	(0.578)	(0.463)	(0.620)	(0.444)
Educational attainment of the primary earner	0.019	0.027**	0.035**	0.050***	0.043***	0.036***	0.056***	0.076***	0.103***
	(0.015)	(0.012)	(0.015)	(0.018)	(0.016)	(0.012)	(0.012)	(0.013)	(0.021)
Industry of the primary earner	0.001	0.002	0.007	0.008*	0.004	0.004	0.002	0.001	0.009*
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.361***	4.300***	4.487***	4.334***	5.281***	5.964***	6.358***	8.039***	8.552***
	(0.530)	(0.585)	(0.434)	(0.683)	(0.773)	(0.816)	(0.906)	(0.954)	(1.170)
N	703								
2018 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	-0.053	-0.058	-0.049	-0.017	-0.002	-0.003	-0.043	-0.075**	-0.057**
	(0.062)	(0.038)	(0.032)	(0.023)	(0.029)	(0.034)	(0.033)	(0.033)	(0.029)
Treatment dummy	0.172***	0.107**	0.083***	0.077***	0.052	0.036	0.039	-0.011	-0.046
	(0.062)	(0.047)	(0.031)	(0.026)	(0.034)	(0.037)	(0.053)	(0.038)	(0.035)
Reform × Treatment (DID effect)	0.166*	0.109*	0.083**	0.048	0.04	0.048	0.029	0.065	0.026
	(0.088)	(0.058)	(0.041)	(0.034)	(0.046)	(0.048)	(0.061)	(0.048)	(0.043)
Age of the primary earner	0.037**	0.021**	0.016***	0.020***	0.019***	0.021***	0.020***	0.019***	0.031***
	(0.019)	(0.009)	(0.005)	(0.004)	(0.005)	(0.006)	(0.006)	(0.003)	(0.004)
Age of the primary earner, squared	-0.000**	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.165	0.125	0.091	0.070*	0.046	-0.013	-0.046	-0.122**	-0.003
	(0.123)	(0.086)	(0.108)	(0.036)	(0.092)	(0.086)	(0.086)	(0.062)	(0.029)
Educational attainment of the primary earner	0.049***	0.064***	0.063***	0.053***	0.050***	0.049***	0.039***	0.034***	0.021***
	(0.017)	(0.014)	(0.010)	(0.007)	(0.009)	(0.010)	(0.010)	(0.007)	(0.007)
Industry of the primary earner	0	0.001	-0.002	0	-0.001	-0.001	0.001	0.004	0.001
	(0.006)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.010***	4.547***	4.752***	4.805***	4.972***	5.096***	5.408***	5.791***	5.505***
	(0.544)	(0.276)	(0.211)	(0.144)	(0.177)	(0.262)	(0.244)	(0.141)	(0.173)
N	4675								
2020 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.327**	0.404***	0.310***	0.314***	0.335***	0.356***	0.359***	0.523***	0.401***
	(0.140)	(0.080)	(0.054)	(0.074)	(0.063)	(0.061)	(0.087)	(0.166)	(0.126)
Treatment dummy	0.05	0.076	0.011	-0.014	-0.034	0.017	-0.039	0.017	-0.088
	(0.135)	(0.057)	(0.056)	(0.046)	(0.034)	(0.033)	(0.051)	(0.091)	(0.113)
Reform × Treatment (DID effect)	-0.222	-0.324***	-0.241***	-0.248***	-0.259***	-0.302***	-0.285***	-0.448***	-0.326**
	(0.140)	(0.080)	(0.056)	(0.076)	(0.065)	(0.064)	(0.089)	(0.169)	(0.131)
Age of the primary earner	0.024***	0.031***	0.028***	0.027***	0.026***	0.015	0.008	0.008	0.013
	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.007)	(0.010)
Age of the primary earner, squared	-0.000*	-0.000***	-0.000**	-0.000**	-0.000**	0.000	0.000	0.000	0.000
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.022	0.081	0.061	-0.027	0.064*	0.104**	0.152***	0.229***	0.218***
	(0.070)	(0.064)	(0.096)	(0.086)	(0.035)	(0.048)	(0.033)	(0.047)	(0.048)
Educational attainment of the primary earner	0.017*	0.012	0.028***	0.032***	0.047***	0.049***	0.051***	0.025*	0.052***
	(0.010)	(0.008)	(0.007)	(0.007)	(0.010)	(0.009)	(0.010)	(0.013)	(0.015)
Industry of the primary earner	0.002	0	0	-0.002	-0.001	-0.003	-0.003	-0.006**	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.818***	4.701***	4.797***	5.071***	5.004***	5.425***	5.733***	5.890***	5.864***
	(0.240)	(0.197)	(0.230)	(0.209)	(0.244)	(0.343)	(0.284)	(0.240)	(0.346)
N	1831								

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Source: Authors' calculations based on JHPS/KHPS microdata.

Table B5. Estimation results from Weighted UQR-DID
(dependent variable: log equivalent household disposable income)

2011 reform (a)	Quantile τ 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.633	-0.42	-0.464	-0.105	-0.165	-0.165	-0.441	-0.411	-0.425
	(0.533)	(0.733)	(0.481)	(0.152)	(0.143)	(0.143)	(0.366)	(0.378)	(0.377)
Treatment dummy	0.341	0.57	0.399	0.179	0.260*	0.260*	0.293*	0.386**	0.386**
	(0.472)	(0.568)	(0.363)	(0.115)	(0.143)	(0.143)	(0.155)	(0.166)	(0.165)
Reform × Treatment (DID effect)	-0.117	1.209*	1.002**	0.315**	0.288*	0.288*	0.628*	0.633*	0.586
	(0.493)	(0.661)	(0.445)	(0.141)	(0.152)	(0.152)	(0.372)	(0.383)	(0.391)
Age of the primary earner	-0.068*	-0.034	0.109	0.033	0.019	0.019	0.016	0.018	0.033
	(0.037)	(0.086)	(0.095)	(0.028)	(0.023)	(0.023)	(0.027)	(0.028)	(0.031)
Age of the primary earner, squared	0.001**	0.000	-0.001	0.000	0.000	0.000	0.000	0.000	0.000
	0.000	(0.001)	(0.001)	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.373	-0.629*	-0.269	-0.031	-0.082	-0.082	0.077	0.082	0.007
	(0.291)	(0.366)	(0.257)	(0.085)	(0.104)	(0.104)	(0.176)	(0.182)	(0.191)
Educational attainment of the primary earner	-0.243	-0.225	-0.187	-0.031	0.006	0.006	0.018	0.046	0.039
	(0.208)	(0.211)	(0.123)	(0.038)	(0.041)	(0.041)	(0.042)	(0.045)	(0.045)
Industry of the primary earner	0.012	-0.002	0.002	0.004	0.012	0.012	-0.011	-0.006	0.005
	(0.032)	(0.040)	(0.027)	(0.010)	(0.010)	(0.010)	(0.024)	(0.024)	(0.026)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.873**	5.897*	2.615	3.125***	3.177***	3.237***	3.540***	3.325***	3.241***
	(2.484)	(3.015)	(2.417)	(0.720)	(0.588)	(0.588)	(0.680)	(0.733)	(0.755)
N					15018				
2011 reform (b)	**Quantile τ 0.1**	**0.2**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.8**	**0.9**
Reform dummy (post = 1)	0.543	-0.114	-0.396	-0.356	-0.186	-0.177	-0.452	-0.424	-0.397
	(0.581)	(0.813)	(0.592)	(0.387)	(0.139)	(0.136)	(0.320)	(0.324)	(0.328)
Treatment dummy	0.737	0.597	0.21	0.231	0.082	0.184	0.212	0.243*	0.275*
	(0.527)	(0.728)	(0.530)	(0.320)	(0.097)	(0.139)	(0.135)	(0.138)	(0.144)
Reform × Treatment (DID effect)	-0.432	0.993	1.353**	0.903**	0.372***	0.289*	0.565*	0.558*	0.577*
	(0.494)	(0.774)	(0.608)	(0.395)	(0.137)	(0.154)	(0.334)	(0.336)	(0.339)
Age of the primary earner	-0.075	-0.086	0.098	0.102	0.027	0.02	0.018	0.019	0.019
	(0.045)	(0.079)	(0.126)	(0.083)	(0.025)	(0.025)	(0.027)	(0.027)	(0.028)
Age of the primary earner, squared	0.001*	0.001	-0.001	-0.001	0.000	0.000	0.000	0.000	0.000
	0.000	(0.001)	(0.001)	(0.001)	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	-0.418	-0.387	-0.166	0.061	0.04	-0.046	0.133	0.119	0.127
	(0.338)	(0.439)	(0.355)	(0.246)	(0.074)	(0.112)	(0.185)	(0.187)	(0.188)
Educational attainment of the primary earner	-0.251	-0.309	-0.256	-0.13	-0.011	0.001	0.006	0.01	0.033
	(0.236)	(0.247)	(0.175)	(0.103)	(0.042)	(0.043)	(0.041)	(0.041)	(0.043)
Industry of the primary earner	0.015	0.02	0.017	0.019	0.01	0.016	-0.008	-0.009	-0.003
	(0.037)	(0.046)	(0.038)	(0.027)	(0.008)	(0.010)	(0.024)	(0.024)	(0.024)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	6.080**	7.253**	2.768	2.157	3.125***	3.200***	3.444***	3.545***	3.386***
	(2.932)	(3.386)	(3.389)	(2.098)	(0.644)	(0.632)	(0.684)	(0.701)	(0.731)
N					12369				
2013 reform	**Quantile τ 0.1**	**0.2**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.8**	**0.9**
Reform dummy (post = 1)					0.16	0.16	0.16	0.282	0.282
					(0.172)	(0.172)	(0.172)	(0.212)	(0.212)
Treatment dummy					0.493	0.493	0.493	0.695	0.695
					(0.459)	(0.459)	(0.459)	(0.498)	(0.498)
Reform × Treatment (DID effect)					-0.363	-0.363	-0.363	-0.575	-0.575
					(0.368)	(0.368)	(0.368)	(0.418)	(0.418)
Age of the primary earner					-0.059	-0.059	-0.059	-0.088	-0.088
					(0.058)	(0.058)	(0.058)	(0.064)	(0.064)
Age of the primary earner, squared					0.001	0.001	0.001	0.001	0.001
					(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex of the primary earner (male = 1)					0.067	0.067	0.067	-0.062	-0.062
					(0.087)	(0.087)	(0.087)	(0.154)	(0.154)
Educational attainment of the primary earner					-0.029	-0.029	-0.029	-0.054	-0.054
					(0.040)	(0.040)	(0.040)	(0.056)	(0.056)
Industry of the primary earner					0.042	0.042	0.042	0.025	0.025
					(0.037)	(0.037)	(0.037)	(0.039)	(0.039)
Region fixed effects					Yes	Yes	Yes	Yes	Yes
Year fixed effects					Yes	Yes	Yes	Yes	Yes
Constant					6.799***	6.821***	6.844***	7.747***	7.768***
					(0.990)	(0.990)	(0.990)	(1.295)	(1.295)
N					303				

2014 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.171	0.125	0.125	-0.159	-0.216	-0.216	0.04	0.395	0.295
	(0.232)	(0.083)	(0.083)	(0.297)	(0.288)	(0.288)	(0.371)	(0.837)	(0.533)
Treatment dummy	0.353	0.164**	0.164**	0.145*	0.272**	0.272**	0.559**	1.599**	1.120***
	(0.226)	(0.076)	(0.076)	(0.087)	(0.111)	(0.111)	(0.276)	(0.656)	(0.416)
Reform × Treatment (DID effect)	-0.237	-0.151	-0.151	0.103	0.136	0.136	-0.121	-0.503	-0.331
	(0.299)	(0.102)	(0.102)	(0.276)	(0.265)	(0.265)	(0.355)	(0.790)	(0.502)
Age of the primary earner	0.093	0.026	0.026	0.011	-0.007	-0.007	-0.017	0.022	0.000
	(0.099)	(0.030)	(0.030)	(0.035)	(0.035)	(0.035)	(0.036)	(0.093)	(0.059)
Age of the primary earner, squared	-0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	0.000	0.000	0.000	0.000	0.000	0.000	(0.001)	(0.001)
Sex of the primary earner (male = 1)	0.272	0.083	0.083	-0.215	0.028	0.028	-0.223	-0.624	-0.3
	(0.256)	(0.090)	(0.090)	(0.311)	(0.350)	(0.350)	(0.419)	(0.921)	(0.585)
Educational attainment of the primary earner	-0.012	-0.001	-0.001	0.038	-0.056	-0.056	-0.015	-0.046	0.012
	(0.044)	(0.016)	(0.016)	(0.042)	(0.112)	(0.112)	(0.119)	(0.255)	(0.173)
Industry of the primary earner	0.013	0.006	0.006	0.029	0.051*	0.051*	0.028	0.024	0.005
	(0.021)	(0.008)	(0.008)	(0.023)	(0.027)	(0.027)	(0.035)	(0.081)	(0.052)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.81	1.739*	1.819**	1.952**	3.259***	3.329***	3.614***	3.249	3.924**
	(3.026)	(0.917)	(0.917)	(0.960)	(1.128)	(1.128)	(1.146)	(2.605)	(1.670)
N					7988				

2015 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)									
Treatment dummy									
Reform × Treatment (DID effect)									
Age of the primary earner									
Age of the primary earner, squared									
Sex of the primary earner (male = 1)									
Educational attainment of the primary earner									
Industry of the primary earner									
Region fixed effects									
Year fixed effects									
Constant									
N					104				

2016 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)			0.706	0.706	1.226	1.45	1.45	1.45	2.867
			(0.735)	(0.735)	(0.906)	(1.023)	(1.023)	(1.023)	(2.800)
Treatment dummy			0.997	0.997	1.829	1.9	1.9	1.9	3.532
			(0.943)	(0.943)	(1.245)	(1.412)	(1.412)	(1.412)	(3.944)
Reform × Treatment (DID effect)			-0.976	-0.976	-1.706	-1.7610	-1.761	-1.761	-2.961
			(0.983)	(0.983)	(1.219)	(1.380)	(1.380)	(1.380)	(3.883)
Age of the primary earner			-0.05	-0.05	-0.089	-0.063	-0.063	-0.063	-0.448
			(0.052)	(0.052)	(0.067)	(0.085)	(0.085)	(0.085)	(0.451)
Age of the primary earner, squared			0.000	0.000	0.001	0.001	0.001	0.001	0.004
			0.000	0.000	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Sex of the primary earner (male = 1)			0.663	0.663	1.02	0.896	0.896	0.896	2.706
			(0.641)	(0.641)	(0.733)	(0.863)	(0.863)	(0.863)	(2.299)
Educational attainment of the primary earner			-0.063	-0.063	-0.014	0.018	0.018	0.018	0.186
			(0.073)	(0.073)	(0.092)	(0.109)	(0.109)	(0.109)	(0.314)
Industry of the primary earner			0.036	0.036	0.006	0.007	0.007	0.007	0.11
			(0.035)	(0.035)	(0.046)	(0.052)	(0.052)	(0.052)	(0.156)
Region fixed effects			Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects			Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant			2.867	2.911	3.081	1.706	1.756	1.805	3.209
			(2.463)	(2.463)	(2.525)	(3.155)	(3.155)	(3.155)	(13.832)
N					401				

2017 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	0.03	0.03	0.888	1.31	1.426*	1.426*	1.930**	2.115**	2.350**
	(0.035)	(0.035)	(0.731)	(0.842)	(0.778)	(0.778)	(0.926)	(0.918)	(0.946)
Treatment dummy	-0.284	-0.284	0.782	1.3	1.452*	1.452*	1.888**	2.150**	2.445**
	(0.291)	(0.291)	(0.705)	(0.879)	(0.812)	(0.812)	(0.901)	(0.938)	(0.978)
Reform × Treatment (DID effect)	0.218	0.218	-0.801	-1.228	-1.367*	-1.367*	-1.872**	-2.064**	-2.305**
	(0.222)	(0.222)	(0.734)	(0.846)	(0.784)	(0.784)	(0.931)	(0.920)	(0.949)
Age of the primary earner	-0.039	-0.039	-0.039	-0.055*	-0.046	-0.046	-0.048	-0.066*	-0.067*
	(0.042)	(0.042)	(0.027)	(0.032)	(0.029)	(0.029)	(0.031)	(0.037)	(0.038)
Age of the primary earner, squared	0.000	0.000	0.000	0.001*	0.001*	0.001*	0.001	0.001*	0.001*
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sex of the primary earner (male = 1)	0.055	0.055	-0.335	-0.504	-0.525	-0.525	-0.69	-0.747	-0.865
	(0.101)	(0.101)	(0.355)	(0.419)	(0.396)	(0.396)	(0.497)	(0.510)	(0.551)
Educational attainment of the primary earner	-0.054	-0.054	0.024	0.053	0.071	0.071	0.100	0.053	0.087
	(0.056)	(0.056)	(0.048)	(0.057)	(0.055)	(0.055)	(0.062)	(0.062)	(0.070)
Industry of the primary earner	0.02	0.02	0.033	0.017	0.019	0.019	0.023	0.011	0.013
	(0.020)	(0.020)	(0.022)	(0.025)	(0.021)	(0.021)	(0.021)	(0.019)	(0.019)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	6.954***	7.068***	5.697***	5.737***	5.159***	5.191***	4.865***	5.777***	5.343***
	(1.717)	(1.717)	(0.798)	(0.800)	(0.905)	(0.905)	(1.038)	(1.129)	(1.231)
N	703								

2018 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	-0.216	-0.944	-1.834*	-0.643*	-0.604**	-0.787**	-0.565**	-0.508**	-0.623***
	(0.255)	(0.667)	(0.991)	(0.354)	(0.273)	(0.324)	(0.257)	(0.235)	(0.224)
Treatment dummy	0.108	0.233	0.46	0.239	0.133	0.089	0.191	0.203	0.166
	(0.117)	(0.190)	(0.355)	(0.149)	(0.119)	(0.144)	(0.137)	(0.125)	(0.110)
Reform × Treatment (DID effect)	0.221	0.937	1.868*	0.655*	0.621**	0.793**	0.572**	0.514**	0.629***
	(0.246)	(0.653)	(0.986)	(0.352)	(0.273)	(0.321)	(0.255)	(0.232)	(0.222)
Age of the primary earner	0.019	-0.045	-0.137	-0.063	0.063	0.214	0.137	0.12	0.097
	(0.051)	(0.094)	(0.147)	(0.054)	(0.115)	(0.187)	(0.140)	(0.125)	(0.110)
Age of the primary earner, squared	0.000	0.000	0.001	0.001	0.000	-0.002	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Sex of the primary earner (male = 1)	-0.101	-0.348	-0.086	-0.082	0.123	0.001	-0.094	0.051	0.114
	(0.269)	(0.417)	(1.019)	(0.368)	(0.504)	(0.556)	(0.416)	(0.410)	(0.374)
Educational attainment of the primary earner	0.058*	0.173	0.294	0.121	0.071	0.075	0.066	0.061	0.032
	(0.035)	(0.129)	(0.229)	(0.084)	(0.075)	(0.082)	(0.063)	(0.057)	(0.054)
Industry of the primary earner	-0.006	0.021	-0.005	0.003	-0.01	0.006	0.016	0.014	0.003
	(0.008)	(0.043)	(0.071)	(0.026)	(0.025)	(0.032)	(0.025)	(0.022)	(0.022)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	2.436**	3.803**	6.469**	4.989***	1.988	-1.908	0.212	0.505	1.523
	(1.139)	(1.679)	(2.787)	(1.025)	(2.809)	(4.839)	(3.630)	(3.249)	(2.850)
N	4675								

2020 reform	Quantile τ								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Reform dummy (post = 1)	-0.766	-0.056	-0.227	0.576	0.816	1.086	1.112	0.867*	1.126**
	(0.563)	(0.079)	(0.188)	(0.602)	(0.803)	(0.723)	(0.688)	(0.493)	(0.516)
Treatment dummy	-0.78	-0.09	-0.075	0.609	1.002	1.300*	1.252*	0.870*	1.167**
	(0.641)	(0.080)	(0.080)	(0.492)	(0.828)	(0.759)	(0.732)	(0.522)	(0.555)
Reform × Treatment (DID effect)	0.895	0.157*	0.295*	-0.4	-0.702	-1.006	-0.892	-0.595	-0.885*
	(0.648)	(0.092)	(0.171)	(0.524)	(0.780)	(0.712)	(0.702)	(0.502)	(0.519)
Age of the primary earner	-0.134	0.004	0.157	0.146	0.135	0.109	0.191	0.158	0.115
	(0.117)	(0.025)	(0.150)	(0.149)	(0.143)	(0.118)	(0.196)	(0.141)	(0.119)
Age of the primary earner, squared	0.001	0.000	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	0.000	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Sex of the primary earner (male = 1)	-0.025	0.007	-0.199	-0.262	-0.264	-0.177	-0.279	-0.168	-0.096
	(0.332)	(0.048)	(0.215)	(0.230)	(0.241)	(0.208)	(0.291)	(0.205)	(0.181)
Educational attainment of the primary earner	-0.315	-0.041	-0.034	0.023	0.06	0.08	0.081	0.034	0.014
	(0.229)	(0.028)	(0.029)	(0.050)	(0.074)	(0.065)	(0.065)	(0.051)	(0.046)
Industry of the primary earner	0.017	-0.003	-0.015	-0.023	-0.04	-0.035*	-0.041*	-0.022	-0.017
	(0.066)	(0.009)	(0.015)	(0.017)	(0.025)	(0.021)	(0.024)	(0.018)	(0.015)
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	11.651**	5.469***	1.426	0.814	0.821	0.985	-1.282	0.073	1.039
	(5.095)	(0.903)	(4.066)	(4.077)	(3.880)	(3.253)	(5.478)	(3.951)	(3.304)
N	1831								

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Notes: For the 2013 reform, the τ = 0.1–0.4 quantiles, for the 2015 reform all quantiles, and for the 2016 reform the τ = 0.1 and 0.2 quantiles are excluded from the analysis because the sample size is too small to ensure reliable estimates. Source: Authors' calculations based on JHPS/KHPS microdata.

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