Does "Welfare-to-Work" Work? Evaluating Long-Run Effects across a Generation of Cohorts*

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Abstract

Work requirements in welfare programs are popular yet controversial. This paper provides a unified evaluation of Denmark's "welfare-to-work" reforms by analyzing their long-term effects across 19 birth cohorts. Impacts vary by age of exposure to the reforms. Adult cohorts incur modest income losses and shift toward crime and disability insurance. In contrast, child cohorts exposed before becoming welfare-eligible reap gains in education, income, and health, likely due to parental spillovers and anticipation effects. Cost-benefit analysis shows welfare-to-work is cost-effective in the long run due to these younger cohorts. These findings help unify the literature and suggest more efficient policy designs.

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1 Introduction

In the last few decades, means-tested cash welfare programs around the world have been radically reformed to include public work requirements, such as job counseling, vocational training, and apprenticeship programs. These "welfare-to-work" reforms represent a generational shift in social policy that "end[ed] welfare as we know it" (Clinton 1996). Yet despite their popularity, work requirements remain a contentious policy debate to this day.[1] Advocates claim that they equip welfare recipients with skills for labor market participation and promote self-sufficiency for younger generations, leading to lower public costs in the long run. Meanwhile, critics argue that work requirements provide a low return relative to the costs borne by recipients.[2] This may incentivize transitions away from welfare toward crime or alternative social insurance programs, raising public costs over time.

Although debates surrounding welfare-to-work reforms are framed by theories of their long-run effects on low-income populations and the government budget, existing empirical evidence is limited in two ways. First, there is little understanding of welfare-to-work's downstream effects on youth and its mechanisms (Ziliak 2016). Yet assessing how youth fared as they aged into this new policy environment is critical for evaluating the reform's impacts over a generational time span. Second, the bulk of the literature focuses on short-run impacts on welfare take-up and labor market outcomes in settings confounded by other policy changes, hampering the scope of policy evaluation. In short, existing work scarcely examines the long-run impacts of welfare-to-work on youth in tandem with adults.

This paper provides a unified evaluation of welfare-to-work. I present new evidence on the long-run effects of welfare-to-work reforms across a generation of birth cohorts up to 20–30 years later by harnessing large-scale administrative data to study Denmark's historical reforms to its cash welfare program, known as "social assistance" (SA). The breadth of cohorts and outcomes I study—including income, education, crime, hospital care, and SA and social insurance participation—allow me to conduct a more comprehensive evaluation of welfare-to-work reforms relative to much existing work. By estimating long-run impacts on youth and adults separately, this paper helps unify an expansive literature that tends to examine welfare-to-work's effects on select cohorts or cross-sections of adults over time.

The Danish context provides an attractive setting for evaluating welfare-to-work. Though Denmark's SA program targeted a broader population—any disadvantaged, able-bodied adult above

1. Work requirements were at the center of the 2023 U.S. debt ceiling crisis. For other recent news coverage, see Kim (2021), Michelmore (2022), Adolphsen (2022), Matthews (2022), and Kuang (2022).

2. See Card et al. (2010, 2018) for reviews of job training programs in the U.S. and Europe. Edin and Lein (1997) provide ethnographic evidence on the costs of participating in U.S. work requirements among single mothers, such as time, childcare, transportation, and administrative burdens.

age 18—than some other settings, its reforms primarily focused on introducing and expanding work requirements, unlike countries like the U.S.[3] The work requirements were onerous: By 1998, many SA recipients were required to participate in 18-month public work programs for at least 30 hours per week. Moreover, Denmark's administrative data afford a unique opportunity to estimate welfare-to-work's effects on a multitude of outcomes. Linkages across tax, housing, school, police, and hospital records provide an unusually rich set of outcomes that is difficult to study using administrative data in other settings. In addition, birth records permit me to observe outcomes of my sample's own children, providing a rare window into intergenerational spillover effects.

I estimate the effects of Denmark's welfare-to-work reforms along two key dimensions. The first is the *starting age* dimension: What are the effects on birth cohorts first exposed to the reforms at different ages? Economic theory predicts that youth not yet eligible for SA may respond differently to the reforms than already-eligible adults since they possess different margins of adjustment (e.g., schooling) and can anticipate facing work requirements in the future. The second is the *outcome age* dimension: What are the reforms' effects on later-life outcomes? Work requirements may directly affect the human capital of SA recipients or incentivize behavioral responses that affect intra- and intergenerational trajectories. This motivates estimating a family of "starting age-outcome age effects" to examine heterogeneity along both dimensions.

To identify starting age-outcome age effects, I leverage variation generated by Denmark's series of reforms during the 1990s. I exploit variation in birth cohorts' timing of exposure to the reforms through a triple difference (TD) strategy that adapts Callaway and Sant'Anna (2021) and Sun and Abraham (2021). Specifically, I difference between life-cycle differences of outcomes of birth cohorts who were "treated" at a particular starting age versus those of older birth cohorts who were "not yet treated" by the reforms. To purge confounding business cycle effects, these double-differenced outcomes are further differenced between disadvantaged sub-groups who are at high- versus low-risk of participating in SA based on pre-determined background characteristics.

I find that the long-run intragenerational effects of welfare-to-work are strikingly heterogeneous across starting age cohorts. Young children who are at high-risk of future SA receipt and between ages 8–15 at the time of the reforms—before they were eligible to participate in SA or leave school—experience significant positive effects in later-life labor market outcomes, schooling, and measures of health based on inpatient hospital care. These effects last throughout young adulthood, and some persist beyond outcome age 30. In contrast, high-risk young adults between starting ages 18–24—who already gained eligibility for SA and suddenly faced the reforms—experience *negative* effects on labor market outcomes that grow in magnitude beyond age 30, null

3. Besides work requirements, U.S. welfare reform included changes to time limits on benefit receipt; allocation of state welfare funds; and expansions of the Earned Income Tax Credit, federal child care funds, and Medicaid/child health insurance. See Fang and Keane (2004) for an effort to decompose the overall effects over these various factors.

effects on schooling and hospital care, and significant positive effects on incarceration and disability insurance (DI) benefits. Meanwhile, for older children between starting ages 16–17 and older adults between starting ages 25–29, estimated effects are far more muted.

I also find suggestive evidence of intergenerational spillover effects of welfare-to-work among a subsample of young mothers. Remarkably, starting age heterogeneity appears to persist to the next generation. Second-generation children whose mothers have starting ages 8–15 have higher income, higher schooling, and lower SA participation. Those born to mothers of older starting ages do not reap these benefits and instead commit more crime.

Starting age heterogeneity is consistent with a model where agents face distinct choice margins at different stages of the life cycle. Cohorts who experience the reforms as a surprise during adulthood lack the schooling margin, resorting to crime or alternative social insurance programs. Meanwhile, high-risk children who are not yet eligible for SA at the time of the reforms adjust along margins that build their human capital. This finding aligns with theories on the "deterrence effect" of work requirements (Besley and Coate 1992) and the role of information in influencing human capital investments (Becker 1962).

Next, I investigate the mechanisms that drive the positive effects among children. Bastian et al. (2021) discuss three channels of welfare-to-work's effects: intent-to-treat (ITT) effects of work requirements, spillovers from their own parents' ITT effects, and anticipation effects.[4] Suggestive evidence is consistent with parental ITT effects and anticipation effects among children. Additionally, I formalize Bastian et al.'s discussion with a potential outcomes framework to decompose starting age-outcome age effects along the three channels. Unique institutional features paired with stronger assumptions allow me to separately identify the anticipation effect for select cohorts. Collectively, these results suggest that parental spillover effects and anticipatory behavioral responses likely explain welfare-to-work's effects on younger cohorts.

With the family of starting age-outcome age estimates at hand, the final part of the paper evaluates the overall effects of welfare-to-work over a generational time span. Aggregating the starting age-outcome age effects over population cross-sections by calendar year reveals that, over time, the positive effects of high-risk children aging into the new policy regime outweigh the negative or null effects among high-risk adults already eligible for SA. Cost-benefit calculations that weigh the fiscal impacts of the reforms over outcomes also reveal meaningful heterogeneity along the starting age dimension: Children provide a *net positive* return on the government budget while adults impose a *net negative or null* return.

My estimates help unify an expansive literature on the impacts of welfare reforms. For example, a survey of prior studies shows mixed evidence on welfare-to-work's effects on high school completion. However, I demonstrate that conditioning on the underlying starting age cohorts of

4. Bastian et al. call these the "adult budget constraint," "resource," and "expectations" channels.

each study yields strong concordance with my main estimates. This indicates that starting age heterogeneity can help reconcile conflicting findings in the literature. Moreover, my calendar time estimates replicate recent studies like Han et al. (2021), who find substantially larger effects of U.S. welfare reforms on economic well-being in the long run than in the short run.

Overall, this paper provides three key takeaways for economists and policymakers. First, welfare-to-work appears to be an effective long-run investment solely because of its positive effects on high-risk children. Meanwhile, they can produce harmful fiscal impacts from high-risk adults *already eligible* for SA who comprise the immediately targeted population. Work requirements have long been promoted to combat long-term dependence on cash welfare. Yet they themselves can create powerful perverse incentives with high social costs among young adults, making the cure worse than the disease. This suggests that redesigning work requirements to target cohorts aging into eligibility and grandfather already-eligible cohorts to the previous policy regime could improve efficiency.

Second, the positive return among younger starting age cohorts does not appear to be driven by the ITT effects of work requirements themselves, implying it would be inappropriate to view welfare-to-work as a direct skill investment that shapes children's later-life outcomes. Rather, the evidence suggests that welfare-to-work "works" by affecting parental behavior and incentivizing human capital-enhancing responses among cohorts before they age into eligibility.

These points lend to the third takeaway: Aggregate estimates on population cross-sections over time, which are popular in the literature, should be interpreted with caution. These estimates mask important heterogeneity as the composition of the "treated" group evolves with the entrance of younger cohorts. Aggregate estimates viewed in isolation may provide misleading interpretations about the efficacy of welfare-to-work over time.

This paper contributes to a wide-ranging literature on the effects of welfare reform. First, to the best of my knowledge, this paper is one of the only studies to estimate effects of welfare-to-work reforms along the starting age dimension—separately for children and adults—in a common setting.[5] Much of the literature estimates effects of reforms on the adult population, though there are some notable exceptions. Hernæs et al. (2017) and Bratsberg et al. (2019) study the effects of increasing work requirements on older children's short-run outcomes, finding positive effects on schooling and negative effects on crime. Other work rely on survey data to examine U.S. welfare reform's effects on teenagers and find mixed results on effects on schooling.[6] In contrast, my paper separately estimates effects for 19 birth cohorts on a multitude of outcomes. Uncovering hetero-

5. Morris et al. (2001), Gennetian et al. (2002), and Bloom and Michalopoulos (2001) synthesize effects from the Manpower Demonstration Research Corporation's U.S. welfare demonstration projects on young children, adolescents, and adults, respectively. However, (i) very few projects solely involved work requirement programs and (ii) all interventions only aimed at adults (i.e., adolescents were not treated later).

6. See Kaestner et al. (2003), Dave et al. (2012), Dave et al. (2021), and Bastian et al. (2021, 2022).

geneous effects among children and adults is crucial for determining how disincentives among certain cohorts weigh against the benefits reaped by others (Aizer et al. 2022).[7]

Second, this paper contributes to our understanding of the long-run effects of welfare-to-work reforms over the outcome age dimension. A burgeoning literature has estimated the long-run intra- and intergenerational effects of cash and in-kind welfare programs.[8] However, evidence on long-run impacts of *reforms* to welfare programs, particularly those involving work requirements, remains nascent,[9] as most studies focus on welfare participation and labor market outcomes after 5 years.[10] One exception is Løken et al. (2018), who find that Norway's welfare-to-work reforms that targeted single mothers did not raise their long-term disposable incomes yet caused modest declines in child test scores. Vaughn (2022, 2023) focus on the effects of early childhood exposure to U.S. welfare reform and find it improved schooling, household structure, and health outcomes in early adulthood. These results are consistent with the starting age heterogeneity in my study.

Third, my paper contributes to work examining anticipatory responses to safety net programs. A sizeable literature considers how time limits of benefit receipt affect the dynamics of SA use and child outcomes.[11] Bastian et al. (2021) interpret the impact of U.S. welfare reform on children as a composite of the anticipation, ITT, and parental ITT effects but do not identify their relative contributions. In the context of DI, Deshpande and Dizon-Ross (2023) finds no evidence of a specific type of anticipation effect—changes in parental investments in child human capital—while Dahl and Gielen (2021) finds evidence suggesting the presence of anticipation effects among adolescents, though their estimates are confounded by parental ITT effects. This paper is among the first to employ a reduced-form strategy to separately identify anticipation effects from other channels. My results suggest they play a role in shaping long-term outcomes in a way that is similar to "threat effects" found in the unemployment insurance literature.[12]

The paper unfolds as follows. Section 2 describes the Danish institutional setting and discusses predictions of welfare-to-work's effects across starting age cohorts. Section 3 describes the data. Section 4 details my research design. Section 5 reports my main estimates of the long-run effects

7. Bitler et al. (2006) and Kline and Tartari (2016) emphasize how mean effects mask heterogeneity. I show that, in spite of this, mean effects are highly heterogeneous across starting age cohorts.

8. Recent work has examined the long-run effects of cash welfare (Aizer et al. 2016; Aizer et al. 2024), food stamps (Hoynes et al. 2016; Bailey et al. 2023), Medicaid (Brown et al. 2020; Goodman-Bacon 2021b), Earned Income Tax Credit (Bastian and Michelmore 2018; McInnis et al. 2024), negative income tax (Price and Song 2018), and disability programs (Deshpande 2016; Deshpande and Mueller-Smith 2022).

9. Hoynes and Schanzenbach (2018) point out that "evidence on... the long-term impact of welfare reform is limited" (p. 106). Moreover, a report by the National Academies of Sciences, Engineering, and Medicine (2019) concludes, "There is insufficient evidence to identify mandatory work policies that would reliably reduce child poverty.... The dearth of evidence also reflects underinvestment over the past two decades in methodologically strong evaluations of the impacts of alternative work programs" (p. 210).

10. See, e.g., Jensen et al. (2003), Dahlberg et al. (2009), Mogstad and Pronzato (2012), and Ziliak (2016).

11. See Grogger and Michalopoulos (2003), Grogger (2004), Chan (2018), and Low et al. (2018).

12. See Black et al. (2003), Geerdsen (2006), and Rosholm and Svarer (2008).

of welfare-to-work. Section 6 probes the mechanisms that affect younger cohorts. Section 7 aggregates effects by calendar time, conducts a cost-benefit analysis, and reconciles some results found in the literature. Section 8 concludes.

2 Institutional Context

2.1 Overview of the Danish Social Assistance Program

Social assistance (SA; *kontanthjælp*) was formally established in Denmark in 1976 to provide financial assistance to disadvantaged adults. SA primarily targets able-bodied adults who have capacity to participate in the labor market but do not qualify for unemployment insurance (UI).[13] Individuals at least 18 years old may apply, though exceptions are made under special circumstances.[14] An individual is eligible if she (1) experienced a social event (e.g., lost a job), (2) is unable to provide for herself (e.g., lack UI), and (3) exhausted her opportunities for work (e.g., unable to find a new job). "Social events" were broadly defined; for example, an individual who exited high school without a job could qualify. Individuals were expected to exhaust their resources before relying on SA, so the second criterion included a liquid asset limit of $550.[15]

SA benefits were generous relative to international standards.[16] For those with no recent earnings, the maximum benefit rate for "older adults" (ages $\geq$25) was $1,038/month (2010 U.S. dollars) in 1993, while that for "young adults" (ages 18–24) was $610.[17] Benefits had a 100% marginal tax rate. Conditional on maintained eligibility, there was no time limit on receiving benefits.[18]

The SA program was locally governed by Denmark's 275 municipal councils. Each municipality had flexibility in how they administered the program so long as they followed national guidelines. Caseworkers also had discretion in handling SA applicants.

2.2 The Welfare-to-Work Reforms

The welfare-to-work reforms were enacted in response to strained economic conditions. Unemployment rates in Denmark were persistently high during the 1980s, swelling above 10% by the

13. Unlike many countries, the Danish UI system is based on voluntary membership.
14. Exceptions are granted to youth without parental support or facing domestic abuse.
15. Liquid assets exclude savings earmarked for education, home improvements, and child care.
16. For reference, the average state-level maximum benefit of the U.S. Aid to Families with Dependent Children (AFDC) program for a mother with two children in 1994 was $609/month (Crouse 1995). See Appendix Table B.2 for a comparison between the Danish SA and U.S. AFDC programs.
17. For individuals who earned income prior to participating in SA, benefit levels were capped by the maximum between this level and 90% of the individual's previous net-of-tax income. Caps were benchmarked to be below UI benefits and state education grants. SA benefits were originally comprised of a basic rate, a housing supplement, and individual assistance. See Hansen and Schultz-Nielsen (2017) for details.
18. See Appendix B for more details.

end of the decade. Policymakers diagnosed that SA disincentivized able-bodied individuals, particularly young adults, from achieving self-sufficiency (Haahr et al. 1997).

The Danish government enacted a series of 7 reforms between 1990 and 1998 aimed at encouraging work among SA recipients. The most prominent feature of the reforms were public work requirements (also known as "activation" or "workfare"), which included job search, vocational training, short-term educational programs, community service, and subsidized apprenticeships in the public or private sectors. While SA recipients could voluntarily participate in these work programs before 1990, these reforms made them mandatory. The reforms also made relatively small changes to benefit levels and rules that affected minor segments of the population.[19] Work requirements were also implemented in DI and UI under different rules and timelines.[20]

SA work requirements were enforced after individuals exhausted a $1/2$–3-month "passive period" of receiving SA benefits. SA work programs were heterogeneous in nature, though were frequently public work projects or apprenticeships involving unskilled labor.[21] Qualitative studies document several cases where activities amounted to "busy work" with onerous schedules that frustrated participants.[22] Failure to complete work requirements led to reductions or discontinuation of SA benefits; some municipalities required repayment of benefits.

Appendix Table A.1 provides a timeline of the national reforms. The first reform enacted in July 1, 1990 required SA recipients between ages 18–19 only to participate in work requirements for 20 hours per week for at least 5 months. By 1992, the targeted age group of recipients expanded to ages 18–24. The 1994 reform marked a significant expansion: *All* SA recipients, irrespective of age, were required to fulfill work requirements. In addition, recipients who failed to exit SA after completing 6 months of work requirements were assigned to a new work program within 3 months. The last three reforms further intensified work requirements. By 1998, recipients below age 30 who lacked vocational credentials were required to work at least 30 hours per week in 18-month programs with mandatory renewals.

Municipal councils had some discretion over local policies. Municipalities could decide the intensity and duration of work requirements for older-aged recipients, but in most cases they applied a uniform policy across all ages.[23] In practice, municipalities adopted work requirements at different times, but typically within 6 months of the national reform dates.[24]

19. See Hansen and Nielsen (2021) and Appendix Table B.1 for details.

20. In my robustness analysis in Section 5.4 and Appendix D.3, I provide additional details and probe whether the DI and UI reforms explain my estimated effects of the SA reforms.

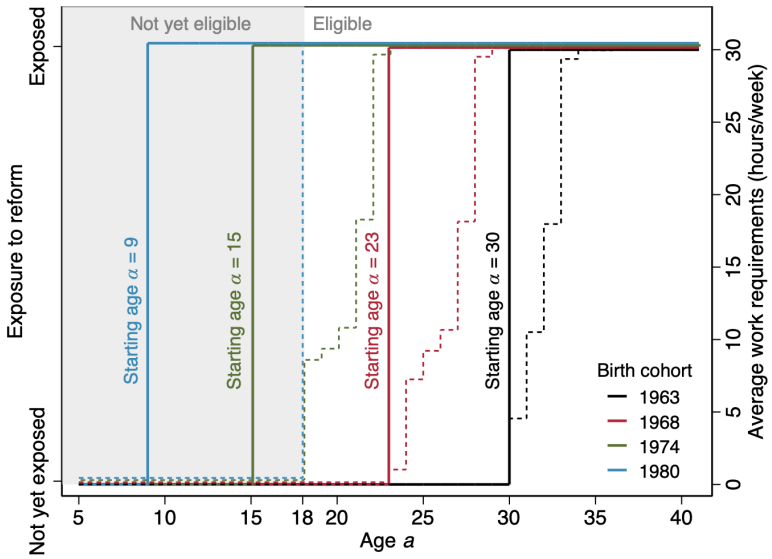
21. Examples include janitorial work, public park maintenance, snow shoveling, daycare assistants, food packing, carpentry, and bricklaying. Individuals could share preferences for work programs to caseworkers.

22. See Mik-Meyer (1999), Larsen (2005), Katznelson (2008), and Andersen (2020).

23. According to a survey of municipal councils prior to the 1996 reform, over 85% of municipalities enforced the same work requirements for recipients above age 25 as for those between ages 18–24 (at least 30 hours per week for 6 months). See Appendix Figure B.2 for the distribution of work requirements across municipalities.

24. There are some outliers like the municipality Farum, which enforced work requirements for all SA recipients

Figure 1: Exposure to reforms and work requirements over outcome ages, by birth cohort



Notes: Solid lines indicate exposure to any SA reform that affects a birth cohort immediately or in the future (left axis). Dashed lines plot the average work requirements (in hours per week) according to the national reforms' rules on the duration of work requirements, intensity of work requirements, mandatory renewals, and passive period before work requirements and between renewals (right axis); see Appendix Table B.1 for more details. The gray region represents ages below 18 before individuals are formally eligible for SA.

Figure 1 illustrates how different birth cohorts were exposed to the national welfare-to-work reforms over the life cycle.[25] Each solid line corresponds to a different birth cohort's "starting age"—that is, the age when the cohort was first exposed to a reform. For example, the solid black line indicates that the 1963 cohort first faced welfare-to-work at age 30 (due to the 1994 reform). Each dashed line corresponds to the average intensity of work requirements faced by each birth cohort over the life cycle. For example, the dashed black line shows that work requirements immediately began at age 30 for the 1963 cohort (since this cohort fell within the targeted age group of the 1994 reform) and quickly intensified due to subsequent reforms. In contrast, for younger cohorts with starting age below 18, exposure to the reforms occurred *before* they were eligible for SA. For example, the solid green line shows how the 1974 cohort was exposed to the reforms starting at age 15 (due to the 1990 reform). While this cohort would not face work requirements until they gained SA eligibility at age 18—as indicated by the dashed green line—they could *anticipate* facing work requirements at age 18 while still in school. For even younger cohorts, their *parents* would face the reforms during their childhood.

This figure offers two key insights. First, it suggests that welfare-to-work may have heterogeneous effects, particularly along the starting age dimension. Youth who could experience parental

starting in 1987 (Fallesen et al. 2018). I examine municipal adoption timing as a robustness check.

25. Appendix Figure A.1 shows corresponding "first-stage" plots of the take-up of SA work programs.

spillover effects and anticipate the reforms may respond differently compared to already-eligible adults. Notably, since individuals could drop out of school at around age 16, cohorts below starting age 16 may have more educational opportunities than their older counterparts. Appendix E.1 sheds light on heterogeneous responses through a dynamic model examining the reforms' impact on life-cycle budget constraints and different cohorts' labor supply, SA participation, and educational attainment. The model predicts that the reforms would deter children from SA and incentivize human capital investments (Becker 1962; Besley and Coate 1992), leading to higher income that may grow over outcome ages.

Second, the figure motivates my research design. It suggests that older starting age cohorts can serve as a comparison group to identify welfare-to-work's effects for younger starting ages. I discuss this strategy in detail after describing the data.

3 Data

I leverage population register data from Statistics Denmark between 1980 and 2019. Anonymized identifiers allow individuals to be linked across registers over time. This permits me to construct panels of various outcomes spanning several decades of the life cycle. Birth records also allow linkages between individuals and their parents and own children. See Appendix C for details.

SA participation data are available through a patchwork of different registers. Records of SA receipt significantly vary within and between registers to adapt to amendments made to national laws governing the SA program during the study period. I track all amendments to national laws between 1980 and 2000 to harmonize SA data across registers over time.[26]

My base sample consists of native Danes born between 1962 and 1981.[27] These cohorts correspond to starting ages $8 \leq \alpha \leq 30$ excluding 20, 21, and 25, which do not exist due to differences in timing and targeted age groups across the reforms. I restrict my attention to these cohorts due to data restrictions and confounding policy changes.[28] Background characteristics $\boldsymbol{X}_i$ include household income, household assets, mother's years of schooling, number of siblings, marital status, and mean neighborhood income. Most are defined before the individual turns age 18 and before 1994 to ensure individuals' parents are not directly affected by the SA reforms.[29]

26. Unfortunately, data on SA *work program* participation is unavailable in certain years; see Appendix C.5.

27. I exclude first- and second-generation immigrants to ameliorate compositional changes in my sample driven by immigration flows. A separate SA program for refugees was governed by a different rules.

28. The 1963 cohort is the earliest one I can observe covariates before age 18. The 1963 cohort is also the first one to experience formal SA eligibility rules at age 18 institutionalized by the "Hunger Circular" (*Sultecirkulære*) of 1981 (Hansen and Schultz-Nielsen 2017). Meanwhile, the 1981 cohort is the final one unaffected by a SA reform in 2006 that mandated SA recipients below age 25 to enroll in a formal vocational education program, which departs from the type of work requirements considered in this paper.

29. Section 6.2 finds no evidence that parents responded to the 1990–1992 reforms from becoming aware *their child* will face work requirements. Nevertheless, I verify my results are robust to measuring $\boldsymbol{X}_i$ before 1990.

In general, individuals most likely to be affected by the reforms come from low-socioeconomic status (SES) backgrounds. My main sample thus restricts the base sample by two background characteristics in $\boldsymbol{X}_i$: (i) household income between ages 12–17 (before 1994) is below the national median, and (ii) the individual's mother has at most 12 years of schooling.

4 Research Design

4.1 Target Parameters

Let $i \in \mathcal{I}$ index individuals, $b \in \mathcal{B}$ index birth quarters, and $a \in \mathcal{A}$ index integer-valued outcome ages. Let $D_i \in \mathcal{A} \cup \{\infty\}$ indicate the starting age α that i is exposed to the reforms, which is an absorbing "treatment" state; $\alpha = \infty$ signifies "never treated." Let $Y_{ia}(\alpha)$ be the potential outcome at outcome age a under starting age assignment $\alpha \in \mathcal{A} \cup \{\infty\}$.

Ideally, one would aim to identify welfare-to-work's effects among individuals who are eligible for SA. Unfortunately, exposure to the reforms could induce behavior that alters SA eligibility, making it an endogenous state. I thus identify effects among individuals with background characteristics making them at "high-risk" of participating in SA later in life. Let $G_i \in \{0, 1\}$ indicate membership in the high-risk group. I describe how risk groups are constructed below.

The target parameters are the family of starting age-outcome age effects for starting ages $\alpha \in \mathcal{A}$ on outcome ages $a \geq \alpha$ among the high-risk population $G_i = 1$:

$$\beta_{\alpha,a} \equiv \mathbb{E}[Y_{ia}(\alpha) - Y_{ia}(\infty) \mid D_i = \alpha, G_i = 1]. \tag{1}$$

In settings with staggered treatment timing and heterogeneous effects, this parameter is the underlying "group-time" or "cohort-time" treatment effect of popular two-way fixed effect estimators (Goodman-Bacon 2021a; Sun and Abraham 2021) and provides a building block for other parameters like aggregate effects over population cross-sections by calendar time (Callaway and Sant'Anna 2021). To focus on starting age heterogeneity, I also identify the average $\beta_{\alpha,a}$ over outcome ages, $\beta_\alpha \equiv \frac{1}{A_\alpha - \alpha + 1} \sum_{a=\alpha}^{A_\alpha} \beta_{\alpha,a}$, where A_α is the highest available age for α.

In practice, the data I observe are realized outcomes where starting ages are determined by birth quarters $b \in \mathcal{B}$, so if b_i denotes i's birth cohort, then $D_i = D(b_i)$. Assuming the low-risk group $G_i = 0$ is unaffected by the reform, realized outcomes are $Y_{ia} = \sum_{\alpha \in \mathcal{A}} Y_{ia}(\alpha) \times \mathbb{1}(D(b_i) = \alpha) \times G_i \; + \; Y_{ia}(\infty) \times (1 - G_i)$. I discuss this assumption and the interpretation of $\beta_{\alpha,a}$ below.

Risk Groups. The "low-" and "high-risk" groups, indicated by G_i, are defined by two predetermined criteria measuring exposure to SA before age 18: (i) the individual's household participated in SA (before the 1994 reform), and (ii) their childhood residential neighborhood block has

an above-median long-run SA participation rate.[30] The high-risk group is comprised of individuals who satisfy both criteria; the low-risk group satisfies neither. Both are theoretically and empirically relevant. Household participation in SA during childhood is motivated by high intergenerational persistence of welfare participation (Dahl, Kostøl, et al. 2014; Hartley et al. 2022). Neighborhood-level SA participation rates may capture spillover effects through local social networks (Bertrand et al. 2000).

Appendix Figure A.2 shows that risk groups are highly predictive of SA participation later in life. On average, the high-risk group participates in SA for 13.3 months longer than the low-risk group. In addition, the low-risk group takes up just 0.5 more months of SA than high-SES individuals excluded from my main sample, suggesting they face very similar exposure to SA as the broader population. Appendix Figure A.4 validates that risk groups predict SA participation just as well as a proxy measure of SA eligibility.

Appendix Table A.2 reports summary statistics of the high-risk group and its differences from the low-risk group. Younger high-risk starting age cohorts $8 \leq \alpha \leq 15$ come from more disadvantaged families than their older counterparts $25 \leq \alpha \leq 30$, revealing negative trends in SES over cohorts. Against this context, it is striking that the long-run outcomes of high-risk group cohorts $8 \leq \alpha \leq 15$ are similar or more favorable than $25 \leq \alpha \leq 30$. These summary statistics motivate me to control for the composition of risk groups and trends over cohorts.

4.2 Identification Strategy

My main analysis employs a triple difference (TD) design that exploits variation between birth cohorts of different starting ages, their outcome ages, and risk groups. Identification of $\beta_{\alpha,a}$ rests on the following assumption:

Assumption 1 (Conditional parallel trends)**.** *For any starting age $\alpha' > a$,*

$$\begin{aligned}
&(\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha, G_i = 1, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i) = \alpha, G_i = 1, \boldsymbol{X}_i]) \\
&\quad - (\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha, G_i = 0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i) = \alpha, G_i = 0, \boldsymbol{X}_i]) \\
&= (\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha', G_i = 1, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i) = \alpha', G_i = 1, \boldsymbol{X}_i]) \\
&\quad - (\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha', G_i = 0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i) = \alpha', G_i = 0, \boldsymbol{X}_i]) .
\end{aligned}$$

Assumption 1 states that, conditional on pre-determined characteristics $\boldsymbol{X}_i$, differences between

30. Specifically, I measure whether any older household member was on SA over the three highest ages available in each cohort (before age 18, before 1994). Neighborhood blocks (developed by Damm and Schultz-Nielsen (2008)) consist of roughly 150 households. Each block's SA participation rate is measured between 1980–1998; the median SA participation rate is conditional on the block's mean income (by $1,000 bins) and urbanicity (based on municipality population size <10,000, between 10,000–50,000, and >50,000) in order to preserve greater balance between risk groups. See Appendix Figure A.3 for each criterion's predictive power of future SA take-up.

the high- and low-risk group's potential outcomes by starting age cohort move in parallel over outcome ages. In other words, calendar time shocks that affect different cohorts do not vary between the high- and low-risk groups.

This assumption is violated if business cycles, structural labor market changes, or increasing returns to schooling unevenly affect low- and high-risk groups over time. Section 5.4 tests and rejects that these confounding factors drive my results. This is likely because my analytical sample is restricted to a low-SES population.[31] This contrasts with prior work on welfare reform's effects on youth that tend to define risk groups by higher- versus lower-income or educated backgrounds,[32] which may be more susceptible to non-parallel trends. Conditioning on $\boldsymbol{X}_i$ also accounts for the possibility that potential outcomes evolve differently by risk group due to varying $\boldsymbol{X}_i$ distributions.

Following standard arguments, Assumption 1 implies the target parameter $\beta_{\alpha,a}$ can be nonparametrically identified via TD. The identifiable family of effects consists of starting ages $\alpha < 30$ and outcome ages below 30 ($A_\alpha = 29$): $\{\beta_{\alpha,a} \,|\, \alpha \leq 29, \alpha \leq a \leq 29\}$. This is because I am limited by the oldest birth cohort in my sample, which has starting age $\alpha = 30$. To get a sense of longer-run impacts, I also consider starting age-outcome age effects *relative to* $\alpha = 30$,

$$\beta^{30}_{\alpha,a} \equiv \mathbb{E}[Y_{ia}(\alpha) - Y_{ia}(30) \,|\, D_i = \alpha, G_i = 1], \tag{2}$$

and define β^{30}_{α} analogously. This parameter is identified using $\alpha = 30$ as the only comparison cohort instead of all $\alpha > a$. The advantage of this parameter is that it can be identified for outcome ages beyond 29. Checking $\beta_{\alpha,a} = \beta^{30}_{\alpha,a}$ for ages $a \leq 29$ tests whether my research design is sensitive to using different starting age cohorts as comparison groups.

I estimate an event-study specification for each starting age α to estimate starting age-outcome age effects. For a given α, I restrict all panels to the range of outcome ages available for starting age α up to age $A_\alpha = 29$, denoted $\mathcal{A}_\alpha$. I further restrict panels of outcomes such that, for each outcome age $a \in \mathcal{A}_\alpha$, individuals belong to either the "treated" or "not-yet-treated" starting age cohorts: $D(b_i) = \alpha$ or $D(b_i) > a$. On this subsample, I estimate my preferred regression specification,[33]

$$\begin{aligned} Y_{ia} = & \sum_{k \in \mathcal{A}_\alpha \setminus \{\alpha-1\}} \beta_{\alpha,k} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a = k) \Big) \\ & + \delta_1 G_i + \delta_{b(i)} + \delta_a + \pi_{12} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \Big) + \pi_{1,a} G_i + \pi_{2,a} \mathbb{1}(D(b_i) = \alpha) \\ & + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b_i + \theta_{m_{\alpha-1}(i)} + \varepsilon_{ia}. \end{aligned} \tag{3}$$

31. Indeed, Appendix Figure A.3 shows that the gap in later-life SA participation between risk groups persists even *conditional on* household income during childhood.

32. Examples include Kaestner et al. (2003), Hao and Cherlin (2004), Offner (2005), and Bastian et al. (2021, 2022).

33. Hence, all parameters in Equation (3) vary by α. I suppress α indices to reduce notational burden.

Equation (3) follows a similar spirit as the interacted event-study model proposed by Sun and Abraham (2021). To estimate $\beta^{30}_{\alpha,a}$, I instead define $\mathcal{A}_\alpha$ to include all outcome ages available for starting age α but subsample the data to individuals with $D(b_i) \in \{\alpha, 30\}$ only.

Parameters $\{\beta_{\alpha,k} \mid k \leq \alpha - 2\}$ serve as the "pre-trends" test of Assumption 1. They capture any differential trends between the risk-by-cohort groups before either face the reforms; accordingly, I should expect $\beta_{\alpha,k} = 0$ for $k \leq \alpha - 2$. However, for some outcomes Y_{ia}, the earliest outcome age available in the data exceeds the starting age of younger cohorts. For example, income is observed beginning at age 16, so there are no pre-trends available for starting ages $\alpha \leq 16$. In these cases, the event-study parameters are either normalized around outcome age 17 instead of $\alpha - 1$ and, if necessary, renormalized to DD estimates at age 17.[34] Given their lack of pre-trends, I conduct a battery of falsification tests to check if these estimates capture confounding factors.

In addition to controlling for $\boldsymbol{X}_i$, Equation (3) also controls for their linear trends over cohorts b and over the life cycle a. These serve as a parametric means of satisfying Assumption 1; nevertheless, my results are robust to excluding $\boldsymbol{X}_i$ and their trends.

I also use a simple pre-post (binarized) version of Equation (3) that estimates β_α (β^{30}_α), the average effect between ages α and $A_\alpha = 29$ ($A_\alpha =$ highest available age) for each starting age α.[35] β_α provides a simple way to capture starting age heterogeneity.

Standard errors are two-way clustered by the municipality of residence at age $\alpha - 1$ and by birth quarter (Cameron et al. 2011). Municipalities had some discretion over implementing local SA work requirements. Birth quarters demarcate birth cutoffs of the starting age cohorts and capture cyclical patterns, such as variation in school starting age.[36]

Limits of difference-in-differences. To justify this TD design, it is useful to consider the limitations of a difference-in-differences (DD) approach. There are three possible DD designs.

The first approach exploits differences between starting ages and outcome ages within the high-risk group only. The identifying parallel trends assumption is that, absent any reforms, the outcomes of starting age $D(b) = \alpha$ would have evolved from ages α to a in parallel to the outcomes of starting age cohorts $D(b) > a$. Nevertheless, it is unlikely this condition is satisfied due to confounding calendar time shocks that affect the potential outcomes of cohorts at specific ages. For example, comparing outcomes at age $a = 28$ between starting ages $\alpha = 8$ (the 1981 cohort)

34. Normalizing at age 17 is innocuous for outcomes that occur at low frequencies before age 18 and thus unlikely to significantly change as a result of the reform (e.g., income, SA participation). For outcomes that frequently occur before age 18 (e.g., education, incarceration), I adjust estimates of $(\beta_{\alpha,a})_{a \in \mathcal{A}_\alpha}$ for $\alpha \leq 17$ by adding the analogous DD event-study estimate on outcomes measured at age 17:
$Y_{i17} = \sum_{\alpha \leq 16,\ \alpha \geq 18} \beta_{\alpha,17} \left(G_i \times \mathbb{1}(D(b) = \alpha)\right) + \delta_1 G_i + \delta_b + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta} \boldsymbol{X}_i \times b_i + \theta_{m_{16}(i)} + \varepsilon_i$.

35. For outcomes that occur at higher frequencies prior to age 18, I adapt the DD regression described in Footnote 34 by replacing the outcome variable with the mean outcome over ages $a \leq 17$.

36. In Denmark, birth years determine when children enter 1st grade, which has implications on later-life outcomes (Landersø et al. 2017,2020).

and $\alpha = 29$ (the 1964 cohort) would be confounded by the 2009 recession, which affects $\alpha = 8$ at $a = 28$ but not $\alpha = 29$ at that age. The TD method purges these confounding effects by further differencing the DDs between risk groups.

The second DD approach compares the evolution of outcomes over the life cycle between risk groups of each starting age α. Identification hinges on the assumption that the high- and low-risk groups' counterfactual outcomes evolve in parallel over the life cycle. However, risk groups possess significant differences in baseline educational attainment and labor force attachment, leading to diverging life-cycle outcomes, even absent the reforms.

The third DD approach relies on variation between risk groups and starting age groups, fixing the outcome variable at a particular outcome age. This approach relies on the strong assumption that there are no cohort trends (e.g., changing returns to education) that differentially affect risk groups. While this assumption unlikely holds, it is perhaps the most reasonable among all of the DD approaches. I thus use this DD design as a robustness check to demonstrate trend breaks in outcomes among cohorts exposed to the SA reforms.

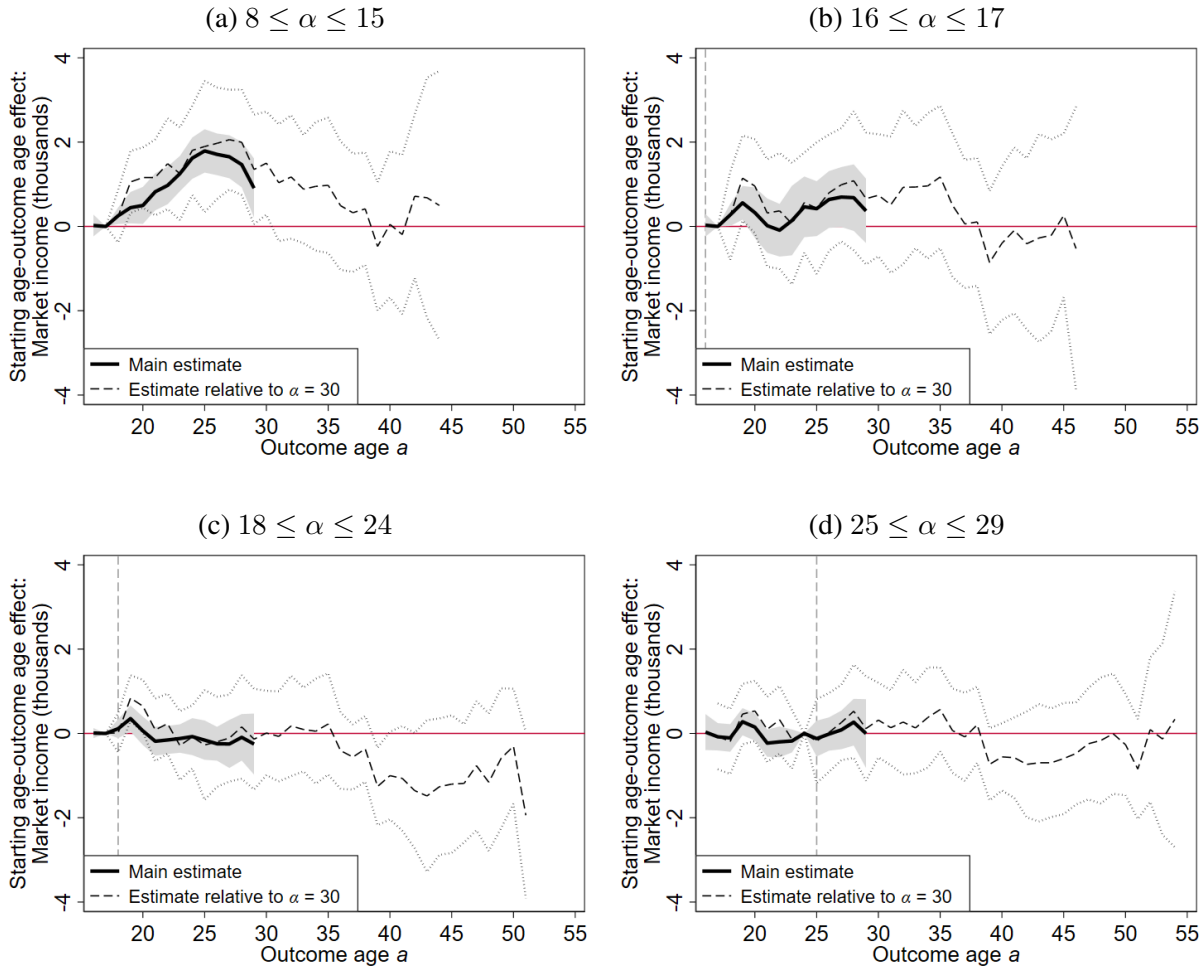
Interpretation of identified parameters. The starting age-outcome age parameter $\beta_{\alpha,a}$ is the overall average effect of a high-risk individual facing the welfare-to-work reforms starting at age α on outcomes measured at age a. This captures several underlying channels, including the intent-to-treat effect of SA work requirements, spillovers from parents affected by the reforms, and anticipating facing SA work requirements before becoming eligible for SA. My main results focus on the composite effect; Section 6 examines its underlying channels for younger cohorts.

Note that the TD strategy compares risk groups with different potential intensities of being affected by the reforms, so it follows a similar spirit as fuzzy DD designs (de Chaisemartin and D'Haultfoeuille 2018). With this in mind, the parameter $\beta_{\alpha,a}$ can be interpreted as a lower bound of the full effect if, on average, the effect of the reforms on the high-risk group is larger than that of the low-risk group (Fricke 2017).

5 Long-Run Effect Estimates of Welfare-to-Work

This section reports my paper's main results. The discussion is organized in five parts. First, I report starting age-outcome age estimates on labor market outcomes. Second, I discuss effects on welfare participation, education, health, and crime over the starting age dimension. Third, I summarize heterogeneous effects across various population subgroups. Fourth, I report a battery of robustness checks to help increase confidence in my results. Finally, I discuss suggestive evidence of intergenerational spillover effects.

Figure 2: Welfare-to-work's effects on life-cycle income, by starting age group



Notes: This figure plot estimates of the starting age-outcome age effects from the event-study regression described in Equation (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% point-wise confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% point-wise confidence intervals. Confidence intervals are based on robust standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform. Vertical dashed lines indicate the minimum α in each starting age group, corresponding to the group's earliest exposure to the reforms.

5.1 Starting Age-Outcome Age Effects on Labor Market Outcomes

Figure 2 plots event-study estimates of the starting age-outcome age effects $\beta_{\alpha,a}$ (solid line) and $\beta^{30}_{\alpha,a}$ (dashed line) for market income (i.e., gross income before taxes and transfers), a key barometer of self-sufficiency, aggregated by four starting age groups. These starting age groups—$8 \leq \alpha \leq 15$, $16 \leq \alpha \leq 17$, $18 \leq \alpha \leq 24$, and $25 \leq \alpha \leq 29$—represent key life-cycle stages related to education, SA, and labor force participation (explained below).

This figure illustrates striking heterogeneity in the effects of welfare-to-work reforms across starting age groups. Focusing on Panel (a), starting age cohorts $8 \leq \alpha \leq 15$ ("young children")—

who are still in compulsory schooling at the time of the reforms—experience a growing *positive* effect on market income that peaks to nearly $2,000 by their mid-20s. Relative to $\alpha = 30$, estimated effects on market income remain positive up to their late-30s, before fading to economically insignificant magnitudes. Meanwhile, in Panels (b)–(d), the starting age-outcome age estimates of older cohorts follow a very different pattern. For starting age cohorts $16 \leq \alpha \leq 17$ ("older children")—who can drop out of high school—their point estimates are also generally positive until the late-30s but are more attenuated and statistically insignificant. Astoundingly, starting ages $18 \leq \alpha \leq 24$ ("young adults")—who have reached the SA eligibility age—incur null or modest *negative* effects during adulthood that grows to statistically significant magnitudes beyond age 40 (relative to $\alpha = 30$). Finally, starting ages $25 \leq \alpha \leq 29$ ("older adults")—who can receive the higher SA benefit tier—have largely statistically insignificant estimates across all outcome ages. Collectively, these estimates reveal that welfare-to-work has disparate impacts across starting age cohorts that can persist for decades. The next subsection rationalizes this starting age heterogeneity in light of patterns in estimated effects found in other outcomes.

Reassuringly, the figure also supports the validity of my research design. Among young and older adults $18 \leq \alpha \leq 24$ and $25 \leq \alpha \leq 29$ where pre-trends are available (Panels (c) and (d)), there are precisely-estimated null effects prior to the reform (indicated by the estimates falling to the left of the vertical dashed line), giving greater confidence in the parallel trends assumption. In addition, estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ track each other quite closely over all common outcome ages $a \leq 29$, showcasing my results are not spuriously driven by certain not-yet-treated cohorts.

Table 1 reports event-study estimates of the starting age-outcome age effects $\beta_{\alpha,a}$ from Equation (3) for other labor market outcomes. To gain precision and improve readability of results, I average event-study estimates over ranges of outcome ages by the four starting age groups; estimates of individual outcome ages are plotted in Appendix Figures A.5–A.7.

Panel A reports that welfare-to-work's effects on disposable income (after taxes and transfers) are far more attenuated than market income. Declines in disposable income among young adults ($18 \leq \alpha \leq 24$) are not statistically significant, indicating that this group is receiving more transfers that counteracts their declines in market income. Panel B reports estimates on net taxes, defined as the difference between gross income tax paid and public transfers received. Relative to $\alpha = 30$, the government raises about $340–1,000 per year from children ($8 \leq \alpha \leq 15$) when they are between ages 18–39 but incurs costs of around $80–400 per year from young adults. This indicates that welfare-to-work can generate uneven fiscal impacts over a generational span of cohorts.

Finally, Panel C reports estimates on employment, corresponding to Denmark's "basic amount" benchmark of self-sufficiency (defined by market income >$7,000). Welfare-to-work increases employment primarily among children before outcome age 30. Beyond age 30, employment (relative to $\alpha = 30$) increases by 1–5 percentage points (1.5–8% relative to the mean), with weaker

Table 1: Welfare-to-work's effects on other labor market outcomes, by starting age-outcome age group

Starting age group	Main estimates ($\beta_{\alpha,a}$)			Relative to $\alpha = 30$ ($\beta^{30}_{\alpha,a}$)		Mean [S.D.]
	$16 \leq a \leq 17$	$18 \leq a \leq 24$	$25 \leq a \leq 29$	$30 \leq a \leq 39$	$40 \leq a \leq 54$	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Disposable income (thousands $)						
$8 \leq \alpha \leq 15$	0.010	−0.002	0.432***	0.424	0.202	21.891
	(0.048)	(0.115)	(0.146)	(0.292)	(0.475)	[6.915]
$16 \leq \alpha \leq 17$	0.001	−0.074	0.207	0.560	0.106	22.754
	(0.049)	(0.125)	(0.164)	(0.344)	(0.471)	[7.255]
$18 \leq \alpha \leq 24$	−0.013	0.079	−0.051	0.181	−0.353	23.007
	(0.021)	(0.064)	(0.108)	(0.187)	(0.323)	[7.322]
$25 \leq \alpha \leq 29$	−0.028	0.053	0.003	0.089	−0.214	23.758
	(0.101)	(0.054)	(0.097)	(0.123)	(0.183)	[7.491]
B. Net taxes paid (thousands $)						
$8 \leq \alpha \leq 15$	0.002	0.840***	1.074***	0.340*	0.031	−0.756
	(0.038)	(0.100)	(0.128)	(0.160)	(0.286)	[8.511]
$16 \leq \alpha \leq 17$	0.013	0.315***	0.354**	−0.054	−0.319	−0.450
	(0.040)	(0.100)	(0.165)	(0.257)	(0.289)	[9.092]
$18 \leq \alpha \leq 24$	0.018	−0.080	−0.148	−0.391*	−0.758**	−0.280
	(0.015)	(0.064)	(0.125)	(0.178)	(0.216)	[9.303]
$25 \leq \alpha \leq 29$	0.004	−0.094	0.039	0.037	−0.214	1.053
	(0.085)	(0.072)	(0.058)	(0.142)	(0.218)	[9.719]
C. Employed (income above basic amount)						
$8 \leq \alpha \leq 15$	0.001	0.003	0.030***	0.037*	0.048**	0.620
	(0.003)	(0.009)	(0.007)	(0.016)	(0.014)	[0.322]
$16 \leq \alpha \leq 17$	0.002	−0.009	0.006	0.035*	0.029*	0.624
	(0.004)	(0.010)	(0.009)	(0.016)	(0.014)	[0.320]
$18 \leq \alpha \leq 24$	0.004	−0.009	−0.006	0.030*	0.031**	0.631
	(0.002)	(0.006)	(0.008)	(0.015)	(0.013)	[0.321]
$25 \leq \alpha \leq 29$	−0.005	−0.003	−0.001	0.010	0.015	0.666
	(0.007)	(0.004)	(0.004)	(0.009)	(0.008)	[0.310]

Notes: Columns (1)–(3) report estimates of $\beta_{\alpha,a}$ from Equation (3) aggregated by groups of starting ages and outcome ages; Columns (4)–(5) report estimates of $\beta^{30}_{\alpha,a}$. Column (6) reports mean outcomes among the high-risk group; standard deviations are in brackets. Shaded cells indicate pre-reform outcome ages. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

effects among older starting ages. The minimal labor supply responses among adults is consistent with recent studies on work requirements (Gray et al. 2023, Cook and East 2024). To better understand labor supply responses, Appendix Figure A.12 plots TD estimates of the probability that income exceeds various thresholds. The reforms increase extensive labor supply across all starting ages but particularly for $\alpha < 18$, with estimates upward of 6 percentage points (9%). Decreased

intensive labor supply explains the modest declines in income among young adults.

5.2 Starting Age Heterogeneity in Effects on Other Outcomes

Table 2 summarizes estimated effects of the welfare-to-work reforms on eight later-life outcomes.[37] Each row reports TD estimates of β_α and β_α^{30} aggregated by the four starting age groups as before. Several of the outcomes reported here—such as hospital care, incarceration, and DI transfers—are scarcely examined in the literature on welfare reforms but are critical ingredients for cost-benefit analysis. Rows 3 and 4 report previously-discussed estimated effects on market income and employment for reference.

The remainder of this subsection discusses welfare-to-work's effects on each outcome in turn. Appendix Tables A.5–A.9 take a closer look at categories of related outcomes. As a visual aid, Figure 3 plots these aggregated group estimates (as horizontal black lines) and individual β_α estimates (as gray dots) for three outcomes.[38] The estimated effects showcase prominent heterogeneity across starting age cohorts, and that the nature of this heterogeneity differs across outcomes. This subsection concludes with a rationale for the heterogeneous patterns found across outcomes.

SA participation. To begin, Table 2, Row 1 reports welfare-to-work's estimated effects on duration of overall SA receipt, which includes the passive period and SA work program participation. Overall, I find that welfare-to-work causes young children ($8 \leq \alpha \leq 15$) to decrease their participation in SA. While this aggregate estimate is statistically insignificant, Figure 3, Panel (a) reveals a strong gradient in starting ages, where the youngest cohorts have significant declines in SA participation by up to -0.35 months per annum. Meanwhile, older children ($16 \leq \alpha \leq 17$) have modestly higher uptake of SA, though this does not appear to persist beyond age 29 based on the β_α^{30} estimate. Adults ($18 \leq \alpha \leq 29$) have no significant changes in duration of SA participation.

Nonetheless, Appendix Table A.5 reveals that long-term take-up of SA along the *extensive margin* (measured by β_α^{30}) declines among *all* starting age groups, with strongest effects among older children and young adults. This implies work requirements deter individuals from SA receipt. Given that overall *duration* on SA remains positive but insignificant among $\alpha \geq 16$, this suggests that work requirements induce a "lock-in" effect among participants (Rosholm and Svarer 2008).

As expected, Row 2 shows all starting age groups experience robust, statistically significant increases in their participation in SA work requirements after the reforms. Pairing these estimates with Row 1 indicates that welfare-to-work successfully substituted passive benefit receipt for work

37. Appendix Tables A.3 and A.4 validate that these results are robust to alternative TD regression specifications. Adding controls like municipality fixed effects improve estimates' precision; adding trends in background characteristics affects the magnitude of results but does not change any qualitative conclusions about starting age heterogeneity. Appendix Figures A.8–A.10 plot event-study estimates of starting age-outcome age effects of additional outcomes.

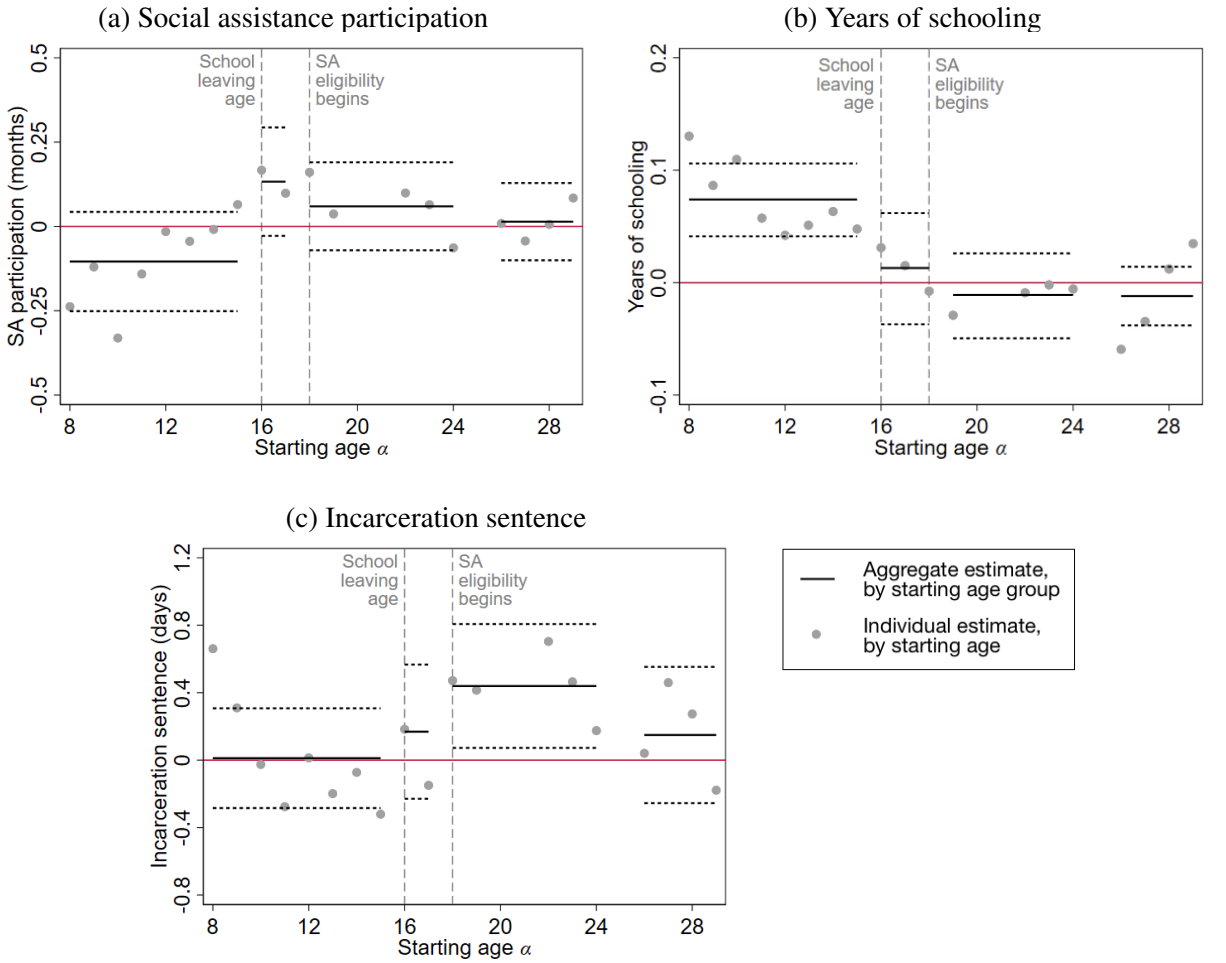
38. Appendix Figure A.11 plots individual β_α estimates with confidence intervals for all eight outcomes.

Table 2: Summary of welfare-to-work's effects on main outcomes, by starting age group

		Starting age group				Mean
		$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	[S.D.] (5)
1. SA participation, total (months)	β_α	-0.104	0.133	0.060	0.014	2.37
		(0.073)	(0.080)	(0.064)	(0.055)	$[3.05]$
	β_α^{30}	-0.123	-0.013	-0.128	-0.002	1.98
		(0.092)	(0.087)	(0.100)	(0.055)	$[2.74]$
2. SA work req's (months)	β_α	0.439^{***}	0.416^{***}	0.189^{***}	0.117^{***}	0.56
		(0.024)	(0.034)	(0.019)	(0.016)	$[1.09]$
	β_α^{30}	0.320^{**}	0.196^{***}	0.051^{**}	0.095^{**}	$[0.52]$
		(0.000)	(0.000)	(0.020)	(0.018)	$[0.98]$
3. Market income (thousands \$)	β_α	1.083^{***}	0.348	-0.043	0.153	16.32
		(0.234)	(0.136)	(0.123)	(0.238)	$[10.76]$
	β_α^{30}	1.123^{**}	0.419	-0.512	-0.235	19.90
		(0.360)	(0.464)	(0.278)	(0.227)	$[12.72]$
4. Employed (income $>$ basic amount)	β_α	0.012	-0.005	-0.003	0.002	0.61
		(0.007)	(0.007)	(0.005)	(0.006)	$[0.32]$
	β_α^{30}	0.041^{**}	0.030^{**}	0.018^{*}	0.014^{**}	0.63
		(0.013)	(0.012)	(0.009)	(0.005)	$[0.31]$
5. Years of schooling	β_α	0.105^{***}	0.040	-0.005	-0.040^{*}	10.50
		(0.022)	(0.036)	(0.021)	(0.021)	$[1.54]$
	β_α^{30}	0.083^{**}	0.035	-0.039	-0.059	10.79
		(0.035)	(0.055)	(0.045)	(0.053)	$[1.74]$
6. Inpatient hospital care (days)	β_α	-0.072^{**}	-0.010	-0.042	0.083^{*}	0.81
		(0.028)	(0.041)	(0.025)	(0.041)	$[1.78]$
	β_α^{30}	-0.048	0.011	-0.063	-0.014	0.72
		(0.061)	(0.067)	(0.050)	(0.037)	$[1.46]$
7. Incarceration sentence (days)	β_α	0.012	0.017	0.446^{**}	0.149	1.94
		(0.147)	(0.195)	(0.193)	(0.192)	$[14.28]$
	β_α^{30}	0.204	0.177	0.321^{*}	-0.146	1.80
		(0.236)	(0.262)	(0.166)	(0.176)	$[12.75]$
8. DI benefits (thousands \$)	β_α	-0.016	-0.074^{**}	0.061	0.004	0.33
		(0.031)	(0.035)	(0.037)	(0.044)	$[1.97]$
	β_α^{30}	-0.002	0.000	0.103	-0.036	0.77
		(0.087)	(0.101)	(0.099)	(0.101)	$[2.94]$

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Row 5 reports estimates of β_α^{30} relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data limitations. All estimates are based on a main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. Column (5) reports the high-risk group's mean outcomes between ages 18–29 (for β_α) or ages 18–37 (for β_α^{30}); standard deviations are in brackets. Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 3: Starting age heterogeneity in welfare-to-work's effects on select outcomes



Notes: This plots estimates of β_α for different outcomes. Horizontal solid lines are aggregate estimates of β_α by starting age group; dashed lines are 95% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities. Gray dots are the point estimates of β_α for each starting age α. Appendix Figure A.11 provides a version of this figure with confidence intervals around each estimate of β_α and additional outcomes.

program participation. The positive effect on SA work program participation decreases in starting age, reflecting the increased intensity of work requirements from reforms over time (see Appendix Table A.1) and the decline in SA receipt during adulthood (see Appendix Figure A.13).

Education. Effects on years of schooling, reported in Row 5 of Table 2, follow a similar pattern as market income: young children reap 0.105 additional years of schooling, while older cohorts $\alpha \geq 16$ incur smaller or null effects. The gray dots in Figure 3, Panel (b) illustrate evidence of a gradient where younger starting age cohorts tend to attain more education. But as soon as α crosses 16—corresponding to the typical school dropout age—the estimates diminish to statistically insignificant levels. These results corroborate a key model prediction: the reforms have

larger impacts on later-life income of among younger starting ages than their older counterparts since work requirements encourage human capital investments through education.

Appendix Table A.6 finds the gains among young children are driven by a 2.0 percentage point (4.6% relative to mean) increase in high school completion and 2.6 percentage point (7.0%) increase in post-secondary school attendance.[39] Increased schooling, coupled with high extensive labor supply response, explains the reforms' large effects on market income among young children.[40] The event-study estimates of Appendix Figure A.8 highlight these gains are concentrated between outcome ages 16–24 before gradually fading out in the long run. This is driven by the counterfactual that, absent welfare-to-work, individuals slowly upgrade their educational level over time (see Appendix Figure A.14). In this way, welfare-to-work accelerates educational attainment among youth. In contrast, the modest negative effect among older adults ($25 \leq \alpha \leq 29$) suggests SA work programs may inhibit educational upgrading, consistent with Riddell and Riddell (2014).

Health. Row 6 shows the reforms reduce duration of inpatient hospital care among young children ($8 \leq \alpha \leq 15$) by 0.072 days per year (-9.2% relative to mean). Meanwhile, there is a break for older cohorts ($\alpha \geq 16$), where effects on hospital care are largely statistically insignificant. If anything, there is some suggestive evidence of an increase in hospital care among older adults, but this does not persist beyond outcome age 30. Given only young children gained higher educational attainment, these results align with the well-documented education–health gradient (Cutler and Lleras-Muney 2008).[41]

Appendix Table A.7 validates that reduced hospital care among young children is robust to excluding birth-related hospital care, indicating this result is not driven by welfare-to-work's effect on fertility. Event-study estimates plotted in Appendix Figure A.10 reveal this decline is concentrated between outcome ages 16–24. Unlike other outcomes, hospital records are available beginning in 1977, permitting me to check there are no pre-trends for all starting age groups.

Crime. Starting age heterogeneity in the effects on incarceration have a starkly different pattern than human capital-related outcomes. Row 7 reports that welfare-to-work increases incarceration

39. Landersø and Heckman (2017) show descriptive evidence that the SA reforms encouraged college attendance.

40. Appendix Figure A.12 finds that welfare-to-work increased extensive-margin labor supply (measured by income $\geq$\$500) among $8 \leq \alpha \leq 15$ by 6 percentage points between ages 18–29; given a mean income of about \$16,306, this explains \$967 of the \$1,083 total increase in market income. If the 0.105 additional years of schooling explains the remaining \$115, this translates to a 6.7% internal rate of return (IRR) to schooling ($\log(1+\frac{\$115}{\$16,306}) \div 0.105 \times 100\% = 6.7\%$). This is nearly identical to the 6.6% IRR estimated in Denmark in 1995 by Harmon et al. (2001).

41. Declines in hospital care may be arguably interpreted as genuine improvements in health rather than changes in receipt of hospital care. First, healthcare in Denmark has been universal and free with automatic enrollment at birth since 1973, implying there were likely limited frictions to receiving hospital care. Second, my measure of inpatient hospital care only includes overnight hospital stays, which capture relatively serious health events that would likely encourage individuals to overcome any remaining frictions to seek care.

sentence among young adults ($18 \leq \alpha \leq 24$) by 0.446 days (23% increase relative to the mean). This increase in crime helps rationalize the modest negative effects on income among young adults. In contrast, I find statistically insignificant increases ranging between 0.012–0.149 days for the other starting age groups. These results highlight that welfare-to-work reforms can unintentionally encourage socially costly behavior, especially among young adults.

Appendix Table A.8 finds that the annual probability of being convicted of crime decreases across all starting age groups, suggesting that welfare-to-work deters crime for the average individual but significantly increases crime among young adults who are already criminally active. Most of the effect on incarceration sentence is explained by increased property crime, consistent with Deshpande and Mueller-Smith (2022), though also partly by increased violent crime. Appendix Figure A.9 illustrates that incarceration sentences for young adults have flat pre-trends, gradually grow to statistically significant levels by age 30, and fade-out thereafter.[42]

Social insurance participation. Finally, Row 8 finds evidence that the reforms decreased DI benefits among children ($\alpha \leq 17$) but increased DI benefits among young adults ($18 \leq \alpha \leq 24$). This suggests that welfare-to-work decreased overall dependence on the social safety net among younger cohorts but incentivized substitution toward alternative programs for some older cohorts. Appendix Table A.9 provides a more comprehensive analysis on DI and UI receipt that supports this view. Children are significantly less reliant on both DI and UI on the extensive and intensive margins. Meanwhile, there are far more muted or opposite-signed effects among adults.

Understanding heterogeneity through choice margins. To make better sense of the starting age heterogeneity found across different outcomes, Appendix Table A.10 describes how starting age groups likely face different choice margins at the time of the reforms.[43] Notably, the education margin is less likely to be available to adults, who were exposed to the reforms *after* they likely left school. Meanwhile, all starting age groups likely possess crime, employment, and SA in their choice sets; DI and UI are only partially available margins since only some can meet their eligibility criteria. Differences in availability of the education choice margin helps rationalize the heterogeneous patterns across starting age groups across outcomes. Children who were already in school opted to attain more education. Young adults likely lacked this margin, and instead responded to SA work requirements by moving toward crime or alternative social insurance programs like DI.

42. The gradual increase in incarceration sentencing is partly mechanical, as court sentencing tend to lag the actual date of offenses by about 200 days and sometimes beyond one year. Kyvsgaard (2004) notes there were no major changes to Danish criminal justice policy in the 1990s and early 2000s, giving greater confidence that the estimated effects are not driven by confounding policy changes.

43. Appendix Figures A.13–A.17 empirically validate this by plotting life-cycle dynamics of each choice.

5.3 Heterogeneity over Population Subgroups

Having reported the overall effects of welfare-to-work, I now examine its effects by gender, household income, education level, participation in SA or crime, and household structure within the high-risk group. Examining heterogeneity across these population subgroups helps clarify the patterns found in my main results. These estimates are also interesting in their own right since these subgroups are commonly targeted by welfare programs.

To explore heterogeneity over subgroups, I estimate a quadruple differences specification

$$\begin{aligned} Y_{ia} = {} & \beta_\alpha\Big(G_i \times \mathbb{1}(D(b_i)=\alpha) \times \mathbb{1}(a \geq \alpha)\Big) + \beta^S_\alpha\Big(Subgroup_i \times G_i \times \mathbb{1}(D(b_i)=\alpha) \times \mathbb{1}(a \geq \alpha)\Big) \\ & + \delta_1 G_i + \delta_b + \delta_a + \pi_{12}\Big(G_i \times \mathbb{1}(D_{ib}=\alpha)\Big) + \pi_{1,a} G_i + \pi_{2,a}\mathbb{1}(D_{ib}=\alpha) \\ & + Subgroup_i \times \Big[\delta^S_1 G_i + \delta^S_b + \delta^S_a + \pi^S_{12}\Big(G_i \times \mathbb{1}(D_{ib}=\alpha)\Big) + \pi^S_{1,a} G_i + \pi^S_{2,a}\mathbb{1}(D_{ib}=\alpha)\Big] \\ & + \boldsymbol{\gamma}\boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b + \theta_{m_\alpha(i)} + \varepsilon_{iba}, \end{aligned} \tag{4}$$

where $Subgroup_i$ is a binary indicator of a subgroup of interest. The parameter of interest $\beta^{Subgroup}_\alpha \equiv \beta_\alpha + \beta^S_\alpha$ reflects the effect of the reforms on the high-risk subgroup. For subgroups not based on gender or background characteristics, I only consider starting ages α older than the time that $Subgroup_i$ is measured to avoid endogeneity bias.

Appendix Tables A.11–A.15 provide results for several main outcomes and reveal three interesting patterns. First, welfare-to-work generates larger increases in income and education for women than men; in particular, men explain the decline in income among young adults ($18 \leq \alpha \leq 24$). Second, young adults who committed crime by age 18 experience the greatest loss in income—a statistically significant \$750 per annum between ages 18–29. This lends support to the hypothesis that increased incarceration and decreased income are driven by those already criminally activated and further rationalizes differences by gender. Indeed, Appendix Table A.14 shows that increases in incarceration among young adults is explained by men. Third, evidence suggests that parents may have benefited more from welfare-to-work than childless adults. Those who were parents by age 25 experienced larger (but imprecise) effects on income; single mothers experienced negative (and precise) effects on hospital care. This suggests welfare-to-work might have spillover effects on these cohorts' own children. I revisit this point in the final subsection.

5.4 Robustness Analysis

The previous results suggest there is significant heterogeneity in welfare-to-work's long-run effects across a generation of starting age cohorts. This section assesses the robustness of this finding by examining potential threats to the internal validity of my research design and using alternative measurement choices and model specifications.

The internal validity of my analysis rests on the parallel trends condition (Assumption 1). While my event-study estimates exhibit flat pre-trends for older starting age cohorts, many outcomes can only be observed after the starting age of younger cohorts. Irrespective of data availability, pre-trend tests do not guard against factors that violate parallel trends, such as other policy or economic changes that differentially affect risk-cohort groups. I focus on key falsification exercises that test for such confounders, followed by a brief summary of other robustness checks; additional details are provided in Appendix D. Overall, my main results are robust to a battery of tests and alternative specifications.

Main falsification tests. Figure 4 plots estimates on market income, years of schooling, and incarceration sentence from several falsification tests. The first row reproduces my main estimates from Table 1 for reference. The remaining rows plot estimates on samples that should be unaffected by the SA reforms. This figure focuses on effects on young children ($8 \leq \alpha \leq 15$) for income and schooling and young adults ($18 \leq \alpha \leq 24$) for incarceration, which are among my most prominent results; Appendix Figure D.6 provides results for all starting age groups.

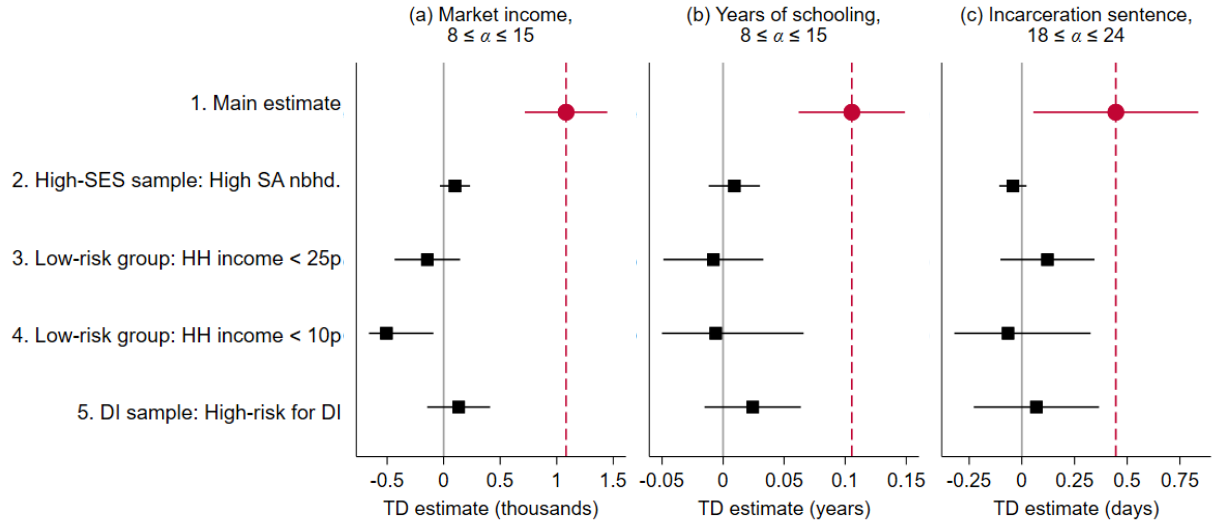
Row 2 finds precise null effects among a high-SES sample who are unlikely to be affected by the SA reforms. In this specification, risk groups are replaced with an indicator of whether an individual grew up in a neighborhood block with an above-median long-run SA participation rate. In this way, this result demonstrates that differences in neighborhood blocks over time, such as school quality or local labor markets, do not lead to diverging later-life outcomes.

Another threat to my research design is the presence of other confounding policy or economic changes that predominantly affect the low-SES population, such as education reforms, structural labor market changes, or increasing returns to schooling. Row 3 tests the presence of such confounders *within* the low-risk group $G_i = 0$ by comparing individuals between the first and second quartiles of the childhood household income distribution. The null estimates suggest these confounders do not drive my results. Row 4 repeats this exercise except splits the sample at the 10th percentile, demonstrating these results are insensitive to my choice of household income cutoff.[44]

One may be concerned that low-SES individuals who belong to the low-risk group are not subject to policy or economic changes that specifically affect welfare recipients. To check this, Row 5 plots estimates from a sample of low-SES individuals at risk of participating in DI, which targeted disadvantaged individuals with limited capacity for work. Here, the "high-risk for DI" group includes individuals who grew up in households on DI but *not* SA, while the "low-risk for DI" group includes those from households on neither program. Work requirements in DI were not introduced until 1998, only applied to DI recipients with partial work capacity, and were less

44. These regression specifications exclude controls for childhood household income and assets to avoid overcontrolling for the low-income group indicators.

Figure 4: Falsification tests for select outcomes and starting age groups



Notes: This figure plots estimates of β_α from variations of the TD specification, aggregated by starting age group. Row 1 plots the main estimates (in red circles); vertical dashed lines signify their magnitude. Rows 2–5 are falsification tests (in black squares). Row 2 uses a high-SES sample (i.e., household income $>$ median or mothers with $\geq$12 years of schooling) and defines treatment and control groups as individuals from high- and low-SA neighborhood blocks, respectively (see main text for details). Row 3 uses low-risk sample and defines treatment and control groups as individuals whose household income (age 12–17) are in the first and second quartiles, respectively. Row 4 repeats the Row 3 except uses the first decile to define treatment and control groups. Rows 3–4 exclude household income and assets as controls in the regression to avoid overcontrolling for risk groups. Row 5 uses a subset of the base sample in which the treatment group is households that participated in disability insurance (DI) and not SA (ages 12–17) and the control group is households that participated in neither DI nor SA. Horizontal lines are 95% confidence intervals based on robust standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform; some intervals are truncated to maintain a reasonable scale.

intensive and more flexible than SA work programs. Thus, estimates for the high-risk for DI group should reflect confounders that also affect those at high-risk for SA. Reassuringly, these estimates are statistically indistinguishable from zero, suggesting that my main estimates genuinely reflect the effects of the SA reforms.

Other robustness exercises. I conduct a variety of other checks that support my results.

Difference-in-differences.—I estimate DD event-study specifications that takes differences between risk groups and between starting age cohorts on outcomes measured at a fixed outcome age $a \in \{16, \ldots, 28\}$. Focusing on this range of outcome ages affords pre-trends for the oldest starting ages $\alpha \geq a$. Appendix Figures D.1–D.3 corroborate the patterns found in my main results and possess either no pre-trends or trend breaks around starting age α.

Business cycles.—Assumption 1 would be violated if calendar time shocks like recessions differentially affect long-run life-cycle trends of risk groups with the same starting age (Stuart 2022). To empirically assess if this is a concern, I check whether estimates of $\beta_{\alpha,a}$ for income are

negatively correlated with recessions or unemployment. Reassuringly, Appendix Figure D.5 and Appendix Table D.2 find weak *positive* correlations.

Structural economic changes.—Similarly, Assumption 1 would be violated if structural labor market changes unevenly affect certain high-risk cohorts. To probe this, I check if effects differ in municipalities exposed to skill-biased technical change based on their historical manufacturing sector size. Appendix Table D.3 finds no evidence that this is true.

Placebo tests with early-life outcomes.—I check for differential trends between risk groups across starting ages in early-life outcomes measured before the reforms, including birth weight and inpatient hospital care during childhood. Appendix Figure D.4 and Appendix Table D.1 show effects across starting ages are statistically indistinguishable from one another.

Validation checks.—Appendix Figure D.6 reports several validation checks. Some test whether a 1994 reform that introduced work requirements for long-term UI recipients drive my results. Recall that most UI members do not participate in SA, so the SA and UI reforms targeted different populations. I re-estimate effects for a subsample of high-risk individuals from households that lacked UI membership; results are similar or larger in magnitude than my main estimates.[45]

Municipal adoption timing.—Recall that starting ages are defined by the timing of the *national* SA reforms. In practice, however, municipalities adopted work requirements at different times. I exploit this variation to validate the heterogeneity in effects across starting ages.[46] Appendix Figure D.8 shows that, as expected, effect patterns among older cohorts from early-adopting municipalities resemble those of younger cohorts from late adopters.

Alternative measurement choices and samples.—Finally, I check whether my results are sensitive to measurement or model specification choices. Appendix Figures D.9–D.11 show my results are robust to using alternative risk groups (e.g., using household SA participation during childhood only), different subsamples, and shortened windows of not-yet-treated cohorts as a comparison group in my TD strategy.

5.5 Intergenerational Spillover Effect Estimates

My results establish substantial heterogeneity in intragenerational effects of welfare-to-work across starting ages, which sometimes persist for decades over the life cycle. Does this heterogeneity spillover to these cohorts' own children? This section summarizes suggestive evidence supporting

45. I also estimate the effects of the *1994 UI reform* on a separate, non-overlapping sample of individuals at high-risk of receiving UI benefits (defined in Appendix D.3). These estimates showcase qualitatively different starting age heterogeneity, providing reassurance that the UI reform does not explain my main results and that my TD design appears to identify effects of distinct policy reforms.

46. Unfortunately, there are no centralized records of the dates that work requirements were adopted by each municipality. To identify municipal adoption dates, I estimate structural breaks in the available time series of SA work program take-up through a procedure similar to Card et al. (2008). See Appendix D.4.

this view. Birth records enable me to link mothers in my main sample to their own children, offering a rare window into intergenerational spillover effects based on the *mother's* starting age.[47]

Nevertheless, these data come with two caveats. First, I restrict my analysis to the 62% of high-risk women who gave birth by age 25 to provide a sufficient time window to observe intergenerational outcomes, since many are first observed during late adolescence.[48] This introduces sample selection if the reforms affected the timing of child birth. Appendix Table A.16, however, finds the reforms have small, statistically insignificant effects on maternal birth age for younger cohorts.[49] Second, limited life-cycle snapshots of second-generation outcomes prevent using my usual TD design. Instead, I employ a DD strategy that compares how differences in second-generation outcomes born to high- and low-risk mothers vary by the mother's starting age, *relative to* starting ages 25–30 (similar to β_{α}^{30}).[50] While this cannot identify the magnitude of welfare-to-work's effects on second-generational outcomes, it can test for *differences* in effects between mother's starting age groups. Given these caveats, these results should be interpreted with caution.

Appendix Figure A.21 and Appendix Table A.17 report intergenerational effect estimates. Fascinatingly, these results suggest that starting age heterogeneity persists to the next generation. Second-generation children born to mothers of starting ages $8 \leq \alpha \leq 15$ receive 1.2 fewer months of SA between ages 18–20 relative to those born to starting ages $25 \leq \alpha \leq 30$. Meanwhile, there are insignificant estimates for children born to older starting age groups. These findings suggest that the reforms reduced SA dependence not only among the youngest starting age group but also their own future children. In contrast, second-generation children born to starting age group $16 \leq \alpha \leq 17$ have statistically significant lower market income between ages 18–20 and higher criminal activity between ages 15–17. To understand these patterns, note that fathers are, on average, 3.8 years older than mothers, and thus primarily belong to starting ages $18 \leq \alpha \leq 24$, who have lower income and commit more crimes due to the reforms. This starting age heterogeneity is amplified along gender lines. Daughters of starting age group 8–15 reap significant increases in educational attainment; sons of starting age group $16 \leq \alpha \leq 17$ drive the effects on crime.

Overall, these findings suggest that starting age heterogeneity persists in intergenerational outcomes. This highlights how welfare-to-work reforms likely has far-reaching consequences that span multiple generations.

47. Second-generation children are assigned to their mother's starting age since nearly all children cohabit with their mothers throughout their childhood.

48. Depending on the age of child outcomes and data availability, I sometimes exclude the youngest starting age cohorts in my estimation sample to preserve balance in maternal ages across cohorts. Appendix Table A.17, Column (1), reports the youngest starting age used in the sample for each outcome.

49. If the reforms delayed childbirth due to increased educational attainment, this implies my sample is *negatively selected*, implying the magnitude of my intergenerational effect estimates are lower bounds of their true effects.

50. Specifically, if $\tilde{\mathcal{A}} \equiv \{[8, 15], [16, 17], [18, 24]\}$ are starting age groups, then the DD regression is $Y_i = \sum_{\tilde{\alpha} \in \tilde{\mathcal{A}}} \beta_{\tilde{\alpha}} G_i \times \mathbb{1}(D(b_i) \in \tilde{\alpha}) + \delta_1 G_i + \delta_{b(i)} + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta} \boldsymbol{X}_i \times b_i + \theta_{m_{\alpha-1}(i)} + \varepsilon_i$.

6 Mechanisms Explaining Effects on Children

A key pattern uncovered in my results is that younger cohorts exposed to the reform *before* reaching the SA eligibility age of 18 tend to fare better than their older counterparts. This section investigates the mechanisms that explain this phenomenon.

As outlined in Bastian et al. (2021), all starting age-outcome age effects are comprised of three channels. The first channel is the *intent-to-treat (ITT) effect*, defined as the effect of potentially facing SA work requirements after becoming eligible for SA. The second is the *parental ITT effect*. Even though most individuals are ineligible for SA before age 18, their parents themselves could be directly affected by work requirements in ways that spillover to individuals. The third is the *anticipation effect*, which is the effect induced from individuals (or their parents) knowing the existence of SA work requirements before actually facing them. I examine each channel in turn.

6.1 Intent-to-Treat Effect

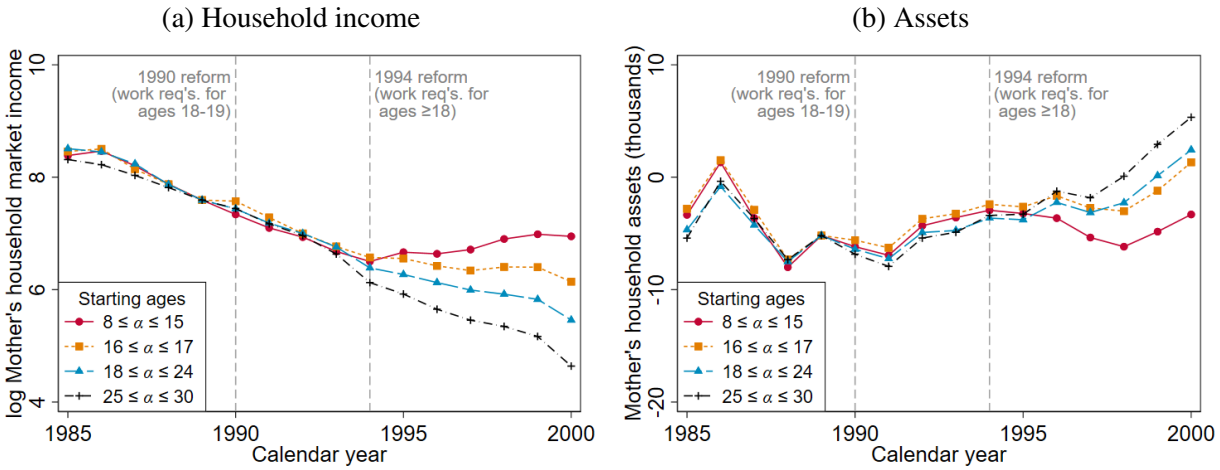
My main results suggest the large positive effects on children may not be explained by ITT effects. This is because adults provide a window into ITT effects: they lack anticipation effects and are likely less sensitive to parental ITT effects, but experienced largely null or negative effects. I formalize this intuition in a decomposition exercise below.

This argument is only valid, however, if ITT effects remain static. ITT effects could drive positive effects among children if SA's work programs improved in quality over time. Municipalities may have been unable to implement high-quality work programs immediately after the national reform to the detriment of older cohorts, but gained capacity to provide better services to younger cohorts aging into SA eligibility.

Unfortunately, measures of SA work program quality are scarce. I therefore develop a proxy for program quality: the municipality's rate of successfully imposing SA work requirements onto long-term SA recipients who exhausted the passive period (see Section 2.2). This "compliance rate" measures a municipality's capacity for serving SA recipients, which likely signals its program quality.[51] Appendix Figure A.18, Panel (a) shows the average compliance rate increases over time while Panel (b) reveals some municipalities have historically higher compliance rates than others. However, Appendix Figure A.19 finds no evidence that starting age effects on human capital-related outcomes are larger in municipalities with higher compliance rates. Overall, this suggests that ITT effects driven by improvements in program quality may not explain welfare-to-work's positive effects on children.

51. Municipal survey data shows that the compliance rate is positively correlated with the number of types of SA work programs (e.g., private sector job training, vocational education programs, etc.) offered by municipalities, which can better meet the diverse needs of SA recipients.

Figure 5: Trends in parents' household resources over time, by starting age group



Notes: This figure plots raw means of individuals' mother's annual household income and assets. Older starting age groups are normalized to the starting age group 8–15's level in 1989. Sample restricted to high-risk group.

6.2 Parental Intent-to-Treat Effect

In contrast, the results reported thus far suggest existence of parental ITT effects. Section 5.5 provided suggestive evidence of intergenerational spillover effects on the cohorts' *own children*. Additionally, Appendix Tables A.11 and A.15 report that parents of starting ages $25 \leq \alpha \leq 29$ saw some improvement in income and health relative to other subgroups of older adults. This suggests the cohorts' *own parents* responded to the reforms that shaped their later-life outcomes.

Unfortunately, it is not possible to separately identify the contribution of parental ITT effects toward the starting age-outcome age effects.[52] Instead, I focus on how the high-risk group's parents' financial well-being evolved around the welfare-to-work reforms.

Figure 5 plots raw trends of household market income and assets among the *mothers* of the high-risk group, by starting age group. Household income and assets move in parallel but sharply diverge after the 1994 reform, which expanded SA work requirements to all adults. Parents of younger starting age cohorts increase their market income but deplete (or borrow) more assets relative to parents of older starting age cohorts. Appendix Figure A.22 shows these trend breaks are not present in the low-risk group, but are also absent in *disposable* income in both risk groups, a finding consistent with Løken et al. (2018). Collectively, these results indicate that parents of younger children are particularly responsive to welfare-to-work through increased labor supply and household consumption, which could affect younger cohorts' later-life outcomes. This can rationalize the starting age heterogeneity present in my main results.

52. This is because (i) parental ITT effects always arise concurrently with or after anticipation or ITT effects, and (ii) no two starting age cohorts share the same timing of anticipation and ITT effects but vary in the timing of parental ITT effects. See Figure 8.

6.3 Anticipation Effects

Finally, I examine evidence of anticipation effects. Economic theory predicts that anticipating work requirements would deter younger cohorts away from SA to other available margins like education (Becker 1962; Besley and Coate 1992). Without data on information sets or subjective expectations, however, it is difficult to definitively conclude the presence of anticipation effects.

Instead, this subsection provides suggestive evidence of anticipation effects in two ways. First, I present motivating evidence consistent with anticipatory behavior. Second, I pursue an econometric strategy to separately identify anticipation effects. I harness unique institutional features of the Danish context and additional assumptions to quantify the anticipation effect's contribution to the overall effects of the reforms for a subset of younger cohorts. These exercises suggest that anticipation effects also help generate welfare-to-work's effects on children.

Kinks in SA take-up and school enrollment. I begin by illustrating that younger cohorts were deterred from immediately participating in SA after the introduction of work requirements. Figure 6, Panel (a) plots monthly SA take-up rates of the high-risk group around the 18th birthday—when most become eligible for SA. Take-up spikes *immediately* after the 18th birthday, establishing that many claim SA as soon as possible. This discontinuous increase suggests that SA eligibility rules are salient and take-up at age 18 may be planned in advance.

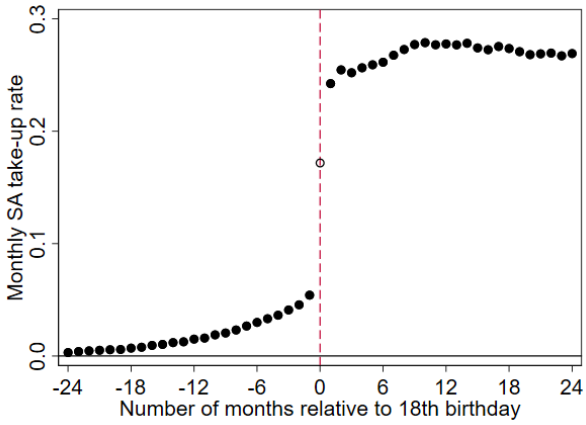
Could starting age cohorts $\alpha \leq 17$ close to gaining SA eligibility anticipate its work requirements? Panel (b) provides suggestive evidence consistent with this idea by plotting how the spike in SA take-up following the 18th birthday evolves over cohorts. There is a *negative* kink in the SA take-up rate among 18-year-olds around July 1st, 1990—the date of the first reform that separates starting ages $\alpha \geq 18$ and $\alpha \leq 17$. In other words, the immediate SA take-up rate declines among starting ages $\alpha \leq 17$ exposed to the reforms *before* reaching age 18. Instead, these individuals appear to invest in their human capital: Panel (c) illustrates a *positive* kink in school enrollment at age 18 around the cutoff.[53]

Though suggestive of anticipatory behavior, kinks around $\alpha = 18$ may be driven by children learning about work requirements after immediately gaining SA eligibility at age 18. Panel (d), however, shows a positive kink around starting age $\alpha = 17$ in school enrollment at age 17—one year before eligibility typically begins. This is consistent with children becoming deterred from SA and investing in human capital in anticipation of work requirements.

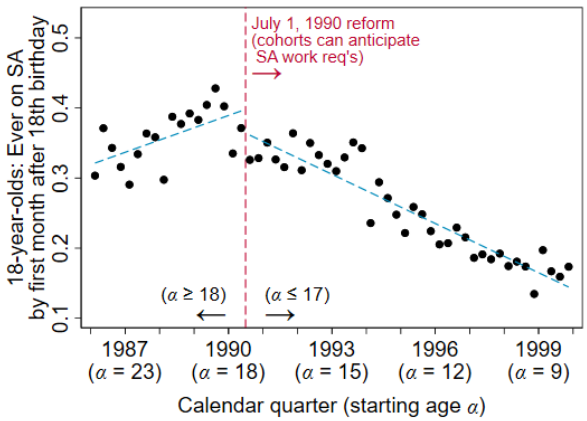
53. Kinks also exist among the low-risk group since age 18 is the most frequent time to participate in SA, but are far less prominent than the high-risk group. This is validated by DD estimates involving risk groups and cohorts; see Appendix Figures D.1 and D.2.

Figure 6: SA participation and school enrollment among anticipating cohorts

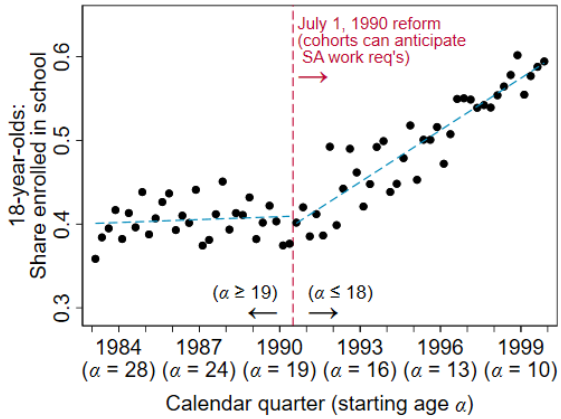
(a) SA take-up spikes after 18th birthday



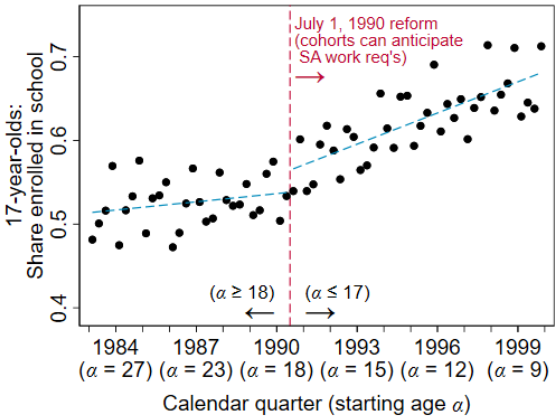
(b) Spike in SA take-up changes around $\alpha = 18$



(c) School enrollment at age 18 around $\alpha = 18$



(d) School enrollment at age 17 around $\alpha = 17$

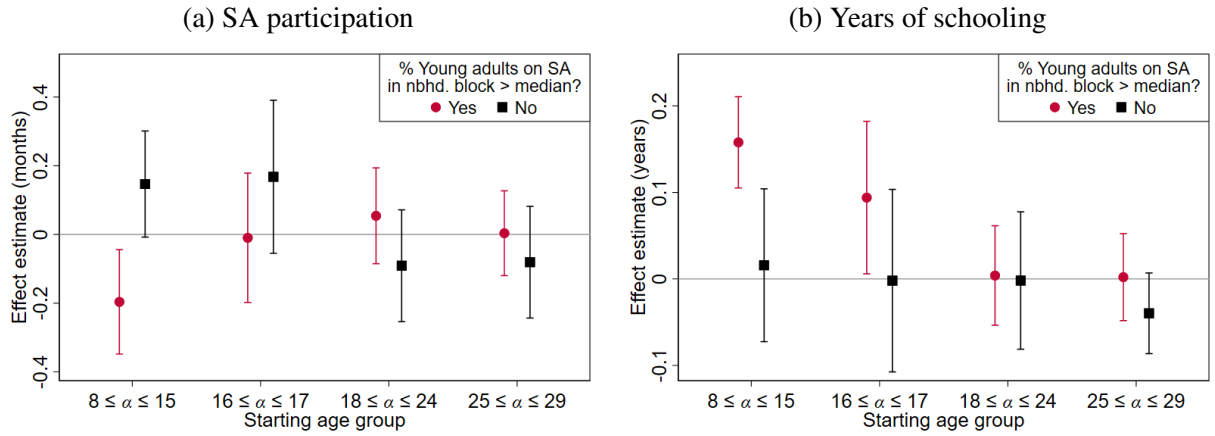


Notes: Panel (a) plots monthly take-up of SA around the 18th birthday. The hollow marker is take-up on the 18th birthday (month 0). Panel (b) plots the take-up rate among 18-year-olds in the first month after the 18th birthday by calendar quarter. Panels (c) and (d) plot similar figures for school enrollment at ages 17 and 18. Since SA take-up is measured at the start of age 18 while school enrollment is measured at the end of age 18, the school enrollment plots' x-axis labels of starting age α in parentheses lead by one year. Dashed blue lines are linear fits on each side of the cutoff. All samples are restricted to the high-risk group; Panels (a) and (b) further restrict to cohorts born after 1968 due to availability of monthly SA data.

Heterogeneity by social networks. Next, I examine whether welfare-to-work's positive effects on children are driven by those exposed to older peers facing reforms. Theoretically, if children learned about the existence and quality of the work requirements through social networks before becoming eligible for SA (Dahl, Løken, et al. 2014), then they may be more inclined toward human capital investments as an anticipatory response.

To test this hypothesis, I estimate Equation (4) where subgroups are defined by whether children resided in a neighborhood block during adolescence (ages 12–17) with an above-median

Figure 7: Heterogeneity in welfare-to-work's effects by social network



Notes: This table reports estimates of β_α and $\beta_\alpha^{Subgroup} \equiv \beta_\alpha + \beta_\alpha^S$ from Equation (4) aggregated by starting age group, where $Subgroup_i$ indicates residing in a 150-household neighborhood block between ages 12–17 in which the share of young adults (19–24 year olds) participating in SA is above the median rate (11.5%). Vertical capped bars are 95% confidence intervals based on two-way clustered robust standard errors.

($\geq$0.115) share of young adults on SA. Recall, by construction, the high-risk group resides in neighborhood blocks with historically high SA participation among the full adult (age 18–60) population. Thus, this quadruple difference analysis conditions on this characteristic; it exploits differences between those who happened to reside in neighborhood blocks with different SA participation rates among young adults during adolescence.

Figure 7 shows that my main effect estimates among high-risk children is driven by the subgroup with greater exposure to these social networks. Children with more older neighborhood peers on SA experienced significant decreases in SA participation and increases in years of schooling. In contrast, those with fewer peers spend *more* time on SA and experience null effects on educational attainment. Differences between subgroups are statistically significant for young children ($8 \leq \alpha \leq 15$).[54] Adults serve as a placebo test; reassuringly, their effect estimates are statistically indistinguishable from zero and each other. Appendix Tables A.18 and A.19 find similar patterns in income and hospital care, and when subgroups are defined by having an older sibling exposed to SA work requirements. Overall, these results align with the theory that children's gains can be rationalized by anticipation effects arising from information transmission channels.

Identifying anticipation effects. Can we quantify how much anticipation effects contribute to welfare-to-work's effects on children? The remainder of this section shows how, under stronger assumptions, anticipation effects can be identified for a subset of younger cohorts.

To proceed, I expand the potential outcomes framework in order to formally decompose starting

54. The significant negative differences in SA duration indicates that the share of older neighborhood peers on SA does *not* reflect children's expected eligibility on SA.

age-outcome age effects into anticipation effects and the other channels. Denote values of the starting age of anticipation, ITT, and parental ITT effects as η, τ, and π; let their realized values given starting age α be η_α, τ_α, and π_α.[55] I define the anticipation effect at starting age-outcome age (α, a) for fixed ITT and parental ITT starting age values (τ, π) as

$$\beta^{Antic}_{\alpha,a}(\tau,\pi) \equiv \mathbb{E}\left[Y_{ia}(\eta_\alpha,\tau,\pi) - Y_{ia}(\infty,\tau,\pi) \mid D_i = \alpha, G_i = 1\right]. \tag{5}$$

This is the causal effect of anticipating work requirements starting at age $\eta_\alpha < 18$ relative to facing work requirements as a surprise $(\eta = \infty)$, holding all else fixed. I discuss the specific (τ, π) pairs for which I can identify $\beta^{Antic}_{\alpha,a}(\tau,\pi)$ below. One can analogously define the ITT and parental ITT, $\beta^{ITT}_{\alpha,a}$ and $\beta^{Par}_{\alpha,a}$, as differences in generalized potential outcomes.[56]

To isolate anticipation effects from the other two channels, I take differences between appropriate starting age-outcome age effects $\beta_{\alpha,a}$. To gain intuition behind this approach, suppose the timing of the reforms was quasi-random (i.e., starting age assignment is independent of birth cohort) and that the reforms did not affect parents (i.e., $\pi_\alpha = \infty$). Then differencing $\beta_{\alpha,a}$ between a younger cohort $\alpha < 18$, who can anticipate work requirements at age $\eta_\alpha = \alpha$ and face ITT effects at age $\tau_\alpha = 18$, and the cohort $\alpha = 18$, who possesses no anticipation effects ($\eta_\alpha = \infty$) but also faces ITT effects at $\tau_\alpha = 18$, would identify the anticipation effect starting at age $\eta_\alpha = \alpha$ over and above the ITT effect: $\beta^{Antic}_{\alpha,a}(18,\infty)$.

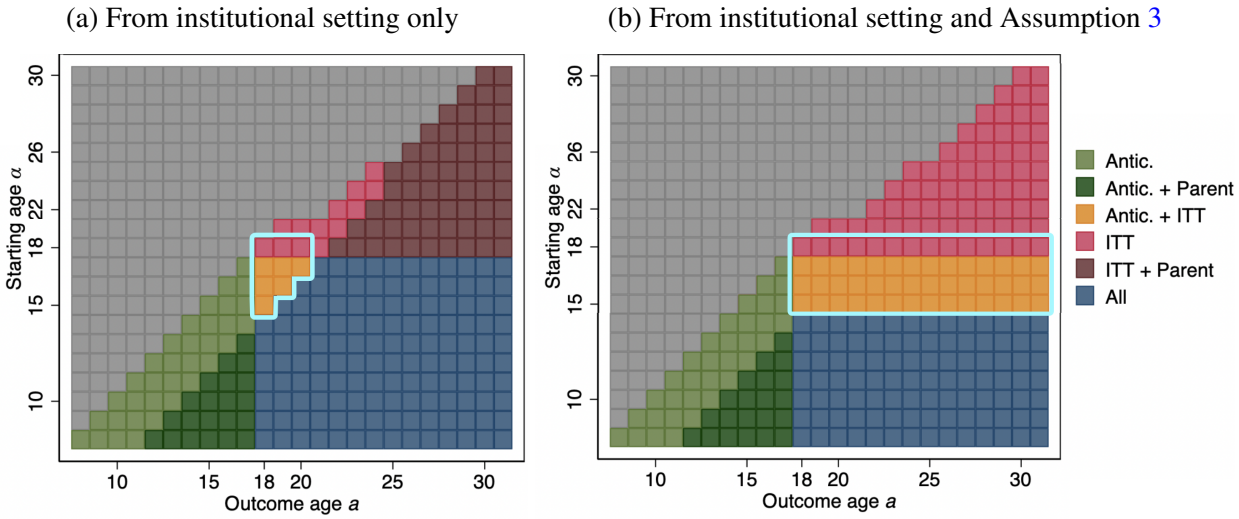
I employ this differencing approach for certain starting age-outcome age effects by (i) exploiting unique institutional features of the Danish setting and (ii) invoking an additional parallel trends assumption. First, recall the first welfare-to-work reform in 1990 targeted only ages 18–19; it was not until January 1st, 1994 that work requirements broadened its reach to all SA recipients (including the cohorts' parents). This feature shuts down the parental ITT effect up to a 4-year window for specific (α, a) pairs. If the SA reforms affected the entire population at once—which is common in other settings—then it would be impossible to isolate the anticipation effect from the parental ITT effect. Figure 8, Panel (a) illustrates how this institutional feature and the SA eligibility age of 18 map the starting age-outcome age effects into different combinations of the three channels. Critically, this mapping reveals a region of starting age-outcome age effects that reflect either an ITT effect only (light red cells) or an anticipation plus ITT effects for starting ages $15 \leq \alpha = \eta_\alpha \leq 17$ (orange cells). For these starting age cohorts, this region spans over outcome ages until the 1994 reform starts affecting their parents (dark red and blue cells).

Second, I invoke the following parallel trends assumption:

55. There is a bijective mapping between α and $(\eta_\alpha, \tau_\alpha, \pi_\alpha)$, so $(\eta_\alpha, \tau_\alpha, \pi_\alpha)$ has the same information as α.

56. Specifically, the ITT for fixed (η, π) is $\beta^{ITT}_{\alpha,a}(\eta,\pi) \equiv \mathbb{E}\left[Y_{ia}(\eta,\tau_\alpha,\pi) - Y_{ia}(\eta,\infty,\pi) \mid D_i = \alpha, G_i = 1\right]$ while the parental ITT effects for fixed (η, τ) is $\beta^{Par}_{\alpha,a}(\eta,\tau) \equiv \mathbb{E}\left[Y_{ia}(\eta,\tau,\pi_\alpha) - Y_{ia}(\eta,\tau,\infty) \mid D_i = \alpha, G_i = 1\right]$.

Figure 8: Underlying components of identified starting age-outcome age effects



Notes: This figure expresses identified starting age-outcome age effects as combinations of anticipation effects, ITT effects, and parental ITT effects defined in the main text under different conditions. Gray cells indicate pre-reform starting age-outcome age combinations (i.e., whenever $a < \alpha$). Highlighted cells are starting age-outcome age estimates used in the decomposition exercise reported in Table 3.

Assumption 2 (Conditional parallel trends with only ITT)**.** *Implicitly condition on* $\boldsymbol{X}_i$ *to reduce notational burden. For starting ages* $\alpha < 18$ *and outcome ages* $a < \pi_\alpha$*,*

$$\begin{aligned}
&(\mathbb{E}\left[Y_{ia}(\infty, 18, \infty) \mid D_i = \alpha, G_i = 1\right] \;-\; \mathbb{E}\left[Y_{i17}(\infty, 18, \infty) \mid D_i = \alpha, G_i = 1\right]) \\
&\quad - \;(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = \alpha, G_i = 0\right] \;-\; \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 0\right]) \\
&= \;(\mathbb{E}\left[Y_{ia}(\infty, 18, \infty) \mid D_i = 18, G_i = 1\right] \;-\; \mathbb{E}\left[Y_{i17}(\infty, 18, \infty) \mid D_i = 18, G_i = 1\right]) \\
&\quad - \;(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = 18, G_i = 0\right] \;-\; \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 0\right]).
\end{aligned}$$

Assumption 2 states that differences in potential outcomes with only ITT effects starting at $\tau_\alpha = 18$ between high- and low-risk $\alpha < 18$ cohorts would evolve over the life cycle in the same way as corresponding differences for the $\alpha = 18$ cohort. This assumption is similar to Assumption 1 but involves potential outcomes of *the ITT starting at age* $\tau_\alpha = 18$ without anticipation or parental ITT ($Y_{ia}(\infty, 18, \infty)$) instead of the pure "untreated" state ($Y_{ia}(\infty)$). Admittedly, this is a strong assumption since it involves a "partly-treated" counterfactual that cannot be validated with pre-trends. A sufficient condition of Assumption 2 is quasi-random timing of the reforms, which implies ITT starting ages are exogenously assigned.

Pairing this assumption with the original parallel trends assumption enables identification of $\beta^{Antic}_{\alpha,a}$ for select (α, a) pairs by taking differences of previously identified parameters:

Proposition 1. *Under Assumptions 1 and 2 and for* (α, a) *pairs with* $15 \leq \alpha < 18 \leq a$ *and* $\pi_\alpha = \infty$*,* $\beta^{Antic}_{\alpha,a}(18, \infty) = \beta_{\alpha,a} - \beta_{18,a}$*.*

Proof. See Appendix E.2. □

From the vantage point of Panel (a) of Figure 8, Proposition 1 states that differences between the highlighted light red cells corresponding to $\alpha = 18$ and the highlighted orange cells corresponding to $15 \leq \alpha \leq 17$ at a given outcome age a identifies the anticipation effect contributing to the orange cells. Since the starting age-outcome age effects in the orange cells can be decomposed as $\beta_{\alpha,a} = \beta^{Antic}_{\alpha,a}(18, \infty) + \beta^{ITT}_{\alpha,a}(\infty, \infty)$, Proposition 1 implies the residual ITT parameter is simply $\beta^{ITT}_{\alpha,a}(\infty, \infty) = \beta_{18,a}$.

As illustrated by the highlighted cells of Panel (a), Assumption 2 provides only a limited number of starting age-outcome age effects that can be used for the decomposition. An additional assumption about parental ITT effects provides a larger range of starting age-outcome age effects:

Assumption 3 (No parental ITT start after age 18)**.** *For any* α *such that* $\pi_\alpha \geq 18$ *and* $a \geq 18$*,* $\mathbb{E}[Y_{ia}(\eta_\alpha, \tau_\alpha, \pi_\alpha) \mid D_i = \alpha, G_i = 1] = \mathbb{E}[Y_{ia}(\eta_\alpha, \tau_\alpha, \infty) \mid D_i = \alpha, G_i = 1]$.

This assumption states that any effect of welfare-to-work on parents does not spillover to an individual's outcomes if the individual is at least age 18.[57] This is a strong condition that cannot be formally tested with the available data. If parental ITT effects occur when individuals live with parents, however, then this assumption is plausible given cohabitation precipitously drops after age 18 (see Appendix Figure A.20). Under Assumption 3, all starting age-outcome age effects after outcome age 18 for starting age $\alpha = 18$ are ITT effects, while those for starting ages $15 \leq \alpha \leq 17$ are anticipation and ITT effects; see Figure 8, Panel (b). This implies I can use the summary starting age effects β_α to decompose anticipation and ITT effects for $15 \leq \alpha \leq 17$.

Table 3 applies Proposition 1 to decompose effects on key outcomes for starting ages $15 \leq \alpha \leq 17$. Columns (1)–(3) report results based on Assumptions 1 and 2 only, relying on a limited number of $\beta_{\alpha,a}$ estimates. The decomposition suggests that anticipation effects generate positive effects for youth, but the estimates are too imprecise to draw strong conclusions. To gain precision, Columns (4)–(6) additionally employs Assumption 3 to decompose estimates of starting age effects β_α that measure effects up to outcome age 29. Comparing Columns (4) and (5) reveals that anticipation and ITT effects tend to be opposing forces. Anticipation effects generate large, statistically significant increases in years of schooling and market income and decreases in incarceration, while the ITT effects are opposite signed and statistically insignificant. Relative to the total effects reported in Column (6), anticipation effects appear to play an outsize role in explaining the long-term effects of welfare-to-work for these cohorts.

57. Note that this is not inconsistent with the intergenerational spillover effects found in Section 5.5: all children of the cohorts I study were below age 18 at the time of the reforms.

Table 3: Decomposing total effects into anticipation and ITT effects (starting ages 15–17)

	Using institutional setting only (outcome ages $18 \leq a \leq 20$)			Using instit. setting & Assump. 3 (outcome ages $18 \leq a \leq 29$)		
	Anticipation (1)	ITT (2)	Total (3)	Anticipation (4)	ITT (5)	Total (6)
1. SA participation (months)	−0.042	−0.022	−0.064	−0.096	0.161	0.065
	(0.116)	(0.104)	(0.117)	(0.113)	(0.109)	(0.088)
2. Years of schooling	0.020	0.029	0.049**	0.090**	0.000	0.090***
	(0.031)	(0.027)	(0.020)	(0.037)	(0.035)	(0.027)
3. Market income (thousands $)	0.183	0.180	0.363**	0.819**	−0.020	0.799***
	(0.375)	(0.340)	(0.169)	(0.396)	(0.396)	(0.246)
4. Incarceration sentence (days)	−0.285	0.271	−0.015	−0.793**	0.472	−0.321
	(0.537)	(0.511)	(0.169)	(0.366)	(0.385)	(0.259)

Notes: This table decomposes the total estimated effect on starting age cohorts $15 \leq \alpha \leq 17$ into anticipation and ITT effects under different assumptions. See main text for more details. Columns (1)–(3) use estimates of select starting age-outcome age effects $\{\beta_{\alpha,a} \,|\, 15 \leq \alpha \leq 18,\ 18 \leq a \leq \min\{\alpha + 4,\ 20\}\}$ (see the highlighted cells in Figure 8, Panel (a)). Columns (4)–(6) use estimates of starting age effects $\{\beta_\alpha \,|\, 15 \leq \alpha \leq 18\}$ under Assumption 3 (see the highlighted cells in Figure 8, Panel (b)). Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. Stars indicate statistical significance: $^*p < 0.10$, $^{**}p < 0.05$, , $^{***}p < 0.001$.

6.4 Discussion

Collectively, the evidence suggests that parental ITT and anticipation effects are the likely mechanisms underlying welfare-to-work's long-run effects on younger starting age cohorts. This has important implications for interpreting the efficacy of welfare-to-work. It suggests that the positive impacts on children are partly through a deterrence effect (Besley and Coate 1992), incentivizing adjustments along alternative human capital-enhancing margins, rather than the ITT effects of work requirements themselves.

As discussed earlier, welfare-to-work increases labor supply and consumption of younger cohorts' households during childhood. A large literature indicates these parental behavioral responses likely affects these cohorts' future outcomes.[58] Candidate mechanisms include human capital investments and role model effects. Quantifying parental ITT effects' contribution to starting age-outcome age effects and distinguishing between these mechanisms are left for future work.

The anticipation effects I identify may capture several factors. First, parents may anticipate that their children may face work requirements in the future, prompting changes in their human capital investments. Recent experimental evidence by Deshpande and Dizon-Ross (2023), however, suggests this might be unlikely. Moreover, Figure 5 shows that parents' financial decisions

58. This includes quasi-experimental studies of the Earned Income Tax Credit (e.g., Dahl and Lochner 2012; Bastian and Michelmore 2018) and structural models of skill development (e.g., Del Boca et al. 2014).

are unresponsive to the 1990–1993 reforms, which targeted only young adults.

This suggests that high-risk children themselves may be forward looking as they reach late adolescence and make critical decisions about their schooling. Though this interpretation might be surprising, existing work supports this view. Ethnographic evidence from Coley et al. (2000) finds that roughly 75% of low-income adolescent girls interviewed at the cusp of U.S. welfare reform were knowledgeable of welfare's eligibility rules and aware of the impending reform. My results also align with Dahl and Gielen (2021), who argue that Dutch DI reforms generated anticipatory human capital investments among children of affected parents. Their estimated effect on years of schooling (0.117) aligns with my estimates among young children (0.105).

Finally, anticipation effects may include the effect of some children *opting into* SA work programs upon gaining eligibility. Note my results focus on the reduced-form effects of welfare-to-work reforms and not the treatment effects of participating in the SA work programs themselves. Disentangling these mechanisms remains an important avenue for future research.

7 Evaluating Welfare-to-Work over Time

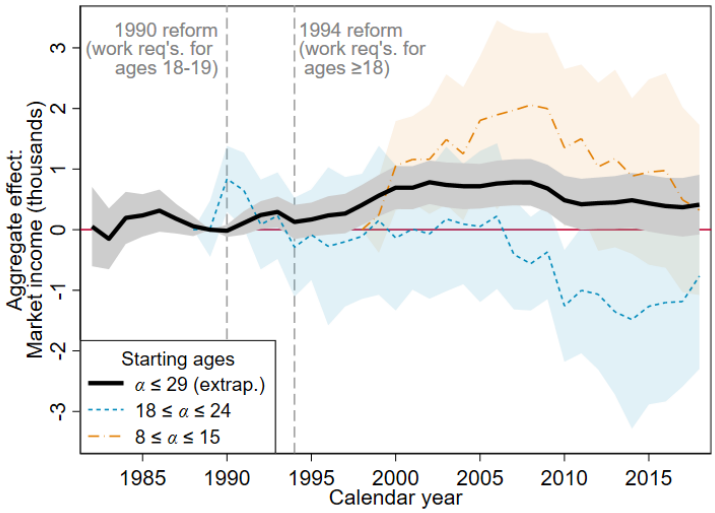
7.1 Aggregate Effects over Calendar Time

Having examined the heterogeneity of starting age-outcome age effects, what are the overall effects of the SA reforms in the population over calendar time? To answer this question, I aggregate the starting age-outcome age estimates over cross-sections of starting age cohorts by calendar year (Callaway and Sant'Anna 2021). These effects are often sought by policymakers and economists for judging whether welfare-to-work is a worthwhile policy in the long run. Critically, however, the starting age-outcome age estimates themselves can provide a more nuanced understanding of the forces that shape these aggregate effects. Since the composition of the population evolves to include younger starting age cohorts over time who possess different choice sets and anticipation effects, this would affect our interpretation of the reform's long-run impacts.

Let $t^\star(\alpha)$ denote the calendar year that starting age α first faces the reform.[59] Let $\mathcal{A}_t$ be the set of starting age cohorts with identified starting age-outcome age effects $\beta^{30}_{\alpha,a}$ in year t, which captures the composition of cohorts in a given calendar year. I opt to use $\beta^{30}_{\alpha,a}$ to include the life-cycle effects of welfare-to-work beyond age 30 in the aggregate effect. To extrapolate effects over longer-run horizons—which include cohorts too young to appear in my main sample—I assume that starting ages $\alpha < 8$ possess the same $\beta^{30}_{\alpha,a}$ effects as $\alpha = 8$ and include these cohorts in $\mathcal{A}_t$

59. $t^\star(\alpha) = 1990 \cdot \mathbb{1}\{\alpha \leq 19\} + 1992 \cdot \mathbb{1}\{22 \leq \alpha \leq 24\} + 1994 \cdot \mathbb{1}\{26 \leq \alpha \leq 29\}$; see Appendix Table A.1.

Figure 9: Aggregate cross-sectional effects of welfare-to-work on income over time



Notes: This figure plots aggregate estimates of the effects on market income for different groups of cohorts. The black series plots estimates of $\bar{\beta}_t$ defined in Equation (6). It extrapolates estimates of $\alpha < 8$ cohorts with $\alpha = 8$ estimates. The dashed lines are event-study estimates of $\beta^{30}_{\alpha,a}$ aggregated between $18 \leq \alpha \leq 24$ and $8 \leq \alpha \leq 15$; these are overlaid over calendar time using the outcome age of the median cohort in each group. Shaded regions are 95% confidence intervals based on two-way clustered robust standard errors.

whenever appropriate. The aggregate effect of welfare-to-work in calendar year t is thus

$$\bar{\beta}_t \equiv \frac{1}{|\mathcal{A}_t|} \sum_{\alpha \in \mathcal{A}_t} \beta^{30}_{\alpha, t-t^\star(\alpha)+\alpha}. \tag{6}$$

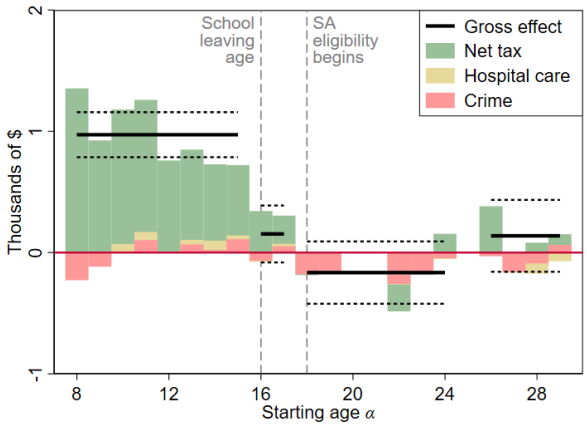
Figure 9 plots estimates of $\bar{\beta}_t$ over calendar time for market income. The aggregate effects on market income (solid line) initially grows unevenly but reaches a statistically significant magnitude by around 1998; thereafter, estimates stabilize at positive levels. Appendix Figure A.23 shows these dynamic effects are robust to alternative aggregation schemes. The figure overlays the aggregated starting age-outcome age event-study estimates for $8 \leq \alpha \leq 15$ and $18 \leq \alpha \leq 24$ (dashed lines). The diverging dynamic effects of these starting age groups showcase how aggregate calendar year effects mask heterogeneity within cross-sections of the population.

7.2 Cost-Benefit Analysis

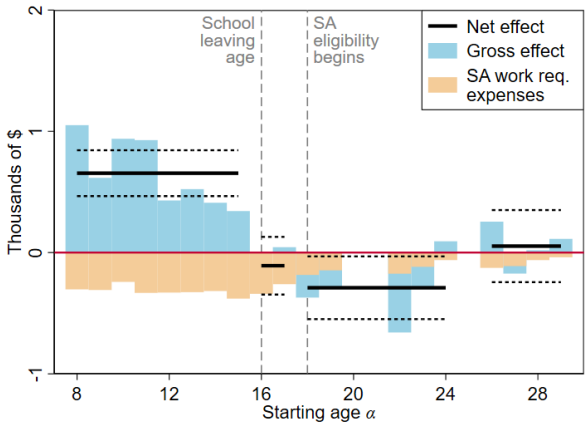
Is welfare-to-work worth it from the government's perspective? To assess this, I conduct a cost-benefit analysis of the fiscal impact of the reforms on the government's budget. I focus on three components that contribute to the reforms' gross fiscal effects: (1) net taxes, defined as tax revenue accrued from market income minus any public transfers allocated for SA, DI, UI, post-secondary education stipends, and other programs (based on my own estimates); (2) expenses from policing

Figure 10: Cost-benefit analysis of welfare-to-work reforms, by starting age

(a) Components of total gross fiscal effect



(b) Net fiscal effect



Notes: Panel (a) decomposes the total gross fiscal effect of welfare-to-work over net taxes, hospital care, and crime over starting ages α; see the main text for details. Panel (b) compares the gross fiscal effect against government expenses incurred from SA work requirements and plots the net fiscal gain over α. Horizontal solid lines are aggregate estimates of the gross and net fiscal effects by starting age group; dashed lines are 95% confidence intervals based on weighted averages of two-way clustered robust standard errors of the relevant outcomes. Colored bars represent estimates of components for each starting age α.

crime, court procedures from convictions, and maintaining and staffing prisons (based on calculations by The Rockwool Foundation Research Unit (2006)); (3) expenses of inpatient hospital care based on bed days and medical staff (provided by Czernichow et al. (2021) drawn from the Danish Health Data Agency).[60] This total gross fiscal effect is assessed against government expenditure to finance additional SA work program participation, which is documented in Bach (2002) and Christensen (2002). Due to limited evidence, I ignore fiscal impacts generated by output from the SA work programs or general equilibrium effects on employers or other job seekers.[61]

Figure 10 illustrates the cost-benefit analysis by starting age α. As shown in Panel (a), net taxes are by far the most significant component of the gross fiscal effect. For young children ($8 \leq \alpha \leq 15$), the net taxes accrued from increased market income generates large, statistically significant benefits for the government. Meanwhile, costs borne from increased crime among young adults ($18 \leq \alpha \leq 24$) generate a negative gross fiscal effect. Panel (b) demonstrates that, after accounting for expenses incurred from increased participation in SA work programs, the government enjoys a large net fiscal gain from young children but suffers statistically significant

60. I estimate costs from crime and convictions by (i) regressing measures of crime and convictions on a quadratic of incarceration sentence length fully interacted with indicators of ever incarcerated on data after 1992 (due to a data break in crime/convictions measures) and all controls and (ii) scaling the coefficients on the incarceration regressors by their respective TD estimates.

61. To the best of my knowledge, no study has estimated the productive gains of the mandated Danish SA work programs. Hussain and Rasmussen (2007) find some evidence of a negative association between firms' formal employment and temporary employment of SA recipients from wage subsidy schemes. Gautier et al. (2018) focus on general equilibrium effects of job search assistance among UI recipients.

losses from young adults. For other starting age groups, there is no significant net fiscal effect.

Dynamics. In light of the evolution of aggregate effects over time, this cost-benefit analysis implies that the government stands to enjoy net fiscal gains in the long run. Appendix Figure A.24 illustrates that aggregate calendar year effects on net taxes increases in magnitude over time while the duration of inpatient hospital care modestly declines over time. All net fiscal gains in the long run are driven by younger starting age cohorts.

Starting age heterogeneity in the net fiscal impact of welfare-to-work highlights that alternative policy designs could enhance efficiency. Targeting welfare-to-work only to cohorts entering SA eligibility and grandfathering already-eligible adults under the previous SA regime with no work requirements would generate even larger long-run effects by preventing young adults from committing more crime or participating in other social insurance programs.

Implications on social welfare. Recall that welfare-to-work reformed SA work program participation from being voluntary to mandatory. Canonical consumption–leisure models of SA participation with rational, forward-looking agents posit that restricting choice sets in this way would decrease lifetime utility. This model is potentially plausible for adults ($18 \leq \alpha \leq 29$). These cohorts experience null or negative returns on their disposable income but increased participation in the labor market and SA work programs, translating to weakly lower consumption and leisure. However, if children ($8 \leq \alpha \leq 17$) have time-inconsistent preferences, they may reap higher utility if the value of consumption from long-run gains in disposable income exceeds the value of leisure lost from schooling, work, or SA work program participation.

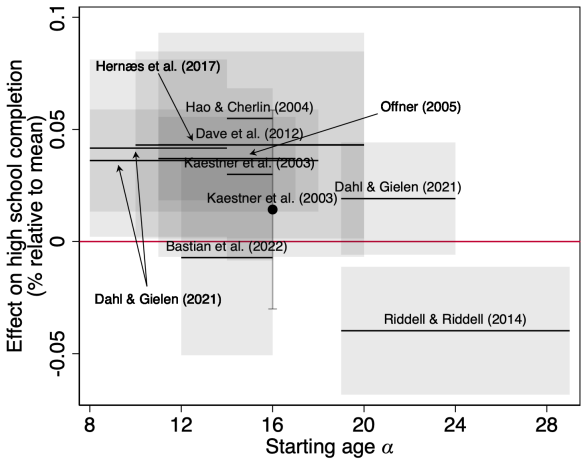
One can couple assumptions about agent's preferences and the cost-benefit analysis to evaluate the SA reforms using Hendren and Sprung-Keyser's (2020) marginal value of public funds (MVPF) metric, which compares the reforms' welfare impacts against their net fiscal effects. The net fiscal effects illustrated in Figure 10(b) is the denominator of the MVPF for each starting age cohort. If children have a welfare gain, then the MVPF for these cohorts is positive infinity since the reforms generate a net positive fiscal return and, thus, a Pareto improvement; if these cohorts incur a welfare loss, then their MVPF is not well-defined (Karapakula 2022). Assuming adults incur a welfare loss, then their MVPF is negative. This implies that welfare-to-work has diverging welfare implications within a generational span of cohorts. Depending on one's assumptions, targeting welfare-to-work to only younger cohorts may improve social welfare.

7.3 Unifying the Literature

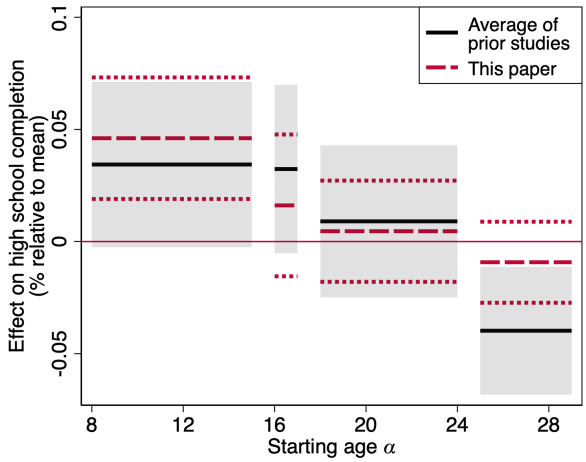
One of this paper's chief contributions is estimating welfare-to-work's effects across a generation of cohorts in a common setting. This provides a unique opportunity to help unify the expansive

Figure 11: Unifying the literature: Welfare-to-work's effect on high school completion

(a) Estimates from prior studies



(b) Comparison with main estimates



Notes: Horizontal black lines in Panel (a) are estimated effects of prior studies plotted against their associated range of starting ages based on available information in each study. Black circles are estimates from studies with a single starting age. Horizontal black lines in Panel (b) pool estimates across prior studies, weighing each study by the fraction that their range of starting ages fall within each starting age group. Shaded areas, vertical capped lines, and dotted red lines are estimated 95% confidence intervals. The confidence interval of Hao and Cherlin (2004) is excluded in order to maintain a reasonable scale. See Appendix F for details on the construction of this figure.

literature on welfare-to-work reforms.

To illustrate this, I review the literature on welfare-to-work's effects on high school completion, a commonly studied outcome for assessing impacts on children and adults that is less prone to measurement issues across institutional contexts or over the life cycle. Effect estimates from the literature, expressed as percent changes relative to the mean, range from -4% to 5%, leading to seemingly inconsistent conclusions about the consequences of welfare-to-work on educational attainment. However, Figure 11, Panel (a) illustrates that a clearer pattern emerges when estimates are conditioned on their underlying starting ages, which are inferred from summary statistics and institutional settings provided in each study (see Appendix F for details). Panel (b) shows that the pooled estimates across studies by starting age group strongly aligns with the magnitude and heterogeneity found in my main results. This suggests that starting age heterogeneity may help reconcile conflicting findings in prior work.

Other work in the welfare-to-work literature estimates aggregate cross-sectional effects by calendar time. More recently, Han et al. (2021) estimate the long-run effects of U.S. welfare reform on consumption among low-educated, single mother-headed households. Their event-study estimates align with Figure 9: initially insignificant effects that gradually grow to significant effects over longer time horizons.[62] Based on my findings in Denmark, the increasing share of younger

62. Han et al. examine repeated cross-sections of adults that fall within a consistent age range. Appendix Figure

starting age cohorts in the population over time might explain Han et al.'s findings.

8 Conclusion

This paper provides a unified evaluation of welfare-to-work by using rich administrative data to estimate the life-cycle impacts of Denmark's SA reforms on a generation of birth cohorts. I document striking heterogeneity in welfare-to-work's effects across cohorts based on their age of exposure to the reforms. Those who were children at the time of the reforms experience gains in labor market, education, and health-related outcomes, consistent with a model where children adjust along human capital-enhancing choice margins in response to the reforms (Becker 1962; Besley and Coate 1992). Meanwhile, welfare-to-work generates socially-costly behaviors among already-eligible young adults, including crime or substitution to DI. Parental spillovers and anticipation effects—rather than the ITT effects of work requirements themselves—likely shape the reform's positive long-term effects on children. These results help unify the literature on welfare-to-work and shed light on the mechanisms that explain their aggregate impacts in the population over time.

In sum, I find that welfare-to-work is cost-effective for the government in the long-run, but this is driven by younger cohorts who could anticipate and behaviorally respond to the reforms during childhood. The net taxes accrued from these cohorts translates to a growing positive effect on the government's budget. On the other hand, the social costs incurred from young adults generate a negative net fiscal impact. Therefore, the body of evidence suggests that welfare-to-work does, indeed, "work" over time, but aggregate effects mask important heterogeneity in the population.

I conclude with two notes relevant for policymakers. First, as usual, one must be cautious with generalizing these findings to countries with different policy contexts. An important determinant of the effects of welfare-to-work reforms that mandate public work programs is the quality of the programs themselves. Qualitative evidence suggests that, in many instances, the Danish work programs were not attractive options to build lasting labor market skills. This is not unlike the experiences of other countries that have implemented national SA work programs, like the U.S. JOBS program (U.S. General Accounting Office 1994).

Second, heterogeneity in net fiscal impacts across cohorts suggests that targeting welfare-to-work to only younger cohorts would improve efficiency and potentially social welfare. However, it is possible that allocating the same public funds to alternative investments in high-risk children could be a more attractive option. It is critical to compare the estimated fiscal and social welfare gains of welfare-to-work against a portfolio of other policies that target this disadvantaged population (Hendren and Sprung-Keyser 2020).

A.23, Panel (e) provides aggregate estimates for 18-to-29-year-olds in each calendar year.

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Online Appendix for "Does 'Welfare-to-Work' Work? Evaluating Long-Run Effects across a Generation of Cohorts"

Neil A. Cholli

January 2025

Table of Contents

A Supplemental Figures and Tables **1**

B Additional Institutional Details **45**
B.1 Overview of Changes to the Social Assistance Program . . . 45
B.2 Comparison with U.S. Aid to Families with Dependent Children . . . 49

C Data Appendix **50**
C.1 Overview . . . 50
C.2 Sampling Scheme . . . 50
C.3 Background Characteristics . . . 50
C.4 Geographic Identifiers . . . 52
C.5 Measuring SA Participation over Time . . . 52

D Additional Details and Results of Robustness Analyses **54**
D.1 Difference-in-Differences Exercises . . . 54
D.2 Checking for Confounding Changes in the Economy . . . 60
D.3 Validation Exercises . . . 62
D.4 Timing of Municipal Adoption of the SA Reforms . . . 65
D.5 Alternative Measurement Choices and Samples . . . 72
D.6 Discussion of the State Educational Grant Reform . . . 76

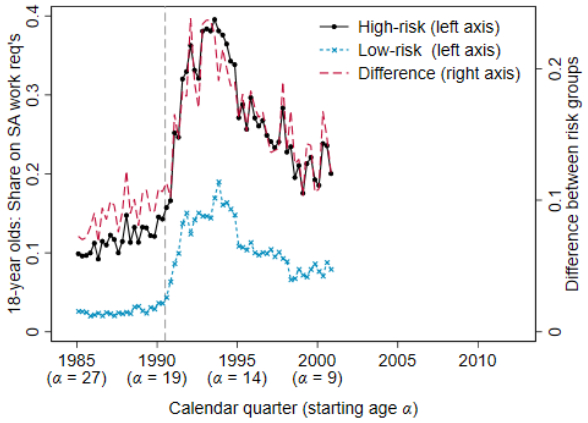
E Technical Appendix **77**
E.1 Model Predictions . . . 77
E.2 Proof of Proposition 1 . . . 81

F Details of Figure 11 **82**
F.1 General Guidelines . . . 82
F.2 Details of Included Studies . . . 82
F.3 Examples of Excluded Studies . . . 84

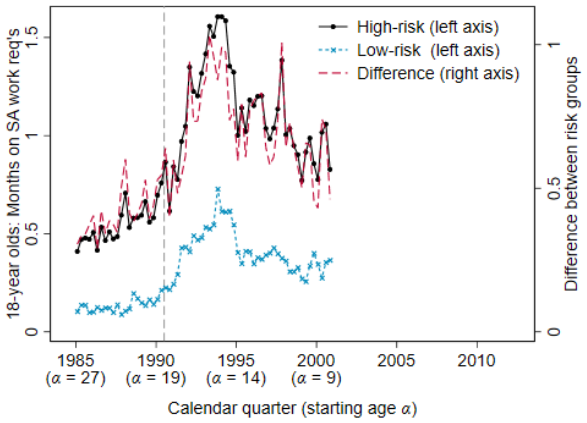
A Supplemental Figures and Tables

Figure A.1: Take-up of SA work requirements across birth cohorts, by outcome age

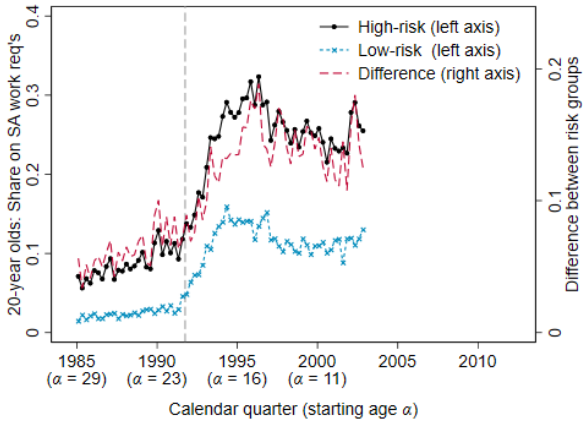
(a) Age 18, share



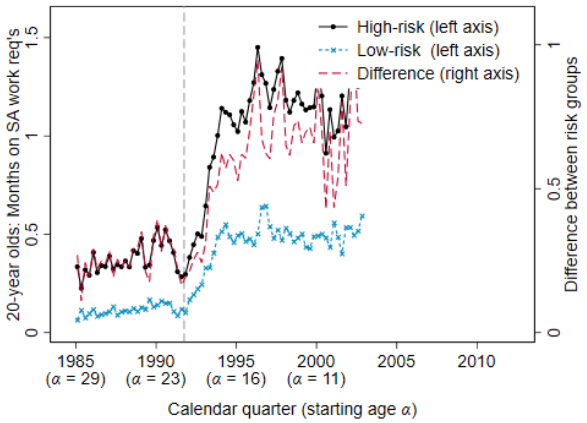
(b) Age 18, duration



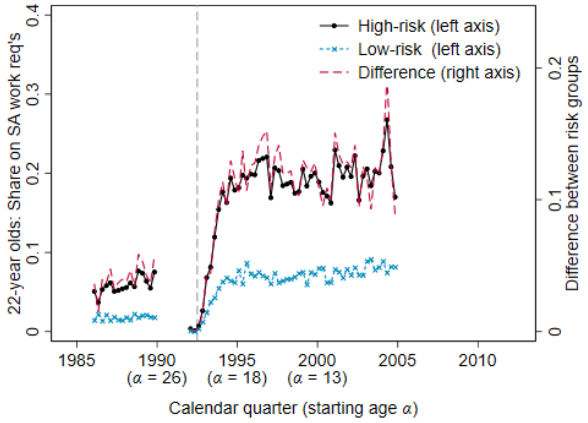
(c) Age 20, share



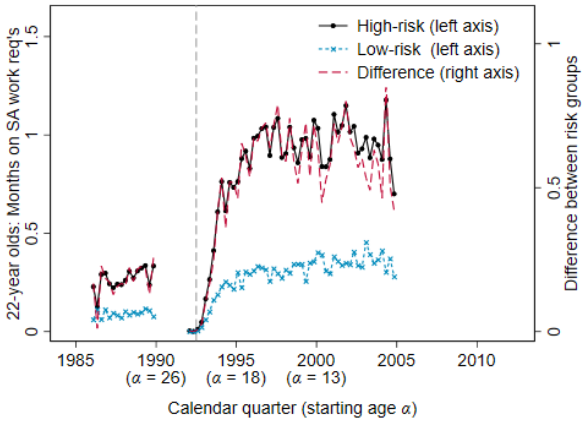
(d) Age 20, duration



(e) Age 22, share



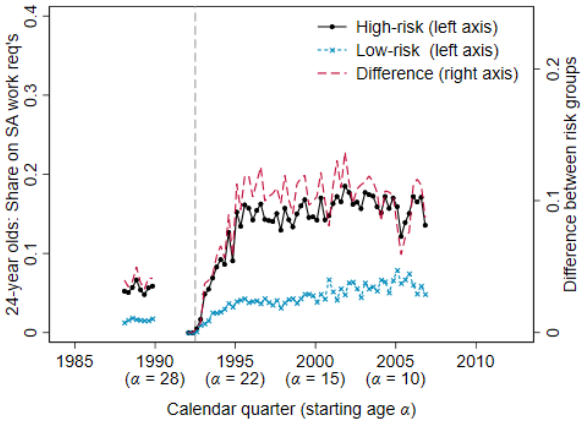
(f) Age 22, duration



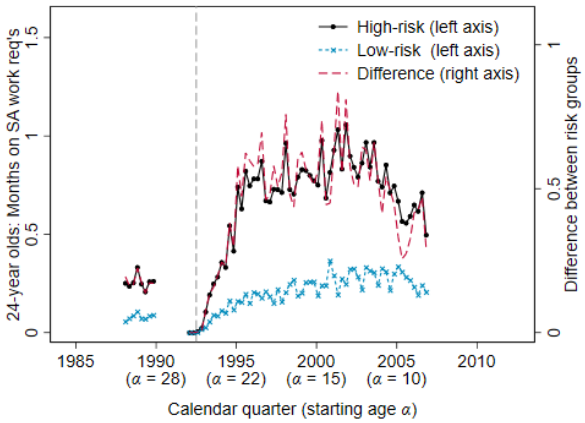
Notes: (see next page)

Figure A.1: (cont'd)

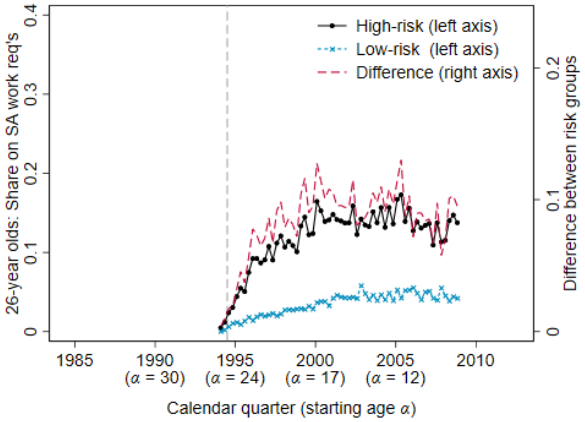
(g) Age 24, share



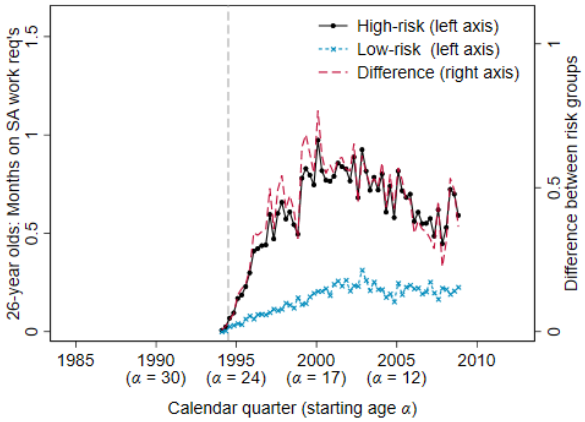
(h) Age 24, duration



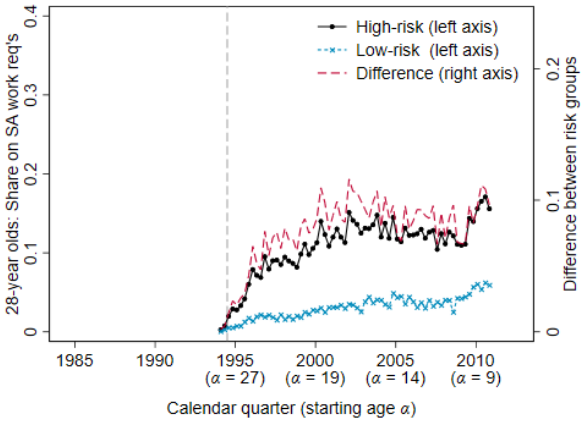
(i) Age 26, share



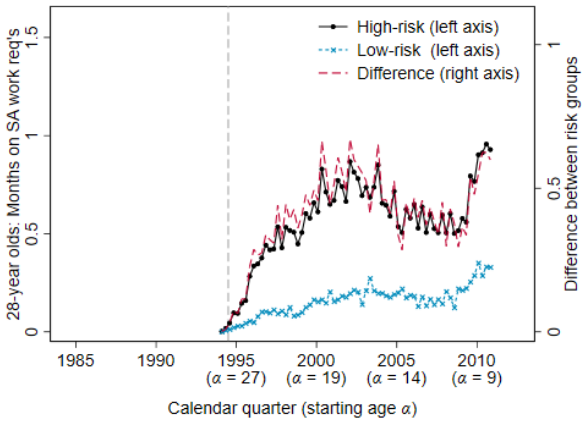
(j) Age 26, duration



(k) Age 28, share

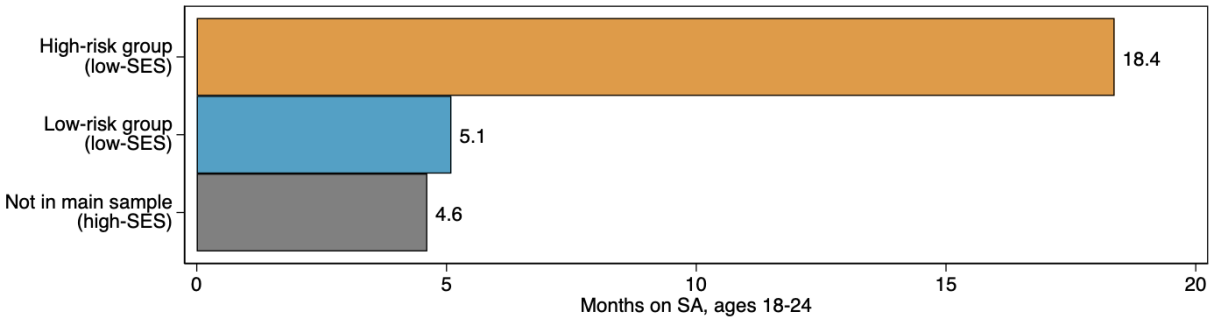


(l) Age 28, duration



Notes: Left panels illustrate participation in SA work requirements along the extensive margin; right panels illustrate the intensive margin (expressed in months). Breaks in data series is due to missing register data; see Appendix C.5.

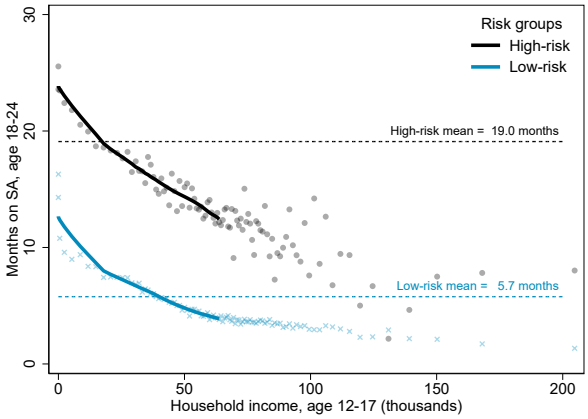
Figure A.2: Risk groups are highly predictive of later-life SA participation



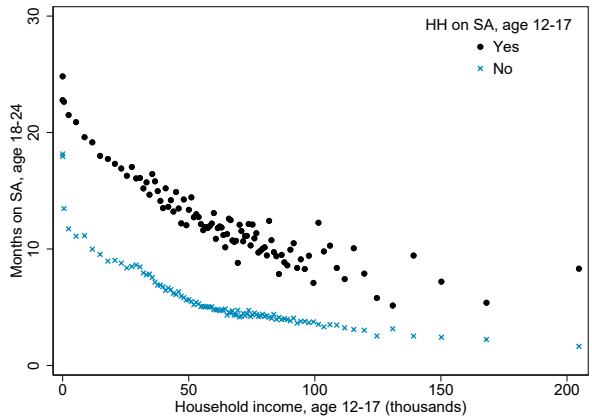
Notes: This figure compares SA participation between risk groups and a high-socioeconomic status (SES) group outside of the main sample, comprised of individuals whose household income is above the national median or mothers with more than 12 years of schooling. Data constrained to individuals born before July 1, 1967 who did not face the SA reforms between ages 18–24 (i.e., $D(b_i) \geq 25$).

Figure A.3: Duration of SA participation, age 18–24, by household income centile and risk group or risk characteristic

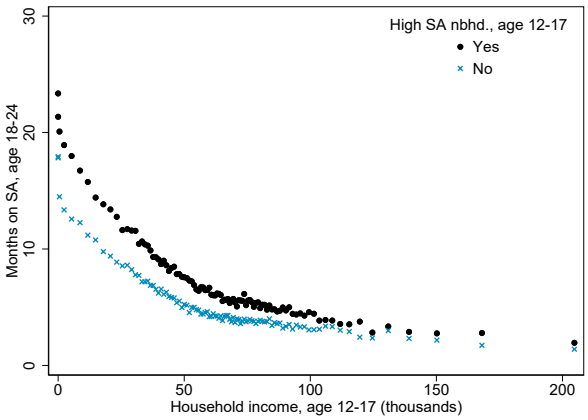
(a) Risk groups



(b) Household on SA



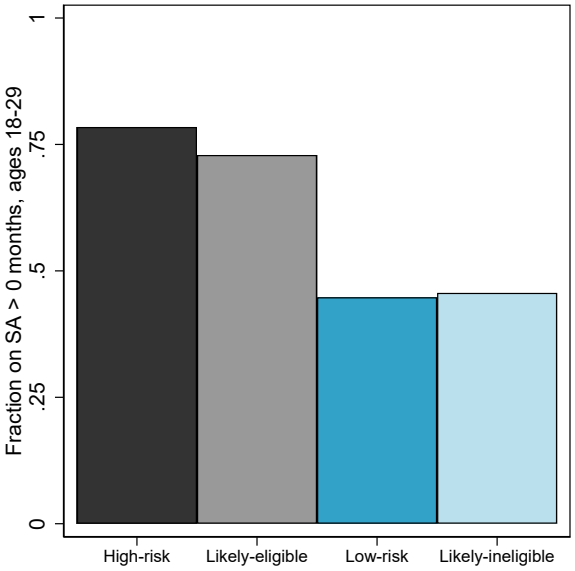
(c) High-SA neighborhood



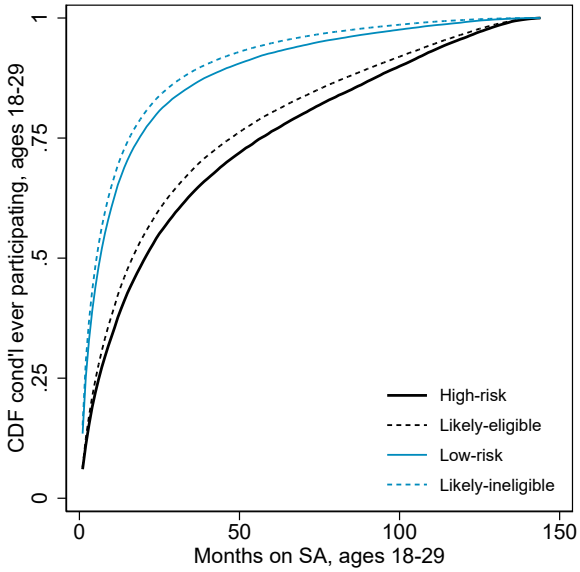
Notes: Scatter plots represent months of SA participation by household income centile bins (based on the household income distribution of the base sample before removing the top half of the distribution but after removing households with mothers with more than 12 years of schooling) and risk groups or risk characteristics defined in the main text. Solid lines are lowess fits below the 50th percentile. This figure uses all individuals whose mothers have no more than a high school education; Panel (a) further constrains the sample to individuals that fall in the high- and low-risk groups.

Figure A.4: Risk groups match SA take-up rates of best possible proxy of eligibility groups

(a) Extensive Margin

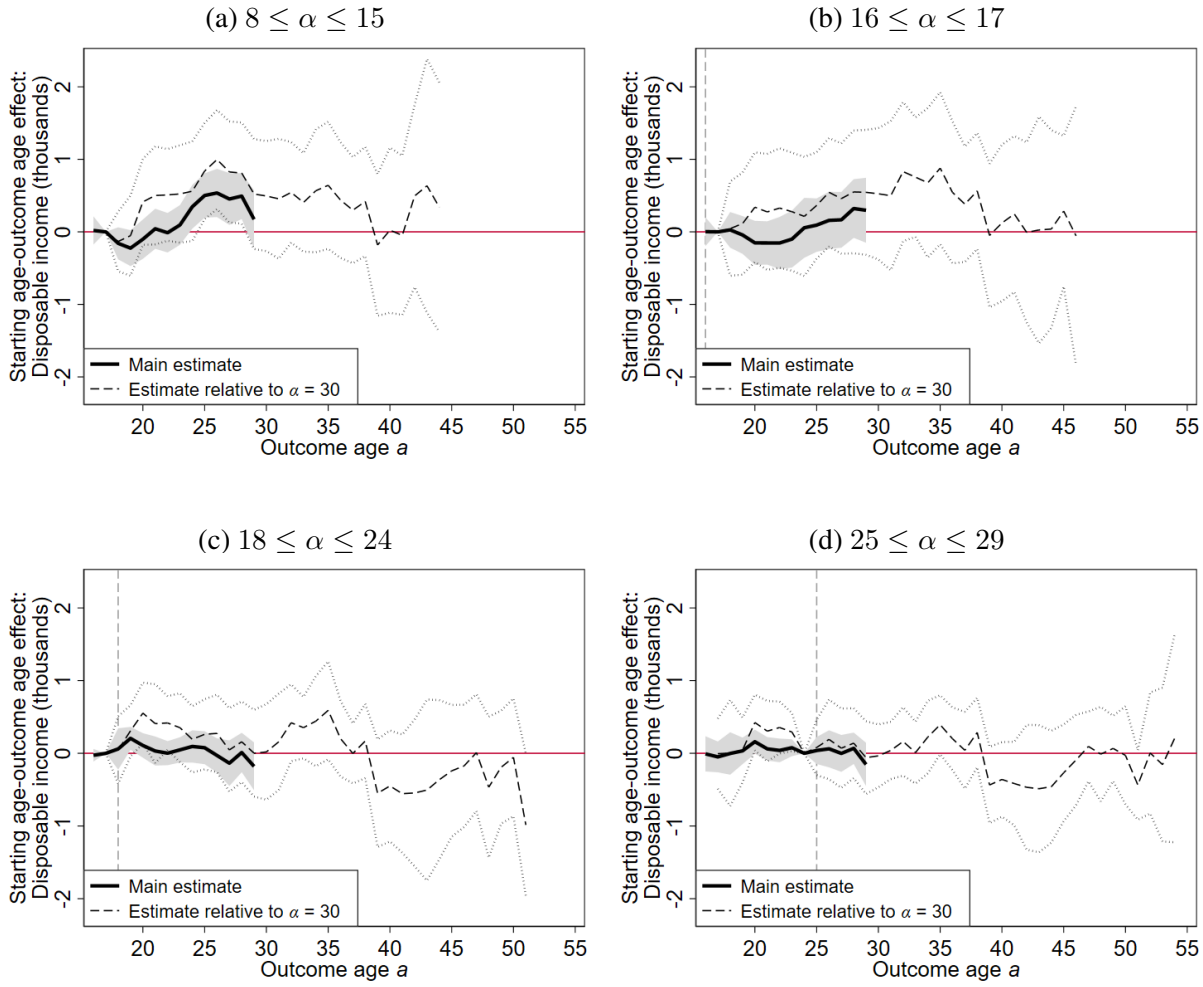


(b) Intensive Margin



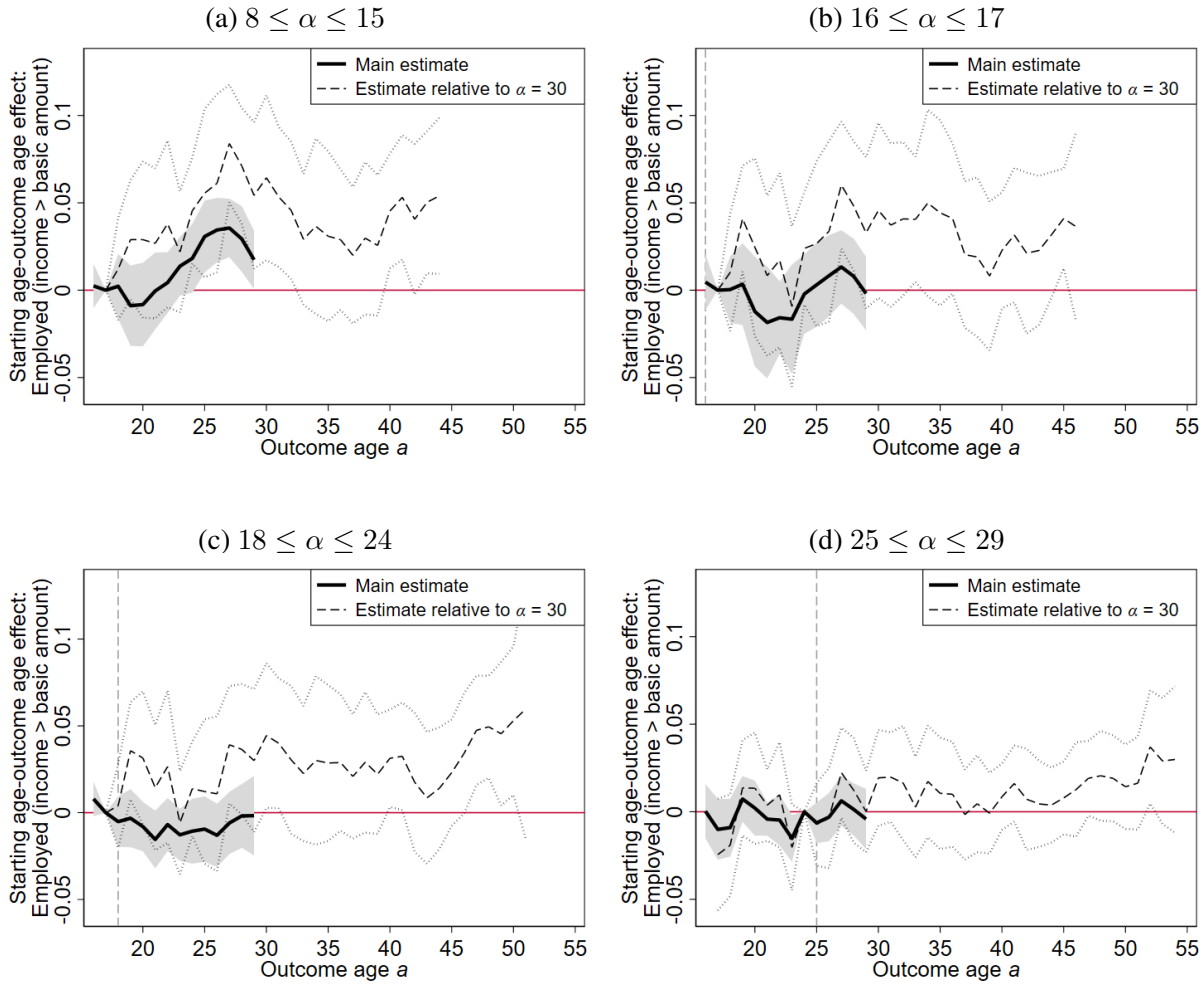
Notes: This figure compares SA participation rates between risk groups and proxies of eligibility groups, "likely-eligible" and "likely-ineligible." Data constrained to individuals with starting age at least 18 to ensure reforms do not affect criteria used to construct likely-eligibility groups. Risk groups are defined by background characteristics measured between ages 12–17; see main text for details. The likely-eligible group is defined by the following criteria measured at age 17: (i) liquid assets (bank deposits and market value of bonds and shares) below $2,000, (ii) market income below $7,000 (the "basic amount"), (iii) not enrolled in school, and (iv) not a member of a UI fund. The likely-ineligible group is defined as the complement. Criterion (i) approximates the SA liquid asset cap, which exempts assets dedicated toward certain (unobserved) needs. Criteria (ii)–(iv) proxy experience of a social event and lack of self-sufficiency.

Figure A.5: Effects on disposable income over the life cycle, by starting age group



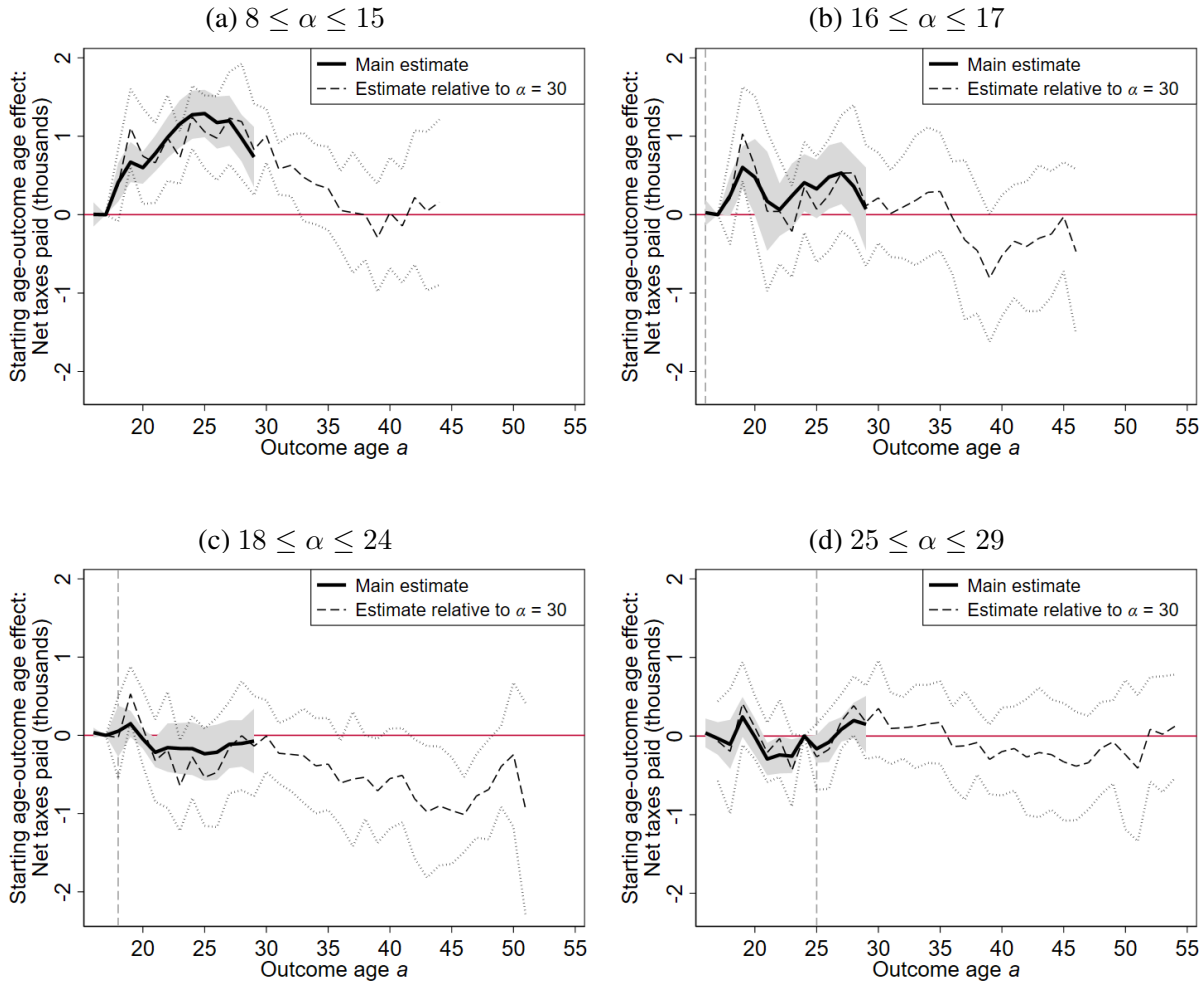
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from Equation (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure A.6: Effects on part-time employment over the life cycle, by starting age group



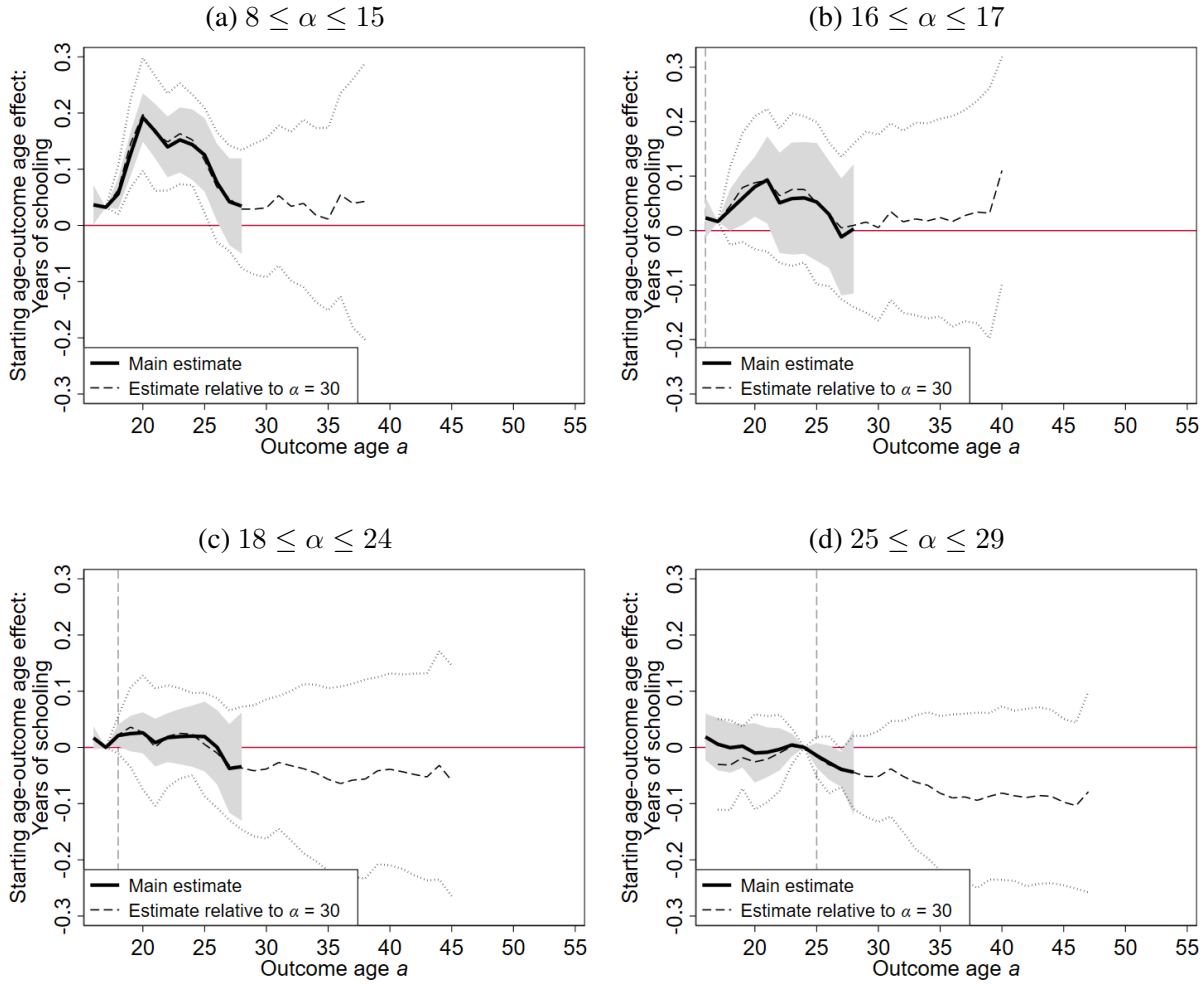
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure A.7: Effects on net taxes paid over the life cycle, by starting age group



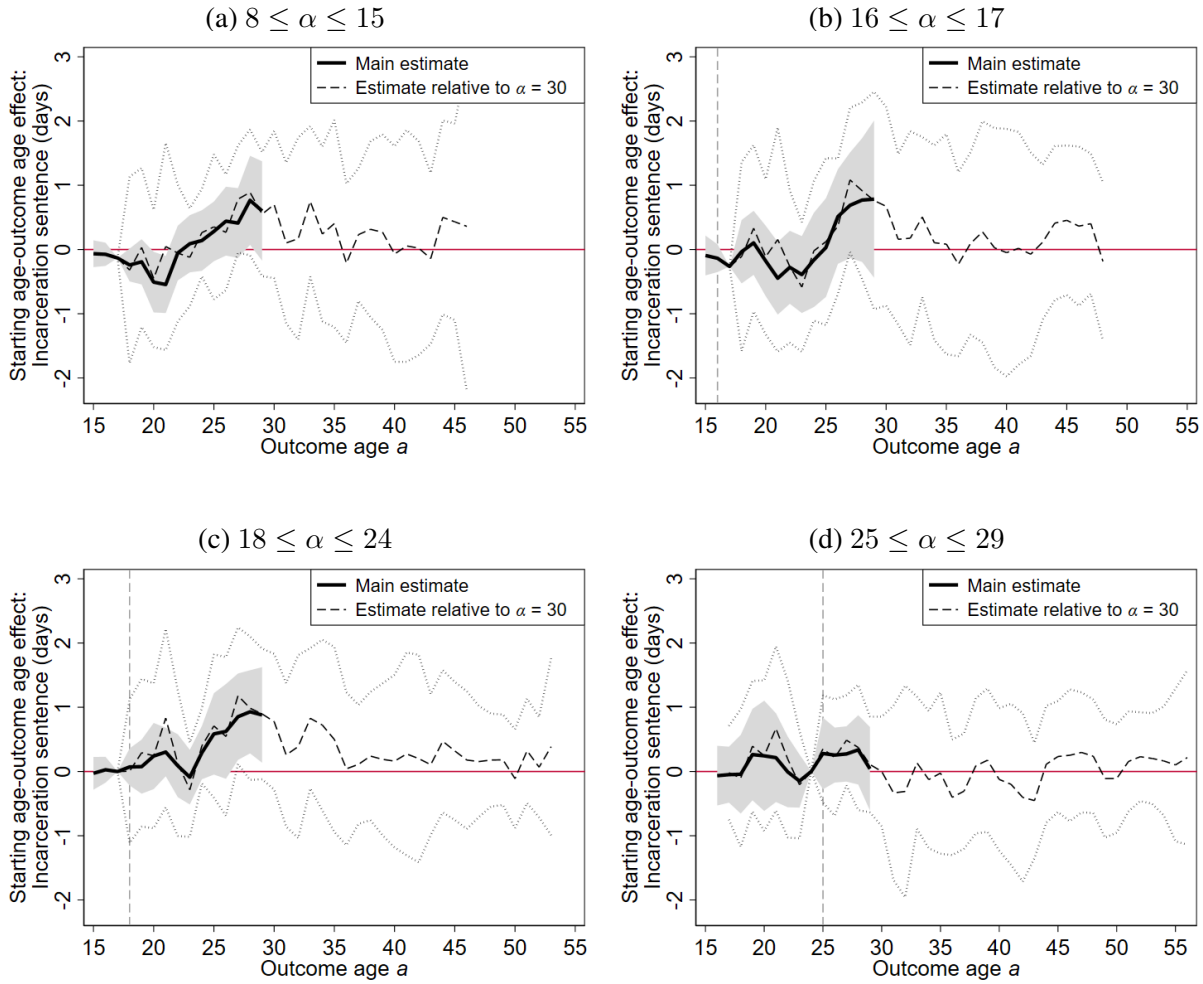
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure A.8: Effects on years of schooling over the life cycle, by starting age group



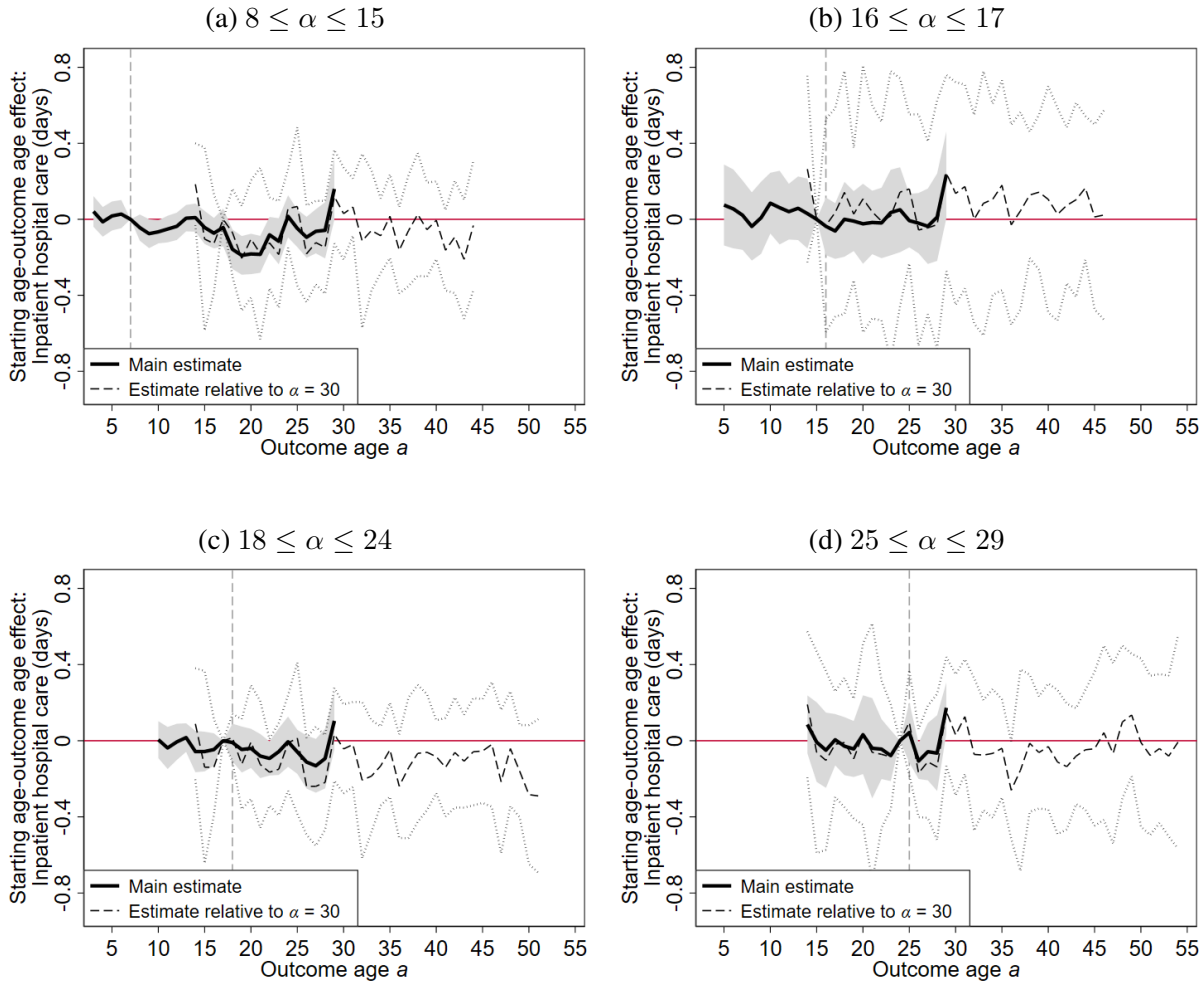
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (3), averaged by starting age groups. Estimates of $\beta^{30}_{\alpha,a}$ are relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data constraints. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals. All event-study estimates of starting ages $\alpha \leq 17$ are re-normalized based on difference-in-difference estimates of welfare-to-work on outcomes measured at outcome age 17; see Footnote 34 for details.

Figure A.9: Effects on incarceration sentence over the life cycle, by starting age group



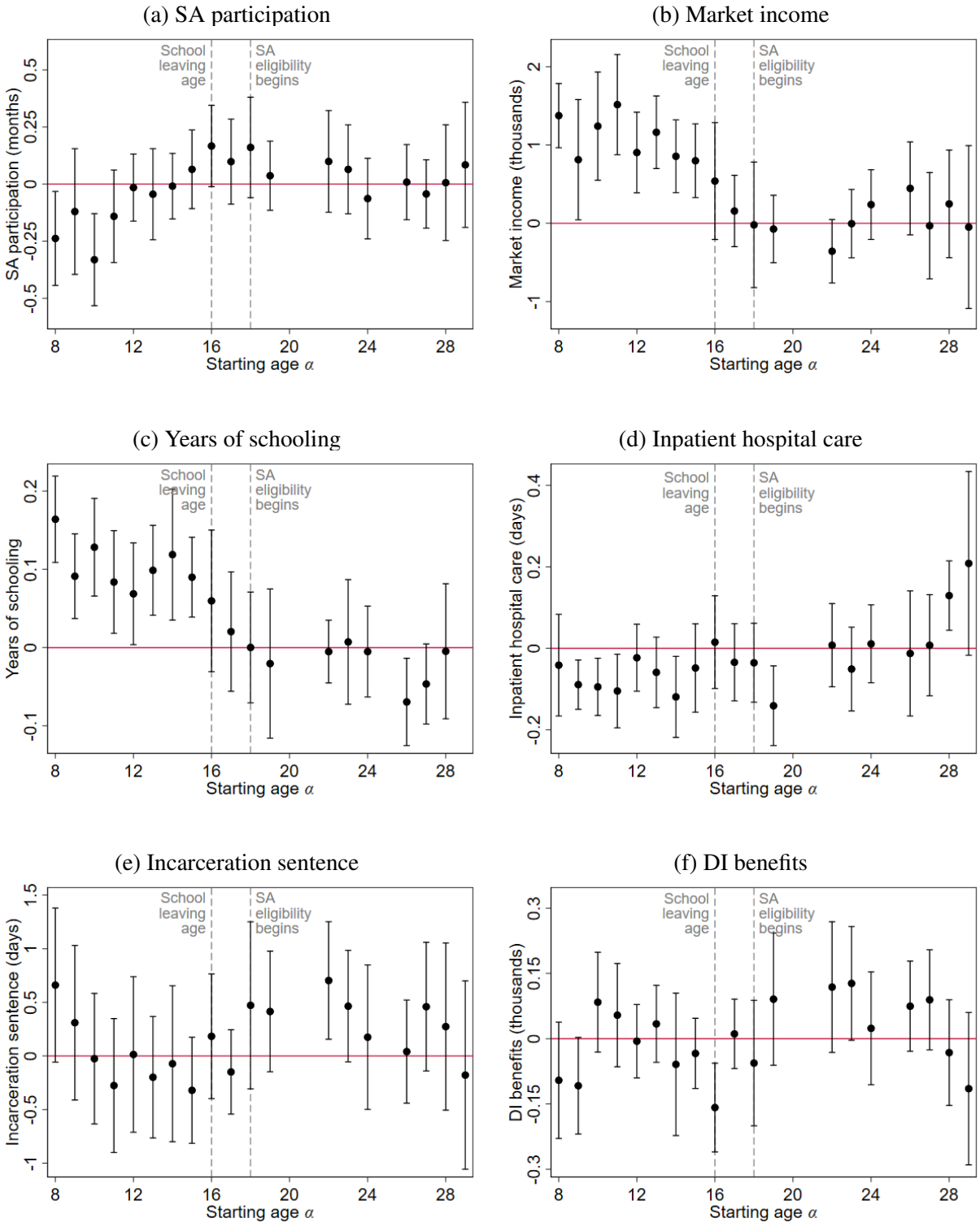
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals. All event-study estimates of starting ages $\alpha \leq 17$ are re-normalized based on difference-in-difference estimates of welfare-to-work on outcomes measured at outcome age 17; see Footnote 34 for details.

Figure A.10: Effects on inpatient hospital care over the life cycle, by starting age group



Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (3), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

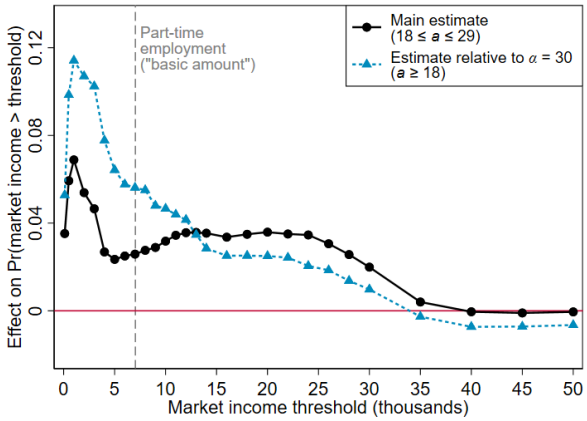
Figure A.11: Heterogeneity of welfare-to-work's effects across starting ages, by outcome



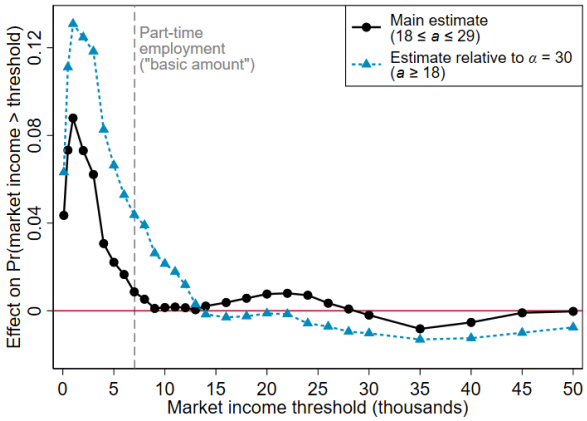
Notes: This plots the main estimates of β_α from the triple difference specification for various outcomes. Vertical bars are 95% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities.

Figure A.12: Effect of welfare-to-work on employment, by different market income thresholds and starting age group

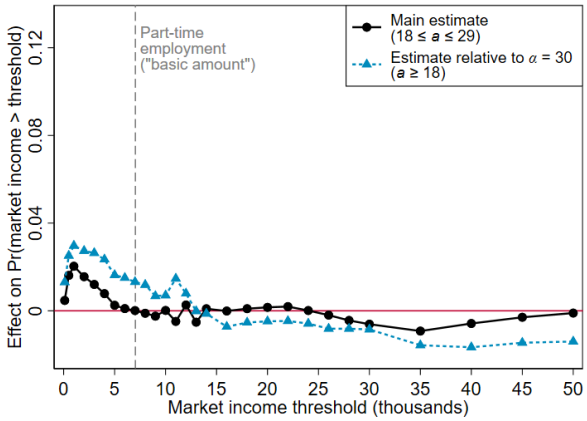
(a) $8 \leq \alpha \leq 15$



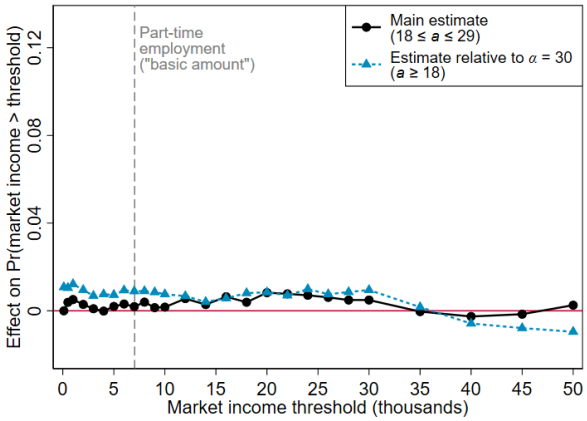
(b) $16 \leq \alpha \leq 17$



(c) $18 \leq \alpha \leq 24$



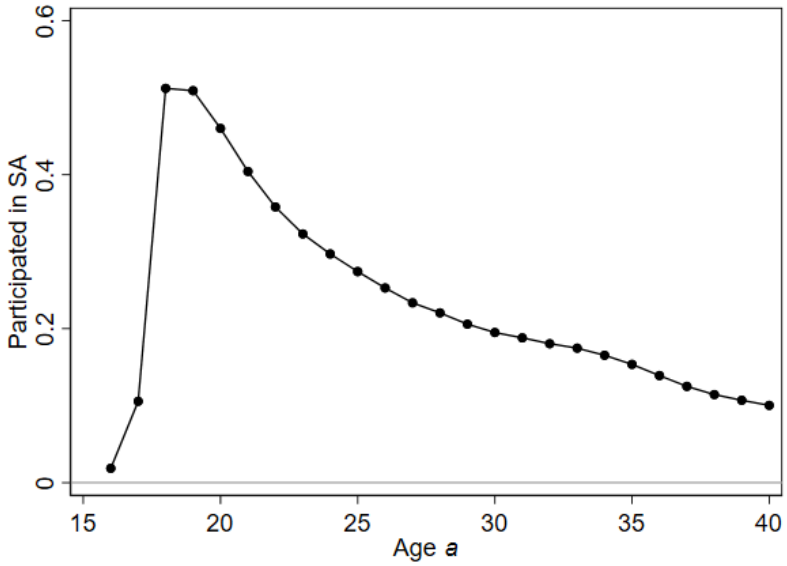
(d) $25 \leq \alpha \leq 29$



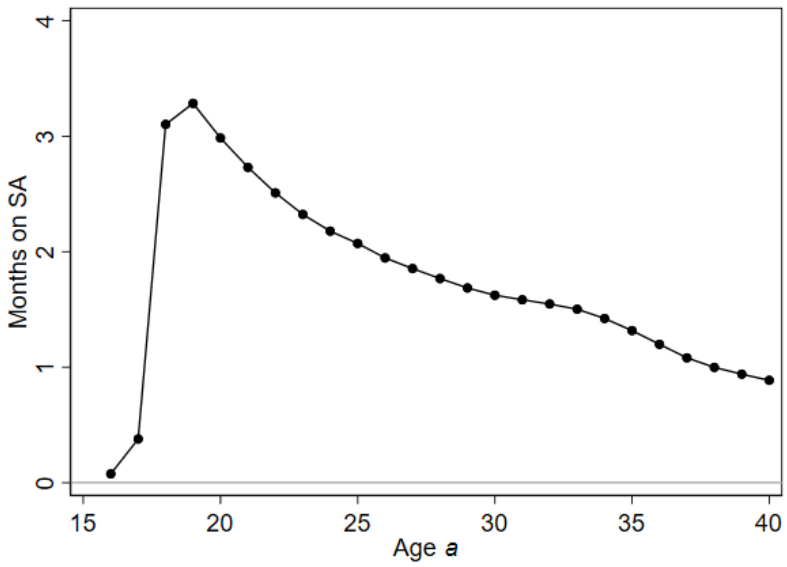
Notes: This figure plots estimates of β_α and β_α^{30}, aggregated by starting age group, where the outcome variables are indicators of market income lying above a given threshold.

Figure A.13: SA participation over the life cycle

(a) Extensive margin



(b) Intensive margin

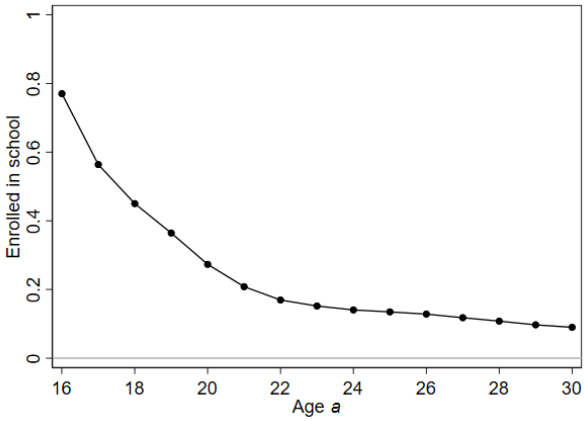


Notes: All figures restrict the main sample to the high-risk group.

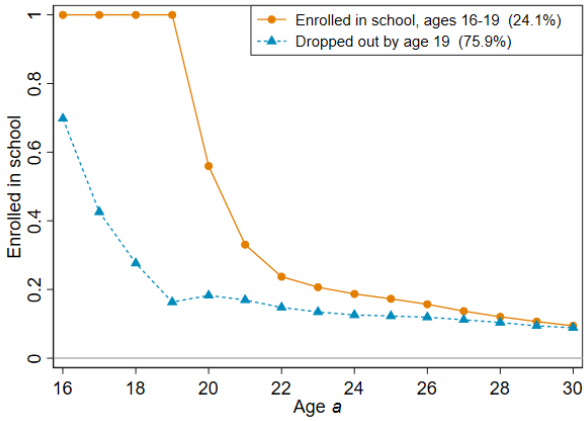
Figure A.14: Education enrollment and attainment over the life-cycle

School enrollment rate

(a) Pooled

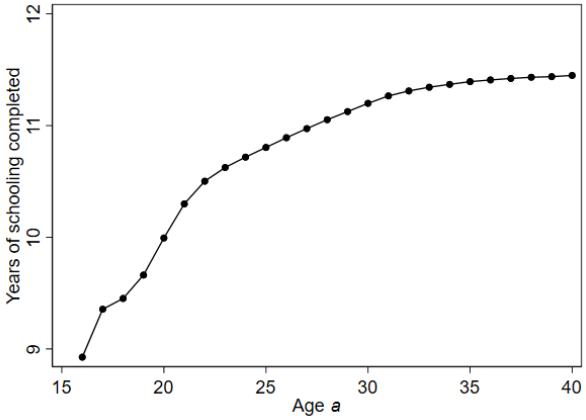


(b) By high school dropout status

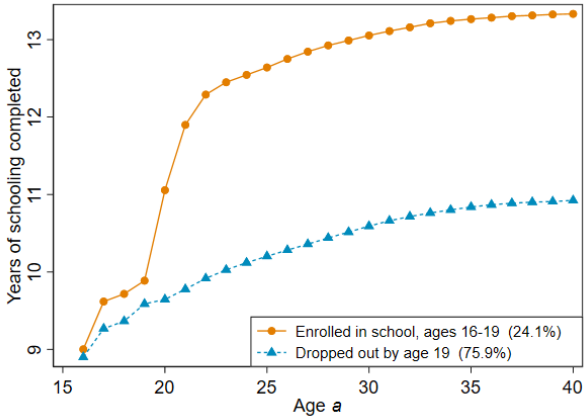


Highest education completed

(c) Pooled



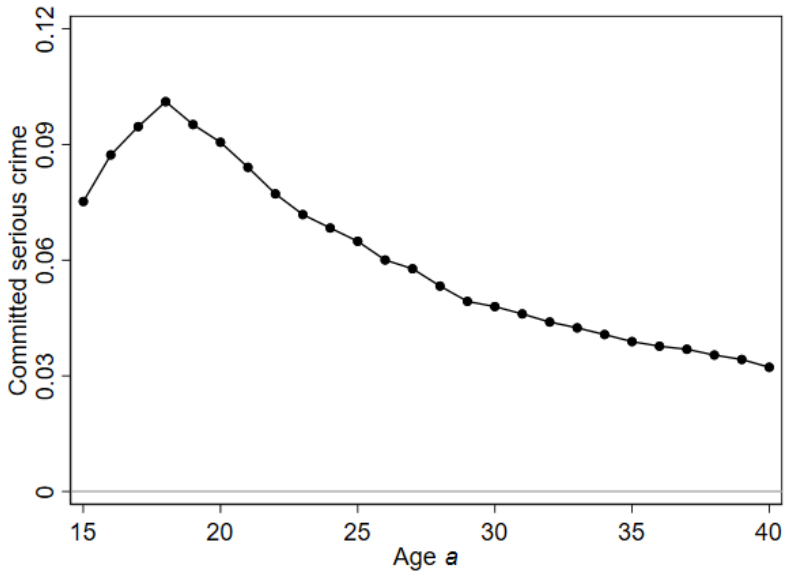
(d) By high school dropout status



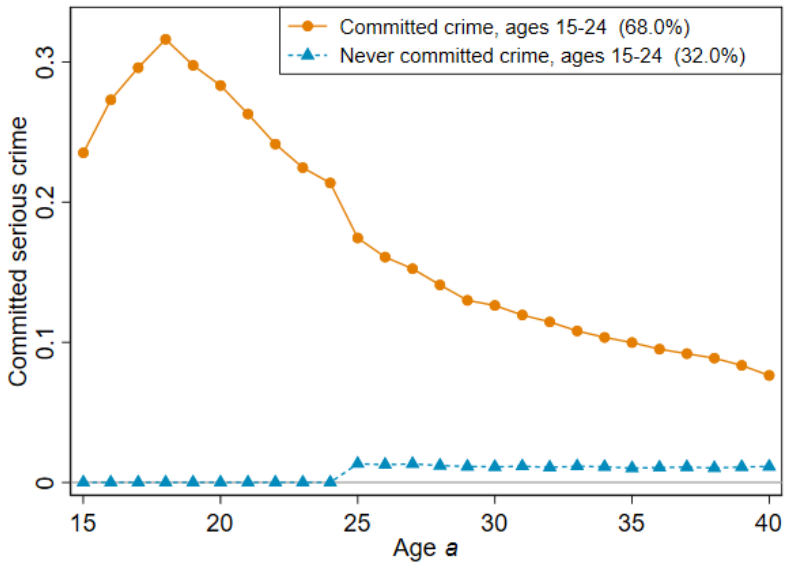
Notes: All figures restrict the main sample to the high-risk group.

Figure A.15: Criminal activity over the life cycle

(a) Pooled

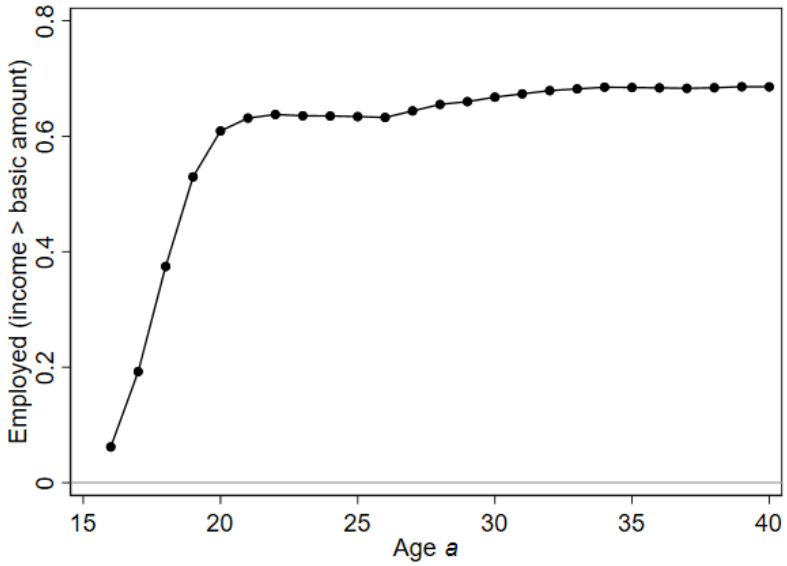


(b) By committed crime, age 15–24



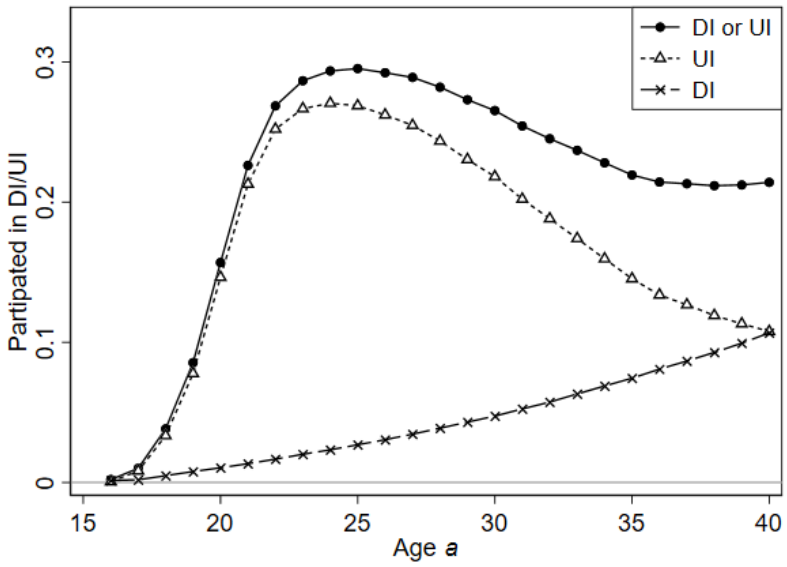
Notes: All figures restrict the main sample to the high-risk group.

Figure A.16: Employment over the life cycle



Notes: This figure restricts the main sample to the high-risk group.

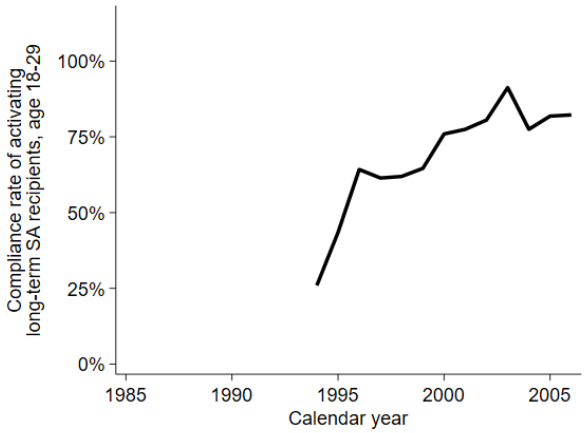
Figure A.17: DI and UI participation over the life cycle



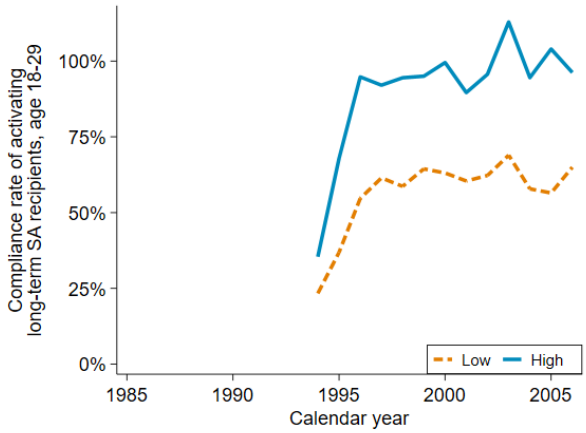
Notes: This figure restricts the main sample to the high-risk group.

Figure A.18: Municipal compliance rate to work requirements over time

(a) National rate



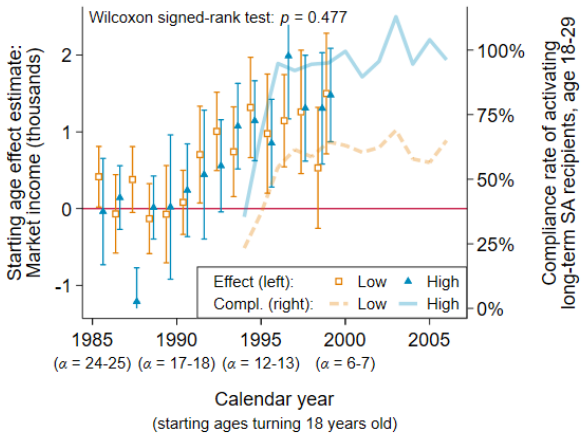
(b) By municipal compliance group



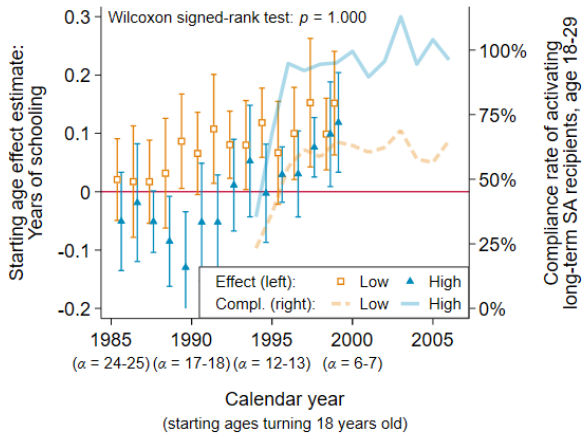
Notes: Municipal compliance rate is defined as the fraction of the number of weeks that individuals in the population between ages 18–29 who continuously participated in SA for at least 4 months were supposed to participate in a work program that actually involved a work program. Panel (a) plots the national average municipal compliance rate over time. Panel (b) plots compliance rates by municipalities with above- or below-median average compliance rate.

Figure A.19: Heterogeneous starting age effects by municipal compliance group

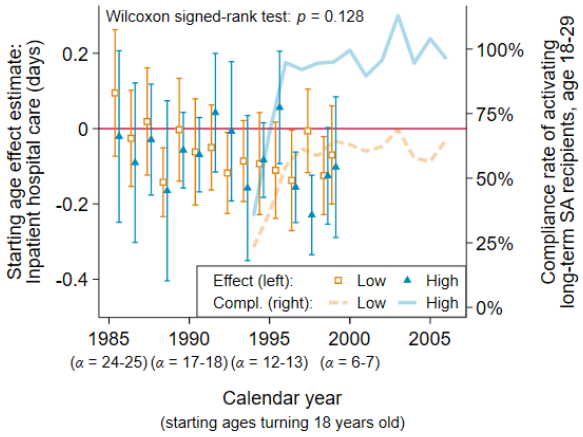
(a) Market income



(b) Years of schooling

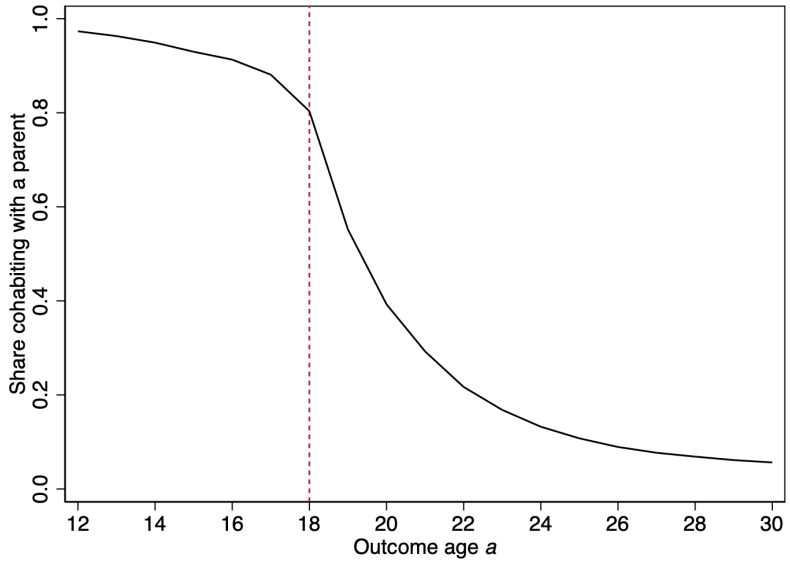


(c) Inpatient hospital care



Notes: This figure overlays municipal compliance group rates (defined in the notes of Figure A.18) with starting age effect estimates of β_α and $\beta_\alpha^{Subgroup}$ from Equation (4), where $Subgroup$ is defined as residing in the high municipal compliance group. These figures also report p-values from Wilcoxon signed-rank tests on the null hypotheses $\hat{\beta}_\alpha^{Subgroup} > \hat{\beta}_\alpha$ (for Panels (a) and (b)) and $\hat{\beta}_\alpha^{Subgroup} < \hat{\beta}_\alpha$ (for Panel (c)). Vertical lines are 95% confidence intervals; some intervals are truncated to maintain a reasonable scale.

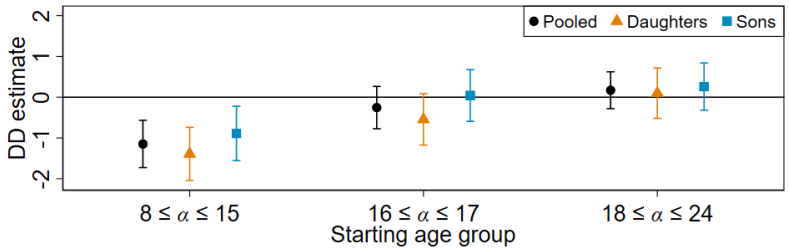
Figure A.20: Cohabitation with at least one parent over the life cycle



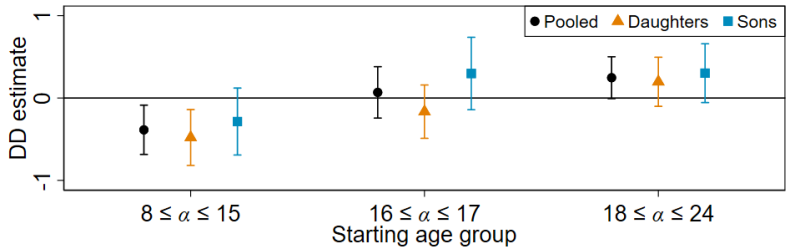
Notes: Sample restricted to the high-risk group.

Figure A.21: Intergenerational spillover effects of welfare-to-work, by mother's starting age group

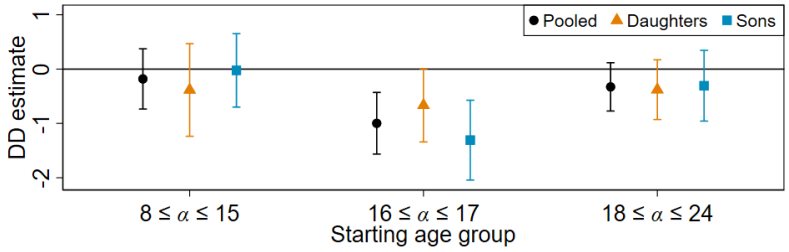
(a) SA participation, total, age 18–20 (months)



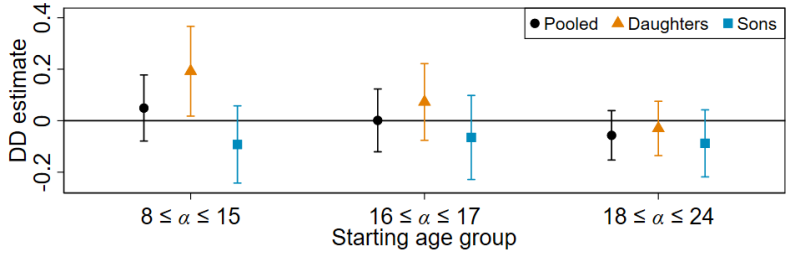
(b) SA work requirements, age 18–20 (months)



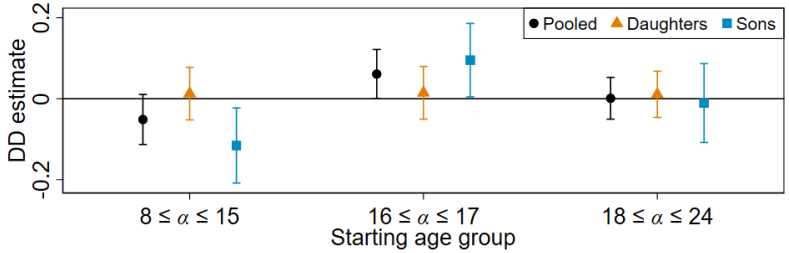
(c) Market income, age 18–20 (thousands)



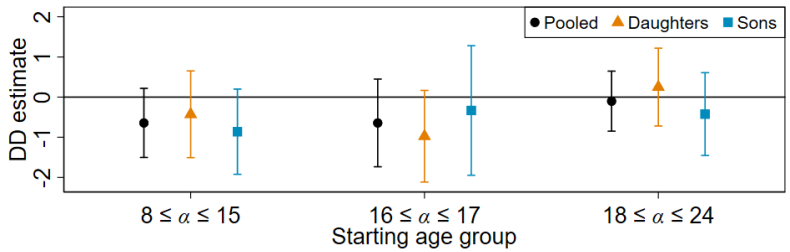
(d) Years of schooling, age 20



(e) Number of serious crimes, age 15–17



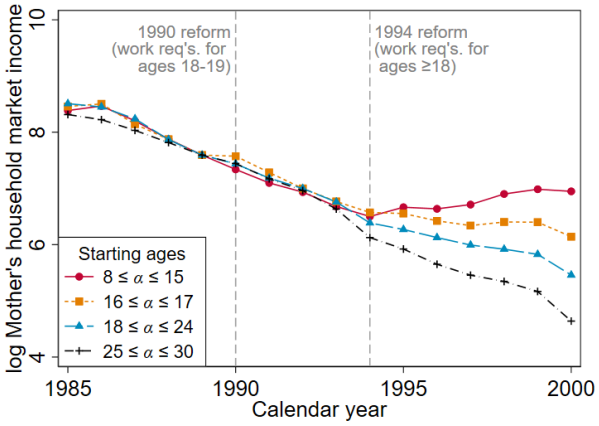
(f) Inpatient hospital care, age 0–6 (days)



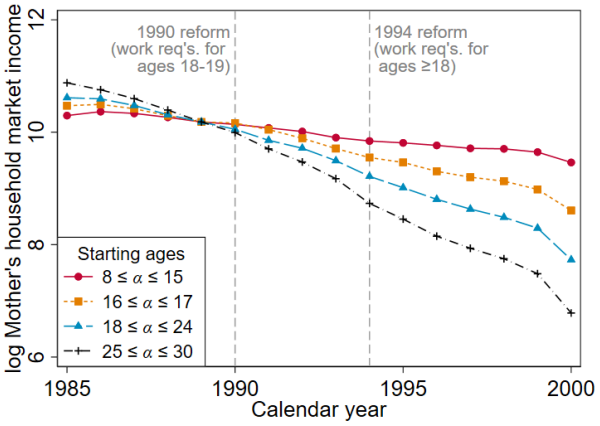
Notes: This figure plots DD estimates of the effect of mother's starting age group on second-generation outcomes relative to starting age group $25 \leq \alpha \leq 29$. Black markers are estimates from the regression described in Footnote 50, which pools over child gender. Orange and blue markers are estimates for second-generation daughters and sons from a TD analog of the regression that includes interactions with second-generation child gender indicator. Vertical capped bars are 95% confidence intervals based on two-way clustered robust standard errors. All estimates conducted on subsamples of young mothers described in the main text; see Appendix Table A.17, Column (1), for the youngest starting age cohort used for each outcome.

Figure A.22: Trends in mother's household financial well-being, by starting age group and risk group

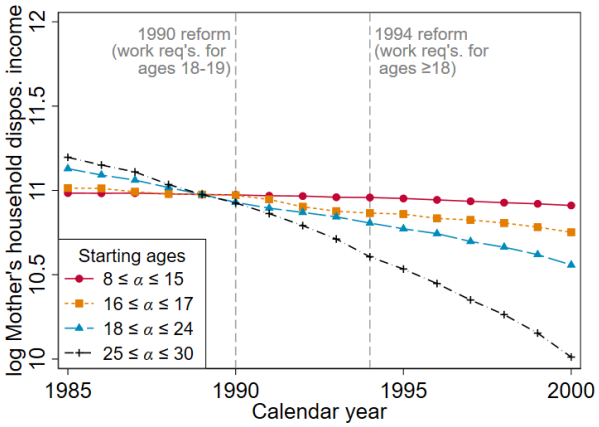
(a) Market income, high-risk



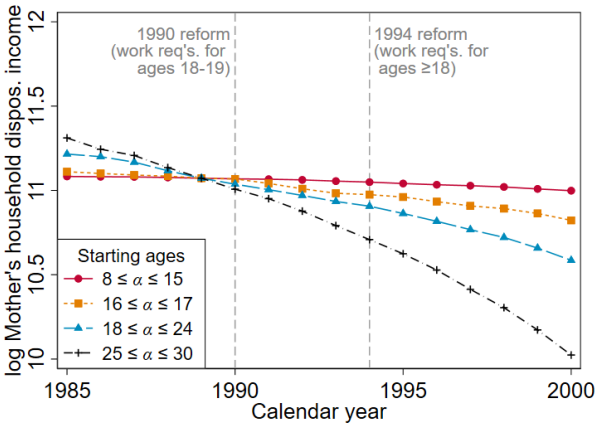
(b) Market income, low-risk



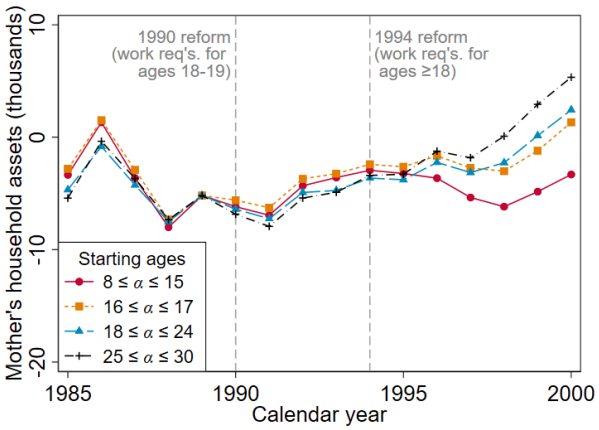
(c) Disposable income, high-risk



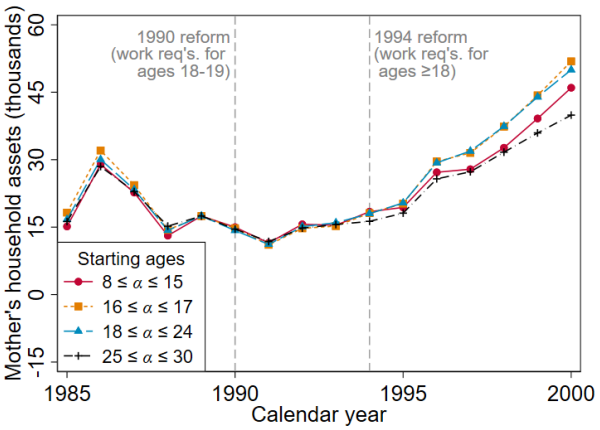
(d) Disposable income, low-risk



(e) Assets, high-risk



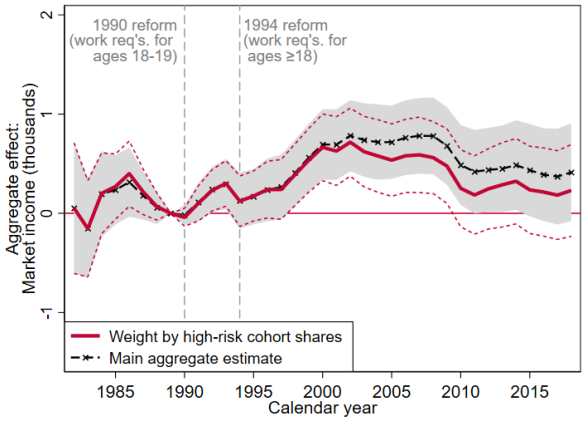
(f) Assets, low-risk



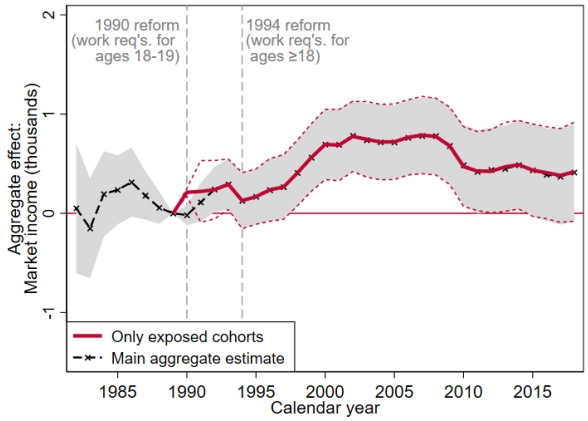
Notes: Sample restricted to the high-risk group.

Figure A.23: Aggregate calendar year effects based on alternative aggregation schemes

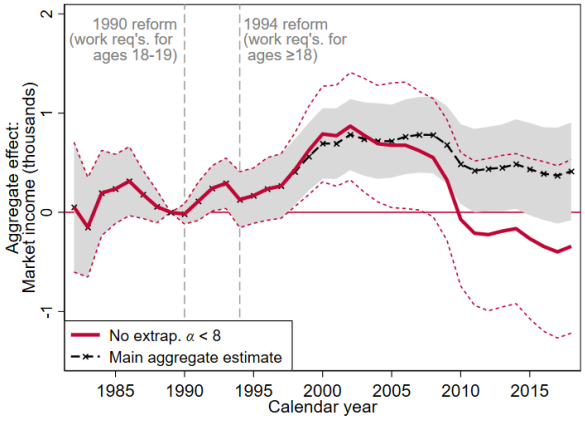
(a) Weighted by high-risk cohort shares



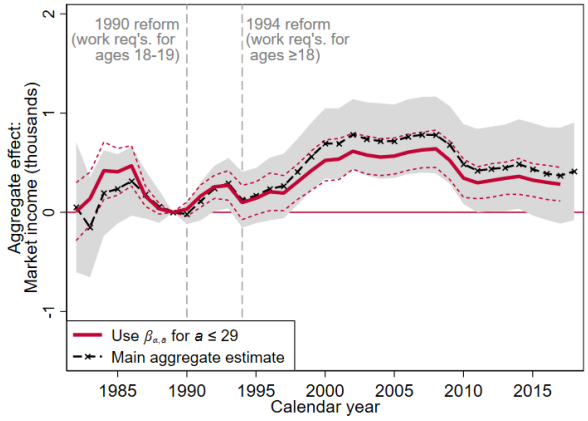
(b) Only exposed cohorts



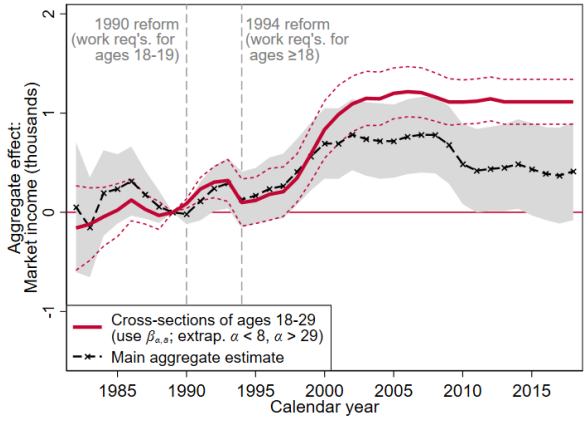
(c) Exclude extrapolated estimates $\alpha < 8$



(d) Use $\beta_{\alpha,a}$ estimates for $a \leq 29$



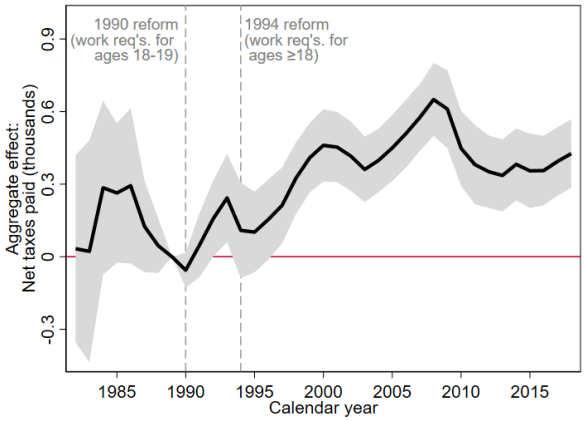
(e) Cross-sectional estimates, ages 18–29
(use $\beta_{\alpha,a}$; extrapolate $\alpha < 8$, $\alpha > 29$)



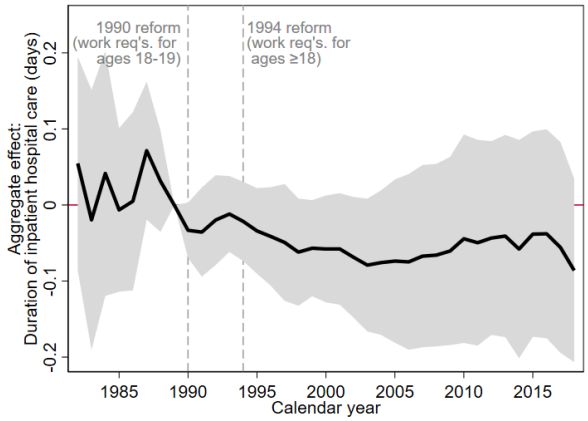
Notes: This figure plots alternative aggregate calendar year estimates of welfare-to-work against the main aggregate estimate reported in Figure 9.

Figure A.24: Aggregate effects of welfare-to-work on outcomes relevant to cost-benefit analysis over calendar time

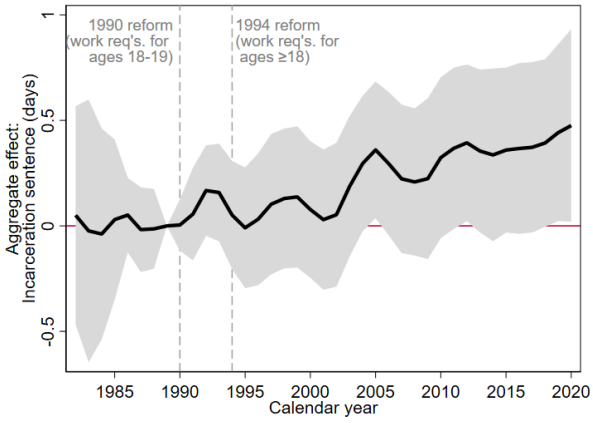
(a) Net taxes paid



(b) Inpatient hospital care



(c) Incarceration sentence (days)



Notes: This figure plots estimates of $\bar{\beta}_t$ defined in Equation (6). It extrapolates estimates of $\alpha < 8$ cohorts with $\alpha = 8$ estimates.

Table A.1: Summary of welfare-to-work reforms, 1990–1998

Date enforced	Ages	Minimum work requirements
Jul. 1, 1990	18–19	5 months, 20 hr./wk.
Oct. 1, 1991	18–20	5 months, 20 hr./wk.
Jul. 1, 1992	18–24	5 months, 20 hr./wk., with optional renewals
Jan. 1, 1994	18–24	6 months, 20 hr./wk., with required renewals
	≥ 25	Mandatory, but duration up to municipality's discretion
Jul. 1, 1995	18–24	6 months, 30 hr./wk.
	≥ 25	Mandatory, but duration up to municipality's discretion
Apr. 1, 1996	18–24	18 months, 30 hr./wk. if education does not qualify for UI membership
	≥ 25	Mandatory, but duration up to municipality's discretion
Jul. 1, 1998	18–29	18 months, 30 hr./wk. if education does not qualify for UI membership
	≥ 30	Mandatory, but duration up to municipality's discretion

Notes: All information based on national laws. Municipalities had discretion to implement stricter policies.

Table A.2: Summary statistics, by risk and starting age group

	Main sample		$8 \leq \alpha \leq 15$		$25 \leq \alpha \leq 29$	
	High-risk (1)	Diff. (2)	High-risk (3)	Diff. (4)	High-risk (5)	Diff. (6)
A. Main outcome variables, age 18–37 (unless otherwise noted)						
SA participation	1.98	1.43	1.93	1.31	1.86	1.38
(months)	(2.74)	[0.46]	(2.68)	[0.42]	(2.65)	[0.47]
SA work requirements	0.52	0.36	0.76	0.49	0.27	0.20
(months)	(0.98)	[0.32]	(1.19)	[0.35]	(0.64)	[0.28]
Market income	19.90	−5.77	19.88	−5.54	20.49	−5.42
(thousands $)	(12.72)	[−0.33]	(12.93)	[−0.31]	(12.63)	[−0.31]
Employed (income	0.63	−0.12	0.61	−0.12	0.66	−0.11
$>$ basic amount)	(0.31)	[−0.31]	(0.32)	[−0.30]	(0.30)	[−0.29]
Years of schooling, age 30	11.18	−1.33	11.48	−1.33	10.95	−1.30
	(2.21)	[−0.43]	(2.24)	[−0.43]	(2.15)	[−0.43]
Inpatient hospital care	0.72	0.15	0.52	0.09	0.90	0.21
(days)	(1.46)	[0.08]	(1.17)	[0.06]	(1.64)	[0.10]
Incarceration sentence	1.80	1.39	1.91	1.40	1.49	1.15
(days)	(12.75)	[0.10]	(12.96)	[0.10]	(11.13)	[0.09]
DI benefits	0.73	0.32	0.86	0.34	0.60	0.27
(thousands $)	(3.02)	[0.08]	(3.42)	[0.08]	(2.64)	[0.08]
B. Background characteristics, age 12–17 (unless otherwise noted)						
Household income	21.77	−20.46	19.16	−25.83	23.83	−14.41
(thousands $)	(18.48)	[−0.82]	(19.15)	[−0.97]	(17.05)	[−0.64]
Household assets	−0.33	−38.41	−5.20	−18.69	4.50	−54.91
(thousands $)	(31.00)	[−0.45]	(27.58)	[−0.24]	(33.87)	[−0.60]
Mother years of schooling	8.05	−0.02	8.74	−0.16	7.56	0.06
	(1.44)	[−0.01]	(1.48)	[−0.07]	(1.20)	[0.04]
Married household	0.34	−0.40	0.30	−0.38	0.42	−0.39
	(0.47)	[−0.62]	(0.46)	[−0.58]	(0.49)	[−0.61]
Single household	0.50	0.30	0.52	0.30	0.44	0.29
	(0.50)	[0.48]	(0.50)	[0.45]	(0.50)	[0.47]
Number of children	1.95	0.27	2.00	0.21	2.08	0.36
in household	(1.21)	[0.18]	(1.12)	[0.15]	(1.28)	[0.23]
Mean parish income, age 15-17	34.13	−0.33	33.53	−1.07	34.87	0.43
(thousands $)	(8.45)	[−0.03]	(8.36)	[−0.10]	(8.50)	[0.04]
Urban municipality, age 17	0.44	0.23	0.43	0.23	0.46	0.23
	(0.50)	[0.35]	(0.50)	[0.36]	(0.50)	[0.35]
N	59,567	170,365	18,982	50,666	18,811	54,019

Notes: Odd-numbered columns report means among the high-risk group pooled over groups of starting age of treatments; standard deviations are in parentheses. Even-numbered columns report differences between the high-risk and low-risk groups; standardized differences scaled by the square root of the sum of risk group variances are in brackets.

Table A.3: Long-run effects of welfare-to-work on labor market outcomes, by starting age group and specification

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
A. Market income (before taxes and transfers, thousands)									
$8 \leq \alpha \leq 15$	0.848^{***}	0.948^{**}	0.960^{***}	0.944^{**}	0.945^{***}	0.939^{**}	1.083^{***}	1.123^{**}	21.135
	(0.177)	(0.306)	(0.177)	(0.308)	(0.182)	(0.318)	(0.181)	(0.360)	[13.961]
$16 \leq \alpha \leq 17$	0.135	0.434	0.237	0.418	0.224	0.404	0.348	0.419	22.305
	(0.196)	(0.348)	(0.199)	(0.352)	(0.227)	(0.382)	(0.234)	(0.464)	[15.001]
$18 \leq \alpha \leq 24$	-0.279^{**}	-0.496^{*}	-0.231^{*}	-0.512^{*}	-0.246^{*}	-0.518^{*}	-0.043	-0.512	22.728
	(0.120)	(0.232)	(0.120)	(0.240)	(0.141)	(0.255)	(0.136)	(0.278)	[15.360]
$25 \leq \alpha \leq 29$	0.236	0.124	0.225	0.109	0.209	0.101	0.153	-0.235	24.811
	(0.235)	(0.172)	(0.229)	(0.185)	(0.251)	(0.221)	(0.238)	(0.227)	[16.142]
B. Disposable income (after taxes and transfers, thousands)									
$8 \leq \alpha \leq 15$	0.090	0.468^{**}	0.141	0.468^{**}	0.132	0.467^{**}	0.152	0.454^{**}	21.891
	(0.092)	(0.156)	(0.095)	(0.158)	(0.096)	(0.164)	(0.098)	(0.190)	[6.915]
$16 \leq \alpha \leq 17$	-0.060	0.457^{*}	-0.013	0.450^{*}	-0.022	0.446^{*}	0.022	0.367	22.754
	(0.109)	(0.212)	(0.111)	(0.212)	(0.120)	(0.224)	(0.123)	(0.254)	[7.255]
$18 \leq \alpha \leq 24$	-0.090	-0.062	-0.063	-0.068	-0.071	-0.069	-0.008	-0.131	23.007
	(0.062)	(0.152)	(0.062)	(0.155)	(0.070)	(0.162)	(0.072)	(0.175)	[7.322]
$25 \leq \alpha \leq 29$	-0.063	0.002	-0.060	-0.006	-0.070	-0.009	-0.093	-0.236	23.758
	(0.079)	(0.097)	(0.077)	(0.103)	(0.093)	(0.125)	(0.094)	(0.140)	[7.491]
C. Net taxes paid (thousands)									
$8 \leq \alpha \leq 15$	0.758^{***}	0.480^{***}	0.819^{***}	0.476^{***}	0.814^{***}	0.473^{**}	0.930^{***}	0.669^{***}	-0.756
	(0.062)	(0.134)	(0.063)	(0.134)	(0.067)	(0.152)	(0.069)	(0.122)	[8.511]
$16 \leq \alpha \leq 17$	0.195^{**}	-0.022	0.251^{***}	-0.033	0.246^{**}	-0.042	0.325^{***}	0.052	-0.450
	(0.085)	(0.174)	(0.086)	(0.175)	(0.093)	(0.185)	(0.095)	(0.184)	[9.092]
$18 \leq \alpha \leq 24$	-0.189^{**}	-0.434^{***}	-0.168^{**}	-0.445^{***}	-0.174^{*}	-0.449^{**}	-0.035	-0.381^{**}	-0.280
	(0.080)	(0.110)	(0.080)	(0.111)	(0.090)	(0.136)	(0.093)	(0.142)	[9.303]
$25 \leq \alpha \leq 29$	0.299^{***}	0.122	0.284^{**}	0.115	0.279^{**}	0.110	0.246^{**}	0.001	1.053
	(0.101)	(0.098)	(0.100)	(0.093)	(0.114)	(0.115)	(0.114)	(0.161)	[9.719]
D. Employed at least part-time (>$7,000)									
$8 \leq \alpha \leq 15$	0.009	0.044^{***}	0.013^{*}	0.044^{***}	0.013^{*}	0.044^{***}	0.012	0.041^{**}	0.620
	(0.007)	(0.011)	(0.007)	(0.011)	(0.007)	(0.012)	(0.007)	(0.013)	[0.322]
$16 \leq \alpha \leq 17$	-0.010	0.033^{***}	-0.006	0.033^{**}	-0.006	0.032^{**}	-0.005	0.030^{**}	0.624
	(0.007)	(0.009)	(0.006)	(0.009)	(0.007)	(0.011)	(0.007)	(0.012)	[0.320]
$18 \leq \alpha \leq 24$	-0.006	0.021^{**}	-0.003	0.021^{**}	-0.004	0.021^{**}	-0.003	0.018^{*}	0.631
	(0.005)	(0.007)	(0.005)	(0.007)	(0.005)	(0.008)	(0.005)	(0.009)	[0.321]
$25 \leq \alpha \leq 29$	0.003	0.017^{***}	0.004	0.017^{***}	0.003	0.017^{***}	0.002	0.014^{**}	0.666
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	[0.310]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: This table reports estimates of β_α and β_α^{30} averaged by groups of starting ages. Standard errors are two-way clustered over birth quarter and municipality of residence and reported in parentheses. Columns (1) and (2) report estimates of a triple difference regression without additional controls. Columns (3) and (4) control for the vector of demographics $\boldsymbol{X}_i$ described in the main text. Columns (5) and (6) also include municipality of residence fixed effects (measured the age before the starting age, or at age 17 for starting ages below 18). Columns (7) and (8) control for linear trends of demographics over birth quarters and over ages. Column (9) reports mean outcomes for each high-risk starting age group measured from age 18 to the last possible age measured in 2018; standard deviations in brackets.

Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Long-run effects of welfare-to-work on other main outcomes, by starting age group and specification

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
A. SA participation, total (months)									
$8 \leq \alpha \leq 15$	-0.081	-0.157^{*}	-0.099	-0.157^{*}	-0.098	-0.157	-0.104	-0.123	1.705
	(0.072)	(0.083)	(0.072)	(0.083)	(0.072)	(0.087)	(0.073)	(0.092)	[2.364]
$16 \leq \alpha \leq 17$	0.167^{**}	-0.043	0.148^{*}	-0.046	0.149^{*}	-0.046	0.133	-0.013	1.766
	(0.077)	(0.073)	(0.077)	(0.075)	(0.079)	(0.083)	(0.080)	(0.087)	[2.388]
$18 \leq \alpha \leq 24$	0.076	-0.153	0.066	-0.154	0.068	-0.154	0.060	-0.128	1.639
	(0.059)	(0.091)	(0.059)	(0.090)	(0.063)	(0.097)	(0.064)	(0.100)	[2.273]
$25 \leq \alpha \leq 29$	0.011	0.003	0.016	0.001	0.019	0.001	0.014	-0.002	1.332
	(0.049)	(0.044)	(0.049)	(0.045)	(0.054)	(0.053)	(0.055)	(0.055)	[2.008]
B. SA work requirements (months)									
$8 \leq \alpha \leq 15$	0.480^{***}	0.289^{***}	0.470^{***}	0.289^{***}	0.470^{***}	0.289^{***}	0.439^{***}	0.320^{***}	0.655
	(0.025)	(0.000)	(0.025)	(0.000)	(0.025)	(0.000)	(0.024)	(0.000)	[1.022]
$16 \leq \alpha \leq 17$	0.462^{***}	0.166^{***}	0.452^{***}	0.166^{***}	0.452^{***}	0.166^{***}	0.416^{***}	0.196^{***}	0.616
	(0.033)	(0.000)	(0.034)	(0.000)	(0.035)	(0.000)	(0.034)	(0.000)	[0.970]
$18 \leq \alpha \leq 24$	0.203^{***}	0.054^{**}	0.197^{***}	0.054^{**}	0.198^{***}	0.054^{**}	0.189^{***}	0.051^{**}	0.398
	(0.015)	(0.017)	(0.015)	(0.017)	(0.016)	(0.019)	(0.019)	(0.020)	[0.749]
$25 \leq \alpha \leq 29$	0.113^{***}	0.088^{***}	0.113^{***}	0.089^{***}	0.114^{***}	0.089^{***}	0.117^{***}	0.095^{***}	0.252
	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.016)	(0.018)	[0.553]
C. Years of schooling									
$8 \leq \alpha \leq 15$	0.065^{***}	0.059	0.079^{***}	0.062^{*}	0.082^{***}	0.067^{*}	0.105^{***}	0.083^{**}	10.925
	(0.021)	(0.031)	(0.021)	(0.032)	(0.022)	(0.035)	(0.022)	(0.035)	[1.648]
$16 \leq \alpha \leq 17$	0.014	0.023	0.027	0.025	0.029	0.027	0.040	0.035	10.958
	(0.033)	(0.049)	(0.033)	(0.050)	(0.034)	(0.056)	(0.036)	(0.055)	[1.781]
$18 \leq \alpha \leq 24$	-0.041^{**}	-0.049	-0.033^{*}	-0.048	-0.033	-0.046	-0.005	-0.039	10.880
	(0.018)	(0.042)	(0.018)	(0.042)	(0.020)	(0.047)	(0.021)	(0.045)	[1.857]
$25 \leq \alpha \leq 29$	-0.028^{*}	-0.049	-0.031^{*}	-0.050	-0.030	-0.049	-0.040^{*}	-0.059	10.927
	(0.015)	(0.044)	(0.016)	(0.044)	(0.020)	(0.052)	(0.021)	(0.053)	[1.902]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: (see below)

Table A.4: (*continued*)

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
D. Inpatient hospital care (days)									
$8 \leq \alpha \leq 15$	-0.096^{***}	-0.057	-0.099^{***}	-0.054	-0.099^{***}	-0.054	-0.072^{**}	-0.048	0.510
	(0.027)	(0.058)	(0.026)	(0.057)	(0.029)	(0.062)	(0.028)	(0.061)	[1.263]
$16 \leq \alpha \leq 17$	-0.035	0.016	-0.036	0.017	-0.035	0.017	-0.010	0.011	0.627
	(0.036)	(0.060)	(0.036)	(0.060)	(0.040)	(0.066)	(0.041)	(0.067)	[1.554]
$18 \leq \alpha \leq 24$	-0.054^{**}	-0.061	-0.053^{**}	-0.060	-0.053^{**}	-0.060	-0.042	-0.063	0.701
	(0.021)	(0.043)	(0.021)	(0.043)	(0.025)	(0.046)	(0.025)	(0.050)	[1.519]
$25 \leq \alpha \leq 29$	0.083^{**}	-0.013	0.086^{**}	-0.013	0.085^{**}	-0.013	0.083^{*}	-0.014	0.776
	(0.032)	(0.032)	(0.031)	(0.032)	(0.036)	(0.036)	(0.041)	(0.037)	[1.475]
E. Incarceration sentence (days)									
$8 \leq \alpha \leq 15$	0.079	0.250	0.058	0.241	0.003	0.182	0.012	0.204	1.173
	(0.119)	(0.176)	(0.120)	(0.177)	(0.145)	(0.227)	(0.147)	(0.236)	[7.918]
$16 \leq \alpha \leq 17$	0.112	0.213	0.098	0.212	0.026	0.136	0.017	0.177	1.250
	(0.145)	(0.209)	(0.148)	(0.210)	(0.189)	(0.253)	(0.195)	(0.262)	[8.852]
$18 \leq \alpha \leq 24$	0.456^{***}	0.294^{**}	0.457^{***}	0.297^{*}	0.459^{**}	0.296	0.446^{**}	0.321^{*}	1.407
	(0.166)	(0.124)	(0.165)	(0.126)	(0.188)	(0.161)	(0.193)	(0.166)	[9.682]
$25 \leq \alpha \leq 29$	0.143	-0.157	0.150	-0.156	0.151	-0.156	0.149	-0.146	1.010
	(0.127)	(0.134)	(0.128)	(0.135)	(0.170)	(0.165)	(0.192)	(0.176)	[7.211]
F. DI benefits (thousands $)									
$8 \leq \alpha \leq 15$	0.038	0.049	0.029	0.050	0.029	0.050	-0.016	-0.002	1.114
	(0.027)	(0.079)	(0.027)	(0.079)	(0.030)	(0.084)	(0.031)	(0.087)	[3.893]
$16 \leq \alpha \leq 17$	-0.029	0.066	-0.036	0.070	-0.037	0.070	-0.074^{**}	0.000	1.335
	(0.028)	(0.097)	(0.028)	(0.095)	(0.035)	(0.103)	(0.035)	(0.101)	[3.908]
$18 \leq \alpha \leq 24$	0.088^{**}	0.138	0.085^{**}	0.141	0.085^{**}	0.141	0.061	0.103	1.615
	(0.034)	(0.091)	(0.034)	(0.090)	(0.037)	(0.099)	(0.037)	(0.099)	[4.239]
$25 \leq \alpha \leq 29$	0.008	-0.066	0.009	-0.060	0.009	-0.060	0.004	-0.036	1.661
	(0.035)	(0.076)	(0.035)	(0.078)	(0.043)	(0.099)	(0.044)	(0.101)	[4.147]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: This table reports estimates of β_α and β_α^{30} averaged by groups of starting ages. Standard errors are two-way clustered over birth quarter and municipality of residence and reported in parentheses. Columns (1) and (2) report estimates of a triple difference regression without additional controls. Columns (3) and (4) control for the vector of demographics $\boldsymbol{X}_i$ described in the main text. Columns (5) and (6) also include municipality of residence fixed effects (measured the age before the starting age, or at age 17 for starting ages below 18). Columns (7) and (8) control for linear trends of demographics over birth quarters and over ages. Column (9) reports mean outcomes for each high-risk starting age group measured from age 18 to the last possible age measured in 2018; standard deviations in brackets.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Long-run effects on SA participation, by starting age group

	Effect	Starting age group				Mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 17$	$18 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
1. Ever participated in SA	β_α	-0.000	0.006	-0.015^{***}	-0.014^{**}	0.299
		(0.009)	(0.010)	(0.005)	(0.006)	$[0.289]$
	β_α^{30}	-0.025	-0.042^{**}	-0.060^{***}	-0.014^{*}	0.239
		(0.014)	(0.014)	(0.008)	(0.006)	$[0.259]$
2. SA participation (months)	β_α	-0.104	0.133	0.060	0.014	2.068
		(0.073)	(0.080)	(0.064)	(0.055)	$[2.657]$
	β_α^{30}	-0.123	-0.013	-0.128	-0.002	1.758
		(0.092)	(0.087)	(0.100)	(0.055)	$[2.444]$
3. Ever participated in SA work req's.	β_α	0.088^{***}	0.080^{***}	0.038^{***}	0.025^{***}	0.109
		(0.005)	(0.005)	(0.004)	(0.003)	$[0.171]$
	β_α^{30}	0.060^{***}	0.035^{***}	0.006	0.018^{***}	0.100
		(0.000)	(0.000)	(0.004)	(0.003)	$[0.155]$
4. SA work req's. (months)	β_α	0.439^{***}	0.416^{***}	0.189^{***}	0.117^{***}	0.502
		(0.024)	(0.034)	(0.019)	(0.016)	$[0.959]$
	β_α^{30}	0.320^{***}	0.196^{***}	0.051^{**}	0.095^{***}	0.485
		(0.000)	(0.000)	(0.020)	(0.018)	$[0.898]$
5. SA benefits (thousands \$)	β_α	0.371^{***}	0.596^{***}	0.552^{***}	0.235^{***}	1.892
		(0.074)	(0.083)	(0.060)	(0.061)	$[2.925]$
	β_α^{30}	0.413^{***}	0.572^{***}	0.457^{***}	0.129^{**}	1.996
		(0.093)	(0.092)	(0.079)	(0.044)	$[3.234]$

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 16–29 (for β_α) or ages 16–37 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Long-run effects on education outcomes, by starting age group

	Effect	Starting age group				Mean
		$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	[S.D.] (5)
1. Years of schooling	β_α	0.105^{***}	0.040	-0.005	-0.040^{*}	11.126
		(0.022)	(0.036)	(0.021)	(0.021)	$[2.167]$
	β_α^{30}	0.083^{**}	0.035	-0.039	-0.059	11.438
		(0.035)	(0.055)	(0.045)	(0.053)	$[2.345]$
2. Completed 10th grade	β_α	0.028^{***}	0.013	0.006	-0.002	0.716
		(0.005)	(0.010)	(0.005)	(0.004)	$[0.451]$
	β_α^{30}	0.060^{***}	0.049^{**}	0.026^{**}	-0.002	0.732
		(0.010)	(0.017)	(0.008)	(0.006)	$[0.443]$
3. Completed high school	β_α	0.020^{***}	0.007	0.002	-0.004	0.434
		(0.006)	(0.007)	(0.005)	(0.005)	$[0.496]$
	β_α^{30}	0.019^{**}	0.010	0.002	-0.003	0.485
		(0.007)	(0.009)	(0.007)	(0.007)	$[0.500]$
4. Completed $>$ high school	β_α	0.026^{***}	0.013^{*}	0.003	-0.009	0.371
		(0.005)	(0.007)	(0.005)	(0.006)	$[0.483]$
	β_α^{30}	0.018^{**}	0.007	-0.009	-0.014	0.443
		(0.007)	(0.011)	(0.010)	(0.011)	$[0.497]$

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group at age 29 (for β_α) or age 39 (for β_α^{30}); standard deviations of outcomes are in brackets. Estimates of $\beta_{\alpha,a}^{30}$ are relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data limitations with education registers. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Long-run effects on types of inpatient hospital care, by starting age group

	Effect	Starting age group				Mean
		$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	[S.D.] (5)
1. Any cause (days)	β_α	-0.072^{**}	-0.010	-0.042	0.083^{*}	0.783
		(0.028)	(0.041)	(0.025)	(0.041)	[1.937]
	β_α^{30}	-0.048	0.011	-0.063	-0.014	0.707
		(0.061)	(0.067)	(0.050)	(0.037)	[1.640]
2. Excluding births or birth-related care (days)	β_α	-0.095^{***}	-0.039	-0.056^{**}	0.054	0.615
		(0.028)	(0.039)	(0.026)	(0.042)	[1.849]
	β_α^{30}	-0.065	0.011	-0.069	-0.024	0.561
		(0.061)	(0.065)	(0.047)	(0.037)	[1.579]
3. Accidents (days)	β_α	-0.007	-0.002	-0.006	-0.015	0.120
		(0.009)	(0.017)	(0.012)	(0.025)	[0.760]
	β_α^{30}	-0.018	-0.008	-0.005	0.006	0.100
		(0.063)	(0.063)	(0.036)	(0.023)	[0.522]
4. Non-metabolic internal disease (days)	β_α	-0.062^{***}	-0.017	-0.025	0.072^{**}	0.449
		(0.018)	(0.020)	(0.015)	(0.025)	[1.248]
	β_α^{30}	0.017	0.072^{*}	-0.007	0.037	0.403
		(0.037)	(0.037)	(0.022)	(0.027)	[1.073]
5. Mental or psychiatric (days)	β_α	0.004	-0.001	-0.004	0.005	0.017
		(0.006)	(0.007)	(0.005)	(0.015)	[0.242]
	β_α^{30}	-0.016^{*}	-0.014	-0.002	0.000	0.023
		(0.008)	(0.011)	(0.007)	(0.009)	[0.282]
6. Metabolic disease (days)	β_α	0.008	0.016	0.012	-0.016	0.057
		(0.013)	(0.016)	(0.011)	(0.014)	[0.942]
	β_α^{30}	-0.028^{*}	-0.018	-0.013	-0.034	0.071
		(0.014)	(0.023)	(0.016)	(0.022)	[0.816]
7. Musculoskeletal disease or issues (days)	β_α	-0.007	0.005	-0.015	0.005	0.057
		(0.007)	(0.014)	(0.012)	(0.017)	[0.487]
	β_α^{30}	-0.013	0.002	-0.032^{*}	-0.010	0.059
		(0.013)	(0.014)	(0.016)	(0.011)	[0.422]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 15–29 (for β_α) or ages 15–39 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Long-run effects on crime outcomes, by starting age group

	Effect	Starting age group				Mean
		$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	[S.D.] (5)
1. Ever convicted	β_α	-0.020^{***}	-0.010^{***}	-0.006^{**}	-0.004	0.070
		(0.002)	(0.003)	(0.003)	(0.004)	[0.147]
	β_α^{30}	-0.035^{***}	-0.028^{***}	-0.020^{***}	-0.008^{***}	0.058
		(0.005)	(0.005)	(0.004)	(0.002)	[0.123]
2. Incarceration sentence (days)	β_α	0.012	0.017	0.446^{**}	0.149	1.590
		(0.147)	(0.195)	(0.193)	(0.192)	[11.723]
	β_α^{30}	0.204	0.177	0.321^{*}	-0.146	1.577
		(0.236)	(0.262)	(0.166)	(0.176)	[11.072]
3. Incarceration sentence , property crime (days)	β_α	-0.116	0.039	0.236^{*}	0.052	0.838
		(0.091)	(0.122)	(0.122)	(0.125)	[7.195]
	β_α^{30}	-0.101	-0.047	0.072	-0.026	0.686
		(0.153)	(0.131)	(0.074)	(0.089)	[5.702]
4. Incarceration sentence, violent crime (days)	β_α	0.081	0.047	0.160^{**}	0.049	0.323
		(0.065)	(0.076)	(0.067)	(0.101)	[3.453]
	β_α^{30}	0.045	0.069	0.096	0.011	0.290
		(0.093)	(0.091)	(0.062)	(0.038)	[2.765]
5. Incarceration sentence, drug-related crime (days)	β_α	0.079^{*}	0.058	0.025	0.094	0.213
		(0.042)	(0.051)	(0.040)	(0.073)	[2.617]
	β_α^{30}	0.119^{**}	0.072	0.030	-0.071^{**}	0.220
		(0.043)	(0.041)	(0.028)	(0.029)	[2.375]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 15–29 (for β_α) or ages 15–39 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Long-run effects on social insurance participation, by starting age group

	Effect	Starting age group				Mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 17$	$18 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
1. Ever on DI	β_α	-0.004^{*}	-0.006^{***}	0.002	-0.002	0.019
		(0.002)	(0.002)	(0.002)	(0.003)	[0.105]
	β_α^{30}	-0.006	-0.006	-0.000	-0.004	0.040
		(0.005)	(0.005)	(0.005)	(0.005)	[0.146]
2. DI benefits (thousands \$)	β_α	-0.016	-0.074^{**}	0.061	0.004	0.334
		(0.031)	(0.035)	(0.037)	(0.044)	[1.972]
	β_α^{30}	-0.002	0.000	0.103	-0.036	0.768
		(0.087)	(0.101)	(0.099)	(0.101)	[2.938]
3. Ever on UI	β_α	-0.018^{**}	-0.019^{***}	-0.009	0.007	0.179
		(0.008)	(0.007)	(0.006)	(0.007)	[0.215]
	β_α^{30}	-0.018^{**}	-0.011^{*}	0.001	-0.003	0.168
		(0.006)	(0.006)	(0.005)	(0.005)	[0.186]
4. UI benefits (thousands \$)	β_α	-0.505^{***}	-0.410^{***}	-0.145^{**}	0.017	1.432
		(0.081)	(0.073)	(0.068)	(0.097)	[2.255]
	β_α^{30}	-0.496^{***}	-0.335^{***}	-0.095^{*}	-0.015	1.285
		(0.060)	(0.055)	(0.048)	(0.063)	[1.804]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30}, aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 16–29 (for β_α) or ages 16–39 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Availability of choice margins vs. effects of welfare-to-work, by starting age group

Choice margin	Starting age group			
	$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 17$	$18 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$
Education	↑↑	↑	—	↓
Crime	—	↑	↑↑	—
Employment	↑↑	↑	↑	↑
SA (total)	↓	↑	—	—
SA work requirements	↑↑	↑↑	↑	↑
Social insurance programs (DI, UI)	↓	↓	↑	—

Notes: This figure illustrates the availability of choice margins for members of different starting age groups at the time of facing the SA reforms. Shaded cells indicate choice margins that are relatively less available to a given starting age group. The ↑↑, ↑, —, ↓, and ↓↓ symbols indicate if starting age group estimates of β_α and β_α^{30} on outcomes associated with the choice margins are positive with relatively large magnitude, positive with relatively small magnitude, null, negative with relatively small magnitude, and negative with relatively large magnitude, respectively.

Table A.11: Heterogeneous effects by subgroup: Market income (thousands)

Subgroup	Starting age group $8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	Mean [S.D.] (5)	N (6)
1. Female	1.161***	0.435	0.130	0.042	13.0	29,248
	(0.199)	(0.266)	(0.180)	(0.354)	[8.8]	
2. Male	1.011***	0.268	−0.197	0.252	19.5	30,194
	(0.238)	(0.280)	(0.191)	(0.248)	[11.5]	
3. Household income ≤ 25th percentile, age 12–17	1.013***	0.383	0.075	0.107	15.8	49,208
	(0.198)	(0.307)	(0.159)	(0.256)	[10.7]	
4. High school dropout	—	—	−0.479**	0.115	15.7	23,736
			(0.184)	(0.294)	[10.5]	
5. Committed crime, age 15–18	—	—	−0.748*	0.686	14.2	6,259
			(0.389)	(0.766)	[10.9]	
6. Participated in SA ≥3 years, age 18–24	—	—	—	0.294	10.9	8,116
				(0.502)	[7.9]	
7. Own household income ≤ 25th percentile, age 18–24	—	—	—	0.130	15.3	8,266
				(0.399)	[11.0]	
8. Childless single adults, age 25	—	—	—	−0.104	17.7	5,440
				(0.386)	[10.8]	
9. Parents, age 25	—	—	—	0.433	15.6	8,204
				(0.294)	[10.5]	
10. Single mothers, age 25	—	—	—	0.250	9.1	1,642
				(0.530)	[7.5]	
Main estimate	1.083***	0.348	−0.043	0.153	16.3	59,442
	(0.181)	(0.234)	(0.136)	(0.238)	[10.8]	

Notes: Columns (1)–(4) report estimates of $\beta_\alpha^{Subgroup} \equiv \beta_\alpha + \beta_\alpha^S$ from Equation (4) aggregated by groups of starting ages for different subgroups. Rows with missing estimates correspond to subgroups that are measured after starting age groups. The final row reports the main estimates of β_α aggregated by groups of starting ages. Column (5) reports the mean market income of the high-risk subgroup measured between ages 18–29 and standard deviation in brackets. Column (6) reports the number of high-risk individuals belonging to the subgroup of the included starting ages α. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. All estimates are expressed as thousands of 2010 U.S. dollars. Stars indicate statistical significance: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: Heterogeneous effects by subgroup: SA participation (months)

Subgroup	Starting age group $8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	Mean [S.D.] (5)	N (6)
1. Female	−0.013	0.218*	0.107	0.056	2.7	29,302
	(0.096)	(0.112)	(0.085)	(0.045)	[3.3]	
2. Male	−0.201***	0.052	0.011	−0.028	2.1	30,265
	(0.071)	(0.081)	(0.075)	(0.062)	[2.8]	
3. Household income ≤ 25th percentile, age 12–17	−0.222***	0.055	0.047	0.009	2.5	49,320
	(0.074)	(0.097)	(0.074)	(0.050)	[3.1]	
4. High school dropout	—	—	0.057	0.034	2.7	23,737
			(0.082)	(0.049)	[3.1]	
5. Committed crime, age 15–18	—	—	0.092	−0.068	3.8	6,259
			(0.164)	(0.197)	[3.5]	
6. Participated in SA ≥ 3 years, age 18–24	—	—	—	−0.120	4.4	8,116
				(0.107)	[3.0]	
7. Own household income ≤ 25th percentile, age 18–24	—	—	—	0.141	3.1	8,266
				(0.082)	[3.3]	
8. Childless single adults, age 25	—	—	—	0.058	2.1	5,440
				(0.080)	[2.7]	
9. Parents, age 25	—	—	—	0.013	2.7	8,204
				(0.043)	[3.2]	
10. Single mother, age 25	—	—	—	−0.032	4.9	1,642
				(0.189)	[3.7]	
Main estimate	−0.104	0.133	0.060	0.014	2.4	59,567
	(0.073)	(0.080)	(0.064)	(0.055)	[3.1]	

Notes: See notes to Table A.11.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Heterogeneous effects by subgroup: Years of schooling

Subgroup	Starting age group $8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	Mean [S.D.] (5)	N (6)
1. Female	0.140^{***}	0.053	-0.013	-0.021	10.5	27,590
	(0.026)	(0.038)	(0.029)	(0.021)	[1.4]	
2. Male	0.063^{**}	0.020	-0.006	-0.020	10.5	27,956
	(0.027)	(0.049)	(0.030)	(0.023)	[1.6]	
3. Household income $\leq$ 25th percentile, age 12–17	0.082^{***}	0.030	-0.014	-0.039^{*}	10.4	46,037
	(0.027)	(0.038)	(0.029)	(0.021)	[1.5]	
4. High school dropouts	—	—	0.003	-0.015	10.0	21,570
			(0.025)	(0.018)	[1.3]	
5. Committed crime, age 15–18	—	—	0.003	-0.003	9.8	5,578
			(0.062)	(0.045)	[1.6]	
6. Participated in SA $\geq$ 3 years, age 18–24	—	—	—	-0.053^{*}	9.8	6,644
				(0.029)	[1.3]	
7. Own household income $\leq$ 25th percentile, age 18–24	—	—	—	0.017	10.1	6,777
				(0.025)	[1.6]	
8. Childless single adults, age 25	—	—	—	0.045	10.6	4,537
				(0.033)	[1.6]	
9. Parents, age 25	—	—	—	-0.033	10.1	6,774
				(0.020)	[1.5]	
10. Single mothers, age 25	—	—	—	-0.078	9.8	1,375
				(0.046)	[1.2]	
Main estimate	0.105^{***}	0.040	-0.005	-0.040^{*}	10.5	55,546
	(0.022)	(0.036)	(0.021)	(0.021)	[1.5]	

Notes: See notes to Table A.11.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.14: Heterogeneous effects by subgroup: Incarceration sentence (days)

Subgroup	Starting age group $8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	Mean [S.D.] (5)	N (6)
1. Female	−0.009	0.023	−0.074	−0.169	0.17	29,302
	(0.090)	(0.135)	(0.087)	(0.144)	[3.70]	
2. Male	0.191	0.158	0.943**	0.451	3.66	30,265
	(0.263)	(0.305)	(0.368)	(0.374)	[19.54]	
3. Household income	−0.064	0.109	0.360	0.011	2.11	49,320
$\leq$ 25th percentile, age 12–17	(0.198)	(0.240)	(0.252)	(0.260)	[15.00]	
4. High school dropouts	—	—	0.324*	0.420**	1.70	23,737
			(0.180)	(0.199)	[13.03]	
5. Committed crime, age 15–18	—	—	0.650	2.067*	7.13	6,259
			(0.766)	(1.113)	[27.07]	
6. Participated in SA	—	—	—	−0.836*	2.99	8,116
$\geq$ 3 years, age 18–24				(0.472)	[17.37]	
7. Own household income	—	—	—	0.044	2.12	8,266
$\leq$ 25th percentile, age 18–24				(0.459)	[14.64]	
8. Childless single adults,	—	—	—	0.815	2.15	5,440
age 25				(0.505)	[14.71]	
9. Parents, age 25	—	—	—	−0.276	1.32	8,204
				(0.321)	[11.37]	
10. Single mothers, age 25	—	—	—	−0.631	0.21	1,642
				(0.644)	[4.01]	
Main estimate	0.012	0.017	0.446**	0.149	1.94	59,567
	(0.147)	(0.195)	(0.193)	(0.192)	[14.28]	

Notes: See notes to Table A.11.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.15: Heterogeneous effects by subgroup: Inpatient hospital care (days)

Subgroup	Starting age group $8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	Mean [S.D.] (5)	N (6)
1. Female	-0.118^{***}	-0.082	-0.055	0.134	1.18	28,779
	(0.041)	(0.055)	(0.042)	(0.081)	[1.94]	
2. Male	0.005	0.064	-0.021	0.032	0.42	26,949
	(0.033)	(0.063)	(0.042)	(0.057)	[1.51]	
3. Household income $\leq$ 25th percentile, age 12–17	-0.079^{**}	-0.044	-0.068^{**}	0.008	0.81	46,170
	(0.030)	(0.046)	(0.031)	(0.048)	[1.79]	
4. High school dropout	—	—	-0.052	0.098	1.05	22,020
			(0.049)	(0.061)	[2.03]	
5. Committed crime, age 15–18	—	—	-0.032	-0.009	0.86	5,774
			(0.075)	(0.147)	[1.85]	
6. Participated in SA $\geq$ 3 years, age 18–24	—	—	—	-0.154	1.36	7,683
				(0.112)	[2.34]	
7. Own household income $\leq$ 25th percentile, age 18–24	—	—	—	-0.044	1.08	7,534
				(0.082)	[2.03]	
8. Childless single adults, age 25	—	—	—	0.214^{***}	0.71	4,802
				(0.056)	[2.26]	
9. Parents, age 25	—	—	—	-0.027	1.43	7,895
				(0.080)	[1.96]	
10. Single mothers, age 25	—	—	—	-0.513^{***}	2.09	1,640
				(0.162)	[2.49]	
Main estimate	-0.072^{**}	-0.010	-0.042	0.083^{*}	0.81	55,728
	(0.028)	(0.041)	(0.025)	(0.041)	[1.78]	

Notes: See notes to Table A.11.
Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.16: Welfare-to-work's effects on maternal age, by starting age group

	$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	Mean (4)	N (5)
Effect relative to $25 \leq \alpha \leq 30$	0.225	-0.183	-0.233	24.58	21,187
	(0.179)	(0.173)	(0.153)	[5.08]	

Notes: This table reports estimates from the regression described in Footnote 50. Estimation sample restricted to mothers. Maternal age is defined as the age at first birth. Column (5) reports the mean of the high-risk starting age group $25 \leq \alpha \leq 30$; standard deviation reported in square brackets. Column (6) reports the number of high-risk mothers in the estimation sample. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: Intergenerational spillover effects, by mother's starting age group and child gender

	$\alpha_{min} \leq \alpha \leq 15$			$16 \leq \alpha \leq 17$		$18 \leq \alpha \leq 24$		Mean		
	α_{min} (1)	Son (2)	Daughter (3)	Son (4)	Daughter (5)	Son (6)	Daughter (7)	Son (8)	Daughter (9)	N (10)
A. Labor market outcomes										
Market income, age 18–20 (thousands)	14	−0.023	−0.384	−1.308***	−0.667*	−0.306	−0.379	11.81	8.76	52,921
		(0.338)	(0.425)	(0.367)	(0.336)	(0.325)	(0.274)	[8.29]	[6.28]	
Disposable income, age 18–20 (thousands)	14	0.032	−0.170	−0.689***	−0.272	−0.172	−0.142	11.07	9.87	52,921
		(0.208)	(0.260)	(0.231)	(0.244)	(0.233)	(0.180)	[5.18]	[4.00]	
Employed (income > basic amount), age 18–20	14	0.006	−0.012	−0.061***	−0.039**	−0.007	−0.016	0.56	0.46	52,921
		(0.020)	(0.024)	(0.016)	(0.018)	(0.017)	(0.014)	[0.40]	[0.38]	
Net taxes paid, age 18–20 (thousands)	14	−0.055	−0.214	−0.619***	−0.395*	−0.134	−0.237	0.74	−1.11	52,921
		(0.201)	(0.254)	(0.221)	(0.214)	(0.142)	(0.165)	[3.81]	[3.92]	
B. Health outcomes										
Birth weight (kg)	8	0.018	0.010	−0.020	0.016	0.013	−0.009	3.42	3.31	64,160
		(0.024)	(0.028)	(0.041)	(0.037)	(0.025)	(0.023)	[0.57]	[0.51]	
Duration of inpatient care, age 0–6 (days)	8	−0.861	−0.427	−0.333	−0.974*	−0.422	0.249	9.58	8.07	65,078
		(0.533)	(0.542)	(0.810)	(0.573)	(0.517)	(0.486)	[12.15]	[10.16]	
Duration of inpatient care, age 7–14 (days)	8	0.010	−0.098	0.011	−0.236	−0.153	0.124	1.09	1.08	65,062
		(0.120)	(0.133)	(0.169)	(0.181)	(0.117)	(0.161)	[3.28]	[3.38]	
Duration of inpatient care, age 15–22 (days)	13	0.084	−0.352*	−0.047	0.050	−0.151	0.134	1.21	2.40	55,705
		(0.157)	(0.209)	(0.145)	(0.179)	(0.155)	(0.203)	[3.81]	[4.96]	
C. Education outcomes										
Grade 9 exam score, age 16 (s.d.)	15	−0.027	−0.028	−0.030	−0.003	−0.010	−0.014	−0.33	−0.13	39,194
		(0.077)	(0.051)	(0.045)	(0.049)	(0.033)	(0.042)	[0.74]	[0.74]	
Years of schooling, age 20	15	−0.052	0.026	−0.092	0.039	−0.040	0.004	−0.12	−0.24	38,121
		(0.080)	(0.071)	(0.059)	(0.056)	(0.047)	(0.051)	[0.98]	[0.98]	
Completed grade 10, age 18	13	0.002	0.026	−0.036	−0.019	−0.019	−0.031	0.77	0.84	52,444
		(0.027)	(0.024)	(0.029)	(0.024)	(0.020)	(0.019)	[0.42]	[0.37]	
Completed grade 12, age 20	15	−0.041	0.051	−0.014	0.016	−0.032	−0.015	0.43	0.45	43,468
		(0.029)	(0.040)	(0.029)	(0.027)	(0.022)	(0.021)	[0.50]	[0.50]	
Attended college, age 20	15	−0.027*	−0.019	−0.002	−0.018	−0.022	−0.003	0.21	0.07	43,291
		(0.016)	(0.018)	(0.017)	(0.015)	(0.014)	(0.012)	[0.41]	[0.26]	

Notes: (see below)

Table A.17: (*continued*)

	$\alpha_{min} \leq \alpha \leq 15$			$16 \leq \alpha \leq 17$		$18 \leq \alpha \leq 24$		Mean		
	α_{min} (1)	Son (2)	Daughter (3)	Son (4)	Daughter (5)	Son (6)	Daughter (7)	Son (8)	Daughter (9)	N (10)
D. Crime outcomes										
Number of serious crimes, age 15–17	11	−0.116**	0.013	0.095**	0.015	−0.011	0.011	0.34	0.08	60,207
		(0.046)	(0.033)	(0.045)	(0.033)	(0.049)	(0.029)	[1.08]	[0.47]	
Number of serious crimes, age 18–20	14	0.034	−0.034	0.171*	0.022	−0.040	0.008	0.65	0.10	53,408
		(0.122)	(0.055)	(0.100)	(0.058)	(0.084)	(0.036)	[1.98]	[0.66]	
Incarceration sentence, age 18–20 (days)	14	1.826	0.265	6.245**	1.253	0.387	−0.434	3.79	0.10	53,408
		(2.756)	(0.663)	(2.782)	(1.234)	(1.935)	(0.420)	[41.24]	[4.68]	
E. Welfare program participation										
Duration on SA, age 18–20 (months)	14	−0.889**	−1.389***	0.043	−0.545*	0.261	0.100	2.56	3.38	52,803
		(0.333)	(0.324)	(0.317)	(0.314)	(0.289)	(0.307)	[6.33]	[7.71]	
Duration in SA work requirements, age 18–20 (months)	14	−0.285	−0.479***	0.298	−0.166	0.302*	0.198	1.24	1.35	52,803
		(0.203)	(0.169)	(0.219)	(0.161)	(0.178)	(0.149)	[3.70]	[3.77]	
Ever on SA, age 18–20	14	−0.070***	−0.073***	−0.021	−0.027	−0.003	0.023	0.25	0.27	52,803
		(0.025)	(0.026)	(0.019)	(0.021)	(0.017)	(0.018)	[0.43]	[0.44]	
Ever on DI, age 18–20	14	−0.001	0.008	0.002	−0.000	−0.001	0.008*	0.01	0.01	52,390
		(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.004)	[0.11]	[0.09]	
Ever on UI, age 18–20	14	−0.002	0.003	−0.002	0.004	−0.003	−0.002	0.03	0.02	52,410
		(0.007)	(0.013)	(0.008)	(0.008)	(0.006)	(0.006)	[0.17]	[0.15]	
UI fund membership, age 20	14	0.041	0.027	−0.008	−0.012	−0.041**	−0.025*	0.19	0.15	52,803
		(0.025)	(0.022)	(0.022)	(0.023)	(0.019)	(0.014)	[0.39]	[0.36]	

Notes: This table reports estimates (relative to starting age of treatment group $25 \leq \alpha \leq 30$) from a TD analog of the regression described in Footnote 50 for children born to mothers in the main sample who are ages ≤ 25 at the time of birth, by the mother's starting age of treatment group and child gender. Column (1) reports the smallest starting age available in the data for the youngest starting age of treatment group $\alpha_{min} \leq \alpha \leq 15$. Columns (2)–(7) report estimates for different starting age groups; even-numbered columns report estimates for sons and odd-numbered columns report estimates for daughters. Columns (8) and (9) report mean outcomes among high-risk individuals with starting ages $25 \leq \alpha \leq 30$ for sons and daughters, respectively; standard deviations are reported in brackets. Column (10) reports the sample size of the estimation samples. Standard errors clustered by birth quarter and municipality of residence in the age prior to treatment are reported in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Heterogeneous effects on human capital outcomes, by social network: Young adults on SA in neighborhood block

	Share $>$ median?	$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	N (5)
SA participation (months)	Yes	-0.196^{**}	-0.010	0.054	0.004	51,383
		(0.075)	(0.093)	(0.069)	(0.058)	
	No	0.147^{*}	0.168	-0.091	-0.081	8,184
		(0.077)	(0.111)	(0.080)	(0.077)	
Years of schooling	Yes	0.158^{***}	0.094^{**}	0.004	0.002	49,070
		(0.026)	(0.044)	(0.028)	(0.024)	
	No	0.016	-0.002	-0.002	-0.040^{*}	7,837
		(0.044)	(0.052)	(0.039)	(0.022)	
Market income (thousands \$)	Yes	1.342^{***}	0.572^{**}	0.025	0.175	50,124
		(0.212)	(0.258)	(0.185)	(0.415)	
	No	0.675^{*}	0.463	-0.097	0.146	7,982
		(0.342)	(0.458)	(0.357)	(0.501)	
Inpatient hospital care (days)	Yes	-0.079^{**}	-0.067	-0.042	0.082	48,173
		(0.037)	(0.050)	(0.038)	(0.150)	
	No	-0.053	-0.014	-0.003	0.056	7,555
		(0.077)	(0.123)	(0.078)	(0.129)	

Notes: Columns (1)–(4) report estimates of $\beta_\alpha^{Subgroup} \equiv \beta_\alpha + \beta_\alpha^S$ and β_α from Equation (4) aggregated by starting age group. Column (5) reports the number of high-risk individuals belonging to the subgroup of the included starting ages α. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses. Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.19: Heterogeneous effects on human capital outcomes, by social network: Older sibling

	Older sibling?	$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	N (5)
SA participation (months)	Yes	−0.081	0.150	0.061	0.021	36,502
		(0.073)	(0.096)	(0.073)	(0.063)	
	No	−0.133	0.122	0.087	−0.123	23,065
		(0.099)	(0.093)	(0.075)	(0.086)	
Years of schooling	Yes	0.138***	0.104**	−0.004	−0.019	34,925
		(0.029)	(0.041)	(0.025)	(0.022)	
	No	0.023	−0.057	−0.032	−0.038	21,982
		(0.037)	(0.054)	(0.039)	(0.048)	
Market income (thousands $)	Yes	1.183***	0.328	0.001	0.186	35,685
		(0.209)	(0.227)	(0.168)	(0.301)	
	No	0.874***	0.138	−0.272	0.024	22,421
		(0.254)	(0.328)	(0.221)	(0.327)	
Inpatient hospital care (days)	Yes	−0.109**	−0.063	−0.064	0.077	34,031
		(0.042)	(0.062)	(0.045)	(0.181)	
	No	−0.070	0.012	0.009	−0.026	21,697
		(0.051)	(0.067)	(0.053)	(0.121)	

Notes: See notes to Table A.18.
Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Additional Institutional Details

B.1 Overview of Changes to the Social Assistance Program

Table B.1 details the timing of legislation, passive benefit duration, severity of sanctions, and exemptions for certain parents.

Nature of work requirements. Work requirements were extremely heterogeneous in nature, depending on the individual needs of the SA recipient and the resources available at the municipality that governed the local program. Activities include job search, vocational training, short-term educational programs, apprenticeships, community service, or subsidized apprenticeships in the public or private sectors. Individual work programs could include combinations of these activities.

Figure B.1 illustrates the availability of activities provided by municipalities. The most popular programs offered included educational programs and job training through private or public apprenticeships. For young adults who have left high school, classroom instruction aimed to teach basic skills and provide direction in formulating future plans for a career; see Mik-Meyer (1999) for illuminating case studies. Job training included a wide spectrum of options. Documented examples include janitorial work, public park beautification, snow shoveling, daycare assistants, food-packing in factories, carpentry, and bricklaying.[1] Despite this large heterogeneity, the intensity of work requirements is remarkably consistent across municipalities and between different age groups—despite the national law's provision that allows municipal discretion of work requirements of older adults. See Figure B.2.

Other changes. The 1994 reform restructured benefits for both youth and adults. Housing benefits were formally separated from SA benefits but still counted toward the total benefit limit. Benefits became taxable, and benefit formulas were inflated to account for the average family's expected tax incidence. There were also some changes in the disbursement of child supplements for families with multiple children. However, Hansen and Schultz-Nielsen (2017) show these changes resulted in very little change in the net benefits received by the vast majority of SA-eligible households.

Two other changes affected eligibility for adult benefit rates over time. First, in 1995, the age threshold for the adult benefit rate was adjusted from age 23 to 25. Second, youth eligibility for the adult basic benefit rate gradually grew stricter over time. For example, cohabitation with a spouse was no longer a sufficient condition for an individual below the age threshold to be eligible for adult benefit rates. See Hansen and Schultz-Nielsen (2017) for more details.

The SA reforms also adapted guidelines on sanctions and exemptions for parents of young children who lacked daycare from work requirements over time. In general, these guidelines loosened over time; see the last two columns of Table B.1. In practice, however, many municipalities did not follow these guidelines and enacted stricter sanctions and some enforced work requirements to parents lacking daycare. Note that the extent that sanctions were loosened and exemptions were adopted would tend to attenuate the estimated effects of facing work requirements.

1. See Mik-Meyer (1999), Larsen (2005), Katznelson (2008), Andersen (2020), and Hansen and Nielsen (2021).

Table B.1: Details of Social Assistance reform work requirements, 1990–1998

Dates			Age	Work req's		Passive period (months)		Guidance	Renewal	Diff. for	Sanctions
Year	Announced	Enforced	group	Hr./wk.	Months	Initial	Pre-renewal	period	policy	parents	
1990	6/13/1990	7/1/1990[a]	18-19	20	5	≤0.5	N/A	≤1.8	None	—"—	DoM⋆
1991	6/6/1991	10/1/1991[b]	18-20	20	5	≤0.5	N/A	≤1.8	None	—"—	—"—
1992	6/24/1992	7/1/1992	18-24	20	5	≤0.5	≤3	≤1.8	Optional	—"—	—"—
1993	12/23/1992	1/1/1993[c]	18-20	20	5	≤0.5	≤3	≤1.8	Optional	—"—	—"—
			21-24	20	5	≤3	≤3	≤1.8	Optional	—"—	—"—
1994	6/30/1993	1/1/1994[d]	18-24	20	6	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	If reject activ. offer, repay benefits; if accept activ. offer, benefits reduced by % absence[e]
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—
1995	12/21/1994	7/1/1995[c]	18-24	≥30	6	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	If reject activ. offer, repay benefits; if accept activ. offer, benefits reduced by % absence up to max. 20%[e]
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—
1996	12/20/1995	4/1/1996[c]	18-24	≥30	6 or 18[f]	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	—"—
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—

Notes: (see below)

Table B.1: (*continued*)

Dates			Age	Work req's		Passive period (months)		Guidance	Renewal	Diff. for	Sanctions
Year	Announced	Enforced	group	Hr./wk.	Months	Initial	Pre-renewal	period	policy	parents	
1998	6/10/1997	7/1/1998[c]	18-29	$\geq$30	6 or 18[f]	$\leq$3	$\leq$3	0	Required	Activation not required if lack childcare	If reject activ. offer, benefits ceased; if accept activ. offer, benefits reduced by % absence up to max. 20%[e]
			$\geq$30	DoM*	DoM*	$\leq$12	N/A	0	None	—"—	—"—

Notes: This table summarizes baselines rules from national reforms to the Social Assistance Act, Municipal Activation Act, and Active Social Policy Act (see retsinformation.dk for the original legislation). The table indicates whenever the national legislation explicitly states that a particular policy rules are left up to the discretion of municipalities (DoM). However, municipalities were granted discretion to implement more extreme rules in activation requirements, passive periods, renewal policies, and sanctions at potentially earlier dates. All dates expressed in MM/DD/YYYY format.

* DoM = policy left up to the discretion of municipalities.

[a] Those already on assistance must be activated before they turn 20 or before 1/1/1991.

[b] Those already on assistance must be activated before they turn 21 or before 1/4/1992.

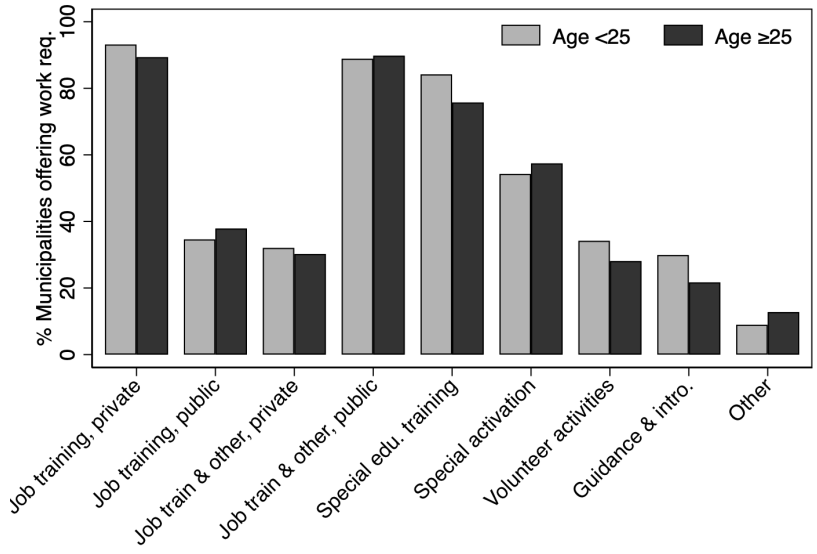
[c] Only applies to those who apply for assistance after start of the law.

[d] Those already on assistance and over age 25 must be activated by 1/1/1995.

[e] Recipients cannot be denied benefits for refusing an activation offer to manufacture munitions.

[f] Recipients whose education level entitles them to membership of an unemployment fund (i.e., vocational credentials) have a 6 month activation period; others 18 months.

Figure B.1: Types of work requirements offered by municipalities

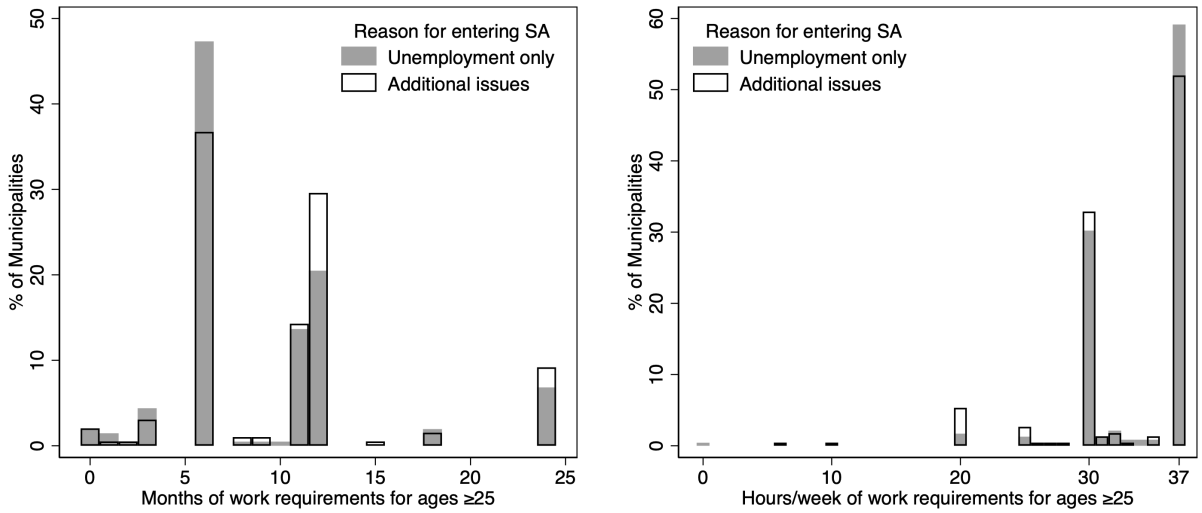


Notes: Data from Questionnaire to Municipalities, *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives.

Figure B.2: Distribution of work requirements for ages 25 or above across municipalities

(a) Months

(b) Hours per week



Notes: Data from Questionnaire to Municipalities, *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives.

B.2 Comparison with U.S. Aid to Families with Dependent Children

Table B.2: Comparison of Denmark's Social Assistance (SA) program and the U.S. Aid to Families with Dependent Children (AFDC) program

	Denmark SA	U.S. AFDC
Target population	Disadvantaged adults (age $\geq$18) (childless and parents)	Disadvantaged single parents (AFDC-UP: 2-parent households)
Eligibility criteria	1. Experienced "social event" 2. Incapable of self-sufficiency 3. Exhausted work opportunities	1. Parent of child age $<$18 2. Earnings $<$ state threshold
Max. benefit	\$1,038/month (age $\geq$ 25 or parent, married, or sufficient prev. income) \$610/month (age $<$ 25 and childless) + housing benefit, individual benefit	\$609/month (single, 2 children)
Asset limit	\$550 (excl. savings for edu, home)	\$1,500 (excl. value of home)
Marginal tax rate	100%	67% (first 4 months) 100% (after 4 months)
Time limits	None	None

Notes: Based on 1993–4 data; monetary values expressed in 2010 U.S. dollars. U.S. benefit levels are averages across states reported in Crouse (1995).

C Data Appendix

C.1 Overview

The administrative register data is provided by Statistics Denmark. Most registers are available from 1980 to 2018, though some begin and end at different years. Section C.2 describes how the main sample is constructed. Section C.3 defines baseline characteristics used to define the main sample and as control variables in my empirical analysis. Sections C.4 and C.5 describe how geographic identifiers and SA participation are defined over time.

C.2 Sampling Scheme

I first select individuals born between 1963 and 1981. I exclude first- or second-generation immigrants from my sample to avoid compositional changes across birth cohorts. This population is comprised of 1.35 million individuals.

Next, I measure the individual's mean household income measured between ages 12–17 and mother's education level by age 17; see Section C.3 for precise definitions. I keep observations whose mean household income falls below the national median (weighted across birth cohorts) and whose mothers have no more than 12 years of schooling (equivalent to a high school education). I also exclude observations with any missing background characteristics. This leaves me with a *base sample* of approximately 429,000 individuals.

Finally, I define risk groups based on two characteristics (precisely defined in Section C.3): (1) growing up in a household that participated in SA and (2) living in a neighborhood with above-median SA participation. The high-risk group ($G_i = 1$) satisfies both criteria; the low-risk group ($G_i = 0$) satisfies neither. My main sample is comprised of only high-risk and low-risk individuals; individuals that satisfy only one criterion are excluded from my main sample. This amounts to a *main sample* of about 230,000 individuals, of which 60,000 belong to the high-risk group.

C.3 Background Characteristics

Individual household identifiers are provided by the Population (*Befolkningen*; BEF) registers. (I specifically use the variable "CNR" for identifiers.) These identifiers allow me to obtain data among all who cohabit with each individual in my main sample at each available year.

- *Female* — dummy indicating whether an individual's gender at birth is female. Available in the Households and Families (*Husstande og Familier*, FAIN) registers.
- *Household market income* — the mean of household's total gross market income (before transfers or taxes) of up to three most recent years available between ages 12–17 of the individual and between 1980–1993. Available in the Income (*Indkomst*; IND) registers.
- *Household net assets* — the mean of household's total net assets of up to three most recent years available between ages 12–17 of the individual and between 1980–1993. Available in IND.

- *Household SA participation* — an indicator of whether any of the individual's older household members (age 18 or above) who received SA benefits between ages 12–17 of the individual and between 1980–1993. I exclude individuals in the main sample from consideration to ensure that this baseline measure is not influenced by young adults affected by the 1990–1992 reforms. Available in IND.

- *Mother's education, age 17* — highest years of completed schooling of birth mothers by the time individuals turn age 17. Since the registers are only available beginning in 1981, I use the mother's years of schooling when the 1963 turns age 18.[2] Available in the Education (*Uddannelseregister*, UDDANY) registers.

- *Number of children, age 17* — the number of children below the age of 18 that cohabit with individuals at age 17. Available in FAIN.

- *Single–parent household* — an indicator of whether an individual lives in a household with only one parent (who could be a birth, step, or adopted parent), measured at the highest age available between ages 12–17 of the individual and between 1980–1993. Available in FAIN.

- *Married household* — an indicator of whether an individual lives in a household where the parent (who could be a birth, step, or adopted parent) is married, measured at the highest age available between ages 12–17 of the individual and between 1980–1993. In nearly all cases, the spouse also cohabits with the individual. Available in FAIN.

- *Mean parish income, age 15–17* — I compute within-parish mean household-level total gross income excluding transfers by parish and year from 1980 to 1998 using the IND registers. Households are defined as individuals living in the same household, including married couples, cohabiting couples, registered partnerships, and children under the age of 18 who are not parents or in a cohabiting relationship.[3] Averages ignore cases missing parishes (which are more common in 1980 and 1981 due to missing parish assignments, as described in Section C.4 below). Since data is only available beginning in 1980, averages are limited to between age 17 and ages 16–17 for the the 1963 and 1964 birth cohorts, respectively. The share of 1963–1968 birth cohorts who move to different parishes between ages 15–17 is extremely small.

- *High-SA neighborhood block* — an indicator of whether an individual's modal residential neighborhood block (comprising of approximately 150 households) had a historical SA participation rate among adults aged 18–60 (averaged measured between 1980–1993) above the median within mean income and urbanicity bins. Neighborhood block identifiers are constructed by Damm and Schultz-Nielsen (2008), which are retroactively matched to 1980–1984 housing registers; see Section C.4. Mean income is measured as the mean household income within the neighborhood between 1980–1993, which are partitioned into $1,000 bins (while neighborhoods at the extreme tails are grouped with their nearest bin); data available in IND. Urbanicity is defined as whether the majority share of household addresses belonging to a neighborhood block belongs to a municipality that is urban ($\geq$50,000 residents),

2. Since nearly all mothers are between 30–60 years old by the time the main sample turns 17, there are little differences in measuring mother's education when the sample turns 18.

3. This is the variable "C_FAMILIE_ID" in the FAIN register dataset.

suburban (1,000–49,999 residents), or rural (<1,000 residents); data available in the Housing Census (*Boligforhold*) register of 1992.

C.4 Geographic Identifiers

I use municipality codes in 2003 to maintain consistency in the geographic boundaries of municipalities over time.[4] Municipality of residence of each residential address are available beginning in 1980 through the Households and Families registers (*Husstande og Gamilier*, FAIN).

While each individual's residential addresses are available beginning in 1980, parish codes are matched with addresses through the Housing Census registers (*Boligtælling*, BOL) beginning in 1982. In an effort to identify as many addresses to parish in 1980 and 1981, I compile the universe of over 3 million addresses matched with parish codes from the 1982–2004 Housing Census registers.[5] I use parish codes in 2007 to maintain consistency in the geographic boundaries of parishes over time. I then retroactively match missing parishes to the 1980 and 1981 addresses that also exist in the 1982–2004 universe. I apply a similar process with neighborhood block identifiers, which are available starting only in 1985.

C.5 Measuring SA Participation over Time

Data available over time. I primarily use the Cash Assistance register (*Kontanthjælp*, KTHJ) to measure duration of SA participation at each age. There are a number of alternative registers, but each has major drawbacks. Figure C.1 summarizes the availability and quality of these registers. To describe their drawbacks:

- The Income register (*Indkomst*, IND) provides annual SA benefit levels each calendar year. Before 1994, IND provides only a single measure of SA participation that does not distinguish between passive and active benefits. Measuring duration of SA participation during the calendar year can only be approximated using SA benefit formulas; measuring participation by ages requires more approximations.

- The Coherent Social Statistics register (*Sammenhængende Socialstatistik*, SHSS) cannot measure passive vs. active between 1990 and 1993 is that the registers combine passive benefit payments under Section 37 of the Development Assistance Act with active benefit payments under Section 49a (the "youth benefit scheme") of the Development Assistance Act. Moreover, measures of activation benefits from voluntary training and education programs (Section 42 of the Development Assistance Act) are replaced with individuals participating in rehabilitation programs beginning in 1990.

- The Labor Market Measures register (*Arbejdsmarkedsforanstaltninger*, AMFO) only provides information on participation in activation programs, not passive SA benefit receipt.

- The DREAM register (maintained by the Ministry of Employment) provides data on participation of a single social benefit/insurance program per week; many programs are prioritized

4. After 2003, there are a few changes in the organization of municipal boundaries, followed by the 2007 Municipal Reform, which reduced the number of municipalities from 275 to 98.

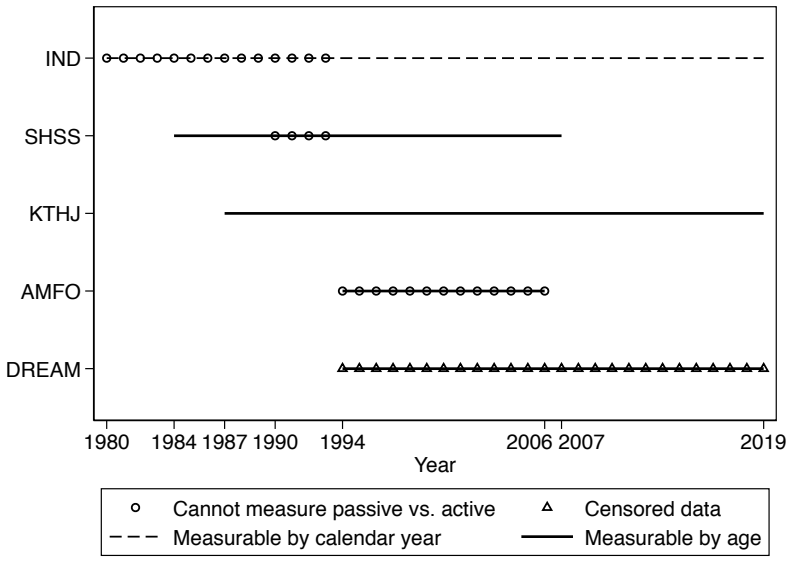
5. Addresses with missing municipalities are removed, since addresses are only unique up to the municipal level.

above SA, leading to censored measures of SA participation. Thus, measuring SA participation in DREAM will be inaccurate for individuals who participate in SA and other transfer programs simultaneously.

The earliest year that SA participation can be measured by KTHJ is 1987. As a result, between 1984–1986, I use SHSS, which is of comparable quality to KTHJ during this time period; between 1980–1983, I use IND, since it is the only register dataset available during this time period that provides some measure of SA participation.

Imputation for 1980–1983 data. I impute the duration of SA participation at ages between 1980–1983 using annual SA benefit data from the IND register. I approximate the number of months of SA participation in each calendar year using the maximum monthly SA benefit using SA benefit rules, which depend on individual demographic characteristics including age, marital status, household structure, and dependent children; see Hansen and Schultz-Nielsen (2017) for more details. For the calendar year where an individual turns 18 years old, I set the maximum possible number of months of SA participation to the number of months that an individual is age 18 during the calendar year; for other ages, I set the maximum possible number of months to 12.I further adjust this approximation in 1983 based on monthly participation data from SHSS for the observed fraction of age in 1984. I then assign individuals to the calendar year approximations based on the age they turn in the calendar year.

Figure C.1: Availability and quality of registers measuring SA participation



D Additional Details and Results of Robustness Analyses

D.1 Difference-in-Differences Exercises

I employ the following difference-in-differences (DD) event-study specification that exploits variation between risk groups and starting age cohorts, holding the outcome age a fixed:

$$Y_{ia} = \sum_{8 \leq \alpha \leq 30,\ \alpha \neq a+1} \beta_{\alpha,a} \left(G_i \times \mathbb{1}(D(b_i) = \alpha) \right) + \delta_{1a} G_i + \delta_{b(i),a} + \boldsymbol{\gamma}_a \boldsymbol{X}_i + \boldsymbol{\eta}_a \boldsymbol{X}_i \times b_i + \theta_{m_{D(b_i)-1}(i),a} + \varepsilon_{ia}.$$

This DD event study is thus an alternative method for identifying the starting age-outcome age effects $\beta_{\alpha,a}$. Figures associated with this DD regression plot estimates of the event-study parameters $\beta_{\alpha,a}$ grouped in consecutive pairs of starting ages α. This grouping is to gain greater precision; ungrouped estimates gives qualitatively similar but noisier results. Note that pre-trend tests are available for starting ages $\alpha \geq a + 1$.

D.1.1 Robustness Checks of Triple Difference Estimates

Figures D.1–D.3 plot the DD event-study estimates of $\beta_{\alpha,a}$ for market income, years of schooling, and incarceration sentence for outcome ages $a \in \{20, 22, 24, 26, 28\}$. Note that estimates are arranged in reverse order relative to conventional event-study plots so that pre-trends consist of the *right-most* estimates.

The estimates are broadly consistent with the heterogeneous patterns found in the main TD estimates. Whenever pre-trends are not flat, estimates tend to show trend breaks. Figure D.2, Panels (a)–(d) additionally report results on school enrollment for outcome ages $16 \leq a \leq 19$ to focus on behavioral responses of human capital investment among younger starting age cohorts.

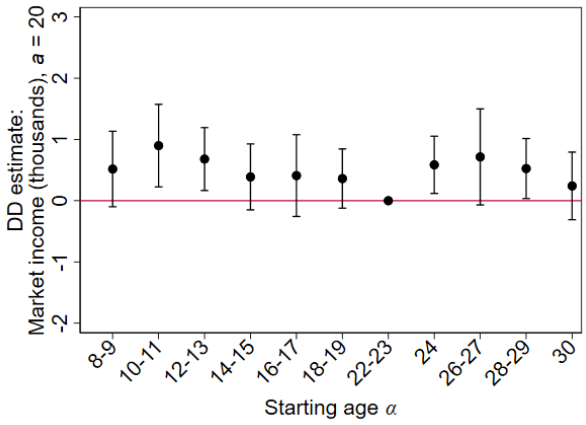
D.1.2 Pre-Treatment Outcomes

As a falsification exercise, I estimate starting age effects on two "pre-treatment outcomes" that should not be affected by the SA reforms: birth weight and inpatient hospital care measured at outcome age 7. I focus on outcomes measured at early life since these cohorts tend to lack pre-trends for my main outcomes. These outcomes are only observed among earlier starting age cohorts, so my sample is limited to starting age cohorts $8 \leq \alpha \leq 17$ and $8 \leq \alpha \leq 22$, respectively.

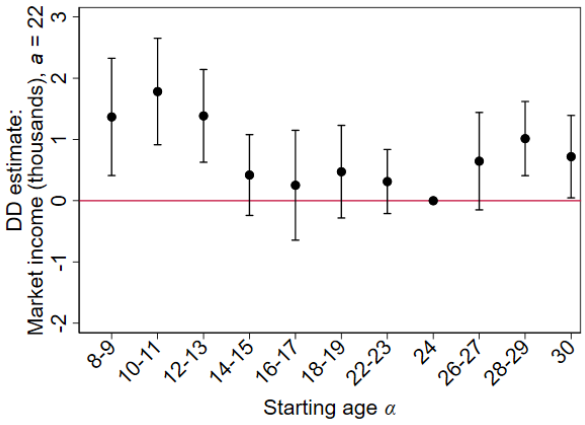
Figure D.4 plots the DD event-study estimates and confirms that nearly all estimates are statistically insignificant at conventional levels. Table D.1 reports joint tests of equality of the DD event-study parameters. This provides a formal check of whether there is pre-existing starting age heterogeneity unrelated to the SA reforms that may confound my identification strategy. For both pre-treatment outcomes, I fail to reject the null that all event-study parameters are equal to one another.

Figure D.1: Robustness check: Difference-in-difference event study on market income measured by outcome age

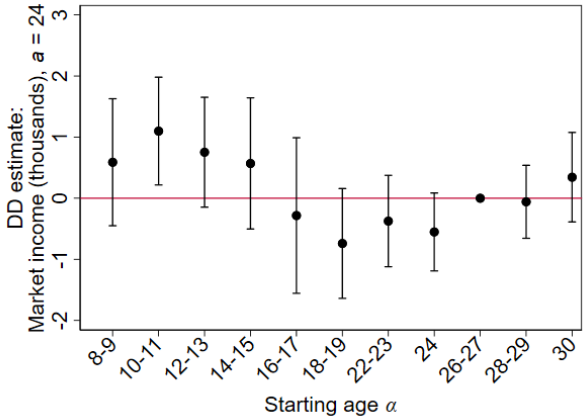
(a) Outcome age $a = 20$



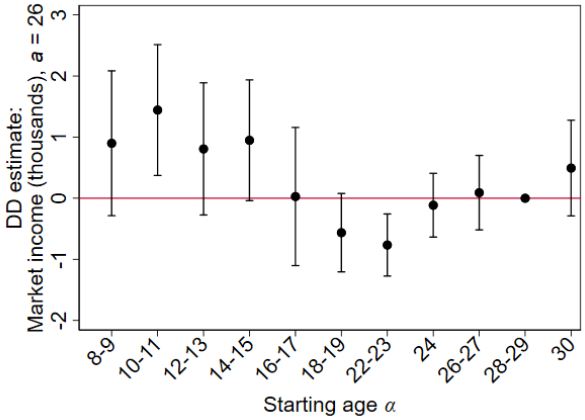
(b) Outcome age $a = 22$



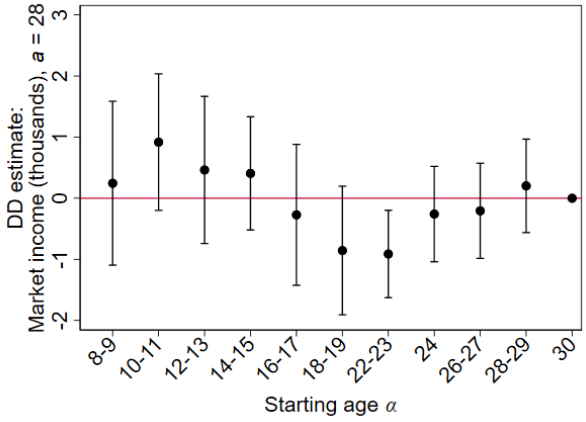
(c) Outcome age $a = 24$



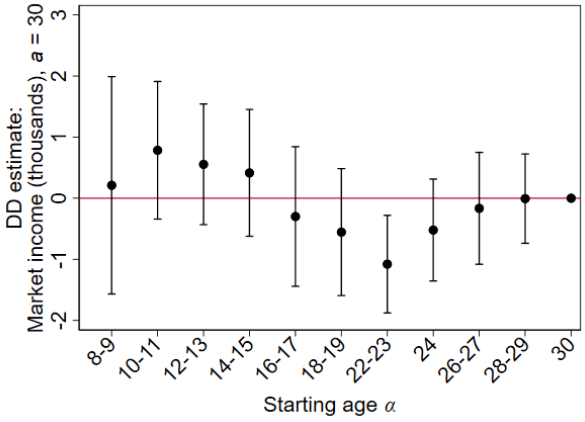
(d) Outcome age $a = 26$



(e) Outcome age $a = 28$



(f) Outcome age $a = 30$

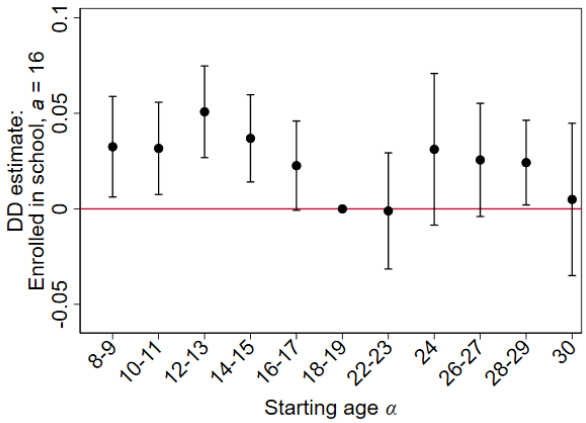


Notes: These figures plot estimates of $\beta_{\alpha,a}$ (binned by consecutive starting ages α, whenever appropriate) from the difference-in-difference event-study model described in Appendix D.1. Vertical bars are 95% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities.

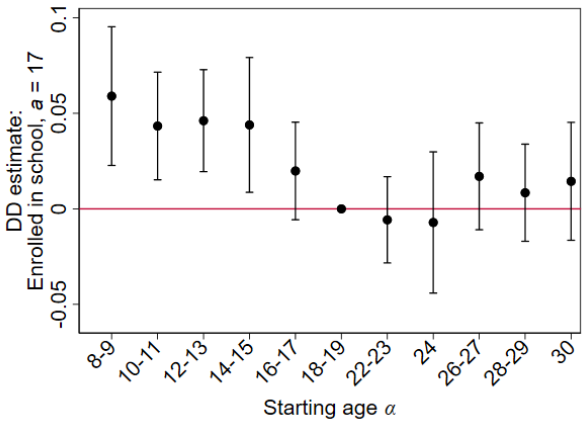
Figure D.2: Robustness check: Difference-in-difference event study
on education outcomes measured by outcome age

Enrolled in school

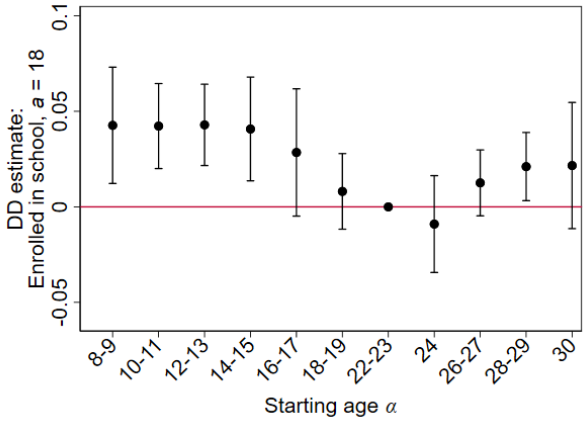
(a) Outcome age $a = 16$



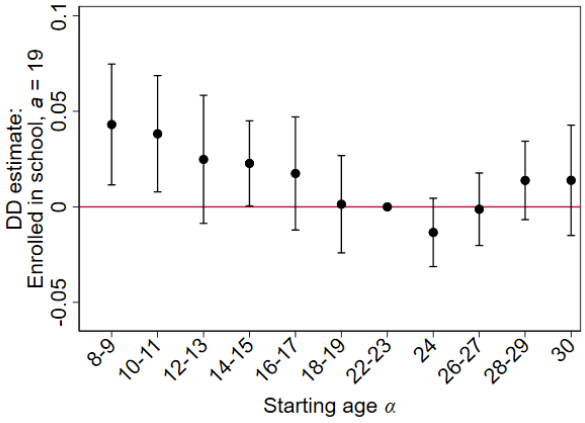
(b) Outcome age $a = 17$



(c) Outcome age $a = 18$



(d) Outcome age $a = 19$

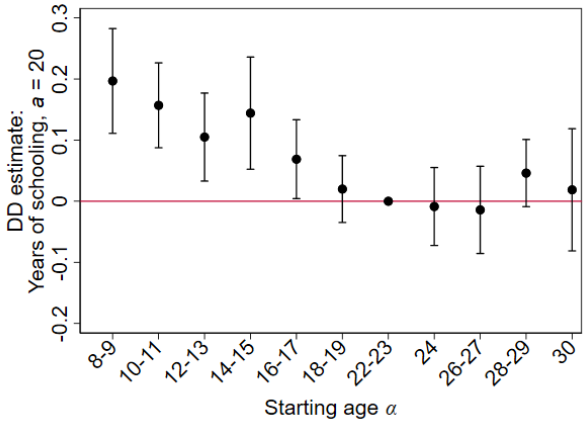


Notes: (see next page)

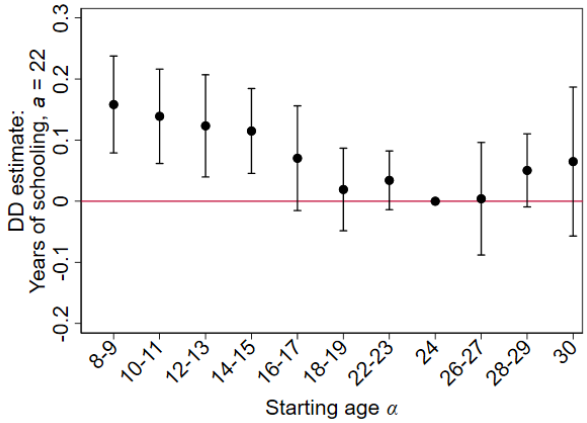
Figure D.2: (cont'd.)

Years of schooling

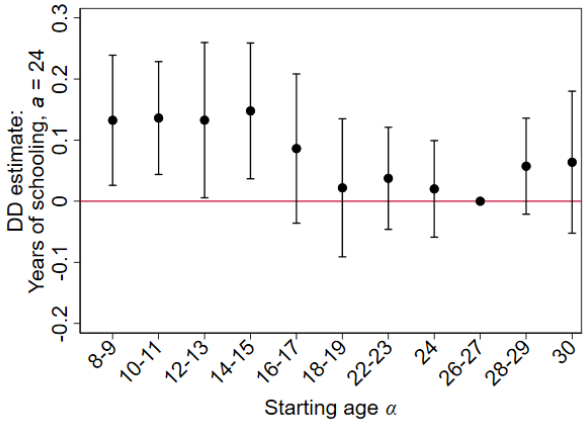
(e) Outcome age $a = 20$



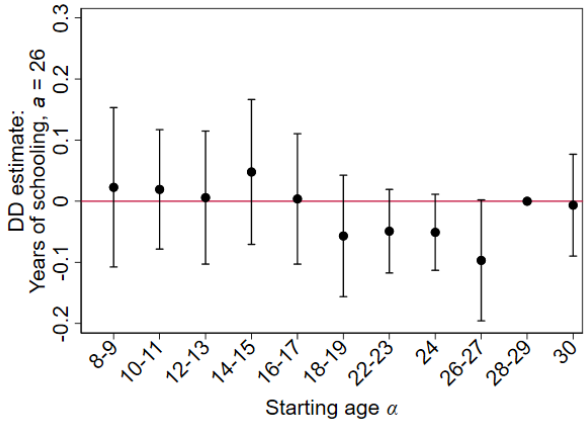
(f) Outcome age $a = 22$



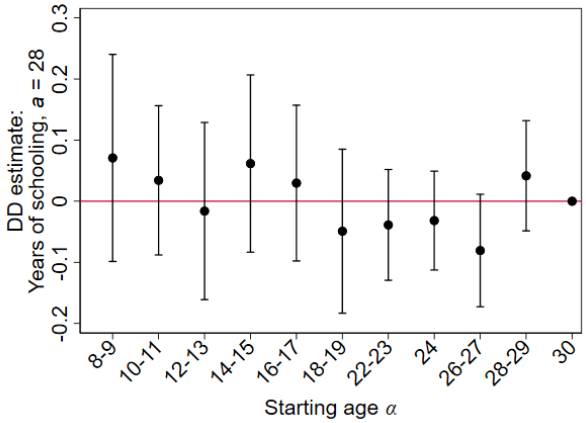
(g) Outcome age $a = 24$



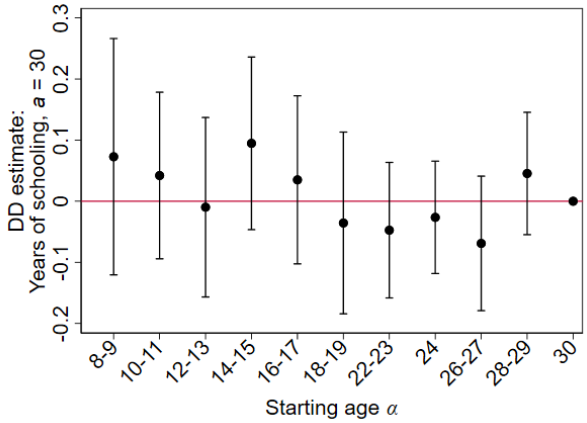
(h) Outcome age $a = 26$



(i) Outcome age $a = 28$



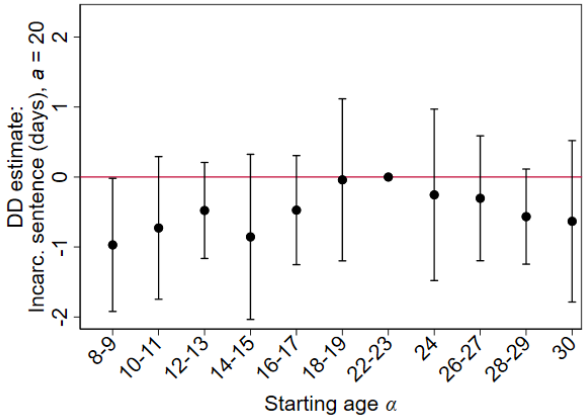
(j) Outcome age $a = 30$



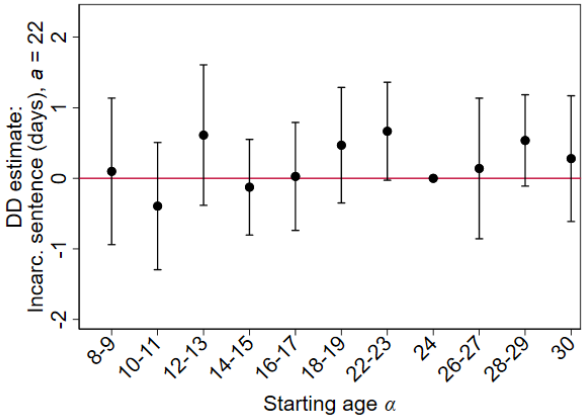
Notes: See notes of Figure D.1 for details.

Figure D.3: Robustness check: Difference-in-difference event study on incarceration sentence measured by outcome age

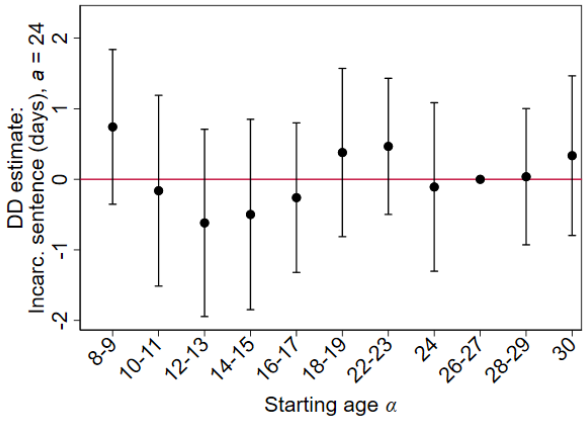
(a) Outcome age $a = 20$



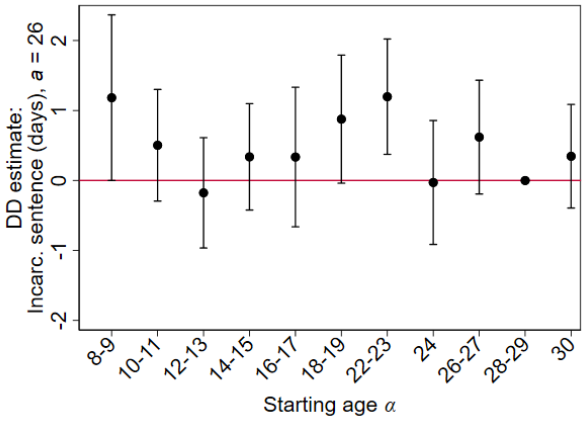
(b) Outcome age $a = 22$



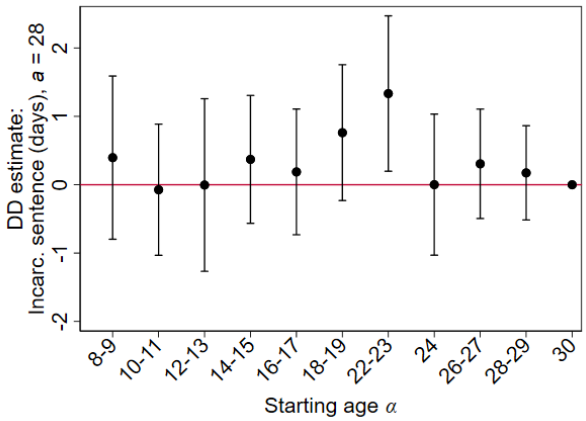
(c) Outcome age $a = 24$



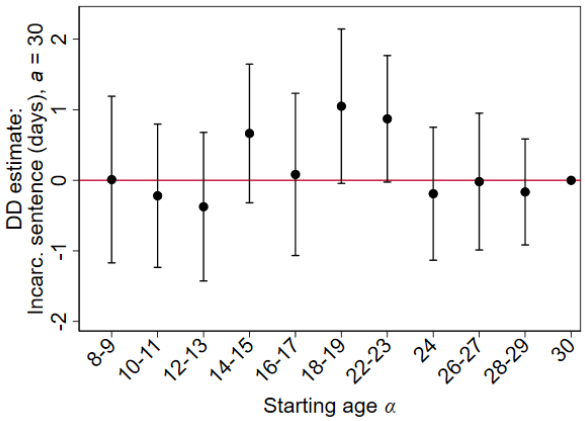
(d) Outcome age $a = 26$



(e) Outcome age $a = 28$



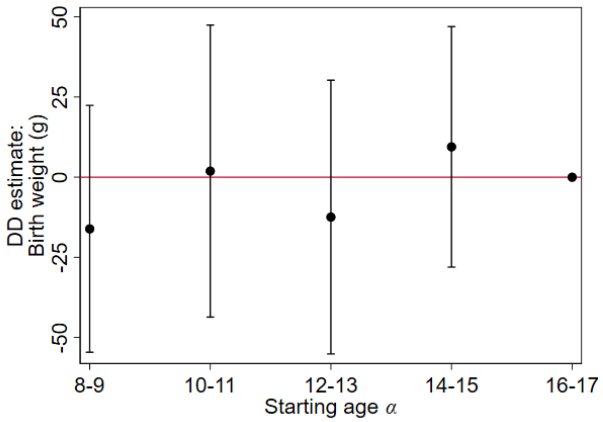
(f) Outcome age $a = 30$



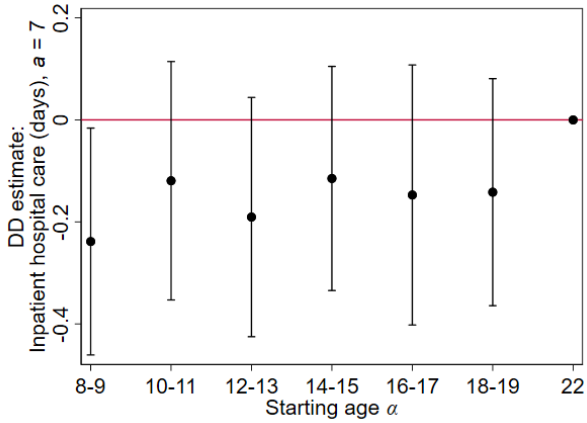
Notes: See notes of Figure D.1 for details.

Figure D.4: Falsification tests: Difference-in-difference event study estimates on pre-treatment outcomes

(a) Birthweight



(b) Inpatient hospital care, outcome age $a = 7$



Notes: These figures plot estimates of $\beta_{\alpha,a}$ (binned by consecutive starting ages α, whenever appropriate) from the difference-in-difference event-study model described in Appendix D.1. Vertical bars are 95% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities.

Table D.1: Falsification tests: Joint test of equality of difference-in-difference event-study parameters on pre-treatment outcomes

	Starting ages α included in sample	Wald test statistic (degrees of freedom)	p-value	Test result
	(1)	(2)	(3)	(4)
Birthweight	8–17	3.05 (4)	0.549	Fail to reject
Inpatient hospital care, $a = 7$	8–22	8.55 (6)	0.201	Fail to reject

Notes: This table reports Wald tests with d degrees of freedom on the joint null hypothesis that the difference-in-difference event-study parameters $\beta_{\alpha,a}$ (binned by consecutive starting ages α, whenever appropriate) on pre-treatment outcomes are equal: $\beta_\alpha = \beta_{\alpha'}, \forall \alpha \neq \alpha'$. Column (2) reports the range of starting ages α included in the sample based on availability of data. Degrees of freedom d are the number of parameters included in the test and are reported in parentheses in Column (3). Columns (4) and (5) report the test results.

D.2 Checking for Confounding Changes in the Economy

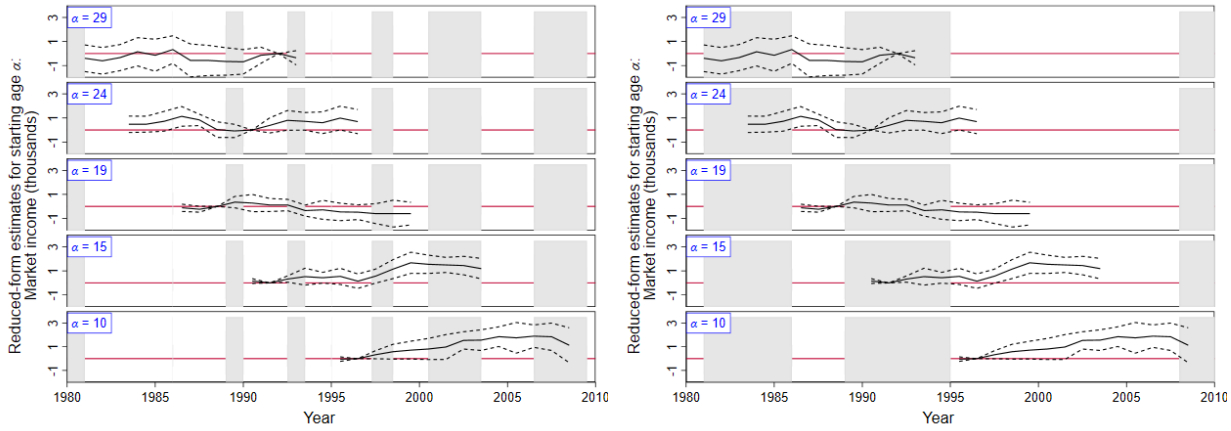
D.2.1 Business Cycles

To check if high- and low-risk groups belonging to the same starting age cohort were differentially exposed to business cycles in ways that affected their life-cycle outcomes, I correlate business cycle measures with estimated starting age-outcome age effects of the appropriate calendar year. Let $t^*(\alpha) \equiv 1990 \cdot \mathbb{1}\{\alpha \leq 19\} + 1992 \cdot \mathbb{1}\{22 \leq \alpha \leq 24\} + 1994 \cdot \mathbb{1}\{26 \leq \alpha \leq 29\}$ indicate the earliest calendar year that starting age α was exposed to SA reform. Thus, $\beta_{\alpha,t-t^*(\alpha)+a}$ is the starting age-outcome age effect of α in calendar year t. Figure D.5 plots $\beta_{\alpha,t-t^*(\alpha)+a}$ over calendar year t for select α. Table D.2 reports correlations between $\beta_{\alpha,t-t^*(\alpha)+a}$ and calendar time business cycle measures. Overall, estimates of $\beta_{\alpha,a}$ are only weakly correlated (< 0.10) with business cycles; in fact, estimated effects on income are weakly *counter-cyclical*, contrary to expectation.

Figure D.5: Business cycles vs. effects on market income over calendar time

(a) OECD recession indicators

(b) High unemployment indicators



Notes: This figure plots starting age-outcome age effect estimates $\beta_{\alpha,a}$ of various starting age cohorts over calendar time. Shaded areas indicate different measures of business cycles: the right panels use dates of recessions defined by the OECD and the left panels use dates where the national unemployment rate exceeded 7%. National unemployment rate is defined as the percent of adults between ages 18–60 that had zero market income reported during the calendar year. Dashed lines are 95% confidence intervals based on robust standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform.

Table D.2: Business cycles do not explain starting age-outcome age heterogeneity

	OECD recession indicator	National unemployment rate
Market income	0.088	0.099
Years of schooling	0.074	0.097
Incarceration sentence	0.017	0.090

Notes: This table reports correlation coefficients between the starting age-outcome age estimates $\hat{\beta}_{\alpha,a}$ from Equation (3) and variables capturing business cycles. The OECD recession indicators are shown in the gray regions of Figure D.5, Panel (a). National unemployment rate is defined as the percent of adults between ages 18–60 that had zero market income reported during the calendar year.

D.2.2 Structural Economic Changes

To check if high- and low-risk groups belonging to the same starting age cohort were differentially exposed to structural economic changes in ways that affected their life-cycle outcomes, I check whether my estimates vary between areas with historically different labor market structures. Specifically, I estimate Equation (4) where $Subgroup_i$ indicates whether an individual's municipality had an above-median share of manufacturing sector in 1980. These historical municipal manufacturing shares predict local labor market's exposure to skill-biased technical change. The key parameter of interest here is β_α^S, the estimated difference in welfare-to-work's effect between municipalities with high versus low manufacturing shares. Table D.3 reports these estimates by starting age group and finds no statistically significant differences.

Table D.3: Heterogeneous effects by historical manufacturing sector size

	Manufacturing sector size	$8 \leq \alpha \leq 15$ (1)	$16 \leq \alpha \leq 17$ (2)	$18 \leq \alpha \leq 24$ (3)	$25 \leq \alpha \leq 29$ (4)	N (5)
Market income (thousands $)	High	1.327***	0.510	0.055	0.013	24,266
		(0.252)	(0.337)	(0.235)	(0.252)	
	Low	0.934***	0.257	−0.080	0.282	31,055
		(0.240)	(0.269)	(0.218)	(0.247)	
	Difference	0.393	0.253	0.134	−0.269	
		(0.288)	(0.345)	(0.341)	(0.377)	
Years of schooling	High	0.061*	0.007	0.012	−0.006	23,816
		(0.032)	(0.051)	(0.037)	(0.031)	
	Low	0.107***	0.053	−0.018	−0.035	30,385
		(0.026)	(0.045)	(0.025)	(0.024)	
	Difference	−0.046	−0.046	0.030	0.029	
		(0.039)	(0.064)	(0.043)	(0.042)	
Incarceration sentence (days)	High	0.210	−0.131	0.421	0.151	25,154
		(0.264)	(0.322)	(0.333)	(0.321)	
	Low	−0.127	0.170	0.396*	0.312	31,577
		(0.234)	(0.225)	(0.233)	(0.233)	
	Difference	0.337	−0.300	0.025	−0.162	
		(0.382)	(0.401)	(0.388)	(0.359)	

Notes: This table reports estimates of $\beta_\alpha^{Subgroup} \equiv \beta_\alpha + \beta_\alpha^S$ ("High"), β_α ("Low"), and β_α^S ("Difference") from Equation (4) where $Subgroup_i$ indicates residing in a municipality with a historically high manufacturing sector size. High manufacturing sector size is defined by municipality possessing above-median (0.158) employment share among adults ages 18–60 in 1980. Column (5) reports the sample size of each high-risk subgroup. Shaded cells highlight each outcome's primary starting age group used in the main robustness checks. Standard errors clustered by birth quarter and municipality of residence in the age prior to treatment are reported in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3 Validation Exercises

D.3.1 The 1994 Unemployment Insurance Reform

Figure D.6 plots all falsification tests described in the main text in Rows 2–6 for all starting age groups. The sixth and seventh rows plot results from two additional validation checks.

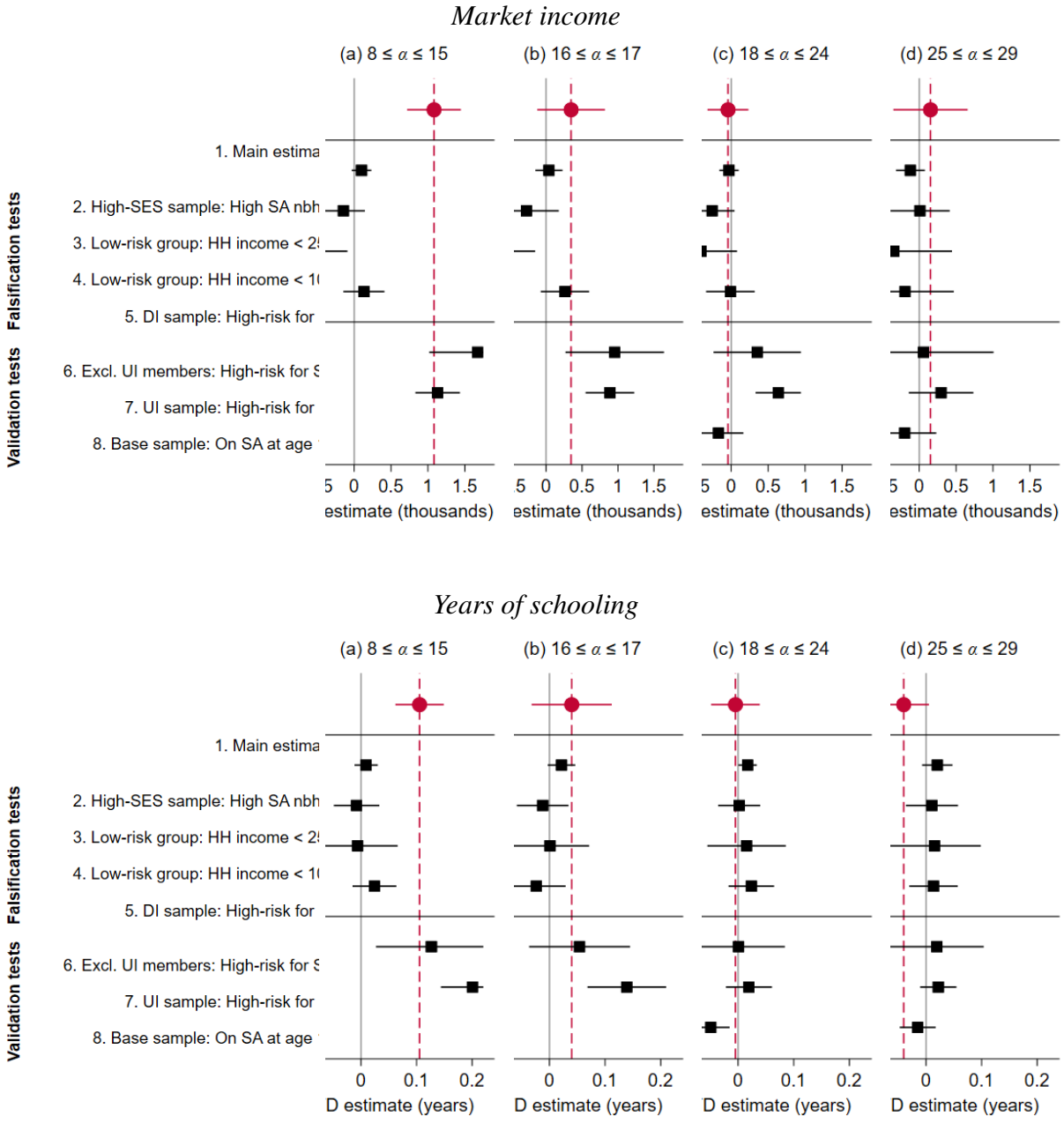
The sixth row checks whether the presence of a 1994 UI reform is responsible for my results. This UI reform introduced work requirements to individuals who received UI benefits for four years. Recall that, in most cases, UI members do not participate in SA, so the SA and UI reforms in principle target different populations. To reduce the risk of my sample being affected by the UI reform, I restrict my main sample to individuals from households where at least 50% of cohabiting adults lack UI membership. Due to their limited exposure to the UI system, these individuals are 20% (12.4 percentage points) less likely to be UI members by age 24 compared to those from UI member households. In this restricted sample, I find qualitatively similar effects across all starting age groups. If anything, estimates for starting ages $\alpha < 18$ grow in magnitude; starting ages $\alpha \geq 18$ remain null.

I also estimate the effects of the 1994 UI reforms, which are plotted in the seventh row. This serves as another way to validate my theory of starting age heterogeneity being driven by anticipatory responses to policy reforms. UI fund membership typically begins after age 21 since membership requires vocational certification and work experience; thus, starting age cohorts $\alpha \leq 21$ could anticipate facing work requirements in the future which could induce anticipatory behavioral response consistent with evidence on "threat effects" of UI work requirements (Rosholm and Svarer 2008). In this exercise, my sample consists of households that did not participate in SA but are UI members, and I estimate effects for individuals from households that received UI benefits and, thus, are more prone to being affected by the UI reform. Reassuringly, Figure 2 and Appendix Figure A.8 show that estimated effects of the UI reform follow qualitatively different starting age heterogeneity than my estimated effects of the SA reform. In contrast to my main estimated effects of the SA reform, the UI reform appears to generate statistically significant increases in market income and years of schooling up to $\alpha \leq 24$ and null effects on incarceration sentence for $\alpha \geq 19$. In this way, this exercise verifies that the null or negative effects of the SA reforms for starting ages $18 \leq \alpha \leq 24$ are meaningful and not driven by confounders.

D.3.2 Defining Risk Groups with SA Participation

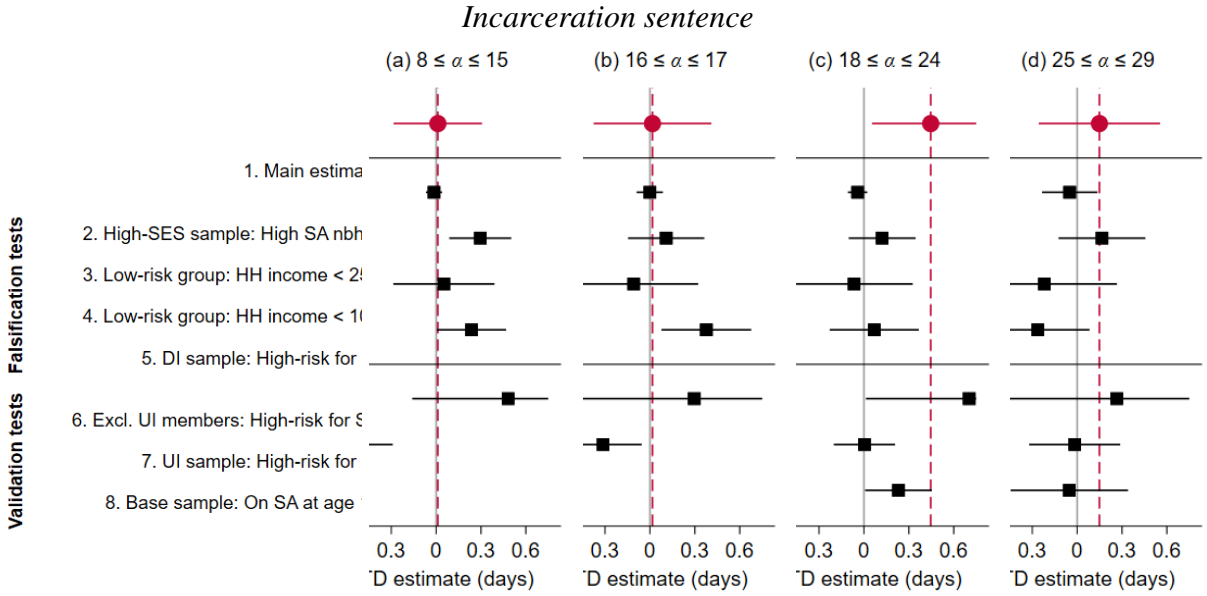
The final rows of Figure D.6 plot estimates from specifications where I use participation in SA at age 18 instead of risk groups for $\alpha \geq 19$. Just like my main estimates, I find largely null effects for these starting age cohorts, demonstrating that my results are not driven by ill-defined risk groups.

Figure D.6: Falsification and validation tests of effects, by starting age group



Notes: (see next page)

Figure D.6: (cont'd.)



Notes: This figure plots estimates of β_α from variations of the triple differences specification, aggregated by starting age group. See notes to Figure 4 for descriptions of Rows 1–5. Rows 6–8 are validation tests. Row 6 restricts the main sample to individuals whose households are not unemployment insurance (UI) fund members (which is defined as at least 50% of working-age cohabiting adults in the household between ages 12–17 are UI fund members). Row 7 uses the base sample and defines the treatment group as UI fund member households that received UI benefits but not SA benefits and the same control group as Row 6. Row 8 uses the base sample and defines treatment and control groups as whether an individual participated in SA at age 18; estimates are missing for age groups $8 \leq \alpha \leq 15$ and $16 \leq \alpha \leq 17$ due to endogeneity. The vertical dashed line signifies the magnitude of Row 1's point estimate. Horizontal lines are 95% confidence intervals based on robust standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform; some intervals are truncated to maintain a reasonable scale.

D.4 Timing of Municipal Adoption of the SA Reforms

The main results were based on the timing of national SA reforms. In reality, there was heterogeneity in the timing that work requirements were adopted across municipalities. Some municipalities adopted work requirements for their SA recipients before the July 1990 reform while others delayed adoption by a number of years.[6] As a validation check, I compare effects of individuals belonging to the same birth cohort who live in municipalities that adopted work requirements in different years. For example, if an individual assigned to starting age cohort α (according to the national reforms) lived in a municipality that actually adopted the reform starting at age $\alpha+2$, then she should experience the same effects as an individual assigned to starting age cohort $\alpha+2$ who lived in a municipality that promptly adopted the reform.

This validation exercise proceeds in three steps. First, I develop a procedure to estimate each municipality's adoption of SA work requirements. Second, I re-estimate effects across starting ages by municipality adoption groups through a quadruple difference regression model. Third, I fit parametric models of the effect estimate over starting ages to better discern differences in the patterns of effect estimates between the adoption groups.

Estimating Adoption Times. To find the adoption date in each municipality, I estimate structural breaks in the time series of participation rates in SA work requirements in my base sample of low-SES individuals among age groups targeted by each reform, by municipality.[7,8] Fortunately, there are measures of take-up of SA work activities before the national reforms in the registry data, allowing me to identify municipalities that likely mandated work activities before the national reforms. Unfortunately, due to breaks in data availability, it is not possible to identify the timing of the implementation of the January 1994 reform; I thus limit my attention to starting age cohorts $\alpha \leq 24$ who were affected by the July 1990 and July 1992 reforms.[9]

To estimate these structural breaks, I use an algorithm that follows a similar spirit as the one used in Card et al. (2008). In what follows, let W_{ia} be individual i's take-up of SA work requirements at age a, $t \in \mathcal{T}_r$ index monthly calendar time over a time range relevant for reform r, $a(t)$ denote the age at time t, $r \in \mathcal{R} \equiv \{1990, 1992\}$ index the reform, and $\mathcal{A}_r$ be the set of newly treated ages in reform year r.

1. Estimate $\bar{W}_{mt,r} \equiv \mathbb{E}[W_{ia(t)} \,|\, m, t, a \in \mathcal{A}_r]$ for each municipality $m \in \mathcal{M}$ and reform year $r \in \mathcal{R}$ at at each time $t \in \mathcal{T}_r$.

6. For example, the municipality Farum instituted work requirements in the late 1980s; see Fallesen et al. (2018).

7. To the best of my knowledge, there are no institutional records of the exact timing that work requirements were implemented across all municipalities.

8. I verified that the base sample is well-represented in each municipality over time. This ensures that changes in take-up rates over time are not driven by changes in the relevant population.

9. I use measures of "voluntary" SA work activities from the Coherent Social Statistics and Cash Assistance registers labeled under the purview of the 1986 Development Assistance Act, Section 42, Subsection 1 (and associated pieces in subsequent revisions of the Act) whenever such data is available. Unfortunately, "voluntary" SA work activities are not recorded after 1990, resulting in breaks in the time series of activation between 1990-1991 for ages 21–24 and 1990–1993 for ages 25 and above. Measures of "voluntary" SA work activity appears to provide an appropriate proxy for municipality-level policy for work requirements: In the case of Farum, which implemented SA work requirements in mid-1987, my algorithm applied to ages 18–19 estimates an adoption date of March 1988.

2. Select the smallest root of the demeaned function $\bar{W}^{d}_{mt,r} \equiv \bar{W}_{mt,r} - \mathbb{E}[W_{ia(t)} \,|\, m, t \in \mathcal{T}_r, a \in A_r]$ with a positive gradient: $t^{\dagger}_{mr} \equiv \min\{t' \in \mathcal{T}_r \,|\, \bar{W}^{d}_{mt',r} = 0, {}^{\partial \bar{W}^{d}_{mt,r}}/_{\partial t}|_{t=t'} > 0\}$.

3. Select the adoption date as $t^{\star}_{mr} \equiv \max\{t' \leq t^{\dagger}_{mr} \,|\, {}^{\partial \bar{W}^{d}_{mt,r}}/_{\partial t}|_{t=t'} = 0 \text{ or } \bar{W}^{ma}_{mt',r} = 0\}$, where $\bar{W}^{ma}_{mt,r} \equiv \bar{W}_{mt,r} - \mathbb{E}[W_{ia(t')} \,|\, m, t' \leq t, a \in A_r]$ is the fitted function demeaned by its moving average.

In the first step, I estimate local linear fits of $\bar{W}_{mt,r}$ using an Epanechnikov kernel with a 90-day bandwidth in years with available SA work activity take-up data up to the year 2000. Using a smoothed estimate of the conditional expectation function, like Card et al., dramatically improves the accuracy of locating adoption dates compared to other methods, especially for municipalities with relatively small sample sizes.[10] The second step, intuitively, locates the "time window" around adoption. If adoption results in a sufficiently large take-up of SA work activities and there are no large changes in take-up of SA work activities before then, then the demeaned function $\bar{W}^{d}_{mt,r}$ will lie below zero before adoption and rise above zero after adoption. The third step aims to find an earlier date within this time window when the take-up of SA work activities in the municipality begins to "ramp up," which provides an indication of adoption timing.[11]

Estimating starting age effects with regression model. For the 1990 and 1992 reforms, I categorize the 275 municipalities into two groups—"early" and "late" adopters—based on whether they adopted work requirements below the median adoption time, indicated by $E_m \in \{0, 1\}$.[12] The average difference in adoption timing between these two groups is roughly two years.

I modify my main regression specification for starting age cohorts $\alpha \leq 24$ to study heterogeneous effects for individuals residing in the different municipality adoption groups in the year prior to the national reform through a quadruple difference design:

$$\begin{aligned} Y_{ia} = {} & \beta^{Late}_{\alpha} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a \geq \alpha) \Big) + \beta^{E}_{\alpha} \Big(E_{m_{\alpha-1}(i)} \times G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a \geq \alpha) \Big) \\ & + \delta^{Late}_{1} G_i + \delta^{Late}_{b} + \delta^{Late}_{a} + \pi^{Late}_{12} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \Big) + \pi^{Late}_{1,a} G_i + \pi^{Late}_{2,a} \mathbb{1}(D(b_i) = \alpha) \\ & + E_{m_{\alpha-1}(i)} \times \Big[\delta^{E}_{1} G_i + \delta^{E}_{b} + \delta^{E}_{a} + \pi^{E}_{12} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \Big) + \pi^{E}_{1,a} G_i + \pi^{E}_{2,a} \mathbb{1}(D(b_i) = \alpha) \Big] \\ & + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b + \theta_{m_{\alpha-1}(i)} + \varepsilon_{ia}. \end{aligned}$$

The parameter β^{Late}_{α} is the effect for those residing in late adopting municipalities, while $\beta^{Early}_{\alpha} \equiv \beta^{Late}_{\alpha} + \beta^{E}_{\alpha}$ is the effect for those residing in early adopting municipalities. Importantly, note that the index α is defined as the starting age relative to the *national reform* as before, and is thus identical between individuals of the same birth cohort, irrespective $E_{m_{\alpha-1}(i)}$'s value.

Validation tests based on parametric models of estimates. Given adoption timing between the $E_m = 0$ and $E_m = 1$ groups was roughly two years apart, one plausible validation test would be

10. Fitting jump or kink discontinuity models with unknown thresholds (Hansen 2000, 2017) to find structural breaks do not perform well in general, especially in cases of municipalities that showcase gradual implementation, non-monotonic behavior shortly after ramp-ups, or small sample sizes with extreme outliers.

11. Using roots of $\bar{W}^{ma}_{mt,r}$ guards against cases of municipalities that experience very slow but continually increasing take-up in the years leading up to the reform (i.e., ${}^{\partial \bar{W}^{d}_{mt,r}}/_{\partial t} > 0$ for $t < t^{\dagger}_{mr}$).

12. I also attempted a validation check where I classify municipalities into three categories—"early," "prompt," and "late"—but the resulting estimates were too imprecise.

to check if $\beta_{\alpha+2}^{Early} = \beta_{\alpha}^{Late}$. However, this test assumes that municipality adoption groups enforced work requirements identically upon adoption, which is rejected based on municipality responses to the Municipal Activation Scheme survey of 1996–1998. Municipalities that reported adopting work requirements for ages 25 and above before the 1994 reform activated significantly larger shares of their SA recipients and imposed more intensive work requirements; see Table D.4.

This implies that an appropriate validation test would test for differences in "global features" that help summarize the overall patterns of $\{\beta_{\alpha}^{Early} \,|\, \alpha \leq 24\}$ and $\{\beta_{\alpha}^{Late} \,|\, \alpha \leq 24\}$ rather than testing for equality of the parameters themselves. The global feature of interest depends on the outcome, which are described in Table D.5, Column (2). For example, in the case of market income, the global feature of interest is the "first α-intercept," which signifies the starting age where the SA reforms' effect fades out. Based on an "eye-ball test," the occurs around starting ages 17–19 in my main effect estimates $\hat{\beta}_{\alpha}$. An appropriate validation test would be to check if the first α-intercept of $\hat{\beta}_{\alpha}^{Early}$ is larger than that of $\hat{\beta}_{\alpha}^{Late}$. In particular, one would expect that the first α-intercept of $\hat{\beta}_{\alpha}^{Early}$ is larger than the intercept of $\hat{\beta}_{\alpha}$, the intercept of $\hat{\beta}_{\alpha}^{Early}$ is smaller than the intercept of $\hat{\beta}_{\alpha}$, and the difference between the intercepts of $\hat{\beta}_{\alpha}^{Early}$, $\hat{\beta}_{\alpha}^{Late}$ is approximately 2 (the average number of years difference between the adoption groups). To conduct these checks more rigorously, I fit parametric models $\hat{\beta}_{\alpha}^{k} = m^{k}(\alpha) + \epsilon_{\alpha}^{k}$ for $k \in \{Early, Late\}$ and compare their estimated global features. This is especially useful given the large estimation error of the individual $\hat{\beta}_{\alpha}^{k}$ estimates.

I discipline the specification of $m^{k}(\cdot)$ based on patterns found in my main starting age effect estimates $\hat{\beta}_{\alpha}$. Specifically, for each outcome I fit threshold models with linear or constant segments; Table D.5, Column (4), reports the exact specification used for each outcome. The threshold α^{*} is selected by finding the R^2-maximizing threshold model over all possible α^{*} values (shown in the gray bars of Figure D.7).[13] Continuing the example of market income, the selected model's global feature—the first α-intercept of $m(\alpha)$ (plotted in the solid black line in Panel (a))—takes the value of around $\alpha = 18.5$. The parametric fit of the selected threshold model is plotted with the original $\hat{\beta}_{\alpha}$ estimates for reference in Panel (b).

With the specification of the parametric model on hand, I subsequently search for the R^2-maximizing threshold models for the municipality adoption group estimates $\{\hat{\beta}_{\alpha}^{k} \,|\, \alpha \leq 24\}$, allowing for different thresholds α^{k*} for each $k \in \{Early, Late\}$, and compare their respective global features according to the tests described in Table D.5, Column (3). Results from the model search and the selected models are illustrated in Figure D.8. Validation tests are met for all outcomes. For example, in the case of years of schooling, the first α-intercept of the selected threshold model for the early-adopting municipality estimates is around $\alpha = 19.5$, which is about two years larger than the late-adopting municipality estimates' intercept. The left panels of Figure D.8 also demonstrate that these results are fairly robust to the choice of threshold. For example, as shown in the orange and blue bars of Panel (a), the R^2-values of the threshold choices $17 \leq \alpha^{k*} \leq 22$ are very close to each other. Yet regardless of which of these threshold α^{k*} is chosen, the first α-intercept value of $m^{Early}(\alpha)$ (the solid blue line) is always above that of $m^{Late}(\alpha)$ (the solid orange line). Overall, these results demonstrate that the heterogeneity of starting age effects is indeed sensitive to the timing that the SA reforms were adopted by municipalities, further validating that my main estimates indeed capture the effects of the SA reforms.

13. Recall that estimates of β_{α}^{k} are restricted to $\alpha \leq 24$. The most striking changes in effect estimates occur around starting age $\alpha = 18$, so I restrict my attention to a symmetric window $12 \leq \alpha \leq 24$.

Table D.4: Differences in municipal work requirement policies, by share of SA recipients activated in work requirement program before 1994 reform

(a) SA recipients facing only unemployment in 1996

	Most activated		*Some activated*		*Few activated*	
	N	Mean [S.D.]	N	Diff. (S.E.)	N	Diff. (S.E.)
A. Coverage of work requirements						
% SA recipients activated	130	90.608 [12.380]	59	-9.108^{***} (2.632)	39	-5.287^{*} (3.047)
B. Intensity of work requirements						
Hours/week of work req's.	131	34.168 [4.757]	62	-0.684 (0.672)	40	-0.268 (0.789)
Months of work req's.	114	9.132 [5.706]	56	-0.149 (0.775)	33	0.020 (1.139)
C. Sanction policy						
Deduct >cash benefits	130	0.846 [0.362]	62	-0.024 (0.058)	40	0.004 (0.065)
Deduct cash benefits	130	0.915 [0.279]	62	-0.061 (0.051)	40	0.010 (0.049)

(b) SA recipients facing additional issues in 1996

	Most activated		*Some activated*		*Few activated*	
	N	Mean [S.D.]	N	Diff. (S.E.)	N	Diff. (S.E.)
A. Coverage of work requirements						
% SA recipients activated	42	83.155 [18.945]	75	-13.955^{***} (4.194)	106	-27.466^{***} (3.968)
B. Intensity of work requirements						
Hours/week (additional reasons)	42	34.810 [3.423]	76	-0.625 (0.703)	103	-3.479^{***} (0.806)
Months (additional reasons)	34	9.235 [5.052]	68	1.765 (1.120)	90	0.698 (1.045)
C. Sanction policy						
Deduct >cash benefits	43	0.837 [0.374]	77	-0.058 (0.074)	107	-0.033 (0.069)
Deduct cash benefits	43	0.837 [0.374]	77	-0.006 (0.071)	107	-0.033 (0.069)

Notes: Data drawn from Questions 16, 19, 22, and 24 from "Questionnaire to Municipalities," *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives. The "Few activated" column includes municipalities who responded "To a lesser degree" or "Virtually none." Differences are relative to the "Most activated" column. Standard deviations are in brackets; standard errors of t-tests are in parentheses.

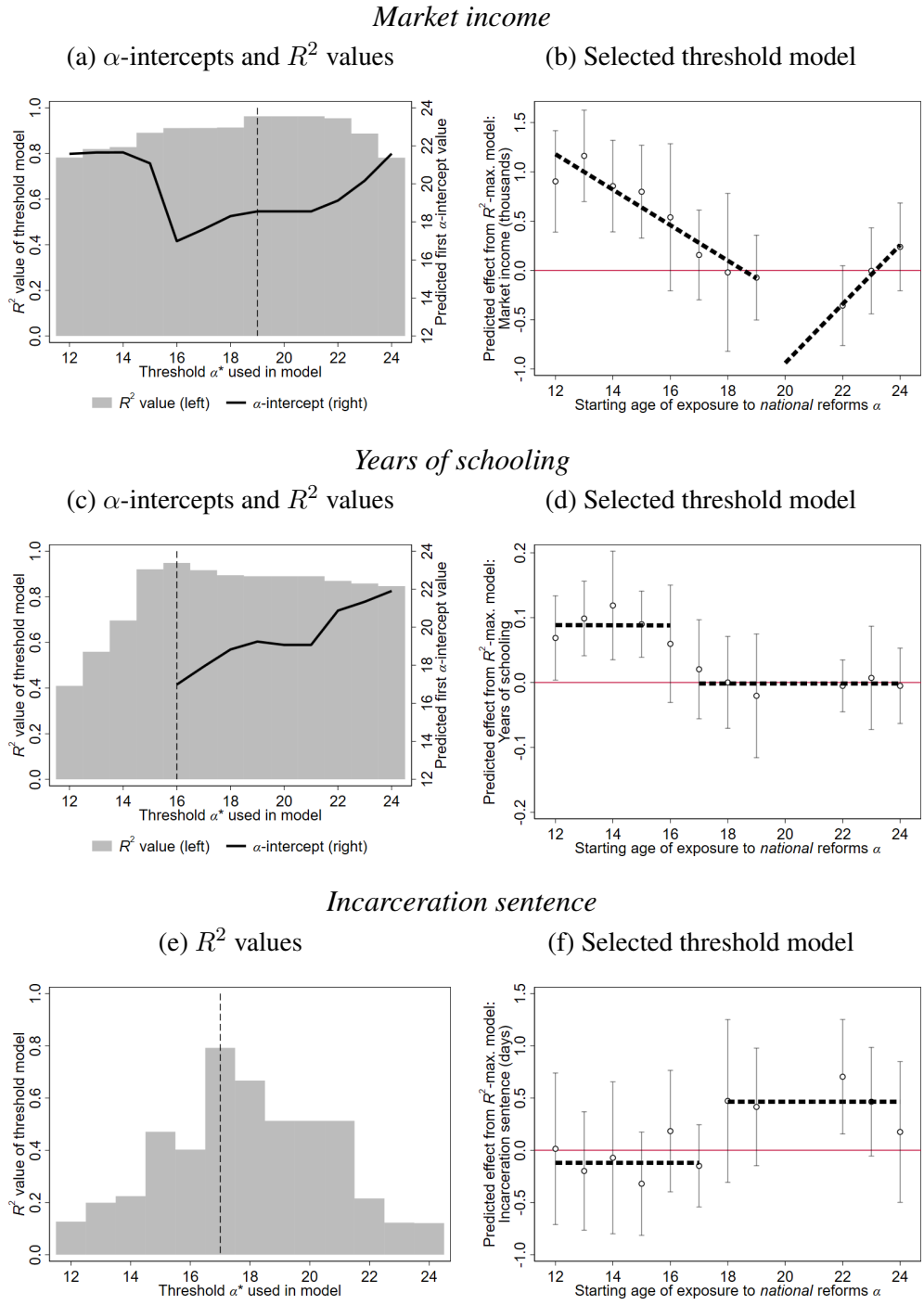
Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.5: Validation tests using parametric fits of starting age estimates in early- and late-adopting municipalities

Outcome	Global feature	Validation test	Parametric specification $m^k(\alpha)$
(1)	(2)	(3)	(4)
Market income	α-intercept	α-intercept of $m^{Early}(\alpha) > \alpha$-intercept of $m^{Late}(\alpha)$	$(\delta^k_{0l} + \delta^k_{1l}\alpha) \cdot \mathbb{1}\{\alpha \leq \alpha^{k*}\} + (\delta^k_{0r} + \delta^k_{1r}\alpha) \cdot \mathbb{1}\{\alpha > \alpha^{k*}\}$
Yrs. of schooling	α-intercept	α-intercept of $m^{Early}(\alpha) > \alpha$-intercept of $m^{Late}(\alpha)$	$(\delta^k_{0l} + \delta^k_{1l}\alpha) \cdot \mathbb{1}\{\alpha \leq \alpha^{k*}\} + \delta^k_r \cdot \mathbb{1}\{\alpha > \alpha^{k*}\}$
Incarc. sentence	threshold α^*	$\alpha^{Early*} > \alpha^{Late*}$	$\delta^k_l \cdot \mathbb{1}\{\alpha \leq \alpha^{k*}\} + \delta^k_r \cdot \mathbb{1}\{\alpha > \alpha^{k*}\}$

Notes: The parametric models $m^k(\cdot)$, $k \in \{Early, Late\}$, are described in Column (3). These models are estimated by weighing the starting age estimates $\hat{\beta}^k_\alpha$ by their inverse standard error for each $k \in \{Early, Late\}$.

Figure D.7: Parametric fits and global features of main starting age effect estimates

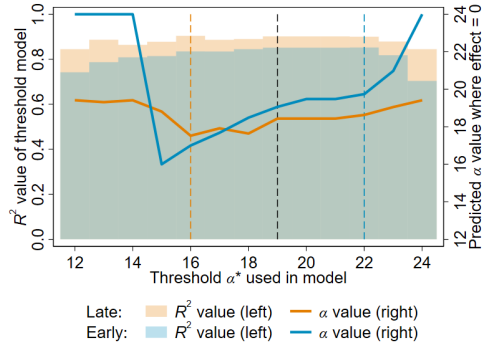


Notes: This figure illustrates parametric model fitting exercise on the main starting age estimates $\hat{\beta}_\alpha$. The left panels illustrate the global feature (black line, if applicable) and R^2 value (gray bar) of each possible threshold model $12 \leq \alpha^* \leq 24$. Vertical dashed lines indicates the selected threshold that maximizes the R^2 value. In Panel (c), the black line is missing between $12 \leq \alpha \leq 14$ since the α-intercept does not exist for these models; Panel (e) lacks a black line since the global feature of interest is the threshold $\alpha^\star$ itself. The right panels illustrate the selected threshold model fitted on the starting age estimates $\hat{\beta}_\alpha$. Dashed lines are fitted values of the selected threshold model. White markers are $\hat{\beta}_\alpha$. Vertical bars are 95% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities.

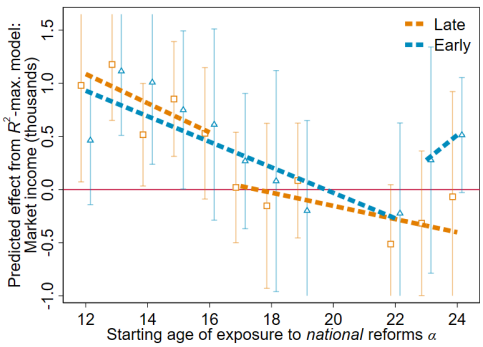
Figure D.8: Parametric fits and global features of starting age effect estimates, by municipality adoption time

Market income

(a) α-intercepts and R^2 values

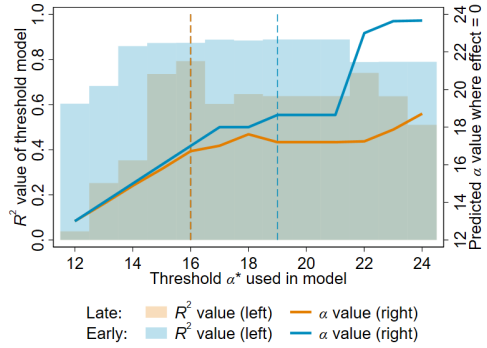


(b) Selected threshold model

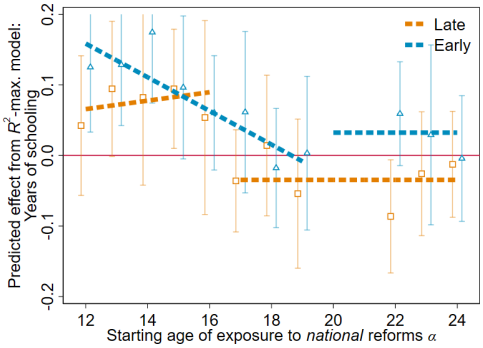


Years of schooling

(c) α-intercepts and R^2 values

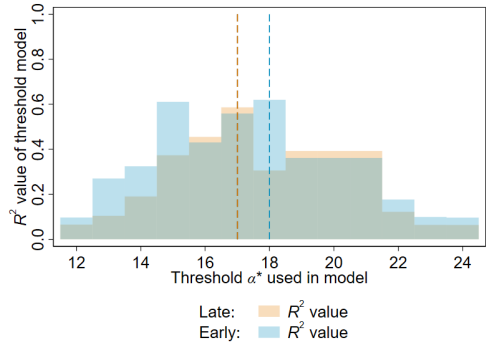


(d) Selected threshold model

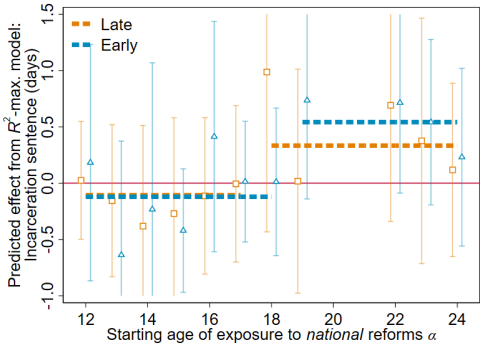


Incarceration sentence

(e) R^2 values



(f) Selected threshold model



Notes: This figure illustrates parametric model fitting exercise on the municipality adoption group starting age estimates $\hat{\beta}^k_\alpha$, $k \in \{Early, Late\}$, described in Appendix D.4. The orange and blue bars, lines, and markers correspond to the late ($E_m = 0$) and early ($E_m = 1$) groups, respectively. The vertical black dashed lines in the left panels correspond to the selected threshold α^* of the main estimates $\hat{\beta}_\alpha$. See notes of Figure D.7 for more details.

D.5 Alternative Measurement Choices and Samples

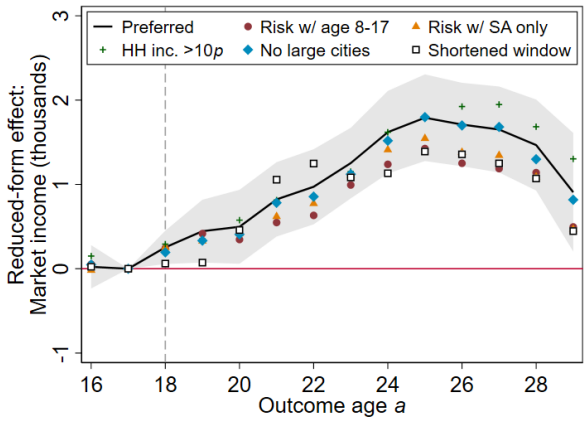
I apply a battery of robustness checks to ensure my main results are unaffected by particular choices made in my main model specification; see Figures D.9–D.11.

First, I examine alternative definitions of risk groups. I try defining the high-risk group as individuals whose households participated in SA between ages 12–17 only without using local neighborhood SA participation rates as a criterion. I also try defining risk groups based on measures of household and neighborhood SA participation defined between ages 8–17 and before 1990 instead of between ages 12–17 and before 1994. While observable household covariates do not appear to respond to the 1990–1992 reforms (results available upon request), measuring data between ages 8–17 before 1990 eliminates the possibility that my main definition of risk groups are affected by household responses to even the earliest reforms. Additionally, for starting age groups $\alpha \geq 18$, I try defining risk groups as individuals who are likely to be eligible for SA based on characteristics measured at age 17 related to liquid assets, UI membership, and employment and education enrollment. I only use likely-eligible individuals in estimates for incarceration sentence, since using likely-eligible individuals for market income or years of schooling would effectively define the "treatment group" on the outcome.

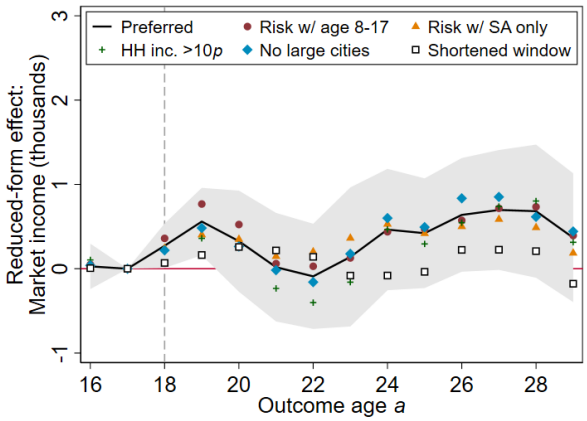
Second, I try different subsamples of my main data. I exclude individuals from the bottom decile of the household income distribution to ensure my main results are not driven by the most disadvantaged subgroup. I also try excluding the four most populous cities from the main sample (Copenhagen, Aarhus, Aalborg, Odense) to ensure that my results persist outside of urban areas with high SA participation rates. Finally, to test if results are sensitive to using significantly older starting age groups who are not-yet-treated at a given outcome age, I shorten the window of comparison cohorts to starting ages at most three years above a given outcome age. All of my results survive these alternative measurement choices and sampling schemes.

Figure D.9: Robustness checks for alternative measurement choices and samples: Market income

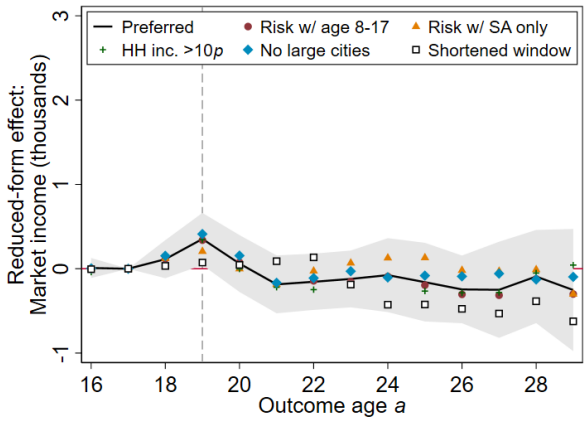
(a) $8 \leq \alpha \leq 15$



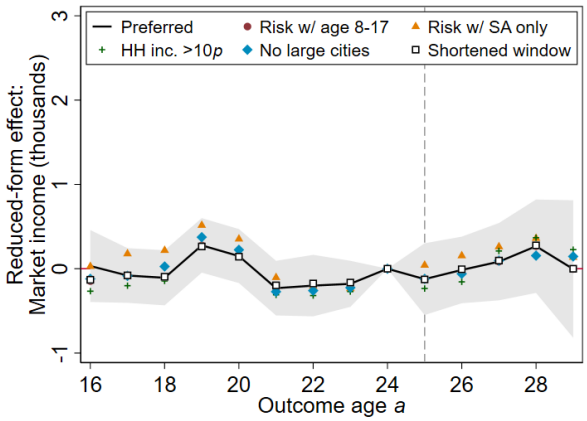
(b) $16 \leq \alpha \leq 17$



(c) $18 \leq \alpha \leq 24$



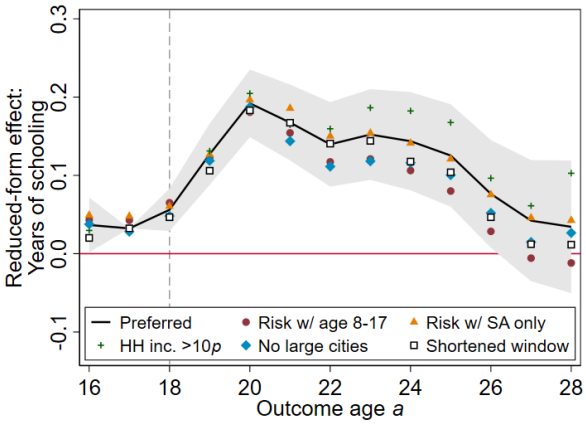
(d) $25 \leq \alpha \leq 29$



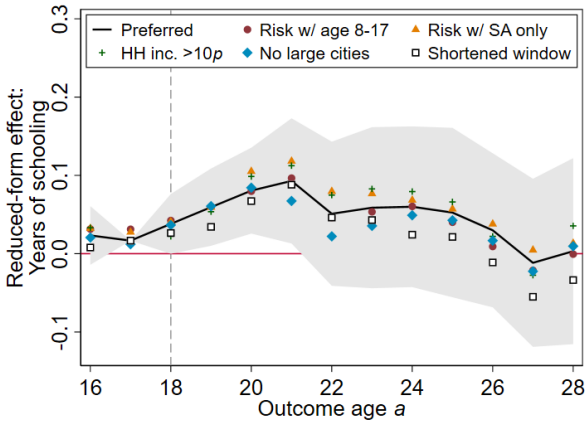
Notes: This figure plots starting age–outcome age event-study estimates $\beta_{\alpha,a}$ from specifications with alternative measurement choices and samples. The solid black line and shaded region represent my main preferred estimates and 95% confidence interval. Red circles are estimates where risk groups are defined by measuring household participation on SA and residence in high-SA neighborhood between ages 8–17 and before 1990. Orange triangles instead define risk groups using household participation on SA only (and thus uses my entire base sample). Green crosses excludes individuals whose household income between ages 12–17 are below the first decile. Blue diamonds exclude the four largest cities in Denmark (Copenhagen, Aarhus, Aalborg, Odense). White squares use a shortened window of starting ages at each given outcome age a as a comparison group, $a+1 \leq \alpha \leq a+3$ (instead of $\alpha \geq a+1$).

Figure D.10: Robustness checks for alternative measurement choices and samples: Years of schooling

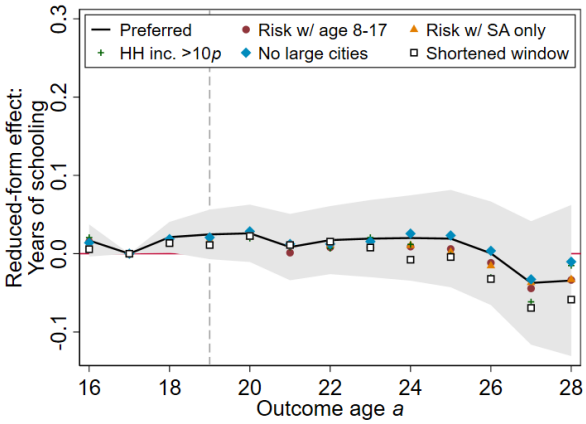
(a) $8 \leq \alpha \leq 15$



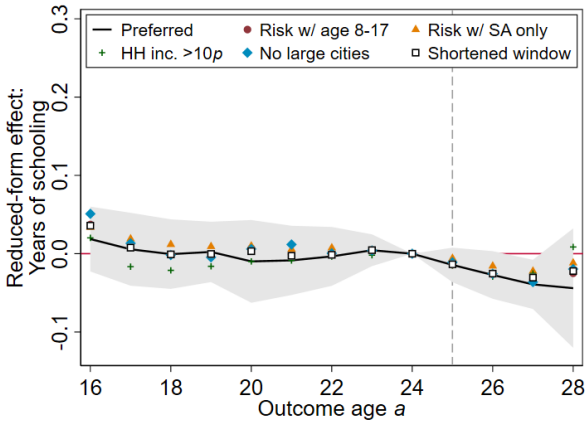
(b) $16 \leq \alpha \leq 17$



(c) $18 \leq \alpha \leq 24$



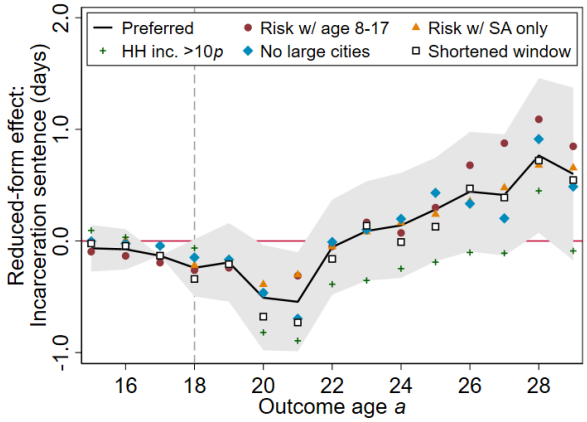
(d) $25 \leq \alpha \leq 29$



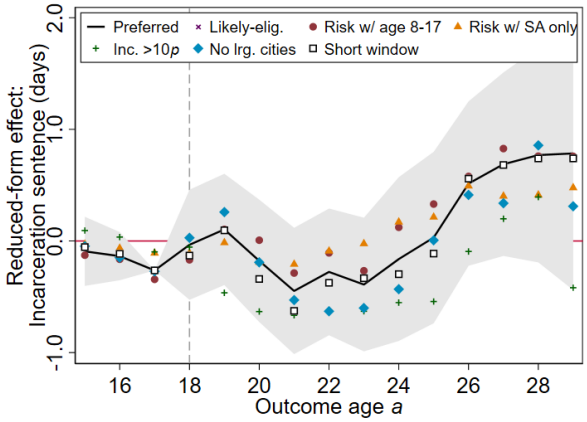
Notes: See notes to Figure D.9. All event-study estimates of starting ages $\alpha \leq 17$ are re-normalized based on difference-in-difference estimates of welfare-to-work on outcomes measured at outcome age 17; see Footnote 34 for details.

Figure D.11: Robustness checks for alternative measurement choices and samples: Incarceration sentence

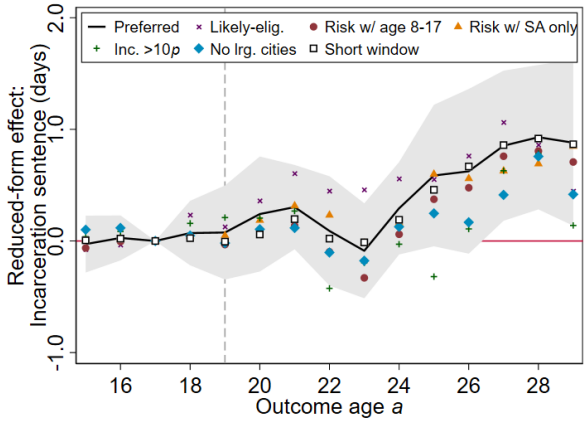
(a) $8 \leq \alpha \leq 15$



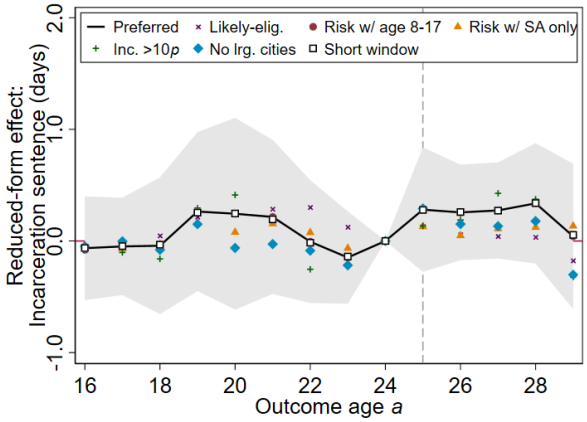
(b) $16 \leq \alpha \leq 17$



(c) $18 \leq \alpha \leq 24$



(d) $25 \leq \alpha \leq 29$



Notes: See notes to Figure D.9. Purple Xes are estimates where risk groups are those likely-eligible for SA; see Appendix D.5 for details. All event-study estimates of starting ages $\alpha \leq 17$ are re-normalized based on difference-in-difference estimates of welfare-to-work on outcomes measured at outcome age 17; see Footnote 34 for details.

D.6 Discussion of the State Educational Grant Reform

One of my robustness analyses compares individuals from particularly low-income households against those from higher-income households. The logic behind this comparison is to check if other policy changes unrelated to SA that may have a disproportionate effect on low-income individuals are driving my main findings.

One policy reform of potential concern is a 1988 reform to state educational grants (*Statens Uddannelsesstøtte*; SU, or student aid), which increased transfers to students attending college by at least 25%.[14] For low-income individuals, this increase in student aid could relax credit constraints and promote college attendance, which could be driving the positive effects on educational attainment and income for younger starting ages. Nevertheless, this policy change likely does not pose a threat to my analysis since SA youth benefit rates actually increased to match the new levels of student aid in 1989 (Hansen and Schultz-Nielsen 2017), which would dampen incentives to substitute from SA to higher education.

Another reason to be skeptical that this reform influences my results is that it actually increased student aid the most for higher-income individuals, since it transformed student aid from a means-tested scheme to a universal conditional cash transfer. This means that low-risk individuals—who are more likely to be higher-income—experienced greater changes in potential aid relative to high-risk individuals, so this reform may generate null differences in outcomes between risk groups.

14. See Nielsen et al. (2010) for more details of the reform. Using the 1985 formula for means-tested student aid scheme they provide, I find that the vast majority of my sample that falls in the bottom quartile of the household income distribution would be eligible for the highest level of aid.

E Technical Appendix

E.1 Model Predictions

What are the expected long-run effects of the welfare-to-work reforms for different starting age cohorts? To answer this question, consider how the reforms' changes to budget constraints induce different behavioral responses in a dynamic labor supply model where each cohort aims to maximize their lifetime value of consumption and leisure. I begin by predicting effects on income, education, and SA participation. I then discuss plausible effects on a broader set of outcomes.

Predicted effects on income, education, and SA participation. Suppose there are four outcome ages of the life cycle indexed by a: childhood C when individuals are not yet eligible for SA (ages $\leq$17), young adulthood YA when individuals become eligible for the SA youth benefit rate (ages 18–24), adulthood A when individuals become eligible for adult benefit rate (ages 25–29), and older adulthood OA (ages $\geq$30). Correspondingly, consider four cohorts with different starting ages of exposure to the welfare-to-work reforms, indexed by $\alpha \in \{C, YA, A, OA\}$.

In addition to choosing consumption and leisure, individuals possess four mutually exclusive discrete choices. The first is school, which is available up to young adulthood, $a \in \{C, YA\}$. The second and third are work and SA participation, which are available starting in young adulthood, $a \in \{YA, A, OA\}$. The fourth choice is an outside option.[15] These choices affect human capital, the key state variable. It appreciates if the individual chooses school or work. If SA possesses (lacks) work requirements, SA participation may appreciate (depreciate) human capital.

Budget constraints and predicted effects by starting age.—How does the reform affect the education and income of different starting age cohorts? Figure E.1, Panels (a) and (b) model the budget constraints for starting ages $\alpha \in \{C, A\}$ in outcome ages $a \in \{YA, A\}$ before and after the reform. In both panels, the black and gray lines are the budget constraints under no reform. The black line $\mathrm{A}_{YA}\mathrm{B}_{YA}\mathrm{C}_{YA}\mathrm{D}_{YA}$ is the budget constraint in young adulthood $a = YA$. The point B_{YA} is the maximum weekly SA youth benefit rate, and the flat segment $\mathrm{B}_{YA}\mathrm{C}_{YA}$ reflects the 100% marginal tax rate on SA benefits. The slope of segment $\mathrm{C}_{YA}\mathrm{D}_{YA}$ is the market wage rate earned from choosing work, where C_{YA} is the labor supply level where income from work breaks-even with SA benefits. Similarly, the gray line $\mathrm{A}_{YA}\mathrm{B}_{A}\mathrm{C}_{A}\mathrm{D}_{A}$ is the budget constraint in adulthood $a = A$, which is shifted higher due to the adult benefit rate and (possibly) higher market wages available to adults.

Now consider budget constraints under the reform. For starting age $\alpha = A$ (Panel (a)), the reform first comes into effect at outcome age $a = A$. The dashed blue line $\mathrm{A}_{YA}\mathrm{B}'_{A}\mathrm{B}''_{A}\mathrm{C}_{A}\mathrm{D}_{A}$ is the new budget constraint, which shifts inwards to reflect the work requirement.

Starting age cohort $\alpha = A$ who would have chosen SA under no reform (point B_{A}) may respond in three possible ways that affect their gross income at outcome age $a = A$. Individuals with stronger preferences for leisure would face a negative effect on gross income (moving to line segment $\mathrm{A}_{YA}\mathrm{B}'_{A}$); those with stronger preferences for consumption would experience increased income (moving to segment $\mathrm{A}_{YA}\mathrm{B}'_{A}$); and some may opt to comply with SA work requirements, resulting in no change in their gross income (moving to point B''_{A}). These heterogeneous effects on gross income result in ambiguous mean effects for the $\alpha = A$ cohort (Bitler et al. 2006). However,

15. Recall that SA eligibility is determined by a lack self-sufficiency. This means individuals may participate in SA if and only if they are not working or in an education program through support from state educational grants.

all three responses could result in higher human capital and, consequently, higher market wages in older adulthood $a = OA$. This implies that the overall effects on income in outcome age $a = OA$ may be larger than in $a = A$.

In the case of starting age cohort $\alpha = C$ (Panel (b)), the budget constraints under the reform may look very different. The reform came into effect before they were eligible for SA, so they could *anticipate* facing work requirements in the future. Moreover, unlike $\alpha = A$, this cohort possesses school as a choice in outcome ages $a \in \{C, YA\}$. In response to anticipating SA work requirements from $a = YA$ onward, individuals who would have chosen SA if there was no reform may be more incentivized to invest in their own human capital through school (Becker 1962, Besley and Coate 1992). Intuitively, if the market wage return to school is sufficiently high to compensate for the utility cost of choosing school, then individuals expecting to supply a certain level of labor under SA in return for the same SA benefit rates may prefer to supply labor in formal work in return for higher income.If the returns to school are sufficiently high, then this could result in fanned-out budget constraints that exceed the SA benefit kinks; this special case is illustrated by the red and blue dashed lines in Panel (b). Alternatively, individuals may opt to participate in SA work requirements to build their human capital and reap higher market wages from work in later ages.

Just like the case of $\alpha = A$, the behavioral responses of the $\alpha = C$ cohort can generate negative, positive, or null effects on gross income. However, there is a higher probability that the mean effect among $\alpha = C$ is higher than the mean effect among $\alpha = A$ since increased incentives for human capital accumulation expand their budget constraints. In particular, the reforms are predicted to increase educational attainment and decrease SA participation of $\alpha = C$ relative to $\alpha = A$.

Similar insights extend to starting age cohorts $\alpha = YA$ and $\alpha = OA$. In general, younger cohorts possess more margins to build their human capital than older cohorts. Conditional on outcome age a, mean gross income is expected to decrease in starting age; conditional on starting age α, it is expected to increase in outcome age.

Predicted aggregate effects.—How does this heterogeneity map onto the overall effects on gross income in cross-sections of the population over time? Panel (c) illustrates how different possible effects by starting age map to a range of mean effects by calendar time period. Each shaped marker is a possible starting age-outcome age effect, and connected lines and colors distinguish the possible outcome age effect trajectories by starting age α. For each starting age $\alpha \in \{C, YA, A, OA\}$, I plot three possible trajectories given the possible positive, negative, or null effects discussed earlier. The gray region illustrates the range of aggregate effects by averaging all possible starting age-outcome ages by calendar time.

While the sign of the overall effects over calendar time remain ambiguous, the simple model predicts the overall effect increases over time as more starting age cohorts (the gray dashed lines) age into eligibility. Steady state is attained when only $\alpha \leq C$ cohorts exist in the adult population, and is expected to be higher than the short-run effects immediately following the reforms.

Predicted effects on other outcomes. Enriching the model with more choice margins and preferences that capture additional outcomes gives rise to complex behavioral responses. I briefly discuss possible effects on other outcomes that are critical for policy evaluation and how they may interact with income and education.

Health.—The SA reforms may enhance the health of younger starting age cohorts given the well-documented relationship between education and health outcomes (Cutler and Lleras-Muney 2008). In contrast, older starting age cohorts' health could diminish from increased stress or accidents from participating in work requirements or the labor market. Effects on health, in turn, may affect income: decreased health among older starting ages could negatively impact labor supply capacity.

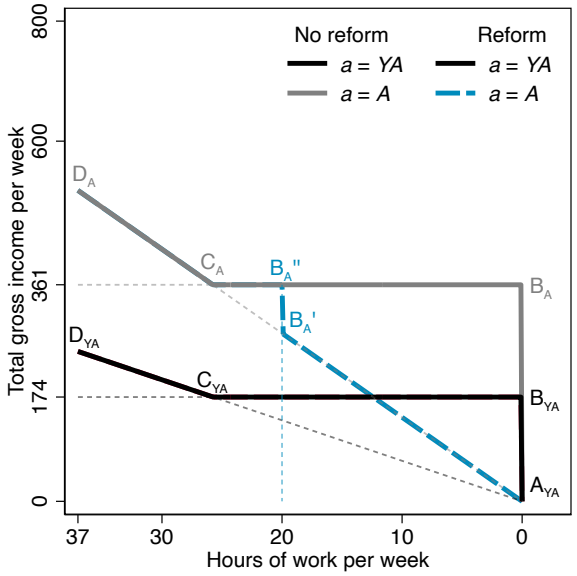
Crime.—Individuals may be incentivized to leave SA to build "criminal capital" instead of human capital. Yet participating in SA work requirements could incapacitate individuals from committing crime. This leads to ambiguous mean effects across starting ages. However, effects are likely magnified in young adulthood when crime peaks during the life cycle. Behavioral responses in crime could also affect income: if the reform increases crime among young adults, this could have scarring effects on their future labor market prospects.

Social insurance program participation.—Older starting age cohorts who lack the education margin would be more incentivized to substitute SA for a different programs like DI or UI. Meanwhile, younger starting ages who gain human capital may be less prone to take-up these welfare programs in the future.

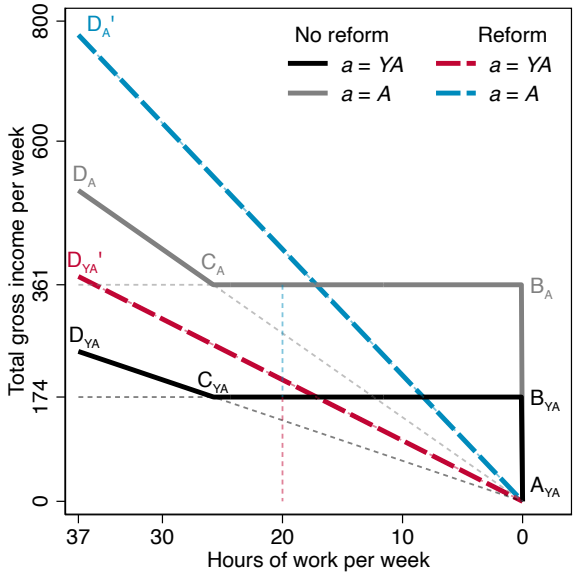
Intergenerational spillovers.—The reforms' effects on choice behavior can affect the development of the next generation via investments, time, and role model effects. In general, I expect children of younger starting age cohorts to fare better than those of older cohorts.

Figure E.1: Effects of reforms on budget constraints and aggregate income over time

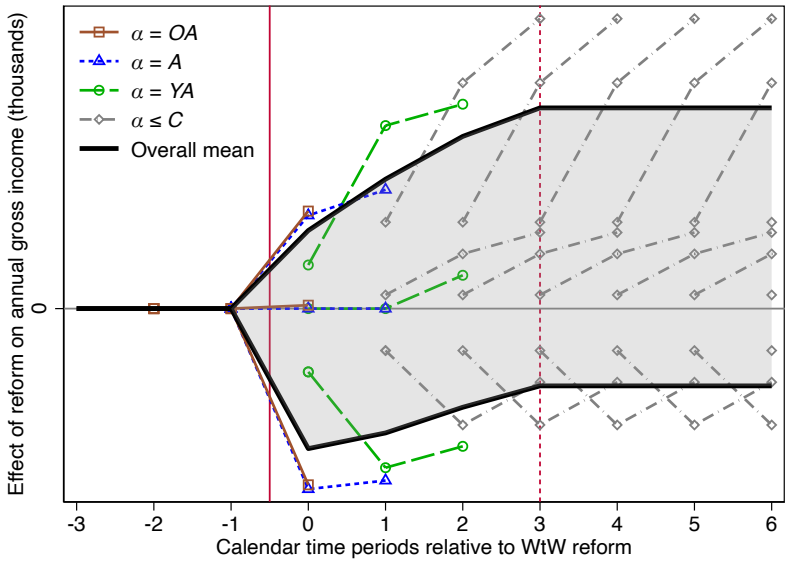
(a) Starting age $\alpha = A$ (no anticipation)



(b) Starting age $\alpha = C$ (anticipation)



(c) Range of aggregate effects over calendar time



Notes: Panels (a) and (b) plot hypothetical weekly budget constraints, with and without work requirements, for starting ages $\alpha = A, C$ at outcome ages $a = YA, A$ based on 1995 SA policy. Panel (c) plots a hypothetical range of effects on average gross income among the eligible SA population over calendar time. See main text for more details.

E.2 Proof of Proposition 1

First, note that

$$\beta_{\alpha,a}^{Antic}(18,\infty) = \beta_{\alpha,a} - \mathbb{E}\left[Y_{ia}(\infty,18,\infty) - Y_{ia}(\infty) \mid D_i = \alpha, G_i = 1\right].$$

Also, note that $\mathbb{E}[Y_{i17}(\infty,18,\infty) \mid D_i = \alpha', G_i = 1] = \mathbb{E}[Y_{ia}(\infty) \mid D_i = \alpha', G_i = 1]$, $\forall \alpha' \in \mathcal{A}$, since ITT effects that start at age 18 cannot affect potential outcomes in previous ages under no anticipation. This implies that Assumption 2 may be rewritten as

$$\begin{aligned}
&\mathbb{E}\left[Y_{ia}(\infty,18,\infty) \mid D_i = \alpha, G_i = 1\right] \\
&= \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 1\right] \\
&\quad + \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = \alpha, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 0\right]\right) \\
&\quad + \left(\mathbb{E}\left[Y_{ia}(\infty,18,\infty) \mid D_i = 18, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 1\right]\right) \\
&\quad - \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = 18, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 0\right]\right).
\end{aligned}$$

Substituting this expression into the first equation and rearranging terms yields

$$\begin{aligned}
\beta_{\alpha,a}^{Antic}(18,\infty) = \beta_{\alpha,a} - \beta_{18,a} \\
&+ \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = \alpha, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 1\right]\right) \\
&- \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = \alpha, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 0\right]\right) \\
&- \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = 18, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 1\right]\right) \\
&+ \left(\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = 18, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 0\right]\right).
\end{aligned}$$

Invoking Assumption 1 completes the proof.

F Details of Figure 11

F.1 General Guidelines

Inclusion criteria. The papers included in Figure 11, Panel (a) (i) study a reform or a randomized control trial involving work requirements among welfare recipients, (ii) employ experimental or quasi-experimental research design, (iii) include high school completion or dropout as an outcome variable, and (iv) includes estimates for more representative demographic groups or populations likely to be affected by work requirements. Table F.1 summarizes the studies included in my analysis. Section F.2 provides details for obtaining the starting age ranges, estimates, standard errors, and means from each study. Section F.3 provides examples of papers that are highly relevant but do not fulfill my inclusion criteria.

Choosing and constructing estimates of individual studies. I focus on reduced-form or intent-to-treat effect estimates in order to make results comparable to my main estimates. Estimates and standard errors expressed as percentage points are scaled relative to the paper's reported appropriate baseline high school completion rate in order to make estimates more comparable across datasets and institutional settings.

If a paper reports estimates for different regression specifications, I use estimates from the specification with the richest number of controls.

I identify ranges of starting ages associated with each study's estimate based on description of the sample and methodology. I do not weight individual starting ages based on their relative sample sizes in the study given that there is plausible treatment effect heterogeneity over individual starting ages within the range. If a paper reports estimates for different groups of starting ages, I include them as separate observations in my analysis.

If a paper reports multiple estimates based on other dimensions (e.g., demographic groups, high-risk group definitions, etc.) and does not provide a pooled estimate, I use the average over all estimates. In this case, estimate standard errors of pooled estimates with the square root of the average squared standard errors across all estimates (weighted by each specification's sample size, if available).

Pooling estimates across studies by starting age group. I estimate mean effects across the prior studies by the four starting age groups ([8, 15], [16, 18], [19, 24], and [25, 29]), weighing each estimate by the fraction that their range of starting ages fall within each starting age group. I estimate pooled standard errors by starting age group using the square root of the weighted mean of the squared standard errors. I obtain extremely similar results if I instead compute pooled estimates by starting age group by additionally weighing individual study estimates by their inverse squared standard error.

F.2 Details of Included Studies

Bastian et al. (2022). Data consists of life-cycle panels of individuals; high-risk group were 12–16 years old in 1996 (p. 4). Though the authors' analysis considers other policy changes during the 1990s, I use 1996 as the reference year for defining starting ages, corresponding to the timing of federal welfare reform. Table 3, Columns (1) and (2) report point (mean) estimates and p-values,

which I use to infer standard errors, separately for men and women. I pool estimates for men and women using high-risk sample sizes reported in the final row of Table 1. Table 2, Columns (1) and (4), report means for high-risk men and women.

Dahl and Gielen (2021). Data consists of life-cycle panels of individuals; "treated" group includes children between 4–24 years old at the time of the reform, based on the "Parent age 40–45" distribution plotted in Online Appendix Figure A.2. Table 8, Row H reports reduced-form effects, standard errors, and means for different ranges of starting ages: [4, 14], [4, 18], and [19, 24].[16] I use these three sets of estimates in my analysis.

Dave et al. (2012). Data consists of cross-sections of 15- to 20-year-olds (p. 261) and spans up to 2001 (p. 257), which is 5 years after the 1996 welfare reform. Thus, starting ages in the reported estimates range from $15 - 5 = 10$ to 20. Table 2, Column (1) report point estimates and standard errors on high school dropout. Table 1, Column (1) reports mean baseline high school dropout rate for the "target" (high-risk) group. I convert the point estimate and mean to represent high school completion.

Hao and Cherlin (2004). Data consists of longitudinal data of 14- to 16-year-olds at the time of welfare reform (p. 184). The final column of Table 6 report difference-in-difference estimates based on 5 alternative "target" or high-risk groups. I use pooled estimates and standard errors in my analysis. The penultimate column of Table 2 reports school dropout rates among the five target groups among the reform cohorts; I use the average in my analysis. I convert point estimates and means to represent high school completion.

Hernæs et al. (2017). Data consists of panels of individuals who turned 21 years old between 1993 and 2005 (p. 147). Treatment is defined by exposure to the policy for at least 1 year, implying that the maximum starting age is 20. Appendix Table A.1 reports the timing of municipal adoption of increased work requirements for social assistance recipients, the earliest of which occurred in 1995; this implies that the youngest starting age is $21 - (2005 - 1995) = 11$. The authors report estimates separately by the quartile of predicted probability of welfare take-up; I focus on estimates among the top quartile. Table 6, Panel B, Column (5) reports the intent-to-treat effect estimates, standard error, and mean.

Kaestner et al. (2003). Data consists of different cohorts with outcomes observed at either age 17 or 19; separate analyses are conducted at each age. High-risk group consists of women aged 17 in 1997 or aged 17 and 19 in 1998 and 1999 (p. 229); given that federal welfare reform occurred in 1996, this implies starting ages ranging between $17-(1999-1996) = 14$ and $17-(1997-1996) = 16$ for the age 17 sample and between $19 - (1999 - 1996) = 16$ for the age 19 sample. The final column of Tables 4 and 6, Row 5, report estimates and standard errors on high school dropout from their richest regression specification. The penultimate column of Table 2, Panel 5 reports means for the (relative) high-risk group for the age 17 and 19 samples. I use both sets of estimates in my analysis. I convert point estimates and means to represent high school completion.

16. The table heading describes the lattermost group as "$Age > 19$," but p. 142 makes it clear that these estimates represent $\alpha \geq 19$.

Offner (2005). Data consists of cross-sectional data on 16- and 17-year olds spanning up to 2001 (p. 308), which is 5 years after the 1996 welfare reform. Thus, starting ages in the reported estimates range from $16 - 5 = 11$ to 17. Table 5 reports the average estimated effect, based on the "relative threshold" and "absolute threshold" coefficient estimates, and high school dropout rate pre-welfare reform. Table 1 reports identical t-statistics of both coefficient estimates; I scale the average estimate by the t-statistic to estimate the standard error. I convert the point estimate and mean to represent high school completion.

Riddell and Riddell (2014). Data is an experimental sample divided into two subsamples: high school dropouts and high school graduates at baseline. Summary statistics only report the mean baseline age of the treatment group's baseline age is 32; Michalopoulos et al. (2002) description of the Self Sufficiency Project states that treatment group was at least 19 years old, so I assume ≥ 19. Table 2, Panel (a) reports treatment effect estimates of completing a high school diploma 54 months after treatment with baseline controls among high school dropouts. Given treatment effects on high school completion is zero among high school graduates, I scale this estimate and its standard error by the fraction of high school dropouts at baseline, based on sample sizes reported in Table 1, to obtain a pooled estimates. The sum of this fraction weighed by the high school dropout control group's 54-month high school diploma completion rate (0.209, reported in Table 2, Column (2)) and the fraction of the treatment group with high school diplomas at baseline gives the mean high school completion rate: $\frac{1,350}{1,110+1,350} \times 0.209 + \frac{1,110}{1,110+1,250} \times 1 = 0.566$.

F.3 Examples of Excluded Studies

Jacobs and Winslow (2003). This paper focuses on enrollment in post-secondary schooling as an outcome variable and does not include any analysis on high school completion.

Koball (2007). This paper focuses exclusively on teen mothers, which is a highly selected sample that is not representative of the larger population of adolescents plausibly affected by welfare-to-work. Pre-reform high school completion rate reported in this study (reported in the first column and last row of Table 1) is 0.38, which is significantly lower than the other included studies.

Table F.1: Estimated effects on high school completion from prior studies

Study	Context	Starting ages α	Estimate (p.p.)	Mean
Bastian et al. (2022)	U.S. welfare reform	[12, 16]	-0.005 (0.018)	0.794
Dahl and Gielen (2021)	The Netherlands' DI reform	[4, 14]	0.031 (0.015)	0.744
		[4, 18]	0.028 (0.009)	0.775
		[19, 24]	0.015 (0.010)	0.783
Dave et al. (2012)	U.S. welfare reform	[10, 20]	0.034 (0.017)	0.785
Hao and Cherlin (2004)	U.S. welfare reform	[14, 16]	0.052 (0.385)	0.947
Hernæs et al. (2017)	Norway's municipal reforms to work requirements	[11, 20]	0.022 (0.013)	0.510
Kaestner et al. (2003)	U.S. welfare reform	[14, 16]	0.026 (0.017)	0.868
		16	0.012 (0.019)	0.840
Offner (2005)	U.S. welfare reform	[11, 17]	0.032 (0.008)	0.865
Riddell and Riddell (2014)	Canada's Self-Sufficiency Project	≥ 19	-0.023 (0.008)	0.566

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