1. Introduction

It is well known that parent well-being affects child well-being. Intergenerational transmission patterns are studied intensely as the transmission of disadvantage from parents to their children indicates inequality of opportunities.[1] This paper investigates the intergenerational transmission of participation in means-tested minimum-income protection programs. The purpose of such welfare programs is to lift households from the most pressing economic troubles and to protect the next generation. If, however, welfare programs cause welfare receipt to be passed from generation to generation then these programs do not work properly for the young. Instead, they impose negative externalities and harm the next generation. Here, welfare reforms that reduce parental participation can be beneficial and pay off for the next generation, as well.

Various mechanisms may determine the intergenerational transmission of welfare benefit receipt: after experiencing parental welfare receipt youths may be better informed about application procedures and institutional features; they may be affected by parental role models and be less subject to stigma concerns; they may know less about the labor market, and receive less parental support with respect to with human capital investments or labor market networks, compared to peers who grow up without welfare. If the experience of welfare receipt in the parental household increases the next generation's welfare receipt by any such mechanism the welfare program has negative externalities. Internationally, most studies confirm positive intergenerational correlations of welfare benefit receipt, but the evidence on causal effects is mixed.

Most of the literature on the intergenerational transmission of welfare covers either the United States or Scandinavian countries such as Sweden and Norway. We are the first to offer evidence on the recent intergenerational transmission of welfare receipt for Germany which

[1] For an early survey see, e.g., Black and Devereux (2011). Later contributions on the transmission of earnings, education, and place-based effects are, e.g., Adermon et al. (2021), Blanden (2013), and Chetty and Hendren (2018a, 2018b).

provides an interesting laboratory to study the causal transmission of welfare benefit receipt and the existence of what the literature termed a 'welfare trap'. Germany is a relevant intermediate case as it ranges between the Scandinavian countries and the U.S. with respect to income inequality, the prevalence of poverty, and the generosity of minimum income protection and social spending (OECD, 2019). Germany has much lower poverty rates after taxes and transfers than the U.S. but higher ones than Sweden (Immervoll et al., 2022). The institutions of the welfare state affect the effectiveness of minimum income protection including its intergenerational effects; therefore, a comparison of intergenerational transmission effects across the different national welfare systems (e.g., following Esping-Andersen, 1990, liberal vs. conservative vs. social-democratic systems) can be informative. While the German welfare system protects the poor comparatively well, we do not know yet whether this relatively generous system also succeeds in an intergenerational perspective, i.e., by protecting the next generation from inherited dependence. This is our research question.

We contribute to the international literature on intergenerational welfare transmission by offering evidence from more than three decades of survey data. Long-running longitudinal data on parents and children are required to examine the transmission of welfare across generations. For most countries, such data is not available. The German Socio-Economic Panel Survey allows us to study the transmission of youth welfare experience for individuals born 1969-1991. We can consider parental welfare receipt when the youth is 10-18 years old and investigate its association with the young person's own welfare receipt at ages 25-29. Thereby our analysis uses wider observation windows than much of the prior literature.[2]

[2] Appendix **Table A.1** characterizes the number of years of observations used in prior studies (see columns entitled "Exposure (t0)" and "Own welfare (t1)"). For parental welfare receipt, Beaulieu et al. (2005) can use 10 years of observations. However, several contributions have fewer years: Antel (1992) and Levine and Zimmerman (1996) observe parental welfare receipt only for one year, Edmark and Hanspers (2015) use only three years, and Boschmann et al. (2019) two years of child welfare outcomes in adult age.

In a first step of our analysis, we describe the correlation between parent and child welfare receipt. We study the correlation patterns before and after a major welfare reform which is useful to assess the sensitivity of correlation patterns to institutional change. In contrast to much of the literature which focuses on mother-daughter pairs, we compare outcomes for young men and women and separately evaluate the transmission from fathers and mothers. Our data allow us to describe the relevance of the age at which youths are exposed to parental welfare receipt and thus to determine the most impressionable years (Krosnick and Alwin, 1989). We look into the potential mediation effect of child educational outcomes.

In a second step, we address the potential impact of unobserved heterogeneities that render parental welfare receipt endogenous to the next generation's outcomes. In particular, parental characteristics, such as human capital, attitudes towards work and family, health, addictions, and emotional well-being may affect both generations' welfare receipt and thus can generate spurious intergenerational welfare correlations. To account for this, we consider the empirical strategy developed by Gottschalk (1996) and apply family fixed effects estimation. The two approaches identify different effects and apply different methods to control for the potential endogeneity of parental welfare receipt. If the identifying assumptions hold, applying both methods allows us to get closer to answering the question of whether parental welfare receipt causally affects the welfare receipt of the next generation in Germany.

We find a strong intergenerational correlation in welfare outcomes for our three welfare indicators. The correlations are larger for females than for males. We do not find important differences in welfare transmission from fathers vs. mothers. Exposure to parental welfare receipt at the ages of 10-12 and 16-18 yields stronger correlation patterns than in the 13-15 age window. Comparing the correlation patterns before and after a major welfare reform we obtain inconclusive results and cannot confirm that intergenerational transmission declined post-reform.

Both, the family fixed effects and the Gottschalk (1996) method identify causal effects under certain, yet different assumptions. In our case, both strategies fail to find evidence of a causal impact of parental welfare receipt on child welfare outcomes. Thus, in the German institutional framework, it does not appear to be the experience of parental welfare receipt that drives subsequent child welfare receipt but the correlation of individual characteristics and circumstances in the child and parent household. Levine and Zimmerman (1996) call this situation a 'poverty trap' as opposed to a 'welfare trap'.

These results have clear policy implications as they show that it is not the character of welfare institutions themselves that leaves the offspring of welfare recipients at an elevated risk of welfare receipt. Therefore, any initiative to reduce intergenerational correlation in welfare receipt must not focus on the institutions of the welfare system but address characteristics at the individual and household level and, e.g., improve human capital, health, and labor market involvement.

In the next Section, we summarize the state of the literature. Section 3 then provides institutional background. We outline our empirical approach in Section 4 and describe our data in Section 5. Next, we present the results of our descriptive analyses of intergenerational transmission patterns and of our causal estimates in Section 6. Finally, we draw conclusions in Section 7.

2. Prior Literature

While a broad international literature describes intergenerational correlation in welfare receipt, fewer studies identify causal effects of minimum income programs; Appendix Table A.1 offers a brief characterization of prior contributions and their results. Early contributions applied structural estimation approaches and U.S. survey data (Antel, 1992 and Levine and Zimmerman, 1996) with opposite results. While Antel (1992) concludes that maternal welfare use causally affects daughters' receipt, Levine and Zimmerman (1996) find only a correlation in incomes. Gottschalk (1996) studies U.S. welfare transmission by applying event study methods and confirms a causal relationship between mothers' and daughters' welfare receipt. Pepper (2000) compares alternative empirical approaches and confirms a causal relationship. Hartley et al. (2022) use instrumental variables and difference-in-differences strategies based on regional heterogeneities and find that mothers' welfare receipt increases the probability of their daughters' welfare participation. However, welfare reforms did attenuate the transmission. Finally, Mitnik (2010) studies the intensive margin of welfare receipt; applying matching and family fixed effects estimators he does not find causal effects.

There are only a few studies covering countries outside the U.S.. Beaulieau et al. (2005) exploit administrative data on social assistance receipt in Quebec, Canada, and confirm causal intergenerational effects. Edmark and Hanspers (2015) apply family fixed effects estimation to Swedish register data and find no causal effects. In their study using administrative data from Norway, De Haan and Schreiner (2018) apply bounds analyses with instrumental variables and confirm significant positive causal transmission effects. Boschman et al. (2019) and Cobb-Clark et al. (2022) apply the Gottschalk (1996) approach to Dutch and Australian administrative data, respectively, and find no significant causal effects for social assistance benefits. Overall, the evidence on the causal intergenerational transmission of welfare receipt is mixed.[3]

So far, little research has addressed intergenerational welfare transmission in Germany. While there are a number of studies on income and unemployment transmission, research on welfare receipt is limited. Closest to our analysis is Siedler (2004): using early data from the German Socioeconomic Panel (1984-2002) he investigates intergenerational correlation in social assistance receipt. He focuses on young adults' benefit receipt at age 22 or above, i.e., at

[3] The literatures on the transmission of disability and unemployment benefits apply similar methods with mixed results: Dahl et al. (2014), Dahl and Gielen (2021), and Grübl et al. (2020) confirm causal intergenerational transmission while Ekhaugen (2009) and Maeder et al. (2015) reject it. Bratberg et al. (2015) and Mueller et al. (2017) find causal transmission patterns for some family relationships but not for others.

an age when almost 40 percent of the sample still live in the parental household. He applies regional characteristics as instruments as well as bounds analyses and concludes that parental benefit receipt is exogenous. Therefore, the correlation patterns are interpreted as causal effects.

3. Institutional Background

The German constitution guarantees each resident the right to a 'dignified life': if an individual or household cannot muster the financial means for a 'dignified life', the person or household can demand the support of the state. Different programs provide assistance for groups such as the unemployed, the elderly, the disabled, and the poor.[4] In our analysis, we jointly consider those branches of the welfare state that provide means-tested minimum income support to individuals below retirement age (for a similar strategy see Boschman et al., 2019).

As the welfare state underwent a major reform in 2005, we distinguish between pre- and post-reform institutions (see Figure 1). We consider the receipt of social assistance (*Sozialhilfe*) and unemployment assistance (*Arbeitslosenhilfe*) before the reform and social assistance, unemployment benefit II (UB II), and social money after the reform to capture means-tested minimum income support. We label the combined institutions "welfare" throughout.

Before the reform, individuals could claim means-tested social assistance (*Sozialhilfe*) if their household income, i.e., the combination of earnings or other income, unemployment benefits, or unemployment assistance, was too low to cover the formally defined financial need of the household. Social assistance provided general income support to the employed, the unemployed, and those out of the labor force. In addition, those who had exhausted their insurance-based unemployment benefits and those who were not (yet) entitled to

[4] Poverty is established in a means test: first, the financial need of a given household is formally determined. It consists of administratively fixed amounts for all household members plus housing expenditures (rent and heating). If household income and wealth are too low to cover the thus calculated financial need the household can claim government support. While institutional regulations are gender neutral, females are more affected by poverty than males.

unemployment benefits were eligible for a second, tax-financed and means-tested unemployment assistance (*Arbeitslosenhilfe*). Unemployment assistance replaced up to 57 percent of previous net labor earnings and in most cases was provided without a time limit, i.e., at most until retirement.

On Dec. 24, 2003, the reform law (*Viertes Gesetz für moderne Dienstleistungen am Arbeitsmarkt* called 'Hartz IV') was passed which came into effect January 1, 2005. Its objective was to reduce transfer dependence and shorten the transfer receipt period. Except for shortened payout periods, the unemployment insurance benefit was not affected by the reform; Riphahn and Schrader (2020) study the effect of the reduced payout period. Figure 1 summarizes the institutional changes caused by the reform: the former unemployment assistance and social assistance programs were combined in the new UB II program, a means-tested and tax-financed benefit for those able to work. Since the reform, individuals who exhaust their unemployment insurance benefit (i.e., UB I) or whose UB I claim is insufficient to cover the household's financial need may be eligible for UB II (possibly in addition to UB I). The UB II benefit covers the legally defined minimum income (household financial need). Generally, all individuals - including those who are employed or out of the labor force - can claim UB II if their household passes the means test and if they are physically able to work at least 15 hours per week. Their children or other household members who are not able to work can claim a similar benefit called social money (*Sozialgeld)*. Independent individual claims against the UB II system are possible starting at age 25; then, financial means are compared to their needs for the young person or the person's own core family. Since the reform, the previous social assistance (*Sozialhilfe*) program is available only for those who are not able to work, e.g., due to sickness, disability, or care responsibilities, and who do not have an employable household member.

The main change induced by the reform was the abolition of the unemployment assistance program. Individuals with high prior labor earnings who previously received unemployment assistance faced cuts: their benefit claims declined and in addition, they had to pass more stringent means tests than before. Those who received social assistance before the reform continued to be eligible for UB II as long as they were able to work; for details on the German welfare system see BMAS (2019, 2020).

Figure 2 describes the utilization of the welfare programs over time. The absolute number of social assistance recipients (dashed line) increased since 1980 from below 1 million to almost 3 million individuals in 2004.[5] Similarly, the number of unemployment assistance recipients (dotted line) increased substantially over time - since 1991 covering East Germany, as well. The thin dotted grey line presents the sum of social assistance and unemployment assistance beneficiaries; as some individuals may have benefitted from both programs the addition generates an overcount. The unemployment assistance program disappeared in 2005. Immediately after the reform, the number of unemployment benefit II (UB II) recipients (bold black line) surpassed 5 million basically continuing where the sum of the two prior benefits left off. The number declined in subsequent years. The number of social money recipients was constant at about 0.8 million and reflects individuals in the household of UB II recipients who cannot work, i.e., mostly children. After the reform, the social assistance benefit was used only by individuals unable to work at least 3 hours per day and dropped. The figure suggests that the joint consideration of the two means-tested programs of social and unemployment assistance before and the UB II program after the reform generates a plausible reflection of welfare receipt.[6]

[5] This covers welfare recipients who live independently (*Hilfe zum Lebensunterhalt außerhalb von Einrichtungen*). The group of handicapped individuals was supported by a different social assistance program (*Hilfe in besonderen Lebenslagen*) and is not reflected in Figure 2.

[6] There is substantial non-take-up in the German welfare system of more than 40 percent of the eligible population. However, this is moderate by European comparison (Eurofund, 2015, Table 1). Also, other social policy programs in Germany feature even higher non-take-up rates (Bruckmeier and Wiemers, 2018, Table 3). These authors point to a complicated benefit structure and argue the expected utility of the entitlements as well as information costs and stigmatization explain take-up behavior. For recent evidence see, e.g., Bruckmeier et al. (2021), Bruckmeier and Wiemers (2017), and for the pre-reform welfare system Riphahn (2001).

4. Empirical Model and Methods

4.1 The model

We are interested in whether the welfare receipt of young adults, i.e., the child generation, is associated with and potentially caused by experiencing the welfare receipt of their parents. We follow the previous literature and model child i's welfare receipt (W_i^C) in observation period t1 as a function of parental welfare receipt (W_i^P) in an earlier observation period t0:

$$W_i^C \quad = W_i^P \beta_0 + \varepsilon_{0i}^C . \qquad (1)$$

The estimate of coefficient β_0 reflects the unconditional intergenerational correlation in welfare receipt. As this correlation may be affected by various factors, we consider an extended specification that controls for a set of individual and household level covariates (X) such as age, gender, migration background, and region of residence:

$$W_i^C \quad = W_i^P \beta_1 + X_i \gamma + \varepsilon_{1i}^C , \qquad (2)$$

where β and γ are coefficients to be estimated. The estimate of β_1 reflects the conditional correlation of welfare receipt across generations. While it may not provide the causal transmission effect, it quantifies the overall association between parent and child outcomes. It is interesting to compare this association for different subgroups and for different types of exposure.

Estimates of β_1 can be interpreted as a causal effect only if parental welfare receipt is exogenous, i.e., uncorrelated with the error term ε_{1i}^C. However, this is unlikely if parent and child welfare participation are affected by unobserved heterogeneities (e.g., tastes, preferences, biological factors, abilities, or unobserved regional characteristics). Let parental welfare receipt be modeled by

$$W_i^P \quad = X_i^P \delta + \varepsilon_i^P . \qquad (3)$$

Then, the error terms for child and parent welfare receipt may follow

$$\varepsilon_i^C \quad = \alpha_i^C + \mu_i^C \qquad (4)$$

$$\varepsilon_i^P \quad = \alpha_i^P + \mu_i^P , \qquad (5)$$

where μ_i^C and μ_i^P are uncorrelated random error components. If there are unobserved family characteristics we expect corr(α_i^C, α_i^P) ≠ 0. This correlation causes a bias in the OLS estimate of β in equations (1) and (2): the coefficient estimate mixes the causal effect of experiencing parental welfare receipt in period t0 and the effects of shared family unobservables.

In the first step of our analysis, we estimate the intergenerational correlation of welfare receipt using two model specifications. In a basic specification, we do not consider a detailed set of control variables. In an extended specification, we account for heterogeneity along individual and parental background dimensions. In particular, we control for characteristics of the individual (year of birth, gender, immigration background, and parity, i.e., the rank position in the family birth order), characteristics of parents (year of birth, parental education) and household characteristics at age 17 of the individual (household size, number of children in parental household, federal state of residence). Holding these dimensions constant enables us to describe conditional correlation patterns that are of more general validity than the results presented in the basic specification.

In the second step of our analysis, we apply two separate strategies to identify two different parameters that reflect causal intergenerational effects. Such causal effect estimates inform about the existence of "family welfare cultures" (Dahl et al., 2014), i.e., situations where the welfare receipt of one generation causes welfare participation of the next generation. A variety of potential mechanisms may determine such intergenerational state dependence: they can relate to parents as role models for their children, the impact of welfare receipt on family beliefs, norms, tastes, preferences, and attitudes toward work and welfare, the susceptibility to stigma effects, the availability of information on welfare institutions and the lack of information on the labor market; finally, parental welfare receipt may affect child educational attainment, e.g., by means of self-esteem, stigmatization, role-model effects (Boschman et al., 2019). The literature applies different approaches to identify this causal effect. While we cannot take advantage of an exogenous shock affecting parental but not child welfare dependence, we exploit two identification strategies that have been used in the literature before and which we now discuss in turn.

4.2 Family fixed effects

Numerous studies of the intergenerational transmission of program participation apply a sibling or family fixed effects approach (see, e.g., Bratberg et al., 2015, Solon et al., 1988, Levine and Zimmerman, 1995, 2005, Ekhaugen, 2009, Edmark and Hanspers, 2015, Mitnik, 2010, and Mueller et al., 2017). Here, the endogeneity of parental welfare use is purged from equation (2) by controlling for family fixed effects in a sample of siblings. If different siblings pass through the family household at different points in time where some do and others do not experience parental welfare receipt or where siblings differ in the age at which they experience parental welfare receipt then comparing their adult outcomes allows us to account for family constant effects. The identifying assumption is that the family background effect is time-invariant. If, however, the relevant family unobservables or their effects are time-varying then the estimator does not generate an unbiased estimate of the causal effect. For example, if parental health worsens over time and intensifying family financial need affects only one of the siblings this is not accounted for by the estimator. To account for such mechanisms, we offer robustness tests where we consider only families where the youngest sibling was exposed to parental welfare receipt but not the older sibling. Also, the estimator is not reliable if siblings differ in unobservable ways that might affect early parental welfare status. In a situation of, e.g., early child health problems parental welfare receipt may be determined by child characteristics instead of vice versa. A final weakness of the approach is that only families with at least two children can be used in the fixed effects estimation. To address this problem, we offer comparisons of the OLS results for the different subsamples.

4.3 The Gottschalk (1996) method

The method introduced by Gottschalk (1996) has been applied frequently (Corak et al., 2004, Ekhaugen ,2009, Maeder et al., 2015, Mueller et al., 2017, Boschman et al., 2019, Cobb-Clark et al. 2022). The key idea is that the total correlation between parental welfare receipt (in period t0) and subsequent child welfare receipt (in period t1) comprises causal and non-causal elements. In contrast, the correlation between parental welfare receipt observed after child welfare receipt (in t2) and child welfare receipt (in t1) entails only non-causal correlation. If both correlation measures, i.e., between period t0 and t1 and between period t2 and t1, are identical then there exists no causal effect from parent (t0) to child receipt (t1) and all within-family correlation in welfare receipt is spurious and due to unobserved heterogeneity. The causal element of the initial correlation can be estimated as the difference between the two correlation estimates. Consider the model

$$W_i^C = W_i^{Pt0}\,\beta_2 + X_i^C\,\gamma + W_i^{Pt2}\,\beta_3 + \varepsilon_{3i}{}^C \quad , \qquad (6)$$

where W_i^C is person i's own welfare receipt as an adult in period t1, W_i^{Pt0} describes parental welfare receipt during i's childhood, and W_i^{Pt2} describes parental welfare receipt after W_i^C is measured. The Gottschalk method uses the difference $\beta_2 - \beta_3$ as an estimate of the causal effect, i.e., after purging pure family-related correlations from the initial estimate.

This method explicitly accounts for the potential endogeneity of parental welfare as a regressor in the child welfare model. This endogeneity is interpreted as an omitted variable characterizing all household-specific unobservables that are constant over time for parent and child (e.g., norms, values and attitudes, health, shared regional and labor market experiences). We measure period t2 parental welfare outcomes when the child is aged 30-35 and control for these in our basic and extended specifications. In this setting, the identifying assumptions are that later parental welfare receipt cannot cause earlier child welfare receipt and that later parent welfare receipt is not caused by earlier child welfare receipt. If, e.g., children with welfare receipt support later parental applications this yields an overestimate of β_3 and a downward bias of the estimated causal effect. In this situation, we may underestimate the causal effect by overstating the family-specific correlation between child outcome and late parent outcomes reflected in β_3; this could also happen if both are jointly affected by regional effects that are correlated over time, e.g., after a slump in the business cycle or a pandemic. Also, changes in the administration of the welfare program can bias the estimates. If eligibility requirements become more stringent over time the selection of parents into welfare receipt is not time constant. If only the neediest parents receive welfare benefits in t2 then the correlation between parent and child welfare receipt may be stronger for future than for past welfare receipt and the causal effect is underestimated.

Additionally, we must assume that families for whom late parental welfare receipt is observable in the data do not differ from families for whom this long-run outcome is missing (we offer comparative descriptive statistics below). This assumption could be violated if parental welfare receipt in period t2 is associated with survey response behavior; differences may result if, e.g., those on welfare have more leisure to respond or if, to the contrary, stigma effects inhibit their response (Lillard and Panis, 1998, Rendtel, 1990). Similarly, mortality differences could bias results. However, in our data, this is unlikely as parents of all groups are in only their 50s. Also, within-family correlation patterns observed for parents with and without late welfare receipt must be identical. Otherwise, the estimate for β_3 would not capture the relevant correlation. Overall, the method may tend to underestimate causal effects.

The two approaches differ in data requirements. The family fixed effects method does not require parental observations after age 25-29 of the child. The Gottschalk method can use observations of children without siblings. Both approaches assume that the family unobservables which may generate a biased estimate in the uncorrected OLS approach are time constant. The fixed effects approach assumes that the unobservable family effect can be differenced out from the linear model. The Gottschalk approach aims at measuring the exogenous causal part of the overall correlation measure using differences of coefficient estimates. The two identification strategies are sensitive and robust to different violations of identifying assumptions. In the end, it is of course possible that both methods yield misleading results. However, we are not aware of any one mechanism that would cause a bias for both methods in the same direction. Therefore, we offer evidence from two independent approaches.

5. Data

We apply data from the German Socio-Economic Panel Study (SOEP) (Goebel et al., 2019). The SOEP is an annual household panel survey which has been running continuously since 1984. We use survey waves from 1984 through 2017. The data is particularly suitable for our purposes as it follows participants and the members of their households over time. Thus, it allows us to connect information on individual welfare receipt as an adult with information on parental welfare receipt in prior survey waves when the individual was a child.[7] Ideally, we would compare the full life course patterns of welfare receipt of parents and children. However, as is common with surveys the data limit observability to a few survey years.

The earliest legal age of individual welfare receipt as an adult is 25. We use an observation window of 5 years (age 25-29) and consider all individuals for whom there are at least two panel observations available in this age window (period t1). Most individuals (1,390 or 58 percent of the sample) are observed for the full five-year period. In addition, 329 / 305 / 379 individuals are observed for 2 / 3 / 4 years within the 25-29 age window. We then gather information on parental welfare receipt when our individuals were aged 10-18 (period t0). We consider all those in our sample for whom information on parental welfare receipt is available for at least four calendar years when they were aged 15-18.[8] With these sample restrictions, our

[7] As is commonly acknowledged (see, e.g., Gottschalk, 1996, Pepper, 2000, Hartley et al., 2022) any panel attrition that is correlated with welfare participation could cause estimation bias. While the SOEP data is generally used in intergenerational mobility studies (see, e.g., Zumbuehl et al., 2021, Maasoumi and Trede, 2001, Angelini et al., 2018), this type of bias cannot be excluded, here.

[8] Information on parental welfare receipt at age 15-18 must be available at a minimum to be included in the sample. If, in addition, information on parental welfare receipt at younger ages (going back to age 10) is available we also consider that in coding our welfare indicators. In our data 301 individuals (12.5 percent of the sample) are observed with the minimum number

main analysis sample comprises 2,403 different individuals for whom information on their own welfare receipt at age 25-29 as well as on parental welfare receipt at age 15-18 is available (birth cohorts 1969-1991). Relative to prior studies (see Table A.1) our coverage of 4-9 years in period t0 and 2-5 years in period t1 should generate reliable welfare indicators and limit measurement error.

When we apply the family fixed effects approach, we use only individuals with a sibling in the data. We can use 414 sibling pairs, 73 triplets, and even 27 families with four or more children in the data. Overall, the family fixed effects sample entails 1,161 different individuals from 514 different families whom we observe in t0 and t1.

When we apply the Gottschalk (1996) approach we focus on the subsample of individuals for whom parental welfare receipt is additionally observed when the child is aged 30-35 (period t2). As we require at least one valid parental welfare indicator in that age bracket this limits the relevant birth cohorts to 1969 to 1987. With this restriction, our sample entails 1,221 different individual observations whom we observe in t0, t1, and t2.

We consider three measures of self-reported welfare receipt for both parent and child observations: a binary indicator of the incidence of welfare receipt, a continuous measure of the number of years for which welfare receipt is observed, and - given that we observe individuals and parents for varying numbers of years - a measure that reflects the share of observation years for which welfare was received. Even though we take advantage of repeated observations per person to code our welfare indicators we use the data cross-sectionally with one observation per person. Panel A of Table 1 shows descriptive statistics for our welfare measures, where the outcomes measured in period t1 (child age 25-29) are dependent variables and the outcomes measured for period t0 (parental welfare when the child is aged 10-18) are key explanatory variables or treatment indicators in our analyses. We find that about 13 and 14 percent of young

---

of 4 years of information on parental household welfare receipt, and 231 / 212 / 238 / 225 / 1,196 observations with 5 / 6 / 7 / 8 / at least 9 years, respectively.

adults (in t1) and parents (in t0) ever received means-tested welfare benefits, respectively. Even though young adults are observed at most for 5 years in period t1 (age 25-29) and parents at most for 9 years in period t0 (age 10-18 of the child) the duration of benefit receipt is similar in both groups with 0.6 years among young adults (in t1) and 0.4 years among parents (in t0). This yields shares of around 6 percent of the observed annual observations.

Panel B of Table 1 shows correlation coefficients for the three welfare measures for parents and their children. Within each generation, the three different welfare measures are highly correlated. In contrast, the intergenerational correlation is weaker with .19 for the welfare incidence, .18 for the number of years, and .24 for the share of observation years on welfare. The data yield the expected positive intergenerational correlation of welfare receipt.

We define a parsimonious basic and an extended specification to capture the vector X of controls (see equation 2) in our multivariate analyses. As we do not observe welfare outcomes in all age years for every individual, we define a vector of missing value indicators. We control for these indicators in the basic specification in order to avoid biases due to selective survey participation; in particular, we use 5 missing indicators for child welfare outcomes at ages 25-29 and 5 indicators for parental outcomes at ages 10-14 of the child. The youngest birth cohort in our data cannot be observed at age 27-29 by construction. It contributes only two years for the age bracket 25-29. In addition to the missing value indicators the reduced number of annual observations is accounted for by controls for birth cohort. The basic specification controls for parental welfare receipt and the missing value indicators and measures the unconditional intergenerational correlation of welfare receipt.

In our extended specification, we control for time-constant characteristics that might be correlated with parental welfare receipt. In particular, we control for characteristics of the individual, the parents, and the household when the youth was age 17. We control for child gender, year of birth, immigration background, and parity. We also consider parental year of birth and indicators of parental education. Finally, we consider household size, the number of children in the parental household, and the federal state of residence when the individual was age 17. The birth cohort controls account for secular time trends and regional heterogeneities. In our main models, we do not consider child education because it may be a mediator of the transmission of welfare receipt. However, we test whether adding child education modifies the observed patterns of intergenerational welfare transmission.

In Table A.2, we present descriptive statistics on explanatory variables for the full sample and separately by welfare receipt in period t1 and period t0. The covariates describing parental welfare benefit receipt in period t2 in the Gottschalk analyses are described in Table A.7. We find that compared to non-recipients, welfare recipients are significantly more likely to be female, born with higher parity, to younger parents, and grew up in larger households. Patterns are similar for those who experienced parental welfare receipt while growing up. In this group, we also observe a significantly higher migration background. Child and parent welfare receipt are associated with lower parental secondary education; Table A.3 provides descriptive statistics for our explanatory variables for the main sample.

6. Results

6.1 Baseline results

Panel A of Table 2 shows our OLS results based on the basic specification. The first set of results confirms the findings reported in Table 1 and indicates that the correlations between parent and subsequent child welfare receipt are positive and highly statistically significant. Having ever experienced welfare receipt in the parental household in period t0 is associated with an increased probability of own welfare receipt as a young adult by 18.7 percentage points. This correlation is large relative to the mean propensity of own welfare receipt of about 13 percent. Similarly, the continuous welfare indicators confirm a strong and significant intergenerational correlation of welfare receipt.

In panel B of Table 2**,** we present the estimates of the extended specification. The controls account for some of the intergenerational correlation in welfare receipt: the coefficients decline in magnitude but remain highly statistically significant. Conditional on individual, parent, and household characteristics young individuals are about 14 percentage points more likely to receive welfare when their parents received welfare during their teen years, a substantial difference; Table A.4 in the Appendix presents the full set of estimation results. The estimate in column 2 shows an increase in the number of own years of welfare experience by about 0.16 for each year of parental welfare receipt. Column 3 suggests that the share of observed years on welfare as an adult is associated with a significant increase of 21 percentage points when parents were on welfare for the full observation period. The reduced intergenerational correlations in Panel B compared to Panel A suggest that the control variables are indeed correlated with the propensity to receive welfare benefits: the intergenerational correlation of welfare receipt is smaller within demographic groups than on average. Below we inspect these heterogeneities in greater detail.

Next, we investigate whether the association between child welfare receipt and the duration of parental receipt is indeed linear. We separately regress the extensive margin of child welfare receipt (i.e., ever welfare in period t1) on having experienced at least x number of years of parental receipt, where x runs from 1 to 9. Figure A.1 in the appendix shows the results for both specifications: the propensity to ever receive welfare increases with the number of years of parental welfare receipt experienced, however, confidence intervals are wide. In Figure A.2 we describe the development of correlation patterns as estimated by the basic regression specification separately for subsequent birth cohorts. We use rolling regressions on three neighboring birth cohorts. The patterns are similar for all three outcomes with a peak in correlations in the early 1970s and a significant positive trend for more recent birth cohorts.

6.2 Heterogeneity by child and parent gender

Next, we follow the literature and investigate whether intergenerational welfare correlation differs for young men and women; descriptive statistics yield higher welfare receipt among females than males.[9] We apply different strategies to describe the gender-specific patterns in our data. First, we re-estimated the extended specification described in Table 2 and additionally interacted parental welfare receipt with child gender. Panel A of Table 3 shows that the correlation between parent and child welfare receipt is substantially but mostly insignificantly higher for females. Panels B-C of Table 3 show separate estimations of the basic and extended specifications by gender and confirm higher intergenerational correlations for females than males across all welfare indicators. This agrees with the literature (e.g., Dahl and Gielen, 2021, Hoynes et al., 2016). One mechanism may be that the single parenthood risk is larger for females and can be transmitted across generations (Musick and Mare, 2004). Also, role model expectations and social norms may contribute to gender differences in economic independence.

In Panels D and E of Table 3, we present separate estimates based on whether maternal or paternal welfare receipt was observed during childhood.[10] Since any differences might be due to living with only one parent rather than to the parental gender in particular, we additionally control for single parenthood in these specifications. Our results yield only minor differences in parent-specific correlation patterns for the basic specification; in separate estimations (not presented to save space) we observe larger intergenerational correlation coefficients if maternal welfare receipt was experienced in a single-parent household. In separate estimations, we considered child gender interaction terms in the estimations for fathers'

[9] In our sample, 14 and 11 percent of females and males ever receive welfare, respectively. For females, we observe on average 0.62 and for males 0.48 years of welfare receipt. The differences in the parent generation are small and insignificant (see Table A.5 for descriptive statistics by gender).

[10] As welfare is provided at the household as opposed to the individual level the welfare outcome in our data was identical for 88 percent of parent couples. The gender-specific effects are identified from separated couples or single parents where the children live with only one of the two parents. In very few cases (28 for mothers and 119 for fathers) we have no information on the person-specific welfare history. There are no major differences by parent gender in extended specification. Results are available upon request.

and mothers' welfare outcomes (see panels F and G of Table 3). These results indicate positive but again mostly insignificant coefficient estimates confirming the stronger correlations for female children but no major differences by parent gender.

Overall, the findings confirm patterns found in other studies: using data for Germany, Mueller et al. (2017) obtained stronger intergenerational unemployment correlations for daughters than for sons. Using Dutch data, Boschman (2019) also found the correlation patterns for maternal and paternal social assistance receipt to be similar.[11]

6.3 Heterogeneity by age of exposure

Numerous contributions discuss the relevance of a child's age at exposure to intergenerational transmission effects. Bratberg et al. (2015) and Dahl and Gielen (2021) study the relevance of age at exposure with respect to the transmission of parental disability.[12] Carneiro et al. (2021) studied the connection between the timing of parental income shocks and the next generation's human capital outcomes. Conditional on household permanent income they find that children benefit most from positive income shocks during age 0-5 and 12-17.

Edmark and Hanspers (2015) and Hartley et al. (2022) compare the relevance of parental welfare receipt across child exposure ages. The former find the strongest intergenerational correlation if the young generation was exposed at age 17-19 and argue that this may reflect role-model or network-related effects that are strongest in the formative years of the late teens. Hartley et al. (2022) find larger correlations for older ages at exposure, i.e., at ages 10-14

[11] The studies on intergenerational transmission of disability benefits in Norway disagree on this issue: while Dahl and Gielen (2021) find larger transmissions from mothers, Bratberg et al. (2015) observe larger effects for fathers. While Dahl and Gielen (2021) find no heterogeneity by child gender, Bratberg et al. (2015) observe larger effects for daughters than sons.

[12] Bratberg et al. (2015) compare child age categories from below 15 to up to 40 and do not find clear heterogeneities for exposure at younger ages. Dahl and Gielen (2021) compare effects for children up to age 14, up to age 18 or at age 19 plus. They find larger intergenerational spillover effects if the younger generation is young at the time of parental treatment.

through 13-17. The authors similarly suggest that learning effects increase when children experience welfare receipt at older ages.

In our analysis, we consider exposure to parental welfare receipt at ages 10-12, 13-15, and 16-18. As our survey does not allow us to go back in time for all individuals, we start out with age-group-specific estimations which vary in sample size. Panel A of Table 4 shows the results. Across all welfare indicators, we find stronger correlations for the youngest and oldest age groups and the smallest correlations for the middle age group of 13-15-year-olds. In order to compare the age-specific correlations for a given yet smaller sample we pooled the three age-group-specific measures in panel B of Table 4 and estimated the correlation patterns in one joint model. We continue to find the weakest correlation for the middle age group and larger impacts for the youngest and the oldest group. Table 4 shows results for the basic specification only. The results are similar when the extended specification is estimated (available upon request). The finding of larger coefficients for the oldest group agrees with the literature. The strong correlation for 10-12-year-olds is somewhat surprising. Possibly it is related to the German secondary schooling system where at around age 10 important tracking decisions are taken. If these decisions are negatively affected by financial problems in the parental household the effects may reduce average human capital with long-run effects. Boschman et al. (2019) consider the heterogeneity of correlation patterns by recency of parental welfare receipt as a potential indicator of the relevance of information transmission. That we find correlations of similar magnitude for 10-12 and 16-18-year-olds does not support the idea of recent information as an important mediator.

6.4 Pre vs. post reform patterns

During our observation period, the German welfare program underwent an important reform in 2005 that is intensely debated to this day. The reform aimed to activate welfare recipients who are able to work (see Section 3). It increased job search monitoring and it reduced benefits for some long-term unemployed. We describe intergenerational correlation patterns before and after the reform; for analyses of pre- and post-reform state dependence in welfare receipt at the individual level over time see, e.g., Riphahn and Wunder (2013, 2016). We consider individuals who reached age 29 before 2005 (birth cohorts 1969-1975) to be subject to the pre-reform welfare regime and those who turned 25 in 2005 and after (birth cohorts 1980-1991) to be affected by the reform. Appendix Table A.6 describes the two groups' welfare outcomes and intergenerational correlation patterns. While the welfare outcomes for the two subsamples in t1 are similar, surprisingly, we find much higher parental welfare receipt for the post reform group (see Panel A). Panel B additionally shows higher intergenerational correlations for the post-reform of, e.g., .24 versus .15 for the 'ever welfare' outcome. This may reflect aggregate trends to higher welfare use over time (see Figure 2).

Table 5 shows the estimation results for both subsamples with the basic and extended specifications in Panels A and B, respectively. Panels C and D offer estimation results on the pooled samples with an interaction term. In Panel A, estimation results for the first two welfare outcomes yield that the intergenerational correlation did not change substantively after the reform. This pattern is not supported by the outcomes reported in Panel B, where correlations declined in the post-reform period; however, the interaction term estimates in Panels C and D are imprecise. In contrast, correlation patterns for the third outcome increased substantially after the reform (see Panels A and B). This is confirmed by the statistically significant estimate of the interaction term coefficient in Panel C. Overall, these results are inconclusive: we find neither strong evidence of increasing nor of decreasing correlation patterns.

6.5 Relevance of mediator variable: child education

It is possible that child education acts as a mediator of the parent-child connection in welfare receipt. If parental welfare receipt negatively affects child educational attainment (e.g., via role-model effects, stigmatization in school, low parental self-esteem, or residential instability) then low child human capital, i.e., cognitive and possibly non-cognitive skills, may limit labor market opportunities and eventually economic independence. We can test whether child educational attainment is a mediator by adding child educational outcomes as a control variable in the estimations shown in Table 2 where they had been omitted so far to avoid endogeneity issues. If the intergenerational correlation declines once we condition on child education then mediation effects are likely which may point to useful policy strategies.

We consider four indicators of the child's highest educational degree obtained. Table 6 shows the estimated correlation patterns that result after adding the child education controls to the set of covariates in the basic and extended specifications. All coefficient estimates continue to be positive and highly statistically significant. However, in comparison to the results in Table 2 they are smaller in magnitude by about 20 percent. Thus, a considerable part of the intergenerational correlation may operate via attenuated educational attainment of children in welfare-receiving households. This agrees well with the literature (see Boschman et al., 2019 or Bubonya and Cobb-Clark, 2021).[13]

6.6 Causal estimation approaches - family fixed effects

The correlations investigated so far cannot generally uncover causal effects. To get closer to causal effect estimation we take advantage of siblings from the same family in the family fixed effects model. This allows us to account for time-constant family unobservables. If these are the only biasing factors then the family fixed effects models provide causal effects (see the discussion in Section 4).

Our family fixed effects sample offers information on 1,161 siblings from 514 different families. Panel A of Table 7 presents the baseline correlation estimates for the basic and

[13] Child education differs significantly for the groups with and without parental welfare receipt. Those with parental welfare receipt are more than twice as likely to be in the lowest (shares of 25 vs. 12 percent) and less than half as likely to be in the highest category (12 vs. 28 percent).

extended specifications for this particular subsample. The results are rather similar to those of the full sample in Table 2. Panel B of Table 7 shows the coefficient estimates on parental welfare when we apply the family fixed effects estimator to both the basic and extended specifications: the positive significant correlation coefficients do not hold up to fixed effects controls. This result does not support the existence of causal intergenerational treatment effects.

As a robustness test, we show the fixed effects results when those families are omitted from the sibling sample where only the older sibling experienced parental welfare receipt. In these cases, the mechanisms that generate intergenerational transmission such as reduced stigma, availability of institutional information might persist in the family even though a welfare receipt is not observed for the younger sibling. Panel C in Table 7 yields that the results obtained so far, i.e., no significant positive effects, are robust to this additional test.[14]

The finding of no causal effects agrees with the family fixed effects estimations for unemployment benefit transmission in Ekhaugen (2009) and Mueller et al. (2017), and for the transmission of maternal (not paternal) benefit transmission in Bratberg et al. (2015). In their fixed effects analyses, Edmark and Hanspers (2015) even obtained negative intergenerational transmission results for welfare receipt in Sweden. The authors argue that either children of welfare recipients are particularly eager to avoid welfare or the coefficients on parental welfare receipt capture other differences between siblings that correlate with the welfare experience.

6.7 Causal estimation approaches - Gottschalk estimation

[14] We pursued two strategies for the robustness test after we determined the set of families where parental 'ever-welfare' outcomes varied across siblings; out of 1,161 children in 514 families, only 103 children in 38 families had varying parental outcomes across siblings. Our first strategy omitted 49 observations from families where already the first-born child experienced parental welfare receipt (estimating with N=1,112 observations) because the one-time experience may affect the family characteristics permanently. In our second strategy, we omitted observations from families where only the first-born child experienced welfare receipt (see Panel C of Table 7). The results hardly differed between the strategies. Nevertheless, we cannot exclude the possibility that families with time-varying benefit receipt differ in unobservable characteristics from other families.

As our second approach to approximate causal welfare transmission effects, we apply the procedure developed by Gottschalk (1996) as characterized in Section 4.3 above. Here, we account for family-specific unobservables that might otherwise bias causal effect estimation by controlling for parental welfare receipt in the period after observing the second generation's welfare receipt. As we do not observe these outcomes for all families the estimation with an additional parental welfare control can only be performed on a subsample. Appendix Table A.7 shows descriptive statistics for the full sample and the Gottschalk subsample for whom information on late parental welfare receipt is available. Not surprisingly, individuals in the Gottschalk subsample and their parents are on average 2 years older than the main sample. The child generation is insignificantly more likely to use welfare (incidence in t1 of 14.3 vs. 12.8 percent) than the main sample whereas the parents are less likely to use welfare in t0. Overall, the subsample characteristics do not appear to differ in important ways (significant age differences are by construction). Next, we investigate whether the correlation patterns in the Gottschalk subsample reflect our results from Table 2. Panel A of Table 8 shows the basic and extended specification estimates for the Gottschalk subsample. The coefficient estimates do not differ in important ways from prior results.

Panels B and C of Table 8 show the estimation results of the actual Gottschalk estimation approach for the basic and extended specifications. Each individual parental welfare receipt indicator yields positive and significant coefficient estimates. The row labeled "Gottschalk effect" in each panel presents the difference between the two parental effects as estimated based on equation (6). In no case do we obtain significantly positive differences which would be indicative of causal intergenerational transmission effects. Therefore, the finding of a lack of causal transmission from the fixed effects estimations is confirmed with the Gottschalk approach. In fact, the overall effects on the incidence of welfare receipt even turn out significantly negative.

While the negative total effect is surprising, it reflects the findings of other authors who studied possibly heterogeneous policy programs in different countries: Ekhaugen (2009) and Mueller et al. (2017) similarly obtained negative estimates for the transmission of unemployment benefits. Also, Boschman et al. (2019) find negative effects for social assistance and disability programs. These authors argue that it is not the experience of the welfare program itself that causes the next generation's participation. Instead, family-specific characteristics such as norms and attitudes that are not attached to actually receiving the benefit may drive the intergenerational correlations. The same patterns appear to hold for our data.

7. Conclusions

The international literature discusses whether experiencing parental welfare receipt in childhood or adolescence is correlated with and causally determines own welfare receipt later in life. This is an important policy question because intergenerational transmission of welfare receipt indicates a failure of welfare programs: government support does not succeed in lifting families out of poverty and may even impose negative externalities on the next generation.

We take advantage of a long-running household panel (SOEP) survey to study the intergenerational transmission of welfare receipt for the case of Germany. Comparative research suggests that Germany offers a relatively generous welfare system; however, its intergenerational characteristics have not been investigated before. The richness of our data allows us to add informative analyses of intergenerational correlation patterns to the literature.

We consider three welfare indicators and find strong intergenerational correlation patterns. The correlations are larger for recent than for older birth cohorts and for females than for males. We do not find important differences in the transmission of welfare from fathers vs. mothers. Exposure to parental welfare receipt at the ages of 10-12 and 16-18 yields stronger correlation patterns than exposure in the 13-15 age window. Child educational attainment appears to be a mediator between parent and child welfare receipt which may offer an opportunity for policy interventions.

We use family fixed effects and the Gottschalk (1996) method to go beyond correlation analyses and to identify causal effects of parental welfare receipt. Both strategies identify causal effects under specific, yet different assumptions and therefore complement each other. Interestingly, both strategies fail to find evidence of a causal impact of parental welfare receipt on child welfare outcomes. Thus, we do not find evidence that it is the experience of parental welfare receipt itself and a 'welfare culture' (Dahl et al., 2014) that drives subsequent child welfare receipt. Instead, the correlation of individual characteristics and circumstances in the child and parent household seems to determine transmission patterns. This suggests that it is not the character of welfare institutions themselves that leaves the offspring of welfare recipients at an elevated risk of welfare receipt. This is a highly policy-relevant finding. It clarifies that any initiative to reduce intergenerational correlation in welfare receipt must not focus on the institutions of the welfare system but more plausibly on characteristics at the individual and household level and, e.g., improve human capital, health, and labor market engagement.

Our conclusions are subject to strong identifying assumptions and should be reinvestigated when larger samples are available. It seems worthwhile to direct future research to study the determinants and relevance of youth educational attainment, which might be malleable by public policy. Also, it is important to re-analyze any changes in intergenerational correlation after the reform of the welfare system. Finally, we agree with Hartley et al. (2022), who point out that in a situation of low benefit take-up, intergenerational spillovers and correlations can be a good thing if they reduce non-take-up.

Bibliography

Adermon, Adrian, Mikael Lindahl, and Mårten Palme (2021), Dynastic Human Capital, Inequality, and Intergenerational Mobility, American Economic Review 111(5), 1523-1548.

Angelini, Viola, Marco Bertoni, and Luca Corazzini (2018), Does paternal unemployment affect young adult offspring's personality? Journal of Human Capital 12(3), 542-567.

Antel, John J. (1992), The intergenerational transfer of welfare dependency: some statistical evidence, Review of Economics and Statistics 74(3), 467-473.

Beaulieau, Nicolas, Jean-Yves Duclos, Bernard Fortin, and Manon Rouleau (2005), Intergenerational reliance on social assistance: Evidence from Canada, Journal of Population Economics 18, 539-562.

Black, Sandra and Paul J. Devereux (2011), Recent Developments in Intergenerational Mobility, in: Ashenfelter O. and D. Card (eds.), Handbook of Labor Economics Vol. 4, North Holland, Amsterdam, 1487-1541.

Blanden, Jo (2013), Cross-country rankings in intergenerational mobility: a comparison of approaches from economics and sociology, Journal of Economic Surveys 27(1), 38-73.

BMAS (Bundesministerium für Arbeit und Soziales) (2019), Übersicht über das Sozialrecht, Bonn.

BMAS (Bundesministerium für Arbeit und Soziales) (2020), Social Security at a Glance 2020, Bonn. https://www.bmas.de/SharedDocs/Downloads/DE/Publikationen/a998-social-security-at-a-glance-total-summary.pdf?__blob=publicationFile&v=1 [last access: July 20, 2021]

Boschman, Sanne, Ineke Maas, Marcus H. Kristiansen, and J. Cok Vrooman (2019), The reproduction of benefit receipt: disentangling the intergenerational transmission, Social Science Research 80, 51-65.

Bratberg, Espen, Øivind Anti Nilsen, and Kjell Vaage (2015), Assessing the intergenerational correlation in disability pension recipiency, Oxford Economic Papers 67(2), 205-226.

Bruckmeier Kerstin and Jürgen Wiemers (2017), Differences in welfare take-up between immigrants and natives: a microsimulation study, International Journal of Manpower 38(2), 226-241.

Bruckmeier Kerstin and Jürgen Wiemers (2018), Benefit Take-Up and Labor Supply Incentives of Interdependent Means-Tested Benefit Programs for Low-Income Household, Comparative Economic Studies 60, 583–604.

Bruckmeier, Kerstin, Regina T. Riphahn, and Jürgen Wiemers (2021), Misreporting of program take-up in survey data and its consequences for measuring non-take-up: new evidence from linked administrative and survey data, Empirical Economics 61, 1567-1616.

Bubonya, Melisa and Deborah A. Cobb-Clark (2021), Pathways of Disadvantage: Unpacking the Intergenerational Correlation in Welfare, Economics of Education Review 80, 102066.

Carneiro, Pedro, Italo López García, Kjell G. Salvanes, and Emma Tominey (2021), Intergenerational Mobility and the Timing of Parental Income, Journal of Political Economy 129(3), 757-788.

Chetty, Raj and Nathaniel Hendren (2018a), The impacts of neighborhoods on intergenerational mobility I: childhood exposure effects, Quarterly Journal of Economics 133(3), 1107-1162.

Chetty, Raj and Nathaniel Hendren (2018b), The impacts of neighborhoods on intergenerational mobility II: county level estimates, Quarterly Journal of Economics 133(3), 1163-1228.

Cobb-Clark, Deborah A., Sarah C. Dahmann, and Anne C. Gielen (2020), The intergenerational effect of requiring unemployment benefit recipients to engage in non-search activities, IZA Discussion Paper No. 13618, IZA - Institute of Labor Economics, Bonn.

Cobb-Clark, Deborah A., Sarah C. Dahmann, Nicolás Salamanca, and Anna Zhu (2022), Intergenerational disadvantage: Learning about equal opportunity from social assistance receipt, Labour Economics 79, 102276.

Corak, Miles, Björn Gustafsson, and Torun Österberg (2004), Intergenerational influences on the receipt of unemployment insurance in Canada and Sweden. In: Corak, Miles (ed.) Generational income mobility in North America and Europe. Cambridge University Press, Cambridge, 245-288.

Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad (2014), Family Welfare Cultures, Quarterly Journal of Economics 129(4), 1711-1752.

Dahl, Gordon B. and Anne C. Gielen (2021), Intergenerational Spillovers in Disability Insurance, American Economic Journal: Applied Economics 13(2), 116-150.

De Haan, Monique and Ragnhild C. Schreiner (2018), The Intergenerational Transmission of Welfare Dependency, CESifo Working Paper No. 7140, CESifo, Munich.

Edmark, Karin and Kajsa Hanspers (2015), Is welfare dependency inherited? Estimating causal welfare transmission effects using Swedish sibling data, European Journal of Social Security 17(3), 338-360.

Ekhaugen, Tyra (2009), Extracting the causal component from the intergenerational correlation in unemployment, Journal of Population Economics 22, 97-113.

Esping-Andersen, Gøsta (1990), The three worlds of welfare capitalism, Princeton University Press.

Eurofound (2015), Access to social benefits: Reducing non-take-up, Publications Office of the European Union, Luxembourg.

Goebel, Jan, Markus M. Grabka, Stefan Liebig, Martin Kroh, David Richter, Carsten Schröder, and Jürgen Schupp (2019), The German Socio-Economic Panel (SOEP), Jahrbücher für Nationalökonomie und Statistik (Journal of Economics and Statistics) 239(2), 345-360.

Gottschalk, Peter (1996), Is the correlation in welfare participation across generations spurious? Journal of Public Economics 63(1), 1-25.

Grübl, Dominik, Mario Lackner, and Rudolf Winter-Ebmer (2020), Intergenerational transmission of unemployment: causal evidence from Austria, IZA Discussion Paper No. 13068, IZA - Institute of Labor Economics, Bonn.

Hartley, Robert Paul, Carlos Lamarche, and James P. Ziliak (2022), Welfare reform and the intergenerational transmission of dependence, Journal of Political Economy 130(3), 523-565.

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond (2016), Long-run impact of childhood access to the safety net, American Economic Review 106(4), 903-934.

Immervoll, Herwig, Rodrigo Fernandez, Raphaela Hyee, and Jongmi Lee (2022), How Reliable are Social Safety Nets? Value and Accessibility in Situations of Acute Economic Need, IZA Discussion Paper No. 15232, Institute of Labor Economics, Bonn.

Kauppinen, Timo M., Anna Angelin, Thomas Lorentzen, Olof Bäckmn, Tapio Salonen, Pais Moisio, and Espen Dahl (2014), Social background and life-course risks as determinants of social assistance receipt among young adults in Sweden, Norway and Finland, Journal of European Social Policy 24(3), 273-288.

Krosnick, Jon A. and Duane F. Alwin (1989), Aging and Susceptibility to Attitude Change, Journal of Personality and Social Psychology 57(3), 416–425

Levine, Phillip B. and David J. Zimmerman (1996), The Intergenerational Correlation in AFDC Participation: Welfare Trap or Poverty Trap? Discussion Paper University of Wisconsin, Institute for Research on Poverty.

Levine, Phillip B. and David J. Zimmerman (2005), Children's welfare exposure and subsequent development, Journal of Public Economics 89(1), 31-56.

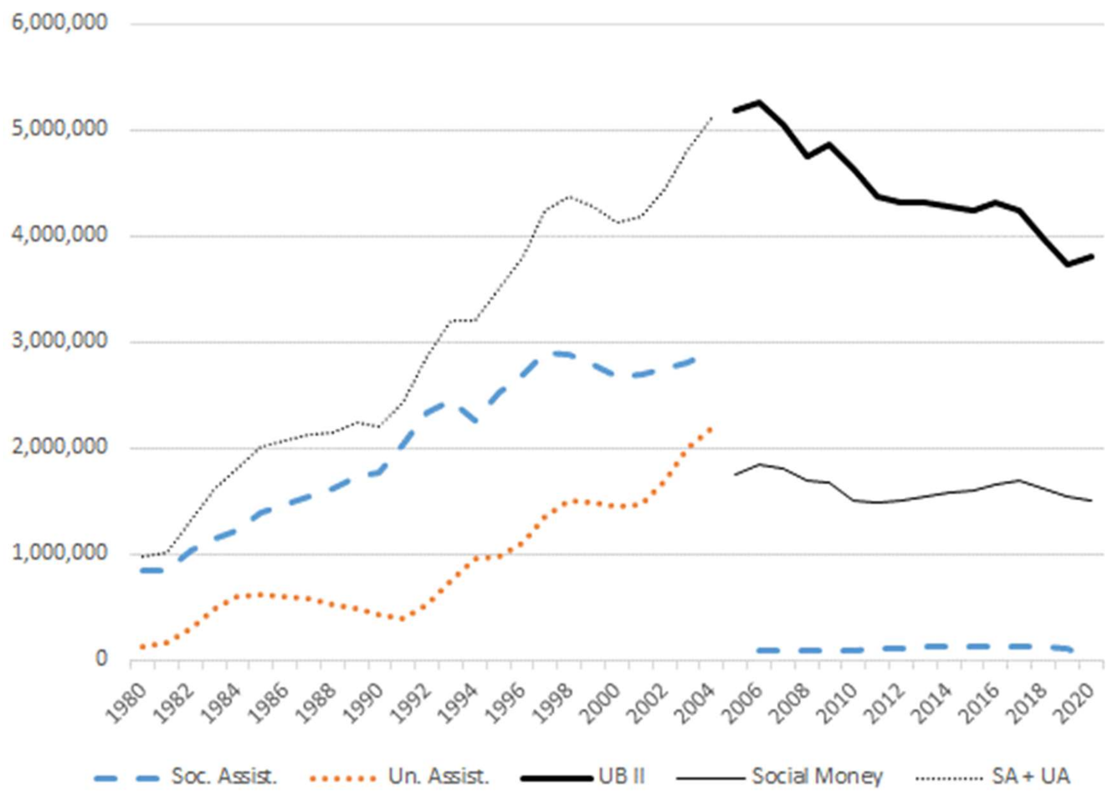
Lillard, Lee and Constantijn W.A. Panis (1998), Panel Attrition from the PSID, Journal of Human Resources 33(2), 437-457.
Maasoumi, Esfandiar and Mark Trede (2001), Comparing income mobility in Germany and the United States using generalized entropy mobility measures, Review of Economics and Statistics 83(3), 551-559.
Maeder, Miriam, Steffen Müller, Regina T. Riphahn, and Caroline Schwientek (2015), Intergenerational transmission of unemployment - evidence for German sons, Journal of Economics and Statistics 235(4+5), 355-375.
Mitnik, Oscar A. (2010), Intergenerational transmission of welfare dependency: The effects of length of exposure, mimeo, University of Miami.
Moisio, Pasi and Timo M. Kauppinen (2011), The intergenerational correlation of social assistance and selection bias in the Finnish population data, Research on Finnish Society 4, 7-15.
Mueller, Steffen, Regina T. Riphahn, and Caroline Schwientek (2017), Paternal unemployment during childhood: causal effects of youth worklessness and educational attainment, Oxford Economic Papers 69(1), 213-238.
Musick, Kelly and Robert D. Mare (2004), Family structure, intergenerational mobility and the reproduction of poverty: evidence for increasing polarization? Demography 41(4), 629-648.
OECD (Organisation for Economic Co-operation and Development) (2019), Society at a Glance 2019: OECD Social Indicators, OECD Publishing, Paris.
Page, Marianne E. (2004), New evidence on the intergenerational correlation in welfare participation, in: Corak Miles (ed.), Generational Income Mobility in North America and Europe, Cambridge University Press, Cambridge, 226-244.
Pepper, John V. (2000), The intergenerational transmission of welfare receipt: a nonparametric bounds analysis, Review of Economics and Statistics 82(3), 472-488.
Ratcliffe, Caroline E. (2002), Premarital childbearing and welfare receipt: the role of mothers' receipt, Industrial and Labor Relations Review 56(1), 160-178.
Rendtel, Ulrich (1990), Teilnahmebereitschaft in Panelstudien: Zwischen Beeinflussung, Vertrauen und sozialer Selektion, Kölner Zeitschrift für Soziologie und Sozialpsychologie 42(2), 280-299.
Riphahn, Regina T. (2001), Rational poverty of poor rationality? The take-up of social assistance benefits, Review of Income and Wealth 47(3), 379-98.
Riphahn, Regina T. and Rebecca Schrader (2020), Institutional Reforms of 2006 and the Dramatic Rise in Old-Age Employment in Germany, Industrial and Labor Relations Review 73(5), 1185-1225.
Riphahn, Regina T. and Christoph Wunder (2013), Patterns of Welfare Dependence Before and After a Reform: Evidence from First Generation Immigrants and Natives in Germany, Review of Income and Wealth 59(3), 437-459.
Riphahn, Regina T. and Christoph Wunder (2016), State dependence in welfare receipt: transitions before and after a reform, Empirical Economics 50(4), 1303-1329.
Siedler, Thomas (2004), Is the receipt of social assistance transmitted from parents to children? Evidence from German panel data, mimeo, Essex and Berlin.
Solon, Gary, Mary Corcoran, Roger Gordon, and Deborah Laren (1988), Sibling and Intergenerational Correlations in Welfare Program Participation, Journal of Human Resources 23(3), 388-396.
Zumbuehl, Maria, Thomas Dohmen, and Gerard Pfann (2021), Parental involvement and the intergenerational transmission of economic preferences, attitudes and personality traits, Economic Journal 131(638), 2642-2670.

Figure 1 Unemployment and welfare institutions for the working-age population in Germany before and after the 2005 reform

	Before the reform (until 31.12.2004)		**After the reform** (since 01.01.2005)
Unemployment Insurance: (not means-tested)	Unemployment benefits (Arbeitslosengeld)	→	Unemployment benefit I (UB I) (Arbeitslosengeld I)
Welfare: (means-tested)	Unemployment assistance (Arbeitslosenhilfe)	→	Unemployment benefit II (UB II) (Arbeitslosengeld II) Social money (Sozialgeld)
		↗	
	Social assistance (Sozialhilfe)	→	Social assistance (Sozialhilfe)

Source: Own illustration.

Figure 2 Utilization of welfare programs over time



Source: Own depiction based on information from different sources. Until 1990 only West Germany, starting 1991 East and West Germany.
Social Assistance (recipients as of 31.12. annually) from https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Soziales/Sozialhilfe/Tabellen/liste-hilfe-lebensunterhalt-empfaenger-zr.html [last accessed July 28.2021]. Unemployment assistance (annual average number of recipients), BA (2020), Arbeitslosengeld und Arbeitslosenhilfe von 1991-2004 (Zeitreihen Monats- und Jahreszahlen); for earlier years: annual publications of Amtliche Nachrichten der Bundesagentur für Arbeit (ANBA). Unemployment benefit II (UB II) and Social Money recipients as of December each year: BA (2021), Strukturen der Grundsicherung SGB II - Deutschland, West/Ost, Länder und Kreise (Zeitreihe Monats- und Jahreszahlen ab 2005), Table 1 (erwerbsfähige und nichterwerbsfähige Leistungsberechtigte).

Table 1 Descriptive statistics and correlation patterns for welfare receipt

Panel A Descriptive statistics

	Observations	Mean	Std. Dev.	Min	Max
Welfare receipt t_1 (age 25-29, child)					
ever (0/1)	2,403	0.1278	0.3339	0	1
number (years)	2,403	0.5502	1.4855	0	5
share (%)	2,403	0.0627	0.1887	0	1
Welfare receipt t_0 (age 10-18, parent)					
ever (0/1)	2,403	0.1382	0.3451	0	1
number (years)	2,403	0.4191	1.3437	0	9
share (%)	2,403	0.0549	0.1741	0	1

Panel B Within and between generation correlation patterns

	Welfare receipt t_1 (child)			**Welfare receipt t_0 (parent)**		
	ever (0/1)	number (years)	share (%)	ever (0/1)	number (years)	share (%)
Welfare receipt t_1 (child)						
ever (0/1)	1.0000	-	-	-	-	-
number (years)	0.9679	1.0000	-	-	-	-
share (%)	0.8688	0.8064	1.0000	-	-	-
Welfare receipt t_0 (parent)						
ever (0/1)	0.1936	0.1765	0.2277	1.0000	-	-
number (years)	0.1989	0.1823	0.2404	0.7791	1.0000	-
share (%)	0.1977	0.1800	0.2381	0.7880	0.9642	1.0000

Source: SOEP (1984-2017), own calculations for sample of 2,403 observations.

Table 2 Baseline results for the basic and extended specifications

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (years)	(3) share (%)
Panel A - Basic specification			
ever (0/1), t_0	0.187^{***} (0.0258)	-	-
number (years), t_0	-	0.204^{***} (0.0309)	-
share (%), t_0	-	-	0.256^{***} (0.0393)
R-Squared	0.0459	0.0558	0.0624
Panel B - Extended specification			
ever (0/1), t_0	0.139*** (0.0255)	-	-
number (years), t_0	-	0.157*** (0.0309)	-
share (%), t_0	-	-	0.211*** (0.0380)
R-Squared	0.105	0.112	0.118

Notes: All estimations use 2,403 observations. Robust standard errors are reported in parentheses. All estimations control for indicators of missing observations at age 10-14 (t_0) and 25-29 (t_1) of the youth. In addition, Panel B controls for child gender, year of birth, immigration background, parity, parental year of birth, and indicators of parental education as well as household size, the number of children in the parental household, indicators of their missing values, and the federal state of residence when the individual was age 17; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.
Source: SOEP (1984-2017), own calculations.

Table 3 Gender-specific effects

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (years)	(3) share (%)
Panel A: Extended specification with gender interaction (N=2,403)			
Parent welfare, t_0	0.100***	0.083**	0.151***
	(0.036)	(0.038)	(0.050)
Female × Parent welfare, t_0	0.074	0.145**	0.117
	(0.050)	(0.059)	(0.039)
Panel B: Male sample (N=1,205)			
Parent welfare, t_0, basic specification	0.161***	0.134***	0.201***
	(0.036)	(0.038)	(0.051)
Parent welfare, t_0, extended specification	0.109***	0.089**	0.154***
	(0.037)	(0.039)	(0.049)
Panel C: Female sample (N=1,198)			
Parent welfare, t_0, basic specification	0.213***	0.269***	0.306***
	(0.037)	(0.047)	(0.058)
Parent welfare, t_0, extended specification	0.166***	0.214***	0.254***
	(0.035)	(0.047)	(0.056)
Panel D: Maternal welfare receipt – basic specification (N=2,375)			
Maternal welfare, t_0	0.195***	0.208***	0.243***
	(0.028)	(0.035)	(0.041)
Panel E: Paternal welfare receipt – basic specification (N=2,284)			
Paternal welfare, t_0	0.197***	0.233***	0.296***
	(0.032)	(0.048)	(0.055)
Panel F: Maternal welfare receipt – ext. specification with gender interaction (N=2,375)			
Maternal welfare, t_0	0.158***	0.126***	0.178***
	(0.039)	(0.042)	(0.054)
Female × Maternal welfare, t_0	0.071	0.154**	0.122
	(0.056)	(0.066)	(0.081)
Panel G: Paternal welfare receipt – ext. specification with gender interaction (N=2,284)			
Paternal welfare, t_0	0.163***	0.165***	0.264***
	(0.045)	(0.060)	(0.084)
Female × Paternal welfare, t_0	0.061	0.126	0.052
	(0.063)	(0.093)	(0.110)

Notes: Each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). Panels D-G additionally control for an indicator reflecting whether an individual ever lived with a single parent (i.e., in a non-couple household). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 2**; *** p < 0.01, ** p < 0.05, * p < 0.05
Source: SOEP (1984-2017), own calculations.

Table 4 Heterogeneity of welfare correlation by age of exposure

	Dependent variables: Welfare receipt t_1		
	(1)	(2)	(3)
	ever (0/1)	number (years)	share (%)
Panel A - Separate estimations by age group - Basic specification			
Age group 10-12, t_0 (N=1,242)	0.225^{***}	0.559^{**}	0.282^{***}
	(0.046)	(0.117)	(0.060)
Age group 13-15, t_0 (N=1,835)	0.189^{***}	0.400^{***}	0.196^{***}
	(0.037)	(0.081)	(0.040)
Age group 16-18, t_0 (N=2,403)	0.201^{***}	0.427^{***}	0.203^{***}
	(0.030)	(0.065)	(0.032)
Panel B - Joint estimations for all age groups (N=1,216) - Basic specification			
Age group 10-12, t_0	0.135^{**}	0.303^{*}	0.147^{**}
	(0.055)	(0.156)	(0.071)
Age group 13-15, t_0	0.051	0.129	0.013
	(0.052)	(0.150)	(0.068)
Age group 16-18, t_0	0.123^{**}	0.325^{***}	0.156^{***}
	(0.048)	(0.121)	(0.056)

Notes: Each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). Robust standard errors are reported in parentheses. For details on the basic specification see notes of **Table 2**; *** p < 0.01, ** p < 0.05, * p < 0.05.

Source: SOEP (1984-2017), own calculations.

Table 5 Pre- vs. post-reform outcomes

	Dependent variables: Welfare receipt t_1		
	(1)	(2)	(3)
	ever (0/1)	number (years)	share (%)
Panel A – Basic specification			
Pre-Reform (N=778)			
Parent welfare, t_0	0.202***	0.216**	0.163**
	(0.0624)	(0.0895)	(0.0746)
Post-Reform (N=1,210)			
Parent welfare, t_0	0.196***	0.218***	0.308***
	(0.0317)	(0.0342)	(0.0475)
Panel B – Extended specification			
Pre-Reform (N=778)			
Parent welfare, t_0	0.184***	0.205**	0.146**
	(0.0620)	(0.0904)	(0.0742)
Post-Reform (N=1,210)			
Parent welfare, t_0	0.143***	0.176***	0.264***
	(0.0313)	(0.0348)	(0.0459)
Panel C – Basic specification with interaction terms			
Full period (N=1988)			
Parent welfare, t_0	0.191***	0.207**	0.159**
	(0.0619)	(0.0880)	(0.0725)
Post × Paternal welfare, t_0	0.006	0.013	0.147*
	(0.0695)	(0.0945)	(0.0865)
Panel D – Extended specification with interaction terms			
Full period (N=1988)			
Parent welfare, t_0	0.170***	0.199**	0.142**
	(0.0603)	(0.0877)	(0.0724)
Post × Paternal welfare, t_0	-0.031	-0.029	0.111
	(0.0679)	(0.0942)	(0.0851)

Notes: In Panels A and B each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 2.** In Panels C and D the pre- and post-reform observations were pooled and an interaction term of the parental welfare indicator with the post-reform indicator was added to the specification. *** p < 0.01, ** p < 0.05, * p < 0.05.
Source: SOEP (1984-2017), own calculations.

Table 6 Controlling for child education as a potential mediator

	Dependent variables: Welfare receipt t_1		
	(1)	(2)	(3)
	ever (0/1)	number (yrs)	share (%)
Parent welfare, t_0 (basic specification)	0.148***	0.161***	0.212***
	(0.025)	(0.031)	(0.039)
Parent welfare, t_0 (extended specification)	0.112***	0.129***	0.180***
	(0.025)	(0.031)	(0.038)

Notes: Estimations used 2,403 observations. Each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 2**; all estimations additionally control for three indicators of child educational attainment.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.
Source: SOEP (1984-2017), own calculations.

Table 7 Family fixed effects estimation

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (yrs)	(3) share (%)
Panel A: OLS results for the FE Sample (N=1,161)			
Parent welfare (basic)	0.199^{***}	0.233^{***}	0.290^{***}
	(0.037)	(0.044)	(0.058)
Parent welfare (extended)	0.132^{***}	0.162^{***}	0.211^{***}
	(0.036)	(0.043)	(0.054)
Panel B: FE Regressions (N=1,161)			
Parent welfare (basic)	-0.007	-0.028	-0.150
	(0.076)	(0.087)	(0.104)
Parent welfare (extended)	0.009	-0.031	-0.163
	(0.78)	(0.089)	(0.115)
Panel C: FE Regressions w/o welfare experience of oldest child (N=1,150)			
Parent welfare (basic)	0.003	0.002	-0.155
	(0.081)	(0.086)	(0.112)
Parent welfare (extended)	0.009	-0.031	-0.163
	(0.078)	(0.089)	(0.115)

Notes: Each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 2;** *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.
Source: SOEP (1984-2017), own calculations.

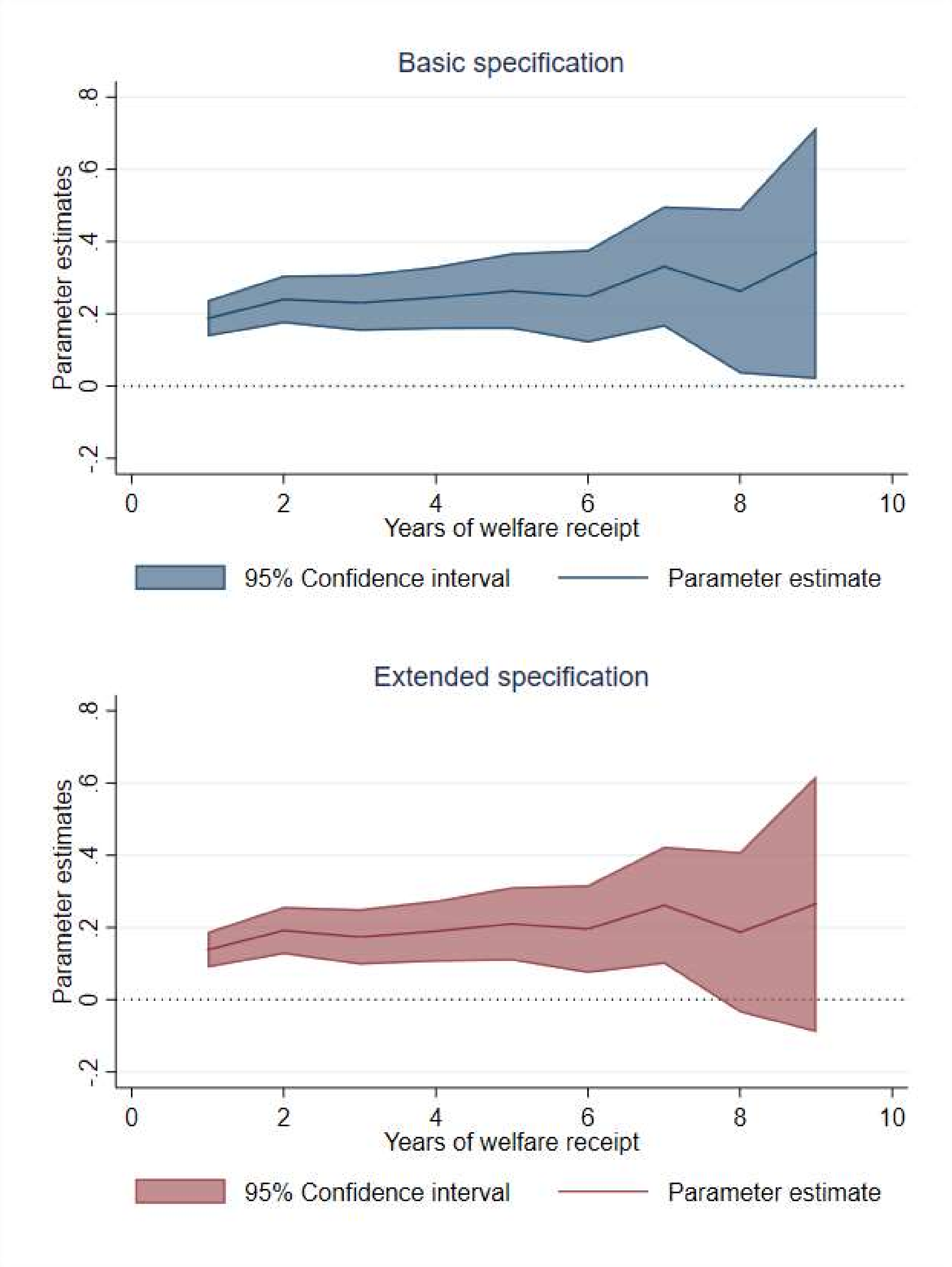
Table 8 Gottschalk estimation

	Dependent variables: Welfare receipt t_1		
	(1)	(2)	(3)
	ever (0/1)	number (years)	share (%)
Panel A: OLS results for Gottschalk estimation sample (N=1,221)			
Parent welfare, t_0, basic specification	0.178***	0.249***	0.260***
	(0.039)	(0.058)	(0.062)
Parent welfare, t_0, extended specification	0.131***	0.192***	0.217***
	(0.038)	(0.057)	(0.060)
Panel B: Gottschalk approach estimation results, basic specification (N=1,221)			
Parent welfare, t_0	0.130***	0.191***	0.201***
	(0.039)	(0.060)	(0.063)
Parent welfare, t_2	0.318***	0.350***	0.188***
	(0.072)	(0.076)	(0.046)
Gottschalk effect	-0.188**	-0.160	0.014
	(0.089)	(0.104)	(0.084)
Panel C: Gottschalk approach estimation results, extended specification (N=1,221)			
Parent welfare, t_0	0.097***	0.149***	0.172***
	(0.039)	(0.059)	(0.061)
Parent welfare, t_2	0.254***	0.271***	0.156***
	(0.073)	(0.080)	(0.046)
Gottschalk effect	-0.157**	-0.122	0.015
	(0.089)	(0.108)	(0.083)

Notes: In Panel A, each cell entry represents a separate regression where the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 2**). In Panels B and C the parent indicators of periods t0 and t2 are controlled jointly in the same regression model. The rows labelled "Gottschalk effect" present the difference between the two period-specific coefficient estimates. Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 2**; *** p < 0.01, ** p < 0.05, * p < 0.05.
Source: SOEP (1984-2017), own calculations.

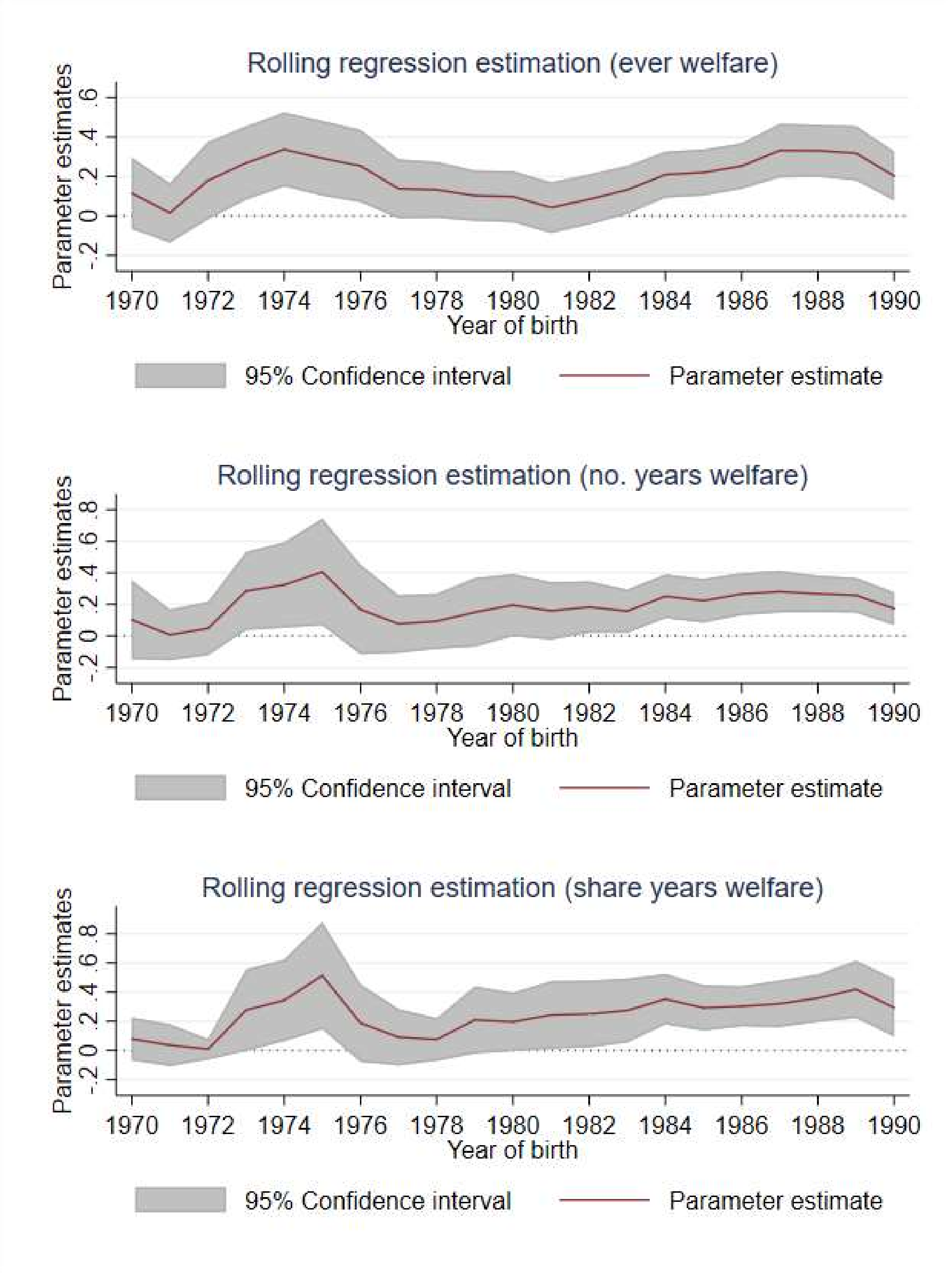
Online Appendix

Figure A.1 Welfare receipt in t0 by minimum number of years of parental welfare receipt



Source: SOEP (1984-2017), own calculations.

Figure A.2 Correlation pattern by birth cohort



Notes: The cohort-specific outcomes are estimated in regressions that consider observations from the specific birth cohort plus the two neighboring birth cohorts (rolling regressions). All estimations use the basic specification as described in **Table 2**.
Source: SOEP (1984-2017), own calculations.

Table A.1 Prior Literature

Author	Country	Publication	Identification	Causality found?	Data	Exposure (t0) Age	#Years	Own welfare (t1) Age	#Years	Parent	Child
Antel	US	1992 REStat	Structural model	yes	Survey: NLSY	14-19	1	20-22…25-27	3	Mother	Daughter
Levine & Zimmerman	US	1996 Disc.Paper	IV	no	Survey: NLSY	16 (?)	1	26 (?)	1	Mother	Daughter
Gottschalk	US	1996 J Pub Econ	Event study	yes	Survey: PSID	8-13	6	14-22	9	Mother	Daughter
Pepper	US	2000 REStat	Bounds / IV	yes	Survey: PSID	12-16	5	24-33	5	Mother	Daughter
Mitnik	US	2010 Disc.Paper	FE / match	no	Admin. Data	13-17 (9-17)	5	19-22&19-26	varies	Mother	Daughter
Hartley et al.	US	2022 J Pol Econ	IV / DID	yes	Survey: PSID	12-18	5	14 or older	5	Mother	Daughter
Beaulieu et al.	Canada	2005 J Pop Econ	Structural model	yes	Admin. Data	7-17	10	18-21	4	Parents	All
Edmark & Hanspers	Sweden	2015 Eur J Soc Sec	FE	no	Admin. Data	17-19	3	24	1	Parents	All
De Haan & Schreiner	Norway	2018 Disc.Paper	Bounds / IV	yes	Admin. Data	13-17	5	18-30	13	Parents	All
Boschman et al.	NL	2019 Soc Sci Res	Gottschalk	no	Admin. Data	17-20	2	28-31	2	Parents	All
Cobb-Clark et al.	Austral.	2022 Labour Econ	Gottschalk	no	Admin. Data	8-14	6	18-22	4	Parents	All

Source: Own compilation.

Table A.2 Descriptive statistics for explanatory variables

Panel A By child welfare receipt in period t_1

		Group Differentiator: Welfare receipt t_1		
	(1)	(2)	(3)	(4)
	Mean (all)	Mean (ever = 0)	Mean (ever = 1)	Difference (2) – (3)
Female	0.499	0.490	0.554	-0.063**
Year of birth	1979.67	1979.74	1979.12	0.624
Migration, first generation	0.084	0.081	0.107	-0.026
Migration, second gen.	0.186	0.184	0.199	-0.015
Parity, first	0.363	0.371	0.306	0.065**
Parity, second	0.346	0.354	0.296	0.057**
Parity, third or higher	0.156	0.142	0.251	-0.109**
Year of birth oldest parent	1949.59	1949.50	1950.20	-0.698
Age mother at birth	26.68	26.84	25.60	1.238***
Age father at birth	29.71	29.88	28.52	1.366***
No. kids in hh at age 17	1.975	1.945	2.179	-0.234***
HH size at age 17	3.791	3.766	3.960	-0.193*
Parental education:				
- Lower second. school	0.236	0.238	0.235	-0.003*
- Secondary school	0.357	0.362	0.357	-0.004*
- Upper second. school	0.191	0.201	0.121	0.191***
- Other	0.216	0.207	0.280	-0.074***
N	2,403	2,096	307	

Panel B By parental welfare receipt in period t_0

		Group Differentiator: Welfare receipt t_0		
	(1)	(2)	(3)	(4)
	Mean (all)	Mean (ever = 0)	Mean (ever = 1)	Difference (2) – (3)
Female	0.499	0.496	0.515	-0.019
Year of birth	1979.67	1979.32	1981.85	-2.531
Migration, first gen	0.084	0.081	0.105	-0.024**
Migration, second gen	0.186	0.182	0.208	-0.026
Parity, first	0.363	0.371	0.310	0.061
Parity, second	0.346	0.347	0.343	0.003*
Parity, third or higher	0.156	0.143	0.238	-0.095**
Year of birth oldest parent	1949.59	1949.18	1952.15	-2.975
Age mother at birth	26.68	26.74	26.27	0.472
Age father at birth	29.71	29.81	29.08	0.730*
No. kids in hh at age 17	1.975	1.926	2.277	-0.351*
HH size at age 17	3.791	3.743	4.089	-0.345*
Parental education:				
- Lower second. school	0.235	0.235	0.238	-0.003*
- Secondary school	0.357	0.355	0.370	0.028***
- Upper second. school	0.191	0.205	0.099	0.106***
- Other	0.216	0.204	0.292	-0.088***
N	2,403	2,071	332	

Notes: The table reports balance tests to compare observable characteristics of individuals with and without welfare benefit receipt in periods t1 (Panel A) and t0 (Panel B). The tests are performed via *t*-tests that explore whether differences in mean characteristics are statistically significant. Indicators for federal states and for missing values on the variables 'No. kids in hh at age 17', parity, HH size at age 17 are considered in the estimations but not described here to reduce clutter. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.
Source: SOEP (1984-2017), own calculations.

Table A.3 Descriptive statistics for all variables

	Obs.	Mean	Std. Dev.	Min	Max
ever (0/1), t_1	2,403	0.12776	0.33389	0	1
number (years), t_1	2,403	0.55015	1.48553	0	5
share (%), t_1	2,403	0.06274	0.18873	0	1
ever (0/1), t_0	2,403	0.13816	0.34514	0	1
number (years), t_0	2,403	0.41906	1. 34372	0	9
share (%), t_0	2,403	0.05491	0.17407	0	1
ever (0/1), t_2	1,221	0.04259	0.20200	0	1
number (years), t_2	1,221	0.24570	0.89456	0	6
share (%), t_2	1,221	0.05164	0.18184	0	1
paternal welfare ever (0/1)	2,284	0.09501	0.29329	0	1
paternal welfare years	2,284	0.23642	0.91474	0	9
paternal welfare share (%)	2,284	0.03471	0.13526	0	1
maternal welfare ever (0/1)	2,375	0.11705	0.32155	0	1
maternal welfare years	2,375	0.35747	1.26210	0	9
maternal welfare share (%)	2,375	0.04788	0.16609	0	1
Missing age 10	2,403	0.47566	0.49951	0	1
Missing age 11	2,403	0.39534	0.48903	0	1
Missing age 12	2,403	0.31003	0.46260	0	1
Missing age 13	2,403	0.22430	0.41721	0	1
Missing age 14	2,403	0.12484	0.33061	0	1
Missing age 25	2,403	0.03412	0.18159	0	1
Missing age 26	2,403	0.03454	0.18265	0	1
Missing age 27	2,403	0.13858	0.34558	0	1
Missing age 28	2,403	0.25968	0.43855	0	1
Missing age 29	2,403	0.35581	0.47886	0	1
Female	2,403	0.49854	0.50010	0	1
Year of birth oldest parent	2,403	1949.59	8.81200	1900	1970
No. children in Hh at age 17	2,403	1.97469	1.03573	1	11
No. children in Hh at 17, missing dummy 1	2,403	0.42697	0.49474	0	1
No. children in Hh at 17, missing dummy 2	2,403	0.12235	0.32775	0	1
HH size at age 17	2,403	3.79085	1.29493	1	12
HH size at age 17, missing dummy	2,403	0.18643	0.38954	0	1
Migration, first gen	2,403	0.08455	0.27816	0	1
Migration, second gen	2,403	0.18560	0.38887	0	1
Parity, second	2,403	0.34623	0.47587	0	1
Parity, third or higher	2,403	0.15605	0.36298	0	1
Parity missing dummy	2,403	0.13483	0.34161	0	1
Cohort (1969-1972)	2,403	0.20183	0.40145	0	1
Cohort (1973-1975)	2,403	0.12193	0.32727	0	1
Cohort (1976-1979)	2,403	0.17270	0.37807	0	1
Cohort (1980-1982)	2,403	0.12443	0.33014	0	1
Cohort (1983-1985)	2,403	0.12651	0.33249	0	1
Cohort (1986-1988)	2,403	0.13525	0.34206	0	1
Cohort (1989-1991)	2,403	0.11735	0.32191	0	1
Parent education					
Lower sec. school	2,403	0.23554	0.42442	0	1
Secondary school	2,403	0.35747	0.47935	0	1
Upper sec. school	2,403	0.19101	0.39318	0	1
Other	2,403	0.21598	0.41159	0	1
Federal States					
Baden-Württemberg	2,403	0.15522	0.36219	0	1

Bayern	2,403	0.14607	0.35325	0	1
Berlin	2,403	0.04536	0.20813	0	1
Hessen	2,403	0.06450	0.24567	0	1
Brandenburg / Mecklenburg-Vorp.	2,403	0.04367	0.20446	0	1
Bremen / Niedersachsen	2,403	0.08864	0.28428	0	1
Nordrhein-Westfalen	2,403	0.21515	0.41101	0	1
Saarland / Rheinland-Pfalz	2,403	0.04744	0.21262	0	1
Sachsen-Anhalt	2,403	0.06950	0.25435	0	1
Sachsen	2,403	0.04078	0.19782	0	1
Hamburg / Schleswig-Holstein	2,403	0.04078	0.19783	0	1
Thüringen	2,403	0.04286	0.20259	0	1
Child education					
Less than upper sec. school	2,403	0.13691	0.34382	0	1
Upper secondary school	2,403	0.42946	0.49510	0	1
Vocational degree	2,403	0.17978	0.38408	0	1
Tertiary degree (BA / MA)	2,403	0.25385	0.43530	0	1

Source: SOEP (1984-2017), own calculations.

Table A.4 Complete estimation results of extended specification in **Table 2**

	Dependent variables: Welfare receipt t_1		
	(1)	(2)	(3)
	ever (0/1)	number (years)	share (%)
ever (0/1), t_0	0.139***	-	-
	(0.0255)	-	-
number (years), t_0	-	0.157***	-
	-	(0.0309)	-
share (%), t_0	-	-	0.211***
	-	-	(0.0380)
Missing age 10	0.0419	0.167	0.0193
	(0.0272)	(0.120)	(0.0152)
Missing age 11	-0.0785**	-0.311**	-0.0324*
	(0.0335)	(0.147)	(0.0194)
Missing age 12	0.0293	0.154	-0.0106
	(0.0284)	(0.128)	(0.0154)
Missing age 13	-0.0645**	-0.304**	-0.0144
	(0.0263)	(0.119)	(0.0143)
Missing age 14	0.0626**	0.280**	0.0235
	(0.0258)	(0.114)	(0.0148)
Missing age 25	0.00503	-0.157	0.0128
	(0.0352)	(0.120)	(0.0215)
Missing age 26	0.0585	-0.0376	0.0557*
	(0.0397)	(0.124)	(0.0289)
Missing age 27	-0.00385	-0.123**	0.00382
	(0.0207)	(0.0584)	(0.0135)
Missing age 28	-0.0221	-0.191**	-0.00777
	(0.0215)	(0.0766)	(0.0124)
Missing age 29	0.00882	-0.116	0.0161
	(0.0218)	(0.0854)	(0.0119)
Female	0.0263**	0.116**	0.0155**
	(0.0133)	(0.0588)	(0.00740)
Year of birth oldest parent	0.00400***	0.0187***	0.00155**
	(0.00122)	(0.00540)	(0.000710)
No. children in Hh. at age 17	0.0102	0.0482	0.00670
	(0.00810)	(0.0361)	(0.00462)
No. children in Hh. at 17, missing dummy 1	0.0147	0.0521	0.0123
	(0.0277)	(0.128)	(0.0158)
No. children in Hh. at 17, missing dummy 2	0.0194	0.137	0.0217
	(0.0259)	(0.116)	(0.0168)
HH size at age 17	-0.000280	-0.00965	-0.00211
	(0.00713)	(0.0326)	(0.00362)
HH size at age 17, missing dummy	0.0300	0.157	0.0205
	(0.0250)	(0.110)	(0.0154)
Migration, first generation	0.0128	0.0664	-0.00750
	(0.0351)	(0.157)	(0.0194)
Migration, second generation	-0.00649	-0.0623	-0.0156
	(0.0234)	(0.105)	(0.0146)

Parity, second	0.0171	0.0718	0.00415
	(0.0156)	(0.0696)	(0.00829)
Parity, third or higher	0.102***	0.417***	0.0552***
	(0.0239)	(0.106)	(0.0142)
Parity missing dummy	0.0433 *	0.133	0.0104
	(0.0250)	(0.112)	(0.0153)
Cohort (1973-1975)	-0.0637**	-0.237*	-0.0334*
	(0.0314)	(0.140)	(0.0179)
Cohort (1976-1979)	-0.0233	-0.0420	-0.00861
	(0.0344)	(0.156)	(0.0203)
Cohort (1980-1982)	-0.00527	0.0297	0.00701
	(0.0471)	(0.212)	(0.0285)
Cohort (1983-1985)	-0.0946**	-0.385*	-0.0365
	(0.0459)	(0.205)	(0.0273)
Cohort (1986-1988)	-0.114**	-0.504**	-0.0497*
	(0.0474)	(0.213)	(0.0281)
Cohort (1989-1991)	-0.168***	-0.639***	-0.0812***
	(0.0519)	(0.226)	(0.0310)
Parent Education			
Secondary school	-0.0366**	-0.127	-0.0202*
	(0.0180)	(0.0793)	(0.0108)
Upper sec. school	-0.0606***	-0.240***	-0.0368***
	(0.0197)	(0.0868)	(0.0113)
Other	0.0177	0.133	0.0119
	(0.0294)	(0.129)	(0.0174)
Federal States			
Bayern	0.00214	-0.00815	-0.00192
	(0.0195)	(0.0844)	(0.0101)
Berlin	0.131***	0.590***	0.0671***
	(0.0400)	(0.174)	(0.0223)
Hessen	-0.00719	0.0100	-0.0140
	(0.0251)	(0.113)	(0.0120)
Brandenburg / Mecklenburg-Vorp.	0.169***	0.628***	0.1000***
	(0.0432)	(0.177)	(0.0270)
Bremen / Niedersachsen	0.0223	0.0911	-0.00436
	(0.0250)	(0.112)	(0.0124)
Nordrhein-Westfalen	0.0566***	0.264***	0.0257**
	(0.0205)	(0.0927)	(0.0114)
Saarland / Rheinland-Pfalz	0.0462	0.222	0.0145
	(0.0332)	(0.156)	(0.0184)
Sachsen-Anhalt	0.141***	0.565***	0.0730***
	(0.0338)	(0.145)	(0.0196)
Sachsen	0.206***	0.910***	0.0901***
	(0.0465)	(0.214)	(0.0272)
Hamburg / Schleswig-Holstein	0.0153	0.0588	0.00887
	(0.0308)	(0.134)	(0.0188)
Thüringen	0.149***	0.591***	0.0828***
	(0.0428)	(0.184)	(0.0263)
R-Squared	0.105	0.112	0.118

Notes: Robust standard errors are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$
Source: SOEP (1984-2017), own calculations.

Table A.5 Descriptive statistics on welfare receipt for male and female observations

	Observations	Mean	Std. Dev.	Min	Max
Panel A - Female observations					
Welfare receipt t_1(age 25-29)					
ever (0/1)	1,198	0.14190	0.34910	0	1
number (yrs)	1,198	0.62104	1.57358	0	5
share (%)	1,198	0.07051	0.19972	0	1
Welfare receipt t_0 (age 10-18)					
ever (0/1)	1,198	0.14273	0.34995	0	1
number (yrs)	1,198	0.42905	1.36291	0	9
share (%)	1,198	0.05638	0.17719	0	1
Panel B - Male observations					
Welfare receipt t_1(age 25-29)					
ever (0/1)	1,205	0.11369	0.31757	0	1
number (yrs)	1,205	0.47967	1.38955	0	5
share (%)	1,205	0.05502	0.17688	0	1
Welfare receipt t_0 (age 10-18)					
ever (0/1)	1,205	0.13361	0.34037	0	1
number (yrs)	1,205	0.40913	1.32485	0	9
share (%)	1,205	0.05345	0.17097	0	1

Source: SOEP (1984-2017), own calculations.

Table A.6 Descriptive statistics and correlation patterns for pre- and post-reform cohorts

Panel A Descriptive statistics

	Observations	Mean	Std. Dev.	Min	Max
(1) Before reform (birth cohorts 1969-1975)					
Welfare receipt t_1(age 25-29)					
ever (0/1)	778	0.11440	0.31850	0	1
number (yrs)	778	0.51928	1.47838	0	5
share (%)	778	0.05124	0.16126	0	1
Welfare receipt t_0 (age 10-18)					
ever (0/1)	778	0.07069	0.25648	0	1
number (yrs)	778	0.19409	0.90166	0	9
share (%)	778	0.02991	0.13468	0	1
(2) After reform (birth cohorts 1980-1991)					
Welfare receipt t_1(age 25-29)					
ever (0/1)	1,210	0.12314	0.32873	0	1
number (yrs)	1,210	0.50331	1.40084	0	5
share (%)	1,210	0.06567	0.20189	0	1
Welfare receipt t_0 (age 10-18)					
ever (0/1)	1,210	0.18678	0.38989	0	1
number (yrs)	1,210	0.62727	1.64030	0	9
share (%)	1,210	0.07973	0.20780	0	1

Panel B Within and between generation correlation patterns

	Welfare receipt t_1 (child)			**Welfare receipt t_0 (parent)**		
	ever	number	share	ever	number	share
(1) Welfare receipt t_1 (child)						
Pre-reform						
ever (0/1)	1.0000	-	-	0.1530	0.1287	0.1224
number (years)	0.9779	1.0000	-	0.1576	0.1261	0.1201
share (%)	0.8847	0.8437	1.0000	0.1706	0.1478	0.1343
Post-reform						
ever (0/1)	1.0000	-	-	0.2399	0.2800	0.2806
number (years)	0.9592	1.0000	-	0.2124	0.2653	0.2626
share (%)	0.8684	0.7955	1.0000	0.2708	0.3136	0.3157
(2) Welfare receipt t_0 (parent)						
Pre-reform						
ever (0/1)	-	-	-	1.0000	-	-
number (years)	-	-	-	0.7809	1.0000	-
share (%)	-	-	-	0.8058	0.9604	1.0000
Post-reform						
ever (0/1)	-	-	-	1.0000	-	-
number (years)	-	-	-	0.7983	1.0000	-
share (%)	-	-	-	0.8010	0.9655	1.0000

Note: Panel B shows all correlations within the pre- and post-reform groups.
Source: SOEP (1984-2017), own calculations.

Table A.7 Comparison of descriptive statistics for full and Gottschalk samples

	(1) Mean (Full Sample)	(2) Mean (Gottschalk Sample)	(3) =(1)–(2)
Dependent variables			
Ever welfare (0/1), t_1	0.128	0.143	-0.015
Number of welfare years, t_1	0.550	0.690	-0.14***
Share of welfare years (%), t_1	0.063	0.064	-0.002
Explanatory variables			
Ever welfare (0/1), t_0	0.138	0.126	0.012
Number of welfare years, t_0	0.419	0.355	0.064*
Share of welfare years (%), t_0	0.055	0.047	0.008*
Female	0.499	0.517	-0.018
Year of birth	1979.67	1977.02	2.65***
Migration, first generation	0.084	0.084	0.001
Migration, second generation	0.186	0.179	0.006
Parity, first	0.363	0.361	0.002
Parity, second	0.346	0.338	0.025
Parity, third or higher	0.156	0.152	0.005
Year of birth oldest parent	1949.59	1947.23	2.358***
No. children in hh at age 17	1.975	1.950	0.024
Hh size at age 17	3.791	3.741	0.050
Parent education - sec. school	0.197	0.174	0.017
Parent education - upper sec. school	0.357	0.333	0.025*
Parent education - other	0.216	0.212	0.004
Age at birth (mother)	26.689	26.354	0.325***
Age at birth (father)	29.711	29.408	0.302**
ever (0/1), t_2	-	0.043	-
number (years), t_2	-	0.246	-
share (%), t_2	-	0.052	-
Number of observations	2,403	1,221	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$
Source: SOEP (1984-2017), own calculations.