

Intergenerational Transmission of Welfare Benefit Receipt: Evidence from Germany

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We study the intergenerational transmission of welfare benefit receipt in Germany. We first describe the correlation of welfare receipt experienced in the parental household and subsequent own welfare receipt of young adults. In a second step, we investigate whether the observed correlations reflect causal effects of past welfare experience. We use family fixed effects estimations and Gottschalk's (1996) approach and take advantage of the long running German Socio-Economic Panel Survey to contribute to a sparse literature. We find strong positive correlations between parental and own welfare receipt. These patterns do, however, not persist after controlling for unobserved heterogeneities. Therefore, our results suggest that the strong intergenerational correlation of welfare benefit receipt is determined by family background rather than by the experience of parental welfare benefit receipt.

Keywords: welfare, social assistance, intergenerational mobility, causal effect, family fixed effects, Gottschalk estimator

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1. Introduction

It is well known that parent well-being affects child well-being. The transmission of disadvantage from parents to their children indicates inequality of opportunities. Intergenerational transmission patterns have been studied intensely with respect to earnings and education outcomes (Adermon et al. 2021, Black and Devereux 2011, Blanden 2013) and recently with respect to place-based effects (e.g., Chetty and Hendren 2018a, 2018b). In this paper, we focus on the intergenerational transmission of welfare receipt, i.e., the participation in minimum income protection programs.

It is important to know whether welfare receipt is transmitted across generations. The purpose of welfare programs is to lift households from the most pressing economic troubles and to safeguard the next generation. If, instead, welfare receipt ends up being passed from generation to generation then not only do the programs not work properly for the young, they may even be harmful. In this situation, reforming a welfare program and reducing 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 financial and non-financial parental support 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 positive causal effects is mixed.

We provide the first empirical evidence on the intergenerational transmission of welfare receipt in Germany. The German case provides an interesting laboratory to study the causal transmission of welfare benefit receipt given the specific institutional framework of welfare

benefits in Germany and the availability of long-running data on parents and children. As most of the literature on the intergenerational transmission of welfare covers either the United States or Scandinavian countries such as Sweden and Norway, Germany is an interesting intermediate case, e.g., with respect to the inequality of the income distribution, the importance of poverty, and the generosity of minimum income protection (OECD 2019). Germany has much lower poverty rates after taxes and transfers than the U.S. and - for workless working-age households - also than Sweden (Immervoll et al., 2022).¹ So, while the German welfare system protects the poor comparatively well we do not know whether such a relatively generous system also succeeds in an intergenerational perspective, i.e., protecting the next generation from inherited dependence.

To examine the transmission of welfare across generations we need data on parents and children that span multiple decades. For many countries, such data is not available. The long running German Socio-Economic Panel Survey allow us to study the transmission of youth welfare experience for individuals born 1969-1991. We 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 25-29.²

In a first step, we describe the correlation of parent and child welfare receipt. We focus on youths' early impressionable years (Krosnick and Alwin 1989) and investigate whether the age of experiencing parental benefit receipt matters. We look into the potential mediation effect of child educational outcomes and compare patterns by child gender. Also, we can study the

¹ The authors show that on average less than one third of poor working-age households in the OECD receive support. This share reaches 75 percent in Germany, topped only by France.

² Some prior studies could use only few years of observations. For example, Antel (1992), Moisiu and Kauppinen (2011), and Kauppinen et al. (2014) observe parental welfare receipt only for one year, Levine and Zimmerman (1996), Siedler (2004), and Edmark and Hanspers (2015) use only one year of child welfare outcomes in adult age, Boschman et al. (2019) look at two years for the outcome measure.

correlation patterns before and after a major welfare reform which is useful to assess the sensitivity of correlation patterns to institutional change.

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. To account for such factors which might generate spurious intergenerational welfare correlations we consider the empirical strategy developed by Gottschalk (1996) and apply family fixed effects estimation. If the identifying assumptions hold applying both methods allows us to get closer to answering the question whether parental welfare receipt causally affects the welfare receipt of the next generation in Germany.

While there is a broad international literature describing intergenerational correlation in welfare receipt the number of studies identifying causal effects of minimum income programs is limited (for a survey see, e.g., Black and Devereux 2011).³ Early contributions applied structural estimation approaches and U.S. survey data: Antel (1992) and Levine and Zimmerman (1996) use data from the National Longitudinal Survey of Youth (NLSY). Antel (1992) concludes that maternal welfare use causally affects daughters' receipt whereas Levine and Zimmerman (1996) suggest that we observe a 'poverty trap' but not a 'welfare trap'.

Gottschalk (1996) studies U.S. welfare transmission applying event study methods to data from the Panel Study of Income Dynamics (PSID). He finds a causal relationship between mothers' and daughters' welfare receipt. Using the same data, Pepper (2000) compares alternative empirical approaches to address the endogeneity of maternal welfare receipt and confirms a causal relationship. Hartley et al. (2022) also use PSID data to study the effect of the U.S. welfare reform on intergenerational transmission between mothers and daughters.

³ Appendix **Table A.1** offers a brief characterization of prior contributions.

They 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.⁴ Exploiting administrative data Mitnik (2010) studies the intensive margin of welfare receipt. He compares individuals whose parents received welfare for different durations. Applying matching and family fixed effects estimators he does not find causal effects.

There are only few studies covering countries outside the U.S.. Beaulieu et al. (2005) exploit administrative data on social assistance receipt in Quebec, Canada. Based on structural estimation approaches they confirm causal intergenerational effects. Edmark and Hanspers (2015) apply family fixed effects estimation to Swedish register data. While intergenerational correlation in welfare receipt is strong they find no causal effects once family fixed effects are considered. In their study using administrative data from Norway De Haan and Schreiner (2018) apply bounds analyses with instrumental variables. They confirm significant positive causal transmission effects. Boschman et al. (2019) apply the Gottschalk (1996) approach to Dutch administrative data. For pooled public transfer programs (disability, unemployment, and social assistance benefits) they confirm significant causal effects but not for social assistance, per se. Overall, the evidence on the causal intergenerational transmission of welfare receipt is mixed.

The literatures on the transmission of disability and unemployment benefits apply similar methods to determine causal transmission effects. They use instrumental variable approaches (Dahl et al. 2014, Grübl et al. 2020), apply regression discontinuity designs (Dahl and Gielen 2021, Cobb-Clark et al. 2020), consider family fixed effects methods (Bratberg et al. 2015), and apply the Gottschalk (1996) approach (Eckhaugen 2009, Maeder et al. 2015, Mueller et al. 2017). Overall, the evidence is mixed: Dahl et al. (2014), Dahl and Gielen (2021), and Grübl et al. (2020) confirm causal intergenerational transmission while Eckhaugen (2009)

⁴ In addition to these causal estimation strategies several contributions describe the intergenerational correlations observed in the U.S. (see e.g. Page 2004, Ratcliffe 2002).

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 (e.g., father-child vs. mother-child or father-son vs. father-daughter).

So far, little research has addressed the intergenerational welfare transmission in Germany. While there are a number of studies on income and unemployment transmission⁵ research on welfare receipt is limited.⁶ 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 a time when almost 40 percent of the sample still live in the parental household. He applies regional characteristics as instruments as well as bounds analyses. When comparing young adults at age 13-16 from households with social assistance receipt to those from low income households without social assistance receipt Siedler (2004) concludes that parental benefit receipt is exogenous. Therefore, the correlation patterns are interpreted as causal effects.

We contribute to the international literature on intergenerational welfare transmission by offering evidence from more than three decades of survey data that has not been investigated before. We describe intergenerational correlation patterns and discuss potential mediating effects. In contrast to much of the literature which focuses on mother-daughter pairs we can compare outcomes for young men and women and separately evaluate the transmission from fathers and mothers. We study intergenerational welfare transmission before and after a reform that was intended to activate benefit recipients. 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. We apply two approaches to account for the potential endogeneity of

⁵ For studies on the intergenerational correlation of earnings see e.g., Schnitzlein (2014, 2016), on the intergenerational correlation of unemployment see Maeder et al. (2015) or Mueller et al. (2017).

⁶ Eberharter (2011) and Groh-Samberg (2014) describe the correlation of different dimensions of poverty. Schels (2018) estimates correlation patterns between parental socioeconomic status and youth welfare benefit receipt in 2005.

parental welfare receipt in order to come closer to the true causal intergenerational transmission effect.

We find 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 find that intergenerational transmission did not decline and may even have increased 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.

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

2. 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 for groups such as the unemployed, the elderly, the disabled, and the poor.⁷ In our analysis, we jointly consider those

⁷ 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

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 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, 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 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 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 to shorten the period of transfer receipt. Except for shortened payout periods, the unemployment insurance benefit was not affected by the reform.⁸

Figure 1 summarizes the institutional changes caused by the reform: the former unemployment assistance and social assistance programs were combined in the new unemployment benefit II (UB II) program, a means-tested and tax-financed benefit for those able to work. Since the

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.

⁸ Riphahn and Schrader (2020) study the effect of the reduced payout period.

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*). Individuals below age 25 are supported only if they remain in the parental household. Independent individual claims against the UB II system are possible starting at age 25. 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.⁹

Figure 2 describes the utilization of the welfare programs over time. The absolute number of social assistance recipients (blue line) increased since 1980 from below 1 mio to almost 3 mio individuals in 2004.¹⁰ Similarly, the number of unemployment assistance recipients (orange line) increased substantially over time - since 1991 covering East Germany, as well. The dotted grey line presents the sum of social assistance and unemployment assistance beneficiaries; as some individuals may have benefitted from both programs the addition

⁹ For details on the German welfare system see BMAS (2019, 2020).

¹⁰ 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**.

generates an overcount. The unemployment assistance program disappeared in 2005. Immediately after the reform, the number of unemployment benefit II (UB II) recipients (grey line) surpassed 5 mio 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 mio 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.¹¹

3. Empirical Model and Methods

3.1 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 $t0$:

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

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 which controls for a set of individual and household level covariates (X) such as age, gender, migration background, and region of residence:

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

¹¹ There is substantial non take-up in the German welfare system of more than 40 percent of the eligible population. For recent evidence see e.g. Bruckmeier et al. (2021), Bruckmeier and Wiemers (2017), and for the pre-reform welfare system Riphahn (2001).

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_i^C . This, however, is unlikely if parent and child welfare participation are both affected by unobserved heterogeneities (e.g., tastes, preferences, biological factors, abilities, or unobserved regional characteristics).¹² Let parental welfare receipt be modelled by

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

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

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

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

where μ_i^C and μ_i^P are uncorrelated random error components. If there are unobserved family characteristics we expect $\text{corr}(\alpha_i^C, \alpha_i^P) \neq 0$. 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 t_0 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

¹² Chetty and Hendren (2018a) study neighborhood effects on intergenerational mobility of income rank positions. While their setting is conceptually related to our problem the endogeneity of a neighborhood (conditional on parental income position) with respect to child outcomes differs from that of parental welfare receipt.

household characteristics at age 17 of the individual (household size, number of children in parental household, federal state of residence).

In the second step of our analysis, we apply two separate strategies to identify 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.

3.2 Family fixed effects

Numerous studies of the intergenerational transmission of program participation apply a sibling or family fixed effects approach.¹³ 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 to account for family constant effects. The identifying assumption is that the family background effect is

¹³ 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).

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.¹⁴

3.3 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). The key idea is that the total correlation between parental welfare receipt (in period t_0) and subsequent child welfare receipt (in period t_1) comprises causal and non-causal elements. In contrast, the correlation between parental welfare receipt observed after child welfare receipt (in t_2) and child welfare receipt (in t_1) entails only non-causal correlation. If both correlation measures, i.e., between period t_0 and t_1 and between period t_2 and t_1 , are identical then there exists no causal effect from parent (t_0) to child receipt (t_1) 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 of two correlation estimates. Consider the model

¹⁴ As German households are comparatively immobile regionally, controlling for family fixed effects in most cases accounts for neighborhood and regional effects (Chetty and Hendren 2018a, 2018b).

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

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 t_2 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 mechanisms that would cause a bias for both methods in the same direction. Therefore, we offer evidence from two independent approaches.

4. 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 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 of

parental welfare receipt in prior survey waves when the individual was a child.¹⁵ 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.¹⁶

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); this limits our sample to birth cohorts 1956 (age 28 in 1984) to 1991 (age 26 in 2017). 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 15-18.¹⁷ This restricts the birth cohorts with full information to 1969 (age 15 in 1984) to 1991. With these sample restrictions our main analysis sample comprises 2,403 different individuals for whom information on own welfare receipt at age 25-29 as well as on parental welfare receipt at age 15-18 is available. 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

¹⁵ 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. However, 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).

¹⁶ Available administrative data on welfare receipt covers the years 2007-2017 (Dummert et al. 2020). This observation period is insufficient to describe welfare use by two generations.

¹⁷ Additionally, we use any available information for the time since they reached age 10 but do not require this information for consideration in the sample.

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 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 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.¹⁸ 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 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 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 t0.¹⁹ We find that compared to non-recipients, welfare recipients are more likely to be female, slightly older, born with higher parity, and grew up in larger parental households. Similarly, those who experienced parental welfare receipt grew up in larger parental households, with younger parents and were younger themselves. Child and parent welfare receipt are associated with lower parental secondary education.²⁰

¹⁸ 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.

¹⁹ The covariates describing parental welfare benefit receipt in period t2 in the Gottschalk analyses are described in **Table A.7**.

²⁰ **Table A.3** provides descriptive statistics for our explanatory variables for the main sample.

5. Results

5.1 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 t_0 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 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 by 18-26 percent 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.²¹ 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 by 21 percentage points when parents were on welfare for the full observation period.

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 t_1) 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

²¹ **Table A.4** in the appendix presents the full set of estimation results.

both specifications: the propensity to ever receive welfare increases slightly with the number of years of parental welfare receipt experienced.²² 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.

5.2 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.²³ 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. Our results yield only minor

²² This result deviates from Hartley et al. (2022) who find that the correlation is insensitive to the duration of parental welfare receipt in their data. In contrast, Levine and Zimmerman (2005) show that child outcomes suffer more if mothers spend a higher share of time on welfare.

²³ 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 more moderate (see **Table A.5** for descriptive statistics by gender).

differences in parent-specific correlation patterns for the basic specification.²⁴ In separate estimations, we considered child gender interaction terms in the estimations for fathers' 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. The studies on intergenerational transmission of disability benefits in Norway disagree: 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.

5.3 Heterogeneity by age of exposure

Numerous contributions discuss the relevance of a child's age at exposure for intergenerational transmission effects. The findings vary depending on the type of treatment. Bratberg et al. (2015) or Dahl and Gielen (2021) study parental disability which in contrast to welfare receipt is typically permanent after onset; therefore, the relevance of age of exposure may differ for treatments connected to parental health as opposed to potentially temporary minimum income support.²⁵ Recently, Carneiro et al. (2021) studied the connection between the timing of parental

²⁴ 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. In a few cases (28 for mothers and 119 for fathers) we have no information on the person-specific welfare history. Similarly, there are no major differences by parent gender in extended specification. Results are available upon request.

²⁵ 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

income shocks and the next generation's human capital outcomes. Conditional on household permanent income they find for the birth cohorts 1971-1980 in Norway, that parental income is least productive during age 6-11 of children. In contrast, younger (age 0-5) and older (age 12-17) children benefit more from positive income shocks.

Edmark and Hanspers (2015) and Hartley et al. (2022) compare the relevance of parental welfare receipt across child exposure ages. The former consider register data on the 1981-1983 birth cohorts and their parents in Sweden. The dependent variable describes the young generation's welfare receipt at age 24. It is regressed on different indicators of parental welfare use during child age groups 9-12, 13-16, and 17-19. The results consistently show the strongest intergenerational correlation if the young generation was exposed at age 17-19. The authors 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) consider U.S. survey data from the PSID on mother-daughter pairs. Their results yield larger correlations for older ages at exposure: the correlation of exposures at ages 4-8 through 9-13 is much smaller than that of exposure at ages 10-14 through 13-17. The authors 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

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.

impacts for the youngest and the oldest group.²⁶ 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 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.

5.4 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 2). It increased job search monitoring and it reduced benefits for some long term unemployed. We describe intergenerational correlation patterns before and after the reform.²⁷ We consider individuals who reached age 25 before 2005 (birth cohorts 1969-1979) 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.²⁸ The samples sizes are comparable with 1,193 and 1,210 observations before and after the reform. Appendix **Table A.6** describes the 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

²⁶ **Table 4** shows results for the basic specification only. The results are similar when the extended specification is estimated (available upon request).

²⁷ 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).

²⁸ In contrast to Cobb-Clark et al. (2020) who study the intergenerational spillover effect of a reform that affects the parent generation we focus on the young generation.

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. The estimation results for the first welfare outcome ("ever welfare") yield that the intergenerational correlation did not change substantively after the reform. In contrast, the intergenerational correlation measure for the quantitative welfare measures increased substantially for the birth cohort who turned 25 after the reform. These results suggest that the reform hardly affected the overall propensity to use welfare. Also, it may not have succeeded in mobilizing recipients to leave the program faster than before. After the reform, the intensity of youth welfare use was more correlated with the intensity of parental welfare use than before the reform confirming the patterns in **Figure A.2**.

5.5 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.²⁹ **Table 6** shows the estimated correlation patterns that result after adding the child education controls

²⁹ We use the information on ISCED levels (International standard classification of education) as provided by the SOEP data and consider no secondary schooling degree or less than upper

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)).³⁰

6. Results and Robustness - Causal estimation approaches

6.1 Family fixed effects

While correlation studies are informative with respect to the intergenerational patterns of welfare receipt their estimates cannot uncover causal effects. We apply two methods to get closer to causal effect estimation. In the family fixed effects model we take advantage of observing siblings from the same family. 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 3).

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 extended specification 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.

secondary school, upper secondary school degree, vocational training degree, academic tertiary education degree (for summary statistics see **Table A.3**).

³⁰ 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).

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.³¹

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.

6.2 Gottschalk estimation

For our second approach to approximate causal welfare transmission effects we apply the procedure developed by Gottschalk (1996) as characterized in section 3.3 above. The key idea is to 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

³¹ 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, surprisingly 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.

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 slightly 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. 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 labelled "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.³²

While the negative total effect is surprising it reflects the findings of other authors. Ekhaugen (2009) and Mueller et al. (2017) similarly obtained negative estimates for the transmission of unemployment benefits. Boschman et al. (2019) study different Dutch programs and find negative effects for the social assistance and disability programs. The authors argue that it is not the experience of the welfare program itself that causes the next generation's

³² The statistical significance of the effect in column 1 - but not its negative sign - disappears when we consider only daughters, only pre-reform observations, only those with at least three measurements in the t2 period, or when we condition on leaving the parental home by age 30.

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. After a recent major welfare reform in Germany intergenerational welfare correlations may even have increased. 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 offspring of welfare recipients at an elevated risk of welfare receipt, which is a highly policy relevant finding.

Our conclusions are subject strong identifying assumptions and should be reinvestigated when larger samples are available. It seems worthwhile to direct future research at the determinants and relevance of youth educational attainment, which might be malleable by public policy. Also, it is important to better understand the mechanisms behind the increase 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-takeup.

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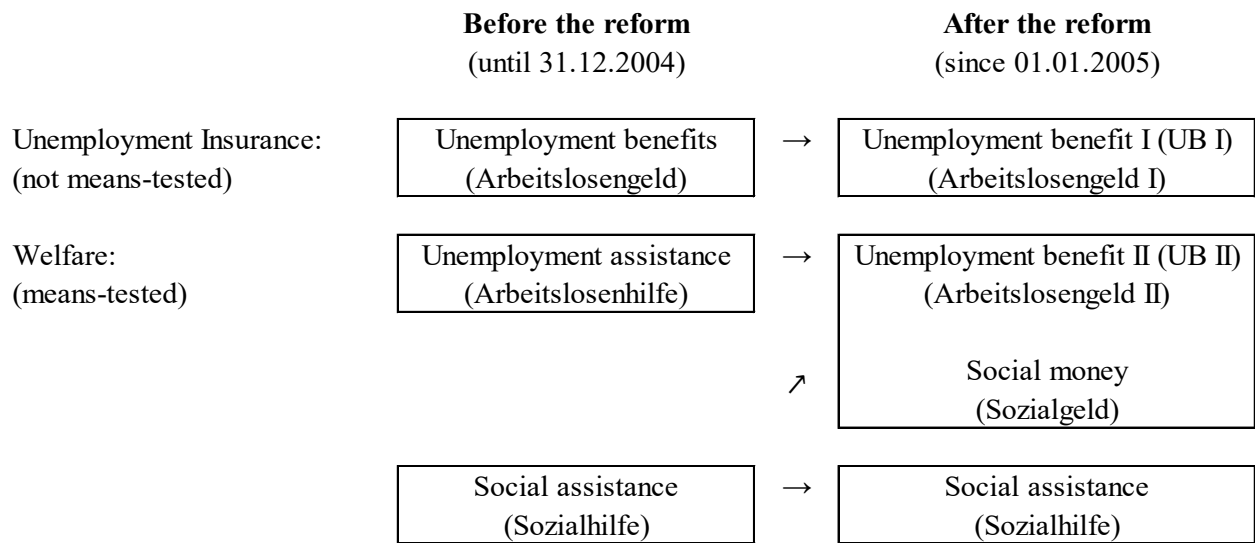
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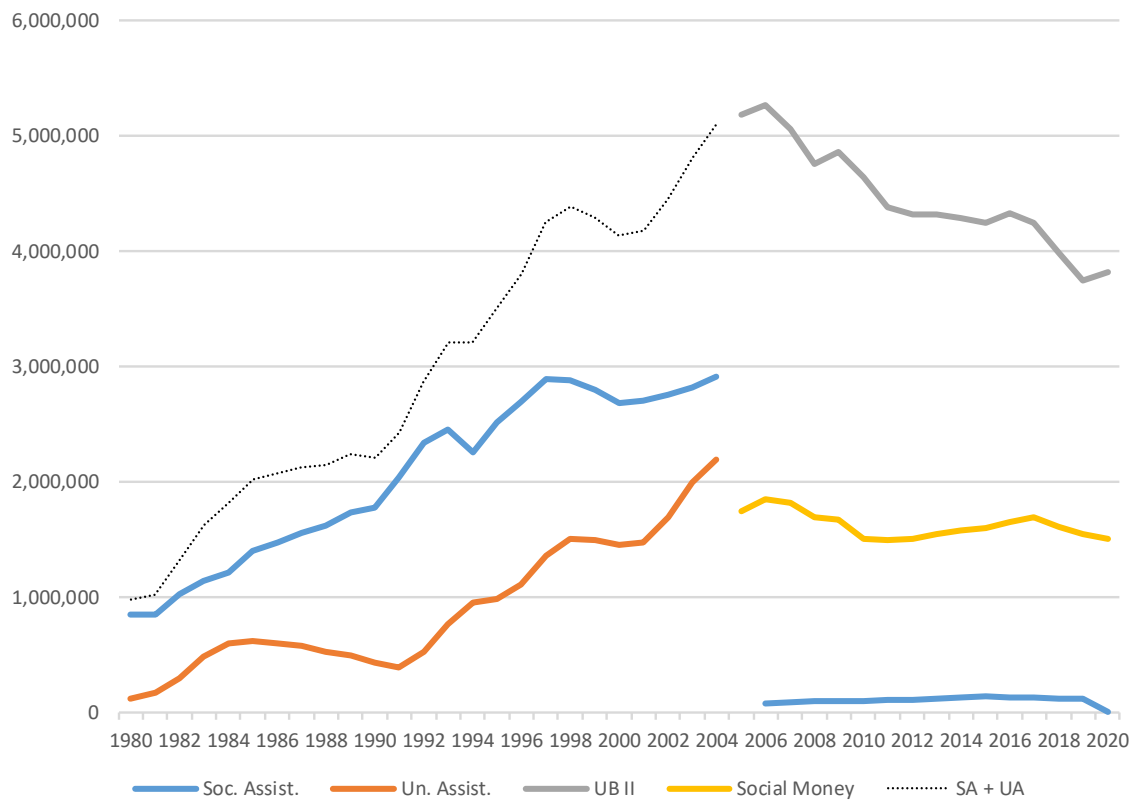
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Figure 1 Unemployment and welfare institutions for the working age population in Germany before and after the 2005 reform



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 Intergenerational correlation

	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	0.9679	0.8688	0.1936	0.1989	0.1977
number (years)	0.9679	1.0000	0.8064	0.1765	0.1823	0.1800
share (%)	0.8688	0.8064	1.0000	0.2277	0.2404	0.2381
Welfare receipt t_0 (parent)						
ever (0/1)	0.1936	0.1765	0.2277	1.0000	0.7791	0.7880
number (years)	0.1989	0.1823	0.2404	0.7791	1.0000	0.9642
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*** (0.036)	0.083** (0.038)	0.151*** (0.050)
Female \times Parent welfare, t_0	0.074 (0.050)	0.145** (0.059)	0.117 (0.039)
Panel B: Male sample (N=1,205)			
Parent welfare, t_0 , basic specification	0.161*** (0.036)	0.134*** (0.038)	0.201*** (0.051)
Parent welfare, t_0 , extended specification	0.109*** (0.037)	0.089** (0.039)	0.154*** (0.049)
Panel C: Female sample (N=1,198)			
Parent welfare, t_0 , basic specification	0.213*** (0.037)	0.269*** (0.047)	0.306*** (0.058)
Parent welfare, t_0 , extended specification	0.166*** (0.035)	0.214*** (0.047)	0.254*** (0.056)
Panel D: Maternal welfare receipt – basic specification (N=2,375)			
Mother welfare, t_0	0.198*** (0.028)	0.209*** (0.034)	0.251*** (0.042)
Panel E: Paternal welfare receipt – basic specification (N=2,284)			
Father welfare, t_0	0.196*** (0.032)	0.231*** (0.047)	0.295*** (0.055)
Panel F: Maternal welfare receipt – ext. specification with gender interaction (N=2,375)			
Mother welfare, t_0	0.106*** (0.039)	0.078* (0.041)	0.138*** (0.051)
Female \times Maternal welfare, t_0	0.088 (0.054)	0.164** (0.065)	0.129 (0.079)
Panel G: Paternal welfare receipt – ext. specification with gender interaction (N=2,284)			
Father welfare, t_0	0.109** (0.045)	0.099* (0.058)	0.209*** (0.080)
Female \times Paternal welfare, t_0	0.077 (0.062)	0.139 (0.091)	0.067 (0.106)

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 4 Heterogeneity of welfare correlation by age of exposure

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (years)	(3) share (%)
Panel A - Separate estimations by age group - Basic specification			
Age group 10-12, t_0 (N=1,242)	0.225*** (0.046)	0.559** (0.117)	0.282*** (0.060)
Age group 13-15, t_0 (N=1,835)	0.189*** (0.037)	0.400*** (0.081)	0.196*** (0.040)
Age group 16-18, t_0 (N=2,403)	0.201*** (0.030)	0.427*** (0.065)	0.203*** (0.032)
Panel B - Joint estimations for all age groups (N=1,216) - Basic specification			
Age group 10-12, t_0	0.135** (0.055)	0.303* (0.156)	0.147** (0.071)
Age group 13-15, t_0	0.051 (0.052)	0.129 (0.150)	0.013 (0.068)
Age group 16-18, t_0	0.123** (0.048)	0.325*** (0.121)	0.156*** (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) ever (0/1)	(2) number (years)	(3) share (%)
Panel A – Basic specification			
Pre-Reform (N=1,193)			
Parent welfare, t_0	0.181*** (0.0453)	0.169** (0.0675)	0.137** (0.0594)
Post-Reform (N=1,210)			
Parent welfare, t_0	0.196*** (0.0317)	0.218*** (0.0342)	0.308*** (0.0475)
Panel B – Extended specification			
Pre-Reform (N=1,193)			
Parent welfare, t_0	0.149*** (0.0444)	0.139** (0.0691)	0.105* (0.0614)
Post-Reform (N=1,210)			
Parent welfare, t_0	0.143*** (0.0313)	0.176*** (0.0348)	0.264*** (0.0459)

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 6 Controlling for child education as a potential mediator

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (yrs)	(3) share (%)
Parent welfare, t_0 (basic specification)	0.148*** (0.025)	0.161*** (0.031)	0.212*** (0.039)
Parent welfare, t_0 (extended specification)	0.112*** (0.025)	0.129*** (0.031)	0.180*** (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*** (0.037)	0.233*** (0.044)	0.290*** (0.058)
Parent welfare (extended)	0.132*** (0.036)	0.162*** (0.043)	0.211*** (0.054)
Panel B: FE Regressions (N=1,161)			
Parent welfare (basic)	-0.007 (0.076)	-0.028 (0.087)	-0.150 (0.104)
Parent welfare (extended)	0.009 (0.78)	-0.031 (0.089)	-0.163 (0.115)
Panel C: FE Regressions w/o welfare experience of oldest child (N=1,150)			
Parent welfare (basic)	0.003 (0.081)	0.002 (0.086)	-0.155 (0.112)
Parent welfare (extended)	0.009 (0.078)	-0.031 (0.089)	-0.163 (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

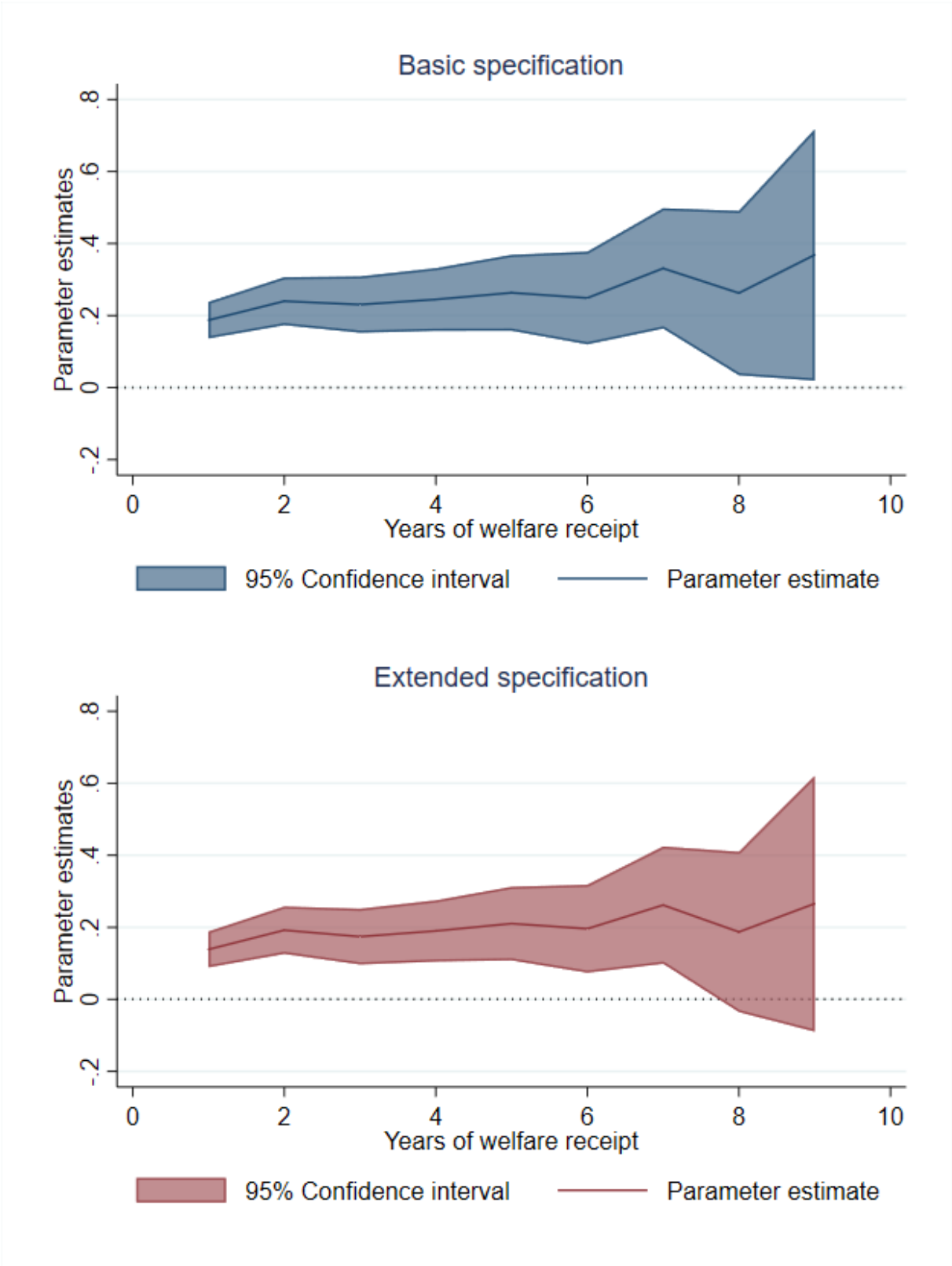
	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (years)	(3) share (%)
Panel A: OLS results for Gottschalk estimation sample (N=1,221)			
Parent welfare, t_0 , basic specification	0.178*** (0.039)	0.249*** (0.058)	0.260*** (0.062)
Parent welfare, t_0 , extended specification	0.131***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

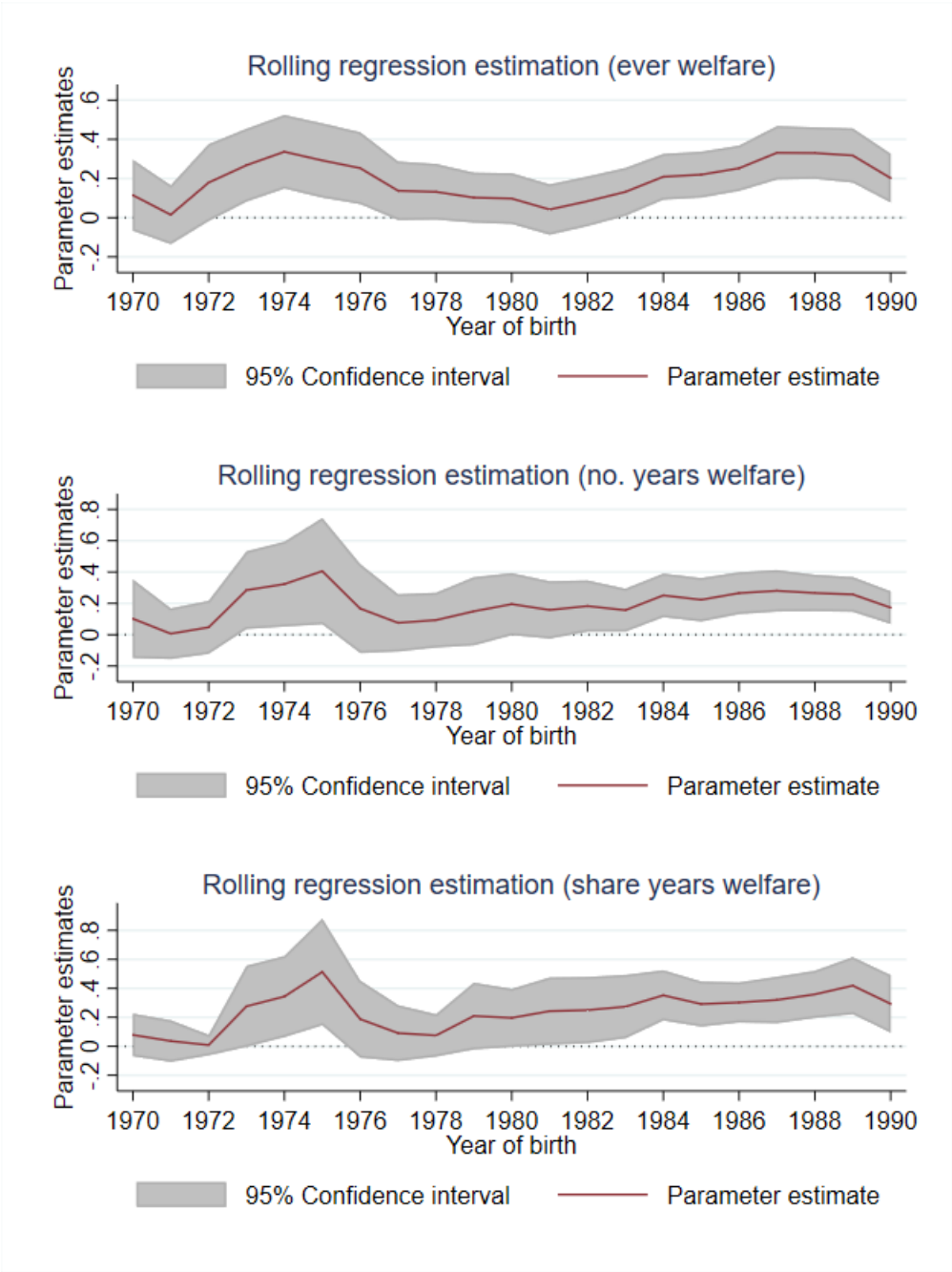
Online Appendix

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	Causal?	Data	Exposure (t0)		Own welfare (t1)		Parent	Child
						Age	#Years	Age	#Years		
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

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) Mean (all)	(2) Mean (ever = 0)	(3) Mean (ever = 1)	(4) 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***
<i>N</i>	2,403	2,096	307	

Panel B By parental welfare receipt in period t_0

	Group Differentiator: Welfare receipt t_0			
	(1) Mean (all)	(2) Mean (ever = 0)	(3) Mean (ever = 1)	(4) 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***
<i>N</i>	2,403	2,071	332	

Notes: Indicators for federal states, 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.
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
father welfare ever (0/1)	2,284	0.09501	0.29329	0	1
father welfare years	2,284	0.23642	0.91474	0	9
father welfare share (%)	2,284	0.03471	0.13526	0	1
mother welfare ever (0/1)	2,375	0.11705	0.32155	0	1
mother welfare years	2,375	0.35747	1.26210	0	9
mother welfare share (%)	2,375	0.04788	0.16609	0	1
miss_10	2,403	0.47566	0.49951	0	1
miss_11	2,403	0.39534	0.48903	0	1
miss_12	2,403	0.31003	0.46260	0	1
miss_13	2,403	0.22430	0.41721	0	1
miss_14	2,403	0.12484	0.33061	0	1
miss_25	2,403	0.03412	0.18159	0	1
miss_26	2,403	0.03454	0.18265	0	1
miss_27	2,403	0.13858	0.34558	0	1
miss_28	2,403	0.25968	0.43855	0	1
miss_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
<i>Parent education</i>					
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
<i>Federal States</i>					
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
<i>Child education</i>					
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) ever (0/1)	(2) number (years)	(3) 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)
miss_10	0.0419 (0.0272)	0.167 (0.120)	0.0193 (0.0152)
miss_11	-0.0785** (0.0335)	-0.311** (0.147)	-0.0324* (0.0194)
miss_12	0.0293 (0.0284)	0.154 (0.128)	-0.0106 (0.0154)
miss_13	-0.0645** (0.0263)	-0.304** (0.119)	-0.0144 (0.0143)
miss_14	0.0626** (0.0258)	0.280** (0.114)	0.0235 (0.0148)
miss_25	0.00503 (0.0352)	-0.157 (0.120)	0.0128 (0.0215)
miss_26	0.0585 (0.0397)	-0.0376 (0.124)	0.0557* (0.0289)
miss_27	-0.00385 (0.0207)	-0.123** (0.0584)	0.00382 (0.0135)
miss_28	-0.0221 (0.0215)	-0.191** (0.0766)	-0.00777 (0.0124)
miss_29	0.00882 (0.0218)	-0.116 (0.0854)	0.0161 (0.0119)
Female	0.0263** (0.0133)	0.116** (0.0588)	0.0155** (0.00740)
Year of birth oldest parent	0.00400*** (0.00122)	0.0187*** (0.00540)	0.00155** (0.000710)
No. children in Hh. at age 17	0.0102 (0.00810)	0.0482 (0.0361)	0.00670 (0.00462)
No. children in Hh. at 17, missing dummy 1	0.0147 (0.0277)	0.0521 (0.128)	0.0123 (0.0158)
No. children in Hh. at 17, missing dummy 2	0.0194 (0.0259)	0.137 (0.116)	0.0217 (0.0168)
HH size at age 17	-0.000280 (0.00713)	-0.00965 (0.0326)	-0.00211 (0.00362)
HH size at age 17, missing dummy	0.0300 (0.0250)	0.157 (0.110)	0.0205 (0.0154)
Migration, first generation	0.0128 (0.0351)	0.0664 (0.157)	-0.00750 (0.0194)
Migration, second generation	-0.00649 (0.0234)	-0.0623 (0.105)	-0.0156 (0.0146)

Parity, second	0.0171 (0.0156)	0.0718 (0.0696)	0.00415 (0.00829)
Parity, third or higher	0.102*** (0.0239)	0.417*** (0.106)	0.0552*** (0.0142)
Parity missing dummy	0.0433 * (0.0250)	0.133 (0.112)	0.0104 (0.0153)
Cohort (1973-1975)	-0.0637** (0.0314)	-0.237* (0.140)	-0.0334* (0.0179)
Cohort (1976-1979)	-0.0233 (0.0344)	-0.0420 (0.156)	-0.00861 (0.0203)
Cohort (1980-1982)	-0.00527 (0.0471)	0.0297 (0.212)	0.00701 (0.0285)
Cohort (1983-1985)	-0.0946** (0.0459)	-0.385* (0.205)	-0.0365 (0.0273)
Cohort (1986-1988)	-0.114** (0.0474)	-0.504** (0.213)	-0.0497* (0.0281)
Cohort (1989-1991)	-0.168*** (0.0519)	-0.639*** (0.226)	-0.0812*** (0.0310)
<i>Parent Education</i>			
Secondary school	-0.0366** (0.0180)	-0.127 (0.0793)	-0.0202* (0.0108)
Upper sec. school	-0.0606*** (0.0197)	-0.240*** (0.0868)	-0.0368*** (0.0113)
Other	0.0177 (0.0294)	0.133 (0.129)	0.0119 (0.0174)
<i>Federal States</i>			
Bayern	0.00214 (0.0195)	-0.00815 (0.0844)	-0.00192 (0.0101)
Berlin	0.131*** (0.0400)	0.590*** (0.174)	0.0671*** (0.0223)
Hessen	-0.00719 (0.0251)	0.0100 (0.113)	-0.0140 (0.0120)
Brandenburg / Mecklenburg-Vorp.	0.169*** (0.0432)	0.628*** (0.177)	0.1000*** (0.0270)
Bremen / Niedersachsen	0.0223 (0.0250)	0.0911 (0.112)	-0.00436 (0.0124)
Nordrhein-Westfalen	0.0566*** (0.0205)	0.264*** (0.0927)	0.0257** (0.0114)
Saarland / Rheinland-Pfalz	0.0462 (0.0332)	0.222 (0.156)	0.0145 (0.0184)
Sachsen-Anhalt	0.141*** (0.0338)	0.565*** (0.145)	0.0730*** (0.0196)
Sachsen	0.206*** (0.0465)	0.910*** (0.214)	0.0901*** (0.0272)
Hamburg / Schleswig-Holstein	0.0153 (0.0308)	0.0588 (0.134)	0.00887 (0.0188)
Thüringen	0.149*** (0.0428)	0.591*** (0.184)	0.0828*** (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 1969-1979)					
Welfare receipt t_1 (age 25-29)					
ever (0/1)	1,193	0.13244	0.33911	0	1
number (yrs)	1,193	0.59765	1.56591	0	5
share (%)	1,193	0.05977	0.17442	0	1
Welfare receipt t_0 (age 10-18)					
ever (0/1)	1,193	0.08885	0.28464	0	1
number (yrs)	1,193	0.20788	0.90600	0	9
share (%)	1,193	0.02973	0.12649	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 Intergenerational correlation

	Welfare receipt t_1 (child)			Welfare receipt t_0 (parent)		
	ever (0/1)	number (years)	share (%)	ever (0/1)	number (years)	share (%)
(1) Welfare receipt t_1 (child)						
Pre-reform						
ever (0/1)	1.0000	0.9679	0.8773	0.1474	0.1556	0.1605
number (years)	0.9773	1.0000	0.8336	0.0933	0.0951	0.1030
share (%)	0.8773	0.8336	1.0000	0.0922	0.0940	0.0989
Post-reform						
ever (0/1)	1.0000	0.9592	0.8684	0.2399	0.2800	0.2806
number (years)	0.9592	1.0000	0.7955	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)	0.1474	0.1556	0.1605	1.0000	0.7351	0.7530
number (years)	0.0933	0.0951	0.1030	0.7351	1.0000	0.9610
share (%)	0.0922	0.0940	0.0989	0.7530	0.9610	1.0000
Post-reform						
ever (0/1)	0.2399	0.2124	0.2708	1.0000	0.7983	0.8010
number (years)	0.2800	0.2653	0.3136	0.7983	1.0000	0.9655
share (%)	0.2806	0.2626	0.3157	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)	(2)	(3)
	Mean (Full Sample)	Mean (Gottschalk Sample)	=(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.