CESifo
CONFERENCES 2021

12th Norwegian German Seminar on Public Economics

Munich, 5 – 6 November 2021

Intergenerational Transmission of Welfare: Evidence from Germany

Jennifer Feichtmayer Regina T. Riphahn

CESifo

NHH

OFS
Oslo Fiscal Studies

Intergenerational Transmission of Welfare: Evidence from Germany

Jennifer Feichtmayer
Regina T. Riphahn

Friedrich-Alexander-University Erlangen-Nürnberg

October 29, 2021

We study the intergenerational transmission of welfare 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 using the Gottschalk (1996) approach and a family fixed effects estimation. We take advantage of the long running German Socio-Economic Panel Survey to contribute to a sparse literature. Our findings of strong positive correlations between parental and own welfare receipt confirm the international literature. However, these correlations do not hold up to controls for unobserved heterogeneities. This suggests that the patterns do not reflect the direct transmission of welfare receipt but instead the intergenerational correlation of relevant characteristics.

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

JEL Code: I32, I38, J62, C36

Corresponding Author:
Regina T. Riphahn
Economics Department
FAU Erlangen-Nürnberg
Lange Gasse 20
90403 Nürnberg, Germany
regina.riphahn@fau.de

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, for surveys see Black and Devereux 2011 or Blanden 2013). 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.

Internationally, most studies confirm, both, positive intergenerational correlations and causal effects of parental welfare receipt on the subsequent welfare receipt of their offspring. Various mechanisms may generate this transmission: after experiencing parental welfare receipt youths may be better informed about application procedures and institutional features, they 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 directly increases the next generation's welfare receipt by any such mechanism the welfare program has negative externalities.

We investigate the intergenerational transmission of welfare receipt in Germany. Our long running panel data allow us to study the transmission of youth welfare experience for individuals born 1969-1991. This is one of the first analyses of intergenerational welfare

transmission for Germany. 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.

In a first step, we describe the correlation of parent and child welfare receipt in terms of incidence and duration. In contrast to some prior studies which could use only few years of observations[1] we have long running data. While we focus on youths' early impressionable years (Krosnick and Alwin 1989) we also investigate whether the age of observing parental benefit receipt matters. We look into the potential mediation effect of child educational outcomes and compare patterns by child gender. Also, we are able to compare the correlation patterns before and after a major welfare reform.

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 this 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). 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

---

[1] For example, Antel (1992), Moisio 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.

(1992) concludes that maternal welfare use causally affects daughters' receipt whereas Levine and Zimmerman (1996) suggest that we do not observe a welfare trap but a poverty trap. They argue that welfare receiving parents transmit low earnings potentials to their children which explains most of the observed correlation in welfare receipt.

Gottschalk (1996) studies U.S. welfare transmission using data from the Panel Study of Income Dynamics (PSID) and applying event study methods. He finds a causal relationship between mothers' and daughters' welfare receipt. Pepper (2000) also uses PSID data and compares alternative empirical approaches to address the endogeneity of maternal welfare receipt. He confirms a causal relationship.

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 of the exposure in the parental household on the next generations' own welfare receipt.

Hartley et al. (2017) use PSID data to study the effect of the U.S. welfare reform on intergenerational transmission. The authors consider mother-daughter pairs with at least 5 years with potential welfare receipt observed for each generation. They use instrumental variables and difference-in-differences strategies based on regional heterogeneities. The authors find that mothers' use of welfare increases the probability of their daughters' welfare participation. However, welfare reforms did attenuate the transmission.[2]

There are only few studies on minimum income and welfare programs in countries outside the U.S.. Beaulieau et al. (2005) exploit administrative data on social assistance receipt in Quebec, Canada. Based on structural estimation approaches they confirm causal intergenerational effects and conclude that parental welfare receipt is most influential when the child is aged either 7-9 or 16-17. Edmark and Hanspers (2015) apply family fixed effects

---

[2] 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).

estimation to Swedish register data. While intergenerational correlation in welfare receipt is strong they find no causal effects once family fixed effects are considered. De Haan and Schreiner (2018) study administrative data from Norway. They apply bounds analyses with instrumental variables to identify causal effects. While a large share of intergenerational correlation is explained by shared characteristics within the family they nevertheless confirm significant causal transmission effects. Boschman et al. (2019) apply the Gottschalk (1996) approach to administrative data in the Netherlands. For pooled public transfer programs (disability, unemployment, and social assistance benefits) they confirm significant causal effects but not for social assistance per se. They study several mechanisms and find a small role for child education as a mediator. Overall, the majority of studies confirms the existence of causal intergenerational transmission of welfare participation.

The literatures on the transmission of disability and unemployment benefits apply similar methods to determine causal transmission effects.[3] 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) 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).

---

[3] Dahl et al. (2014), Bratberg et al. (2015), de Haan and Schreiner (2018), and Dahl and Gielen (2021) study the transmission of disability benefits whereas Ekhaugen (2009), Maeder et al. (2015), Mueller et al. (2017), Grübl et al. (2020) focus on unemployment benefits.

So far, little research has addressed the intergenerational welfare transmission in Germany. While there are a number of studies on income and unemployment transmission[4] research on welfare receipt is limited.[5] Closest to our analysis is an unpublished study by Siedler (2004): based on early data from the German Socioeconomic Panel (1984-2002) he investigates intergenerational correlation in social assistance receipt. He focuses on young adults' benefit receipt observed 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 from households with social assistance receipt at age 13-16 to those from low income households without social assistance receipt at that age Siedler (2004) concludes that parental benefit receipt is not endogenous. Therefore, the correlation patterns are interpreted as causal effects where sons respond stronger to parental benefit receipt than daughters.

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 offer a broad description of intergenerational correlation patterns and discuss potential mediating effects. In contrast to much of the prior 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 the heterogeneity of 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 different approaches to account for the potential endogeneity of parental welfare receipt in order to come

---

[4] For contributions on the intergenerational correlation of earnings see e.g., Schnitzlein (2014, 2016); for intergenerational correlation of unemployment see Maeder et al. (2015) or Mueller et al. (2017).

[5] 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.

closer to the true causal intergenerational transmission effect. As both approaches require different sets of identifying assumptions it is reassuring that they yield comparable results.

We differentiate three different welfare indicators and find strong intergenerational correlation in welfare outcomes. 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 is more detrimental than in the 13-15 age window. Comparing the correlation pattern before and after a major welfare reform we find that intergenerational transmission did not decline and may even have increased post-reform. Child educational attainment appears to be an important mediator between parent and child welfare receipt. Both, the family fixed effects and the Gottschalk (1996) method identify causal effects under certain, yet different assumptions. Both strategies fail to find evidence of a causal impact of parental welfare receipt on child welfare outcomes. Thus, it is not the experience of parental welfare receipt that drives subsequent child welfare receipt. Instead, the correlation of individual characteristics and circumstances in the child and parent household generate intergenerational correlations in welfare receipt.

In the next section, we provide institutional background for our analyses. 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 welfare state institutions provide for groups such

as the unemployed, the elderly, the disabled, and those with insufficient financial means.[6] 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). We label these transfers "welfare" benefits. As the welfare state underwent a major reform beginning January 1, 2005 we distinguish the pre- and post-reform institutions (see **Figure 1** for illustration).

Generally, the unemployed receive benefits from the unemployment insurance. Unemployment benefits (*Arbeitslosengeld*) replace up to 67 percent of previous net labor earnings. After the reform, the benefit was renamed unemployment benefit I (*Arbeitslosengeld I*). The reform shortened the maximum potential benefit duration from 32 to 18 months. As unemployment benefits are not means tested they are not part of our measure of welfare benefits.

Before the reform, those who had exhausted their unemployment benefit entitlement as well as those who were not (yet) entitled to unemployment benefits were eligible for a second tier unemployment benefit: unemployment assistance (*Arbeitslosenhilfe*) was a tax-financed means-tested benefit. 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.

In addition, individuals could claim (additional) means tested social assistance (*Sozialhilfe*) if their household income, i.e., the combination of any income, unemployment benefits or unemployment assistance, was too low to cover the formally defined financial need of the household. Social assistance was a means-tested program for general income support. Since the means test considers household size, a given household income leaves large households (e.g., with many children) more likely to be eligible for social assistance than

---

[6] "Sufficiency" is established as follows: 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 calculated financial need the household can claim government support.

smaller households (e.g., without children). Although social assistance was never intended to support employable clients, about one in six unemployment assistance claimants also received regular social assistance benefits.

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. It intended to activate the unemployed based on the idea of "assist and demand" (*fordern und fördern*). Since the reform, the employment offices explicitly demand individual effort from those members of a household who are able to work for at least three hours per day or 15 hours per week. Job search incentives were increased by reduced durations of unemployment benefit payouts and by an intensified use of sanctions. The 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 2005 reform.[7]

Figure 1 summarizes the institutional changes implied by the reform: the reform combined the former means tested unemployment assistance and social assistance programs in the new unemployment benefit II (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) and, in contrast to the pre-reform unemployment assistance program, is not related to prior earnings. Generally, all individuals - including those who are employed - can claim UB II if their household passes the means test and 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*). Since the reform, the previous social assistance (*Sozialhilfe*) program is available

---

[7] The unemployment benefit payout period was shortened for recipients age 45 and above starting in 2006. Riphahn and Schrader (2020) study the effect of the reduced payout period.

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.

Regarding the eligibility of unemployed young adults below age 25 the UB II regulations were initially more generous after the reform than prior social assistance provisions. In particular, single unemployed young adults could leave the parental home and establish an independent household at the expense of the welfare office. This aspect of the reform was abolished quickly within one year. Since 2006, benefit eligibility of single unemployed individuals below age 25 is conditional on remaining in the parental household. Independent individual claims against the UB II system are possible starting at age 25.

In our study, we consider the receipt of unemployment assistance (*Arbeitslosenhilfe*) and social assistance (*Sozialhilfe*) before the reform and UB II, social money, and social assistance after the reform to capture the means tested minimum income support of interest. We label the combined institutions "welfare" throughout.

The main change induced by the (*Hartz IV*) reform was the abolition of the unemployment assistance program. Individuals with high prior labor earnings who previously received unemployment assistance lost out due to the reform. Their benefit claims declined and in addition, they had to pass more stringent means tests than under the unemployment assistance program. About 7 percent of former unemployment assistance recipients lost eligibility due to the more stringent means test (BA 2005). In contrast, those who received social assistance before the reform continued to be eligible for UB II as long as they were able to work. Their benefit payouts were slightly adjusted up or down depending on individual circumstances. In most cases benefits increased after the reform, although some benefit components were abolished.[8]

---

[8] For details on the German welfare system see BMAS (2019) or the English language summary in BMAS (2020).

Figure 2 describes the development of the utilization of the welfare programs. The absolute number of social assistance recipients (blue line) increased since 1980 from below 1 mio to almost 3 mio individuals in 2004.[9] 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 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. However, the number came down almost monotonously in subsequent years. The number of social money recipients has been rather constant at about 0.8 mio and reflects individuals living in the household of UB II recipients who cannot work (mostly children). After the reform, the social assistance benefit was used only by individuals unable to work at least 3 hours per day. Therefore, its utilization dropped to rather low levels. 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.[10]

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

---

[9] This covers welfare recipients who live independently and outside of institutions (*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**.

[10] 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).

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 in a basic specification:

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

The estimate of coefficient β_0 describes 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:

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

β and γ are coefficients to be estimated. The estimate of β_1 describes the conditional correlation of welfare receipt across generations. While it may not provide the causal effect of intergenerational transmission 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.

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. 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). Let parental welfare receipt be modelled by

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

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

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

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

where μ_i^C and μ_i^P are uncorrelated random error components. If there are unobserved family background characteristics we expect corr(α_i^C, α_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 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 different specifications of the regression model. 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, parity), 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).

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 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 (Boschmann 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 contributions in the literature on the intergenerational transmission of program participation apply a sibling or family fixed effects approach.[11] Here, the endogeneity of parental welfare use is purged from equation (2) by controlling for family fixed effects in a sample with more than one sibling per family. If siblings were exposed to parental welfare receipt at different ages then sibling differences can net out time constant family background effects. The identifying assumption is that the family background effect is time constant. If different siblings pass through the family household at different points in time comparing their adult outcomes allows to account for family constant effects. 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. Similarly, the approach is not reliable if an early exposure to welfare receipt has permanent within family effects independent of subsequent welfare participation. 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 may directly 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.

3.3   The Gottschalk (1996) method

The method introduced by Gottschalk (1996) has been applied in various contributions before (Corak et al. 2004, Ekhaugen 2009, Maeder et al. 2015, Mueller et al. 2017, Boschmann et al.

---

[11] 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).

2019). 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 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 of two correlation estimates. Consider the model

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

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 correlation 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 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 (for similar applications of the method see Gottschalk 1996, Ekhaugen 2009, Maeder et al. 2015, Mueller et al. 2017, and Boschman et al. 2019). 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, the estimate may underestimate the causal effect by overstating the family specific correlation between child

outcome and late parent outcomes; 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. 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.

In contrast to the family fixed effects method the Gottschalk method can use evidence from children without siblings. Also, it is robust to some violations of the identifying assumptions of the fixed effects approach. 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 study the evidence from both 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 current individual welfare receipt with parental responses in prior survey waves.[12]

As we are interested in describing intergenerational welfare receipt our sample considers all individuals aged 25-29 (period t1) for whom there are at least two panel observations available in this age window; this limits our sample to birth cohorts 1956 (age 28 in 1984) to 1991 (age 26 in 2017). We then connect the information observed for the age window 25-29 with data on the parental household during these individuals' youth. We 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.[13] 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.

When we apply the family fixed effects approach to account for time constant family fixed effects we use only individuals with a sibling in the data. We can use 414 sibling pairs, 73 triplets, 24 quadruplets, and even three families with five or more children in the data. Overall, the family fixed effects sample entails 1,161 different individuals from 514 different families.

When we apply the Gottschalk (1996) approach to account for endogenous parental welfare receipt we focus on the subsample of individuals for whom parental welfare receipt is observed when the child observation is aged 30-35 (period t2). As we require at least one valid

---

[12] The Institute for Employment Research (IAB) provides administrative data on welfare receipt covering the years 2007-2017, the Sample of Integrated Welfare Benefit Biographies (SIG) (Dummert et al. 2020). As this observation period is insufficient to describe welfare use by two generations our SOEP survey data are not only a valid source of high quality data but also the only data available to study the intergenerational transmission of welfare receipt in Germany.

[13] 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.

parental welfare indicator in that age bracket this limits the relevant birth cohort to 1969 to 1987. With this restriction, our sample entails 1,221 different individual observations.

We consider three measures of self-reported welfare receipt for both parent and child observations. First, a binary indicator of the incidence of welfare receipt, second, a continuous measure of the number of years for which welfare receipt is observed and third - given that we observe individuals and parents for varying numbers of years - a measure that reflects the share of observation years for which welfare receipt is observed. 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. **Table 1** shows descriptive statistics for our welfare measures, where period t1 (child age 25-29) outcomes are dependent variables and the period t0 (parent welfare when child is below age 18) outcomes 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. 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.

Table 2 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.[14] The basic specification only controls for parental welfare receipt and the missing value indicators and thus offers a measure of the unconditional intergenerational correlation of welfare receipt.

In our extended specification, we control for 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 mediator of the transmission of welfare receipt. However, we test whether adding child education modifies the observed patterns intergenerational welfare transmission. In **Table 3**, we present descriptive statistics on explanatory variables for the full sample and separately by welfare receipt in period t1 and period t0. We find that females, older individuals, those born with higher parity and growing up in larger parental households have a higher risk of own welfare receipt. Similarly, larger parental households, older parents and younger own age are correlated with a higher propensity of parental welfare receipt. The secondary schooling indicators for parental education yield that those with higher education are less likely to experience welfare receipt than those with only lower secondary schooling (see bottom panel). The correlation patterns are similar for welfare receipt in the next generation as well (see top panel).[15]

---

[14] The youngest birth cohort in our data cannot be observed at age 27-29 by construction. It contributes only the minimum of two years of data for the age bracket 25-29. In addition to the controls for individual missing value indicators the reduced number of annual observations is accounted for by controls for birth cohort.

[15] **Table A.1** in the appendix provides descriptive statistics of our full set of explanatory variables for the main sample.

5.   Results

5.1   Baseline results

Panel A of **Table 4** shows our OLS estimation results based on the basic specification. In these models, we control for total observed parental welfare receipt when the individual was a teenager. The first set of results confirms **Table 2** and indicates that the correlations between parent and subsequent child welfare receipt are positive and highly statistically significant. Having experienced welfare receipt in the parental household 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 correlation patterns of welfare receipt.

In panel B of **Table 4** we present the estimates of intergenerational welfare correlation that obtain when we consider a set of covariates; controlling, e.g., for parental education helps separate the relevance of transfer receipt from that of low education and income. The results indicate that controls account for some of the intergenerational correlation in welfare receipt: the coefficients decline by 18-26 percent but continue to be 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 effect.[16]

The estimates in column 2 show an increase in the number of own years of welfare experience by about 0.16 for each year of parental welfare receipt; i.e., 6 years of parental welfare receipt goes along with one additional year as an adult. Column 3 suggests that the

---

[16] **Table A.2** in the appendix presents the full set of estimation results.

share of observed years on welfare significantly increases by .2 percentage points if parents were on welfare for the full observation period (100 percent).

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 slightly with the number of years of parental welfare receipt. This deviates from Hartley et al. (2017) who find that the correlation is insensitive to the duration of parental welfare receipt in their data. Levine and Zimmerman (2005) show that child outcomes decline more if mothers spend a higher share of time on welfare. However, the negative marginal effect of maternal time on welfare on child outcomes declines.

In **Figure 3** we describe the development of the intergenerational correlation patterns as estimated by the basic regression specification over subsequent birth cohorts. The patterns are similar for all three welfare measures. We see a brief peak in correlations for the early 1970s birth cohorts and a significant positive trend for more recent birth cohorts.

5.2   Heterogeneity by child and parent gender

In our next step, we follow the literature and investigate whether the intergenerational welfare correlation differs for young men and women; descriptive statistics yield higher welfare receipt among females than males.[17] We apply different strategies to describe the gender specific patterns in our data. First, we re-estimated the extended specification described in **Table 4** and additionally interacted parental welfare receipt with child gender. Panel A of **Table 5** shows

---

[17] 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.3** for descriptive statistics by gender).

that the correlation between parent and child welfare receipt is substantially but mostly insignificantly higher for females. Panels B-C of **Table 5** show separate estimations of the basic and extended specifications by gender and confirm higher intergenerational correlations for females than for males across all three welfare indicators. This agrees well with the literature (e.g., Dahl and Gielen 2021, Hoynes et al. 2016). One mechanism may be that the likelihood of single parenthood is larger for females than males which 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 5** we present separate estimates based on whether maternal or paternal welfare receipt was observed during childhood. 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 the parent couples. In a few cases (28 for mothers and 119 for fathers) we have no information on the person-specific welfare history. Our results yield only minor differences in parent-specific correlation patterns for the basic specification.[18] Surprisingly, some paternal correlations are slightly larger than the maternal ones. In separate estimations, we considered child gender interaction terms in the estimations for fathers' and mothers' welfare outcomes. We show the results for the extended specifications in Panels F and G of **Table 5**. They 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 German data Mueller et al. (2017) similarly obtained stronger intergenerational transmission of fathers' unemployment for daughters than for sons. With Dutch data, Boschmann (2019) also found similar intergenerational correlation patterns for maternal and paternal social assistance receipt. The studies on intergenerational transmission of disability benefit in Norway disagree: while

---

[18] There are no major differences by parent gender in extended specification. Results are available upon request.

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.[19] Recently, Carneiro et al. (2021) carefully studied the relevance of the timing of parental income shocks for the next generation's human capital outcomes. Conditional on household permanent income they find for the entire 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. (2017) 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

---

[19] 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 implementation 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.

formative years of the late teens. Hartley et al. (2017) consider U.S. survey data from the PSID on mother-daughter pairs. The authors show age-specific intergenerational transmission effects by estimating rolling estimation windows for 5 and 10 years of age. Again, the 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. Unfortunately our survey does not allow us to go back in time for all individuals. Therefore, we start out with age-group specific estimations which vary in sample size. Panel A of **Table 6** 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 effects for a given sample we pooled the three age-groups specific measures in panel B of **Table 6** and estimated the correlation patterns in one joint model and with a smaller sample. We continue to find the weakest correlation for the middle age group and larger impacts for the youngest and the oldest group, depending on the specific welfare measure applied. The finding of larger coefficients for the oldest group agrees with the literature. The strong effect 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 transitions are made. If these decisions are negatively affected by financial problems in the parental household the effects may reduce average human capital with long-run effects.[20] Boschmann et al. (2019) consider heterogeneity of correlation patterns by recency of parental welfare receipt as a potential indicator of the relevance of information transmission. As we find correlations of similar magnitude for 10-12 and 16-18 year olds this does not support the argument of recent information as an important mediator.

---

[20] **Table 6** shows results for the basic specification only. The results are similar when the extended specification is estimated (available upon request).

5.4 Pre vs. post Reform patterns

During our observation period, the German welfare regime 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). Therefore, it increased job search monitoring and it reduced benefits for some long term unemployed (for a recent evaluation see, e.g., Hochmuth et al. 2021). In contrast to Hartley et al. (2017) who study the causal the effect of the 1996 U.S. welfare reform on intergenerational spillovers we only describe intergenerational correlation patterns before and after the reform.[21] Hartley et al. (2017) use an instrumental variables approach which exploits that different states adopted welfare reforms at different points in time; they control for whether daughters' welfare receipt takes place before or after the reform and interact maternal welfare receipt with the post-reform indicator. The authors find that intergenerational transmission within the welfare program declined after the reform.

We compare intergenerational welfare correlation patterns before and after 2005. 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.[22] While the welfare outcomes for the two subsamples themselves in t1 are similar, surprisingly, we find much higher parental welfare receipt in the post reform group (see Appendix **Table A.4** for descriptive statistics on the two groups' outcomes). This may reflect aggregate trends to higher welfare use over time (see **Figure 2**).[23]

---

[21] For analyses of pre- and post-reform state dependence in welfare receipt see, e.g., Riphahn and Wunder (2013, 2016).
[22] In contrast to the interesting contribution by Cobb-Clark et al. (2020) who study the intergenerational spillover effect of a reform that affects the parent generation we follow Hartley et al. (2017) and focus directly on the young generation.
[23] Period t0 for the pre-reform cohorts started 1979-1989, whereas period t0 for the post-reform cohorts started in 1990-2002.

Table 7 shows the results for both subsamples with the basic and extended specifications in Panels A and B, respectively. The samples sizes are comparable with 1,193 and 1,210 observations before and after the reform, 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 quantitive welfare measures increased substantially for the birth cohort who turned 25 after the Hartz reform. These results suggest that the reform did not affect the overall propensity to use welfare but it may not have succeeded in mobilizing recipients to leave the program faster than before. After the reform the intensity of youth welfare was much closer correlated with the intensity of parental welfare use than before the reform which confirms **Figure 3**.

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 takes on a mediator role by adding child educational outcomes as a control variable in the estimations shown in **Table 4** where they had been omitted so far to avoid endogeneity issues. If the intergenerational correlation declines once we condition on child education it is likely that education is a mediating factor. This information potentially opens up useful strategies to counteract intergenerational welfare transmission.

We consider four indicators of the highest educational degree obtained: no secondary schooling degree or less than upper secondary school, upper secondary school degree,

vocational training degree, academic tertiary education degree.[24] **Table 8** 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 4** all coefficient estimates are smaller in magnitude by about 20 percent. This suggests that a considerable part of the intergenerational correlation operates via the attenuated educational attainment of children in welfare receiving households.[25]

Our findings agree well with the literature. Boschmann et al. (2019) offer a similar analysis for Dutch benefit programs and find a decline intergenerational transmission once child education is accounted for. Bubonya and Cobb-Clark (2021) offer an interesting analysis which separates the relevance of alternative mechanisms for intergenerational welfare transmission in Australia. They apply a comprehensive mediation analysis and find that all considered potential mediators jointly explain a third of intergenerational welfare correlation. The authors see the failure to complete high school as the key mechanism behind intergenerational welfare correlation. While across countries there are important institutional differences, e.g., in the education and the welfare systems the mechanisms of the transmission of disadvantage may carry over and be similar. Our findings agree with their conclusions.

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 do not provide causal effects. We apply two methods to get closer

---

[24] We use the information on ISCED levels (International standard classification of education) as provided by the SOEP data. We cannot evaluate the validity of the information (for summary statistics see **Table A.1**).

[25] 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 education category (.25 vs. .12) and less than half as likely to be in the highest category (.12 vs. .28).

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 sample offers information on 1,161 siblings from 514 different families. Panel A of **Table 9** presents the baseline estimation results for the basic and extended specification for this particular subsample. The results are rather similar to those of the full sample in **Table 4**. Panel B of **Table 9** shows the coefficient estimates on parental welfare when we apply the family fixed effects estimator to both the basic and extended specifications. Clearly, the positive significant correlation coefficients do not hold up to fixed effects controls. This result therefore does not support the existence of causal intergenerational treatment effects.

As a robustness test we consider the fixed effects results when those families are omitted from the sibling sample where only the older sibling experienced parental welfare receipt. In this case, it is possible that the mechanisms that generate intergenerational transmission such as reduced stigma, availability of institutional information persist in the family even though a direct welfare receipt is not observed for the younger sibling. Panel C in **Table 9** yields that the fixed effects results obtained so far, i.e., the lack of significant positive effects, are robust to this additional test.[26] Therefore, we find no causal effect of intergenerational welfare transmission under the assumptions of fixed effects estimation.

This finding agrees with the family fixed effects estimations on unemployment benefit transmission in Ekhaugen (2009) and Mueller et al. (2017), and on the transmission of maternal

---

[26] We pursued two strategies for the robustness test with the same result. In step one we determined the set of families where parental 'ever-welfare' outcomes varied across siblings; out of 1,161 children in 514 families, xx children in yy families had varying parental outcomes across siblings. First, we omitted 49 observations from families where the first born child experienced parental welfare receipt (estimating with N=1,112 observations), then we omitted observations from families only the first born child experienced welfare receipt. The latter yielded N=1150 observations. Panel C of **Table 9** shows the results. Not surprisingly, the results hardly differed between both procedures.

(not paternal) benefit transmission in Bratberg et al. (2015). In their fixed effects analyses, Edmark and Hanspers (2015) even obtained negative and partly statistically significant intergenerational transmission results of welfare receipt in Sweden. The authors argue that either children of welfare recipients are particularly eager to avoid welfare receipt 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 after observing the second generation's welfare receipt. As we do not have long-running data for all families the estimation with an additional parental welfare control can only be performed on a subsample of our full analysis sample. Therefore, we first investigate whether the correlation patterns in the "Gottschalk subsample" reflect our results from **Table 4**. We show the basic and extended specification estimates for the Gottschalk subsample with 1,221 observations in Panel A of **Table 10**. The coefficient estimates do not differ in important ways from prior results.[27]

Panels B and C of **Table 10** show the estimation results of the Gottschalk 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

---

[27] In addition, we compare the characteristics of the subsamples for whom information on late parental welfare receipt is available to those of the full sample in Appendix **Table A.5**. Not surprisingly, individuals in the Gottschalk subsample and their parents are on average 2 years older than the main sample. Given cohort trends, the parents are less well educated. The child generation is slightly more likely to use welfare (incidence of 14.3 vs. 12.8 percent) than the main sample whereas the parents are less likely to use welfare.

(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 estimations in section 6.1 is confirmed with the Gottschalk approach. In fact, the overall effect 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. Ekhaugen (2009) and Mueller et al. (2017) similarly obtained negative estimates for the transmission of unemployment benefits in Norway and Germany, respectively. Boschmann et al. (2019) study different Dutch programs and find significant positive causal effects for unemployment benefits and 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 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 would hold for our data.

7.   Conclusions

Most of the international literature suggests that experiencing parental welfare receipt in childhood or adolescence is correlated with and causally determines own welfare receipt later in life. This paper takes advantage of a long running household panel (SOEP) survey to address the topic of intergenerational transmission of welfare receipt for the case of Germany, which has not been studied before. The richness of our data allows us to add informative analyses of intergenerational correlation patterns to the literature and to determine the extent to which intergenerational transmission patterns might be causal.

We differentiate three different welfare indicators and find strong intergenerational correlation in welfare outcomes for all three measures. The correlations are larger for recent than for older birth cohorts. The correlations are larger 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 is more detrimental than exposure in the 13-15 age window. After a major welfare reform in Germany the intergenerational welfare correlations may even have increased. Child educational attainment is a mediator between parent and child welfare receipt. Possibly this offers an opportunity for policy intervention to improve opportunities for the children of welfare recipients.

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. Interestingly, both empirical strategies fail to find evidence of a causal impact of parental welfare receipt on child welfare outcomes. Thus, it is not 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 drive transmission patterns.

Even though it is not the character of welfare institutions themselves that leaves offspring of welfare recipients worse off, it seems worthwhile to direct future research to investigate the relevance of poverty and welfare receipt for the next generation's educational attainment. Also, it is important to understand why the intergenerational correlation increased since a major reform of the welfare system. We agree with Hartley et al. (2017) 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. We conclude with Gottschalk (1996, p. 22): "The results in this paper are offered tentatively as initial estimates that hopefully will be refined through future research."
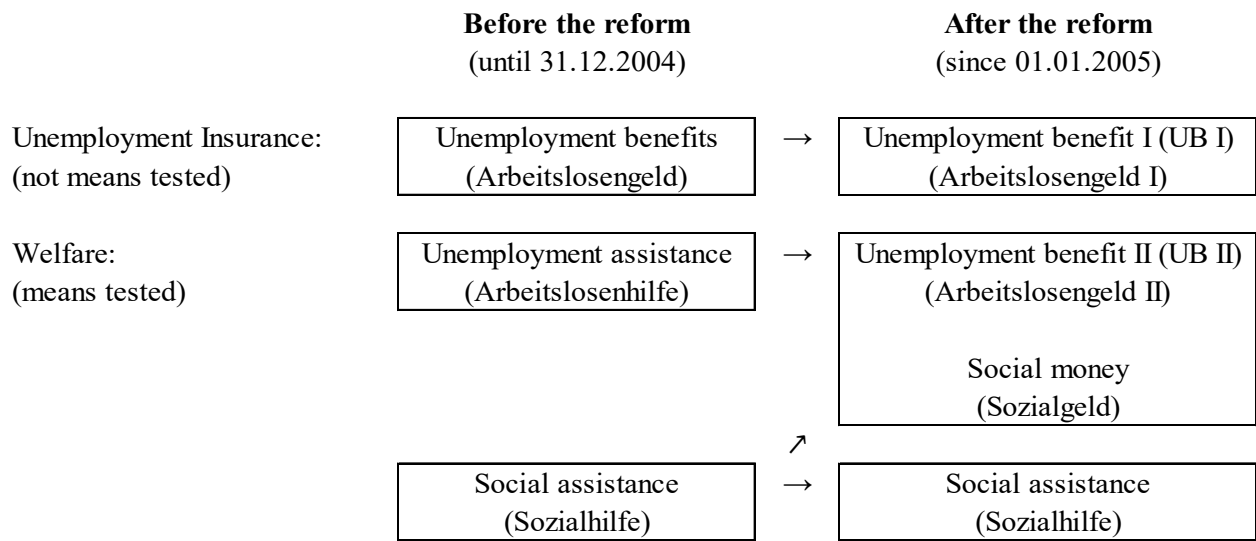
Bibliography

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

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

BA (Bundesagentur für Arbeit) (2005), Der Übergang von der Arbeitslosen- und Sozialhilfe zur Grundsicherung für Arbeitssuchende, Sonderbericht, Bundesagentur für Arbeit, Nürnberg.

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, DOI: 10.1016/j.ssresearch.2019.02.009

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, 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.

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.

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.

Dummert, Sandra, Philipp Grunau, Katrin Hohmeyer, Torsten Lietzmann, Kerstin Bruckmeier, and Martina Oertel (2020), Sample of Integrated Welfare Benefit Biographies (SIG) 2007 - 2017, FDZ Datenreport 02|2020, Nürnberg.

Eberharter, Veronika (2011), The Intergenerational Transmission of Economic Disadvantages and Social Exclusion - Constraints on Social Mobility, Schmollers Jahrbuch 131(2), 287-299.

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.

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.

Groh-Samberg, Olf (2014), No Way Out - Dimensionen und Trends der Verfestigung der Armut in Deutschland, Sozialer Fortschritt 12/2014, 307-315.

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 (2017), Welfare reform and the intergenerational transmission of dependence, IZA Discussion Paper No. 10942, IZA - Institute of Labor Economics, Bonn.

Hochmuth, Brigitte, Britta Kohlbrecher, Christian Merkl, and Herrmann Gartner (2021), Hartz IV and the Decline of German Unemployment: A Macroeconomic Evaluation, Journal of Economic Dynamics and Control 127, 104114.

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.

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.

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.

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.

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, DOI: 10.1111/j.1475-4991.2012.00518.x

Riphahn, Regina T. and Christoph Wunder (2016), State dependence in welfare receipt: transitions before and after a reform, Empirical Economics 50(4), 1303-1329 DOI: 10.1007/s00181-015-0977-0

Schels, Brigitte (2018), Young adults' risk of long-term benefit receipt and parents' socioeconomic background, Acta Sociologica 61(1), 17-33. https://doi.org/10.1177/0001699316681828

Schnitzlein, Daniel D. (2014), How important is the family? Evidence from sibling correlations in permanent earnings in the USA, Germany, and Denmark, Journal of Population Economics 27, 69-89.

Schnitzlein, Daniel D. (2016), A new look at intergenerational mobility in Germany compare to the U.S., Review of Income and Wealth 62(4), 650-667.

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.

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
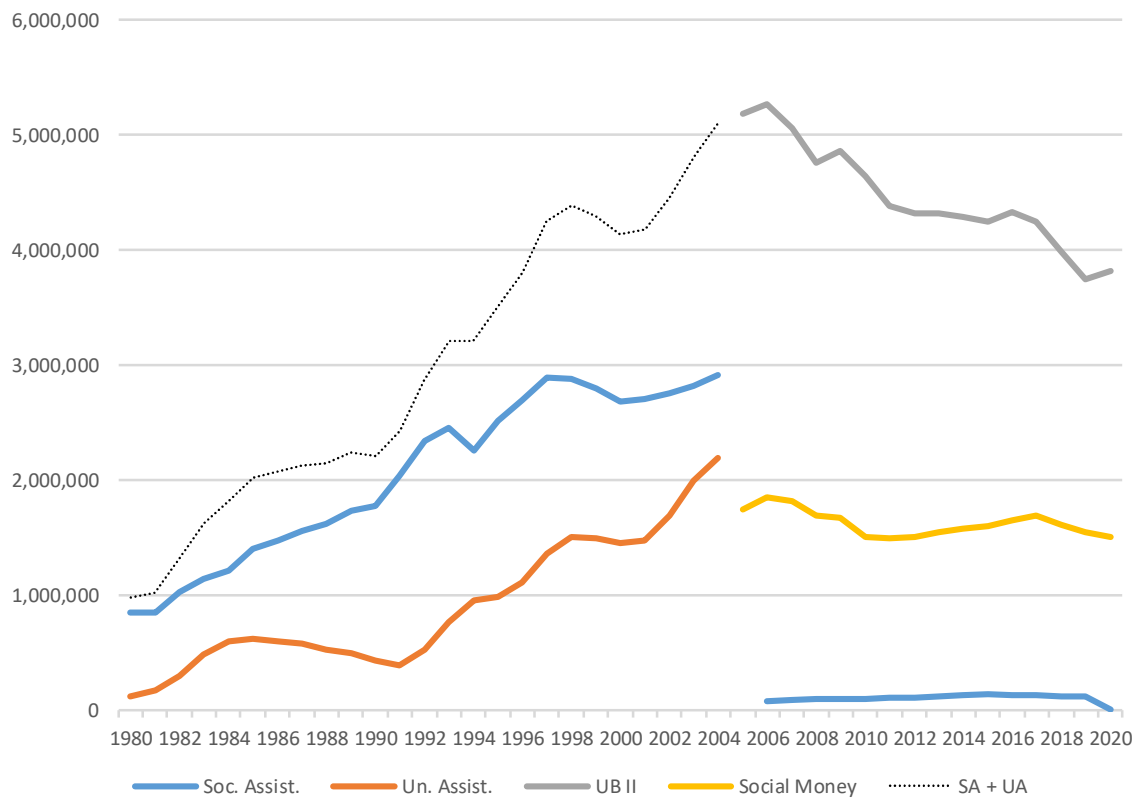
Source: Own depiction based on information from different sources. Until 1990 only West Germany, starting 1991 East and West Germany.

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).

Figure 3   Correlation pattern by birth cohort
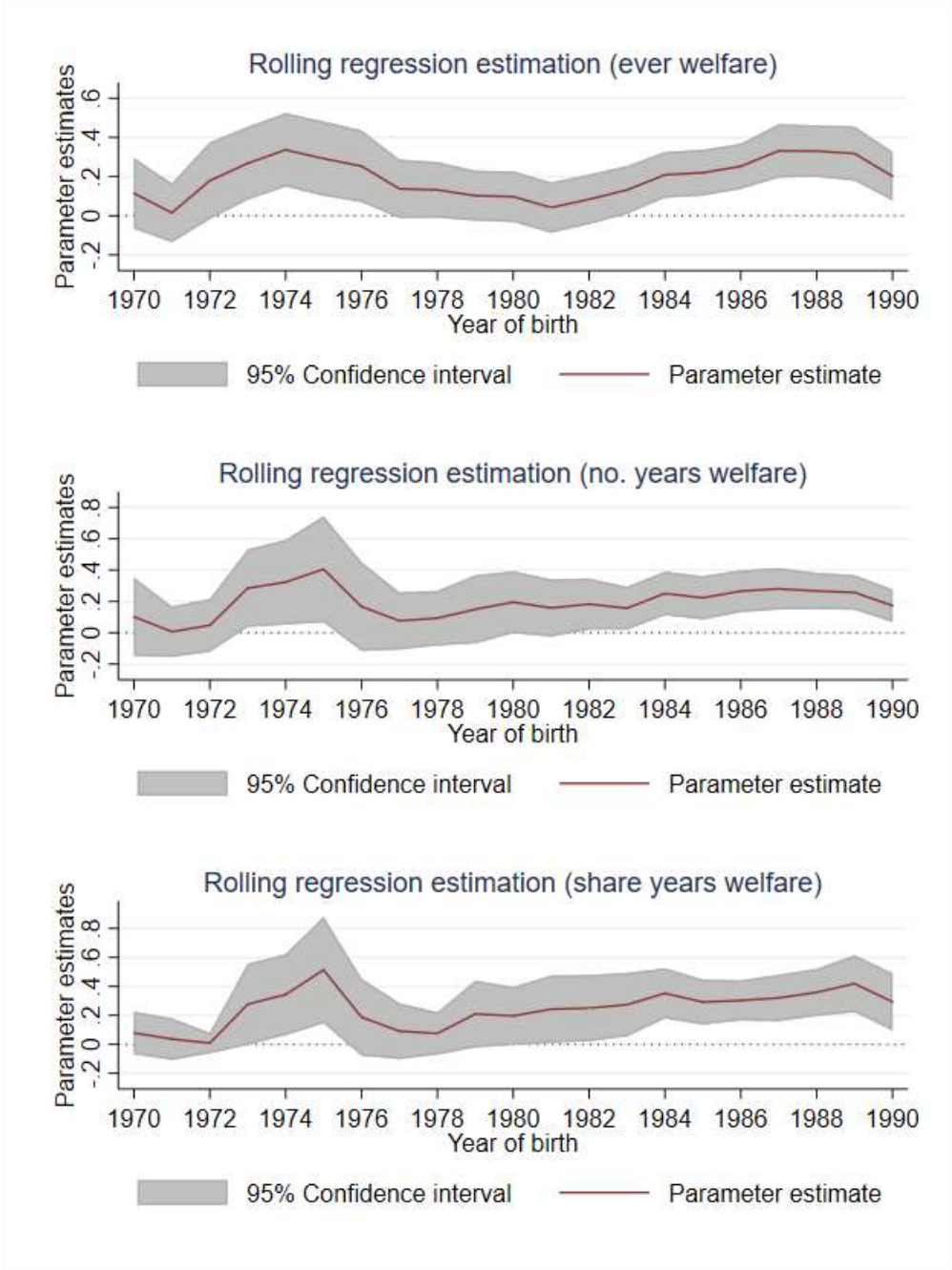
Source: SOEP (1984-2017), own calculations.

Table 1   Descriptive Statistics on Measures of Welfare Receipt

	Observations	Mean	Std. Dev.	Min	Max
Welfare receipt t_1(age 25-29, child)					
ever (0/1)	2,403	0.12776	0.33389	0	1
number (years)	2,403	0.55015	1.48553	0	5
share (years)	2,403	0.06274	0.18873	0	1
Welfare receipt t_0 (age 10-18, parent)					
ever (0/1)	2,403	0.13816	0.34514	0	1
number (years)	2,403	0.41906	1.34372	0	9
share (years)	2,403	0.05491	0.17407	0	1

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

Table 2   Correlation Patterns of Welfare Receipt Indicators

	Welfare receipt t_1 (child)			**Welfare receipt t_0 (parent)**		
	ever (0/1)	number (years)	share (years)	ever (0/1)	number (years)	share (years)
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 (years)	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 (years)	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 3 Descriptive Statistics for Explanatory Variables

3.1 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
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	

3.2 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
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	0.008	0.008	0.024	-0.088***
N	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 4   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 5   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)			
Mother welfare, t_0	0.198***	0.209***	0.251***
	(0.028)	(0.034)	(0.042)
Panel E: Paternal welfare receipt – basic specification (N=2,284)			
Father welfare, t_0	0.196***	0.231***	0.295***
	(0.032)	(0.047)	(0.055)
Panel F: Maternal welfare receipt – ext. specification with gender interaction (N=2,375)			
Mother welfare, t_0	0.106***	0.078*	0.138***
	(0.039)	(0.041)	(0.051)
Female × Maternal welfare, t_0	0.088	0.164**	0.129
	(0.054)	(0.065)	(0.079)
Panel G: Paternal welfare receipt – ext. specification with gender interaction (N=2,284)			
Father welfare, t_0	0.109**	0.099*	0.209***
	(0.045)	(0.058)	(0.080)
Female × Paternal welfare, t_0	0.077	0.139	0.067
	(0.062)	(0.091)	(0.106)

Notes: Each cell entry represents a separate regression where the type of the parental welfare measure matches the dependent variable as listed in the column headers (see **Table 4**). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 4**; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$
Source: SOEP (1984-2017), own calculations.

Table 6   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 4**). Robust standard errors are reported in parentheses. For details on the basic specification see notes of **Table 4**; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.

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

Table 7   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 4**). Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 4**; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.

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

Table 8   Controlling for child education as a potential mediator

	Dependent variables: Welfare receipt t_1		
	(1) ever (0/1)	(2) number (yrs)	(3) share (%)
Parental welfare, t_0 (basic specification)	0.148***	0.161***	0.212***
	(0.025)	(0.031)	(0.039)
Parental welfare, t_0 (extended specification)	0.112***	0.129***	0.180***
	(0.025)	(0.031)	(0.038)
Observations	2,403	2,403	2,403

Notes: Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 4**; 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 9   Family Fixed Effects Estimation

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

Notes: Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 4;** *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.
Source: SOEP (1984-2017), own calculations.

Table 10   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)			
Parental welfare, t_0, basic specification	0.178*** (0.039)	0.249*** (0.058)	0.260*** (0.062)
R-Squared	0.037	0.038	0.060
Parental welfare, t_0, extended specification	0.131*** (0.038)	0.192*** (0.057)	0.217*** (0.060)
R-Squared	0.112	0.112	0.127
Panel B: Gottschalk approach estimation results, basic specification (N=1,221)			
Parental welfare, t_0	0.130*** (0.039)	0.191*** (0.060)	0.201*** (0.063)
Parental welfare, t_2	0.318*** (0.072)	0.350*** (0.076)	0.188*** (0.046)
Gottschalk effect	-0.188** (0.898)	-0.160 (0.104)	0.014 (0.084)
Panel C: Gottschalk approach estimation results, extended specification (N=1,221)			
Parental welfare, t_0	0.097*** (0.039)	0.149*** (0.059)	0.172*** (0.061)
ever (0/1), t_2	0.254*** (0.073)	0.271*** (0.080)	0.156*** (0.046)
Gottschalk effect	-0.157** (0.896)	-0.122 (0.108)	0.015 (0.083)

Notes: Robust standard errors are reported in parentheses. For details on the basic and extended specification see notes of **Table 4**; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.05$.

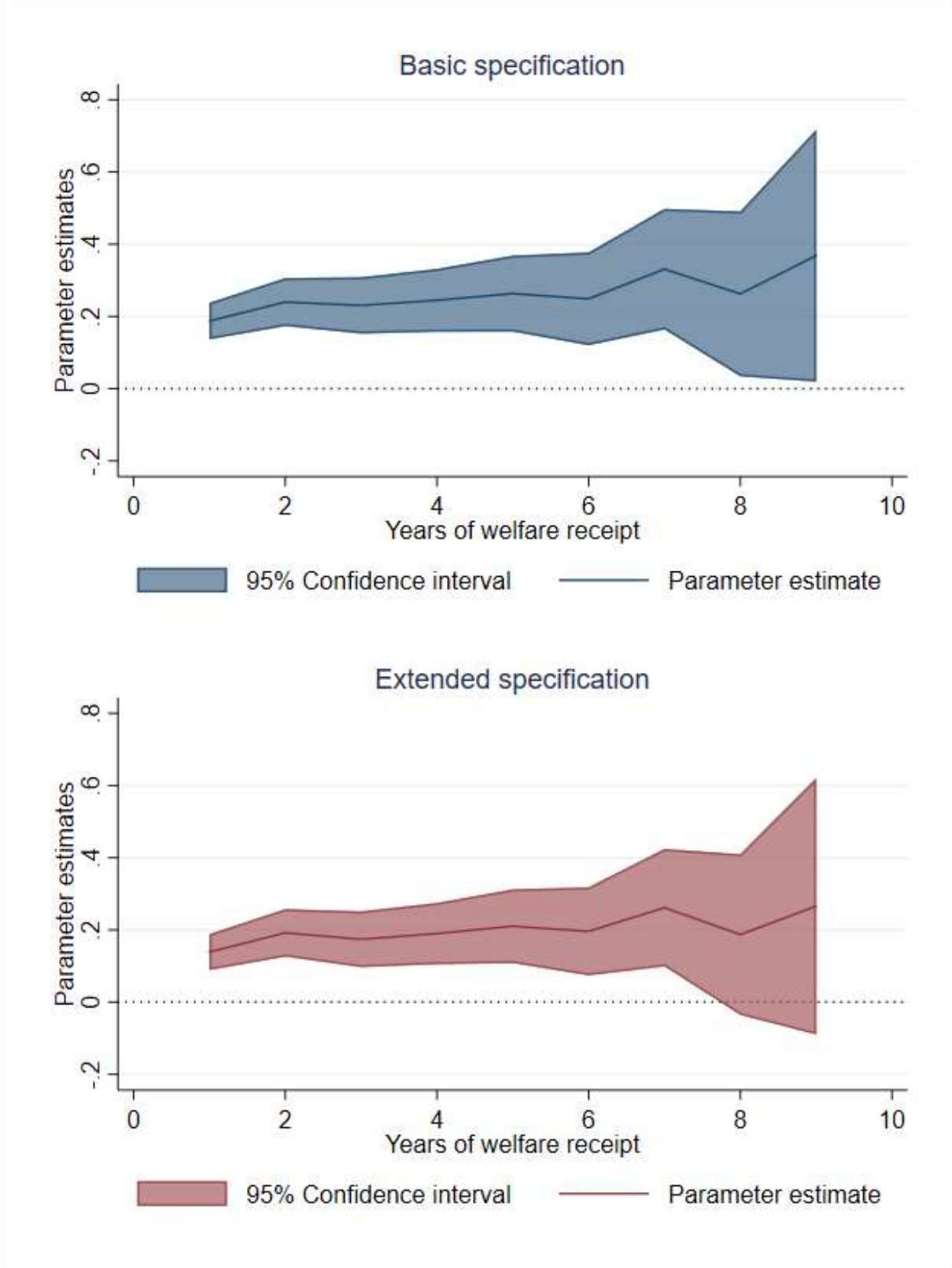Source: SOEP (1984-2017), own calculations.

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

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

Table A.1   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
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
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
Mecklenburg-Vorpommern	2,403	0. 04367	0. 20446	0	1
Niedersachsen	2,403	0. 08864	0. 28428	0	1
Nordrhein-Westfalen	2,403	0. 21515	0. 41101	0	1
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

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
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

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

Table A.2   Complete estimation results of extended specification in **Table 5**

	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)
miss_10	0.0419	0.167	0.0193
	(0.0272)	(0.120)	(0.0152)
miss_11	-0.0785**	-0.311**	-0.0324*
	(0.0335)	(0.147)	(0.0194)
miss_12	0.0293	0.154	-0.0106
	(0.0284)	(0.128)	(0.0154)
miss_13	-0.0645**	-0.304**	-0.0144
	(0.0263)	(0.119)	(0.0143)
miss_14	0.0626**	0.280**	0.0235
	(0.0258)	(0.114)	(0.0148)
miss_25	0.00503	-0.157	0.0128
	(0.0352)	(0.120)	(0.0215)
miss_26	0.0585	-0.0376	0.0557*
	(0.0397)	(0.124)	(0.0289)
miss_27	-0.00385	-0.123**	0.00382
	(0.0207)	(0.0584)	(0.0135)
miss_28	-0.0221	-0.191**	-0.00777
	(0.0215)	(0.0766)	(0.0124)
miss_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)
Mecklenburg-Vorpommern	0.169***	0.628***	0.1000***
	(0.0432)	(0.177)	(0.0270)
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)
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)
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)
Observations	2,403	2,403	2,403
R-Squared	0.105	0.112	0.118

Adjusted R-Squared		0.089	0.096	0.102

Table A.3   Descriptive statistics on welfare receipt for male and female observations

	Observations	Mean	Std. Dev.	Min	Max
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
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.4   Descriptive statistics on welfare receipt for pre- and post-reform cohorts

	Observations	Mean	Std. Dev.	Min	Max
Panel A - 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
Panel B - 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

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

Table A.5   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
N	2,403	1,182	

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