How Family Transfers Crowd-out Social Assistance in Germany

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Abstract

The non-take-up of social assistance has been receiving increased attention among policy makers in recent years as it would apparently underpin the effectiveness of public intervention in alleviating poverty. We examine whether receipt of private transfers affects the household decision to take-up social assistance in Germany between 2009 and 2011. We exploit the follow-up of households in the SOEP to reconstruct family links and estimate a model of welfare participation with endogenous private transfers and sample selection of the instruments. We find that 20% of the non-take-up rate is due to monetary substitution of private transfers lowering the welfare program costs. However, we find that social assistance is more effective in alleviating poverty and its intensity than private transfers.

Keywords: welfare participation, private transfers, family networks
JEL codes: D31, D64, I38

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1 Introduction

Many people in Europe do not receive social assistance despite the fact that they are fully eligible.[1] This non-participation phenomenon has became a major concern among economists and policy-makers in recent years as it would apparently lower the effectiveness of public intervention in alleviating poverty. For instance, this has been used as an argument for promoting the universal basic income and, more generally, universalism against targeting of welfare benefits by Benoît Hamon during the 2017 French Presidential election.

Traditionally, economists view the non-take-up as a rational choice wherein individuals compare their marginal utility to the costs associated with participation. In a seminal paper, Moffitt (1983) finds that welfare participation is associated with (unobservable) costs. This includes the so-called welfare stigma, the lack of information about entitlements, or even transaction costs, see Currie (2004) for a review of these costs. As a consequence, alternative sources of income might be substituted to social assistance as long as the associated costs are lower than those of claiming social assistance. In theory, these alternative sources of income should be included in the assessment of claimant entitlements. However, in practice, various reasons can explain why these alternative sources of income might not affect claimant entitlements. Among others, Duclos (1995) and Duclos (1997) analyses how the non-take-up can be explained by the presence of errors in assessing benefit eligibility by the welfare agency (clerical error), but also, because claimants have not declared them to the authorities in the claiming process (deliberately or by error). In this paper, we argue that the non-take-up of social assistance can be due to the receipt of private transfers. Indeed, individuals may receive financial support from their family and relatives outside of their own household and which might be only partially observed by the welfare agency. Because, one of the most basic assumption of the economic theory postulates that the utility derived from the consumption of an additional unit of a good, here income, is decreasing; then, private transfers are expected to reduce incentives to claim social assistance as they lower the marginal utility of the public benefit.

The aim of the present paper is to empirically examine whether receipt of private transfers affects the propensity to take-up social assistance. Although non-take-up of social assistance is an important concern for most of developed economies (Hernanz et al. 2004), we restrict our attention to the case of

[1]For instance, Hernanz et al. 2004 provides evidence of non-take-up using a survey covering OECD countries while Riphahn (2001) estimates that for Germany 63% of eligible households did not take-up social assistance in the 1990s.

social assistance in Germany because of the availability of accurate data on both public and private transfers. While private transfers can take many different forms such as informal loans, transfers in kind, or assistance with housework or child-care (Laferrere and Wolff 2006), the empirical analysis in this paper focus on financial transfers occurring between households. About one third of elderly people made a transfer to the younger generation of between 2 550 and 3 570 euros per year in the 1990s in Germany (Reil-Held 2006). On the contrary, private transfers from the younger to older generations are unusual. The question of whether private transfers affect welfare participation is important because the interaction between public and private transfers may generate complex distributional effects underpinning the effectiveness of public policies. Very few studies have investigated such public crowding-out effect wherein private transfers would discourage the use of public transfers. A notable exception is Robins (1986) who provides empirical evidences that the enforcement of child support policies reduces the provision of welfare benefit in the United-States.

Assessing the causal impact of private transfers on the take-up of social assistance involves a range of methodological challenges. The *first* and most obvious relates to the measurement of eligibility itself. Indeed, the eligibility status is observed only for those who are eligible and who actually receive the benefit while we are interested in those who do not take-up the benefit while being eligible. The growing availability of micro-simulation models paves the way for simulating eligibility and evaluating redistribution policies in an accurate way. Such methods dictate the use of precise data on household characteristics in order to imitate as close as possible the entitlement process. In this attempt, Bargain et al. (2012) have promoted the use of administrative data. However, administrative data on private transfers are likely to be biased as individuals have monetary incentives to under-report the private transfers they receive. This suggests the use of survey data instead. Consequently, we use the German Socio-Economic Panel (SOEP) which is a representative survey of households living in Germany covering a wide range of topics including public and private transfers. A *second* empirical challenge relates to the joint determination of private transfers and welfare participation. The literature on the crowding-out effect abounds evidences that public transfers may crowd-out private transfers (e.g. Albarran and Attanasio 2002, Angelucci and De Giorgi 2009, Cox et al. 2004, Jensen 2004). This stems from the fact that individuals (e.g. parents) consider the well-being of other family members (e.g. children) when maximizing their own utility (Becker 1974, Barro 1974). To fix ideas, altruistically-motivated transfers are reduced as potential recipients get richer (when taking-up social assistance). Therefore, one cannot rule out the possibility that welfare par-

ticipation affects in turn private transfers received. In order to address this simultaneity problem, we consider a simultaneous model of welfare participation with endogenous private transfers. We use an instrumental variable approach to identify the model. Precisely, the propensity to receive private transfers for a given household is instrumented by unexpected windfall transfers (e.g. lottery winnings, inheritance) received by its family network. Because of altruism, one can expect a household whose family had received windfall transfers to receive more private transfers without affecting directly its propensity to claim social assistance. However, by construction, this instrument is missing for the sub-sample of households for whom we do not observe family link in the SOEP, then we control for potential non-random sample selection of the instruments too.

Using the waves 2009-2011 of the SOEP, we estimate a trivariate probit model of welfare participation with endogenous private transfers and control for sample selection of the instruments. We find that about 20% of the non-take-up is due to private transfers lowering substantially the welfare program costs. However, we find that social assistance is more effective than private transfers in alleviating poverty head-count and poverty intensity. We point out that this relative inefficiency depends on the network of private transfers (Bourles et al. 2017). Because it is not clear whether private transfers are observed by the welfare office and thus whether we should take them into account when simulating the entitled benefit, we assess the sensitivity of our results in this direction. Furthermore and because the maximum likelihood estimator relies strongly on the normality assumption of the probit model, we relax this assumption by using common linear estimators as specification checks. Last, we exploit the linear framework to control for unobserved heterogeneity at the household and family levels using fixed effects. Indeed, social interactions may generate different family welfare cultures through the establishment of different norms toward social assistance (e.g. Bertrand et al. 2000, Dahl et al. 2014). Our results are robust to these different econometric specifications.

The paper is organized as follows. In the next section, we present the conceptual framework and existing evidences of the interplay between public and private transfers. In section 3, we provide an overview of the German social assistance system and we present the SOEP data. We conclude this section by presenting micro-simulation results as well as a portrait of the prevalence of public and private transfers. Section 4 presents the methodology and the estimation strategy with a particular attention devoted to the endogenous nature of private transfers and to the potential sample selection of the instruments. We present the main empirical results in section 5 together with sensitivity and specification checks. Then, in a counterfactual

exercise, we quantify the substitution effect of social assistance by private transfers in terms of poverty. The last section concludes.

2 Conceptual framework and existing evidences

Most developed economies provide social assistance schemes to guarantee a minimum level of resources for poor households. However, many empirical studies,[2] have provided evidences that a non-negligible proportion of eligible households actually do not take-up the benefit they are entitled to. Since Moffitt (1983), economists have understood this seemingly self-detrimental behaviour as resulting from welfare stigma - that is, from disutility arising from the participation in a welfare program. Even if individuals when deciding to participate are supposed to maximize their utility, the non-take-up is understood as a limiting factor of the effectiveness of redistribution policies (Bargain et al. 2012). This is particularly true when the so-called welfare stigma, which actually captures many potential unobservables, arises from informational problems or administrative costs fostering the persistence of inequality and potentially leading to poverty traps (Bertrand et al. 2000). The interest on non-take-up has been growing along the availability of micro-simulation models. Because the eligibility status and the entitled benefit are unobserved for those who do not claim the benefit, various methods, more or less sophisticated, have been proposed. An early and simple attempt is provided by Cox and Jakubson (1995). In a first step, they regress the observed value of the welfare benefit for the sample of participants on incomes and household characteristics. In the second step, they compute the entitled benefit a household can claim for the whole sample (of both participants and non-participants) as the linear predictions of the first step. Then, a household is said to be eligible if the predicted benefit is positive. Despite its simplicity, this methods has some shortcomings. From a practical perspective, the linear assumption does not account for the complex and non-linear nature of the eligibility process resulting into a weak predicting power. From an econometric perspective, this method ignores that unobservables that may affect the decision to take-up the welfare benefit. This introduces a common sample selection bias in the first step and thus systematic errors in the predicted values. Instead, the growing availability of micro-simulation models paves the way for simulating eligibility and evaluating redistribution policies in a more accurate way (Bourguignon and Spadaro 2006). Our micro-simulation model follows a simplified procedure of the STSM-IAB model adapted for the SOEP that is extensively described in Bruckmeier and Wiemers (2012).

[2] See Hernanz et al. (2004) for a survey of the literature.

Because welfare participation is costly, then alternative sources of income might be preferred to social assistance (Moffitt 1983). Assuming that the utility function is concave, then an increase in the means of the household - through private transfers - reduces the utility derived from an additional income. Consequently, the marginal utility derived from the public benefit is reduced for those having received private transfers. Then, receipt of private transfers lowers the propensity to take-up social assistance (for a constant disutility of participating). In such a case, private transfers are substitutes to public transfers. This is a public crowding-out effect which arises because of the costs associated with welfare participation. The aim of this paper is to empirically examine this question; that is, whether private transfers crowd-out social assistance, inflating the non-take-up rate.

An important empirical challenge concerns the joint determination of private transfers and welfare participation. The literature on the crowding-out effect has abounded evidences that public transfers may crowd-out private transfers (e.g. Albarran and Attanasio 2002, Angelucci and De Giorgi 2009, Cox et al. 2004, Jensen 2004). This stems from the fact that individuals (e.g. parents) consider the well-being of other family members (e.g. children) when maximizing their own utility (Becker 1974, Barro 1974). The great majority of these transfers flow from parents toward children as intergenerational transfers (Reil-Held 2006). In this case, the utility of the parents depends on their own consumption and on the utility of the child. Thus, a welfare program that forces a transfer from child to parents (e.g. an increase in taxes to pay for public pensions) but leaves aggregate family income unchanged will have no effect on any family member's consumption. The parents will increase private transfers (either transfers inter vivos or bequests) by the exact amount of the forced public transfer to maintain consumption of both entities (child and parents) at the previous level. Therefore, changes in public transfers should lead to compensating changes in private transfers. In contrast to this view, Cox (1987) argue that if private transfers are not motivated by altruism, but are instead part of an exchange of services between parents and child, such crowding-out effect may not occur. In this case, people give to others because they expect to get something back in return.[3] The effect on crowding out is a priori unclear in exchange models (Cox and Jakubson (1995)). Although both motivations of transfers imply that transfers decrease as the income of the parents increases; altruism implies that they decrease as the income of child increases while exchange motives

[3]The nature of the service can be very broad, we can think of help for past home production but also about more subtle type of services that entails behavioural constraints associated with social interactions (e.g. conforming to social norms).

imply that transfers may increase in response to an increase in the child's income as the child now demands greater compensation to provide the same amount of service (Cox 1987). Consequently, one cannot rule out the possibility that welfare participation affects in turn private transfers received. In order to deal with this simultaneity problem, we consider a simultaneous model of welfare participation with endogenous private transfers. We use an instrumental variable approach to identify the model. Precisely, the propensity to receive private transfers for a given household is instrumented by the receipt of lottery winnings or inheritance of the her extended family. As discussed before, both motivations of private transfers imply a positive correlation between the donor's wealth and the receipt of private transfers (Cox 1987). Other things being equal, if the wealth of the donor increases then the demand for informal goods and services increases while the demand for altruistic redistribution increases as well. Therefore, we posit that the only channel through which these instruments would affect welfare participation operates through the monetary substitution of private transfers received.

We exploit the sample design of the SOEP to identify family links. As years go on, the children of the first SOEP wave reach age-eligibility and become panel members. If they move out of their initial household to form their own household, they are, as well as their new household, still part of the survey and receive a new household identification number. Therefore, the SOEP distinguishes the current household identifier from the original household identifier. We assume that the set of households with the same original household identifier form a family network wherein all family members are assumed to be connected each other with the same intensity. Hence, each household belongs exclusively to one family network. Consequently, the instrument is only observed for those with observed family links and is missing for the remaining sample. Because observability of family links relies on the sample design but also on the household course of life, there are reasons to suspect that the instrument is not missing at random. Usually, researchers simply limit the analysis to the sub-sample where the instrument is non-missing. Obviously, this introduces a common sample selection bias (Heckman 1979) wherein the instrumental sample is not representative of the population. Again, we use exclusion restrictions to identify the structural parameters. Specifically, we used the sub-sample membership as an instrument for the propensity to observe family links. Indeed, the SOEP is a collection of sub-samples which have been constructed in order to improve the representativeness of the initial sample but also to enlarge the sample so as to cover groups of special interest or for covering the former German Democratic Republic (GDR) after the reunification (Wagner et al. 2007). While households from the initial sample have a higher propensity of observing

family links than those from refreshment samples, this is clearly orthogonal to both private transfers received and take-up of social assistance. These instruments have been commonly used in the literature to deal with sample selection issues in survey data, see for instance Cappellari and Jenkins (2004) for year-on-year attrition in the BHPS.

In summary, we estimate a trivariate probit model of welfare participation with endogenous private transfers that controls for the sample selection of the instruments. In addition, we control for observed and unobserved heterogeneity at the household and the family levels. We investigate the robustness of our results with respect to different specifications of the micro-simulation model, but also with respect to different econometric specifications. Our results are robust to these different variants. Our paper differs from the existing literature on private transfers and welfare benefits as our interest relies mainly on the effect of private transfers on welfare participation whereas economists have usually been interested in the substitution of private transfers to public benefits, the well-known crowding-out effect. Indeed, the question of whether public transfers crowd-out private transfers remains central for the analysis of welfare policies. An illuminating example is Cox and Jakubson (1995) who investigate the anti-poverty effectiveness of public transfers taking private-transfer responses into account. Particularly, they compare the actual poverty rate with what would have been poverty without public transfers taking into account private-transfer responses. In this paper, we consider the reverse question; that is, what would have been poverty without private transfers and taking into account social assistance participation responses. Very few studies have investigated such a public crowding-out effect wherein private transfers discourage the use of public transfers. A notable exception is Robins (1986) who provides empirical evidences that the enforcement of child support policies reduces the provision of the welfare benefit.

3 Context and data

3.1 Social assistance in Germany

Welfare programs are usually distinguished on whether they are considered as providing an insurance benefit wherein their provision depends on previous contributions (e.g. unemployment benefit) or whether they are considered as providing an assistance principle wherein the provision of the benefit depends on the means or resources of the beneficiary (e.g. social assistance). The later is intended to ensure that the poor can meet their basic needs such as food

expenditures and housing costs. Social assistance is an important source of redistribution in Germany since around 4.5 million households receive it in 2011. Let us present briefly the social assistance system in Germany. Since the Hartz reforms[4] (2003-2005), the main German social assistance program is the unemployment benefit II ("Arbeitslosengeld II", ALG II) codified in the book SGB II of the Social Code ("Sozialgesetzbuch"). Although it refers to unemployment, it is designed as an assistance program which guarantees a minimum income to cover basic needs. Specifically, the ALG II provides social assistance for employable persons between 15 and 65 years old who are not employed and not in receipt of unemployment insurance benefits. Hence, this benefit is means-tested with respect to income and wealth and does not depend on previous work history. More precisely, the entitled benefit is determined by the difference between the household needs and the household means. Household needs are determined by the household composition while the household means include net income (earned income, self-employed income, capital income, rental income, pensions minus social security contributions, income tax and alimony payments), but also prioritized benefits (child benefits, unemployment benefits, ...) from every household member. Thus, eligibility is defined at the household unit and a household is said to be eligible if its entitled benefit is positive, i.e. if the household means do not cover the household needs.

It has to be noted that the ALG II benefit can be completed by an additional child benefit ("Kinderzuschlag") for households with children.[5] The maximum amount of this transfer was 140 euros in 2005 per month for children under 18 years living in the parental household. The entitlement rules of the ALG II have not been modified substantially since 2005, see Bruckmeier and Wiemers (2012) and Steiner et al. (2012) for exhaustive presentations of the German welfare system and of the Hartz reforms.

3.2 Entitled benefit and eligibility status

The most obvious challenge when evaluating the impact of family transfers on take-up relates to the question of what determines the eligibility itself and the value of the entitled benefit. As discussed before, social assistance

[4]The Hartz reforms are a set of reforms proposed by the Committee for Modern Services in the Labour Market (Kommission für moderne Dienstleistungen am Arbeitsmarkt) to the federal government of Germany led then by Gerhard Schröder that came into effect between 2003 and 2005. The reforms aimed at activating the labour participation through the introduction of low-paid jobs (Minijob, Midijob) but also by reforming deeply the German welfare state system.

[5]We are grateful to H. Stichnoth for mentioning us this benefit.

is means-tested. Therefore, the entitlement benefit can be written, without loss of generality, as:[6]

$$b_i = \max(g(X_i) - r\, y_i - s\, t_i, 0), \tag{1}$$

where $g(.)$ are the household needs determined by a non-trivial function of the household characteristics X_i, y_i are the household means before public and private transfers (mainly labour and capital earnings), r is the marginal tax rate on the household means and s the marginal tax rate on private transfers t_i. Generally, r varies between labour and capital earnings in order to provide incentives to participate to the labour market. Obviously, the welfare benefit b_i cannot be negative. More precisely, when the benefit is zero then the household is said to be non-eligible and when it is strictly positive then the household is considered as eligible. In sum, equation (1) makes explicit that there are two different types of non-participation: the non-participation of households with means (or private transfers) too high to be eligible, and the non-participation of eligible households who choose not to participate. Thus, our interest relies on the effect of private transfers on take-up for those who have too low means or private transfers such that they are eligible.

The question of whether private transfers are actually taken into account by the welfare agency when determining eligibility is of primary importance. If they are taken into account (i.e. when $s > 0$), it becomes clear that they affect eligibility directly and indirectly. They lower by a fraction s the value of the entitled benefit. This directly affect incentives to be on welfare. While private transfers should, in theory, be included in the assessment of claimant means, various reasons suggest that, in practice, this is not the case. For instance, Duclos (1995) and Duclos (1997) finds that non-take-up can be empirically explained by the presence of errors in assessing benefit eligibility by the welfare agency (clerical error), but also, because claimants have not declared them to the authorities in the claiming process (deliberately or by error). In addition, official documentations are not clear on how private transfers should be taken into account by the welfare agency. This mainly depends on the use of these private transfers which is unobserved. In any case, we cannot rule out that private transfers are, at least partly, observable by the welfare office. Consequently, we investigate two scenarios in simulating the entitled benefit. In our baseline specification, we assume that private transfers are unobserved $s = 0$ by the welfare agency and thus they do not affect the entitled benefit. In the second scenario, we assume that the welfare agency has a perfect information and reduces the entitled benefit by the same

[6]The entitlement process is described explicitly in Appendix B.

value of private transfers received i.e. $s = 1$. We investigate this case in the sensitivity analysis.

Because the value of the benefit b_i and thus the eligibility status are unobserved for those who do not claim the benefit, we have used a micro-simulation model, described in Appendix B, which follows a similar procedure of the STSM-IAB model adapted for the SOEP (and which is extensively described in Bruckmeier and Wiemers 2012).

The SOEP contains individual and household data on the different sources of earnings, the tax paid, the welfare benefits received, ... on a yearly basis. Among these, respondents have been asked whether they receive social assistance together with the value of the received benefits. In addition, all information relevant for the purpose of simulating the benefit have been reported in the SOEP.[7] Table 1 provides summary statistics of observed and simulated social assistance variables. About 8% of households receive social assistance with an average value of the benefit among them of 6 067 euros. However, very few households receive the additional child benefit. Supposing that the welfare agency has no information on private transfers ($s = 0$), we obtain 19.7% of eligible households with an average value of the benefit among the eligibles of 7 899 euros and 18.9% and 7 700 euros under perfect information ($s = 1$).[8] We find that 65% of households do not take-up social assistance while they are eligible. Only few empirical studies have reported non-take-up rates in Germany. Bruckmeier and Wiemers (2012) finds a non-take-up rate of 49% in Germany in 2005 while Frick and Groh-Samberg (2007) reported a non-take-up rate of 67% for the year 2003. As a quality indicator of the simulation, we find that 83% of the observed receivers are simulated as being eligible. This is a reasonable level of quality for a micro-simulation model, see Bargain et al. (2012) for a discussion on the accuracy of micro-simulation methods. We discuss more extensively the potential sources of micro-simulation errors in Appendix B. Interestingly, the non-take-up rate slightly decreases (64%) when $s = 1$ as some households (263) at the eligibility frontier are no longer supposed to be eligible. However, we find that the share of eligible households among the receivers decreases when we assume $s = 1$ or conversely the share of ineligible households among the receivers increases suggesting that we can reject the hypothesis of perfect information of the welfare agency.

[7] Remark that the household community has been assumed to be the household observed in the SOEP. Although they are theoretically two different notions, in practice they are very close.

[8] The private transfer variable is briefly described in the next section and more extensively in Appendix A.

Table 1: **Welfare participation and eligibility:** summary statistics

		Among recipients/eligible		
	Frequency	Mean	C.V.	Maximum
Observed social assistance	8.22	6 067	63	29 196
- ALG II	8.07	6 124	62	29 196
- Kinderzuschlag	0.31	1 339	140	14 628
Simulated social assistance, $s = 1$	18.9	7 700	69	39 017
Simulated social assistance, $s = 0$	19.7	7 899	67	39 017

Pooled waves (2009-2011), 30 508 households.

3.3 Private transfers and family networks in the SOEP

Several types of private transfers have been reported by individuals sampled in the SOEP. We use the most accurate data on private transfers (available between 2009 and 2011) indicating whether a sampled individual has personally received, in the last year, payments or financial support from relatives or from other people *outside of his or her own household*. Respondents are then asked to specify the value and the origin of these transfers (i.e. from parents/parents-in law, from children/in-law children, from spouse or divorced spouse, from other relatives and from unrelated persons). Then, these individual data are aggregated at the level of the household unit. Data on private transfers are detailed in Appendix A with summary statistics provided in Table 9. It is worth mentioning that the great majority of private transfers are from parents toward children. Remark that this variable covers only monetary transfers while not observing the purpose of these transfers. It is worth mentioning that less households declare receiving public transfers (8.22%) compared to private transfers (10.20%), demonstrating, if necessary, the importance of private redistribution in a developed economy. However, public transfers are less unequal and more generous than private ones, 6 067 and 4 039 euros respectively on average. Another type of transfers have also been reported in the SOEP, namely unexpected windfall transfers. In this case, the household's head were asked if he or another member of his household have received a large sum of money or other forms of wealth (more than 500 euros) as inheritance, gift, or lottery winnings during the last year. Again, respondents are then asked to specify the value of this unexpected windfall and whether it was due to a lottery win, inheritance, or gift. Hence, this variable is directly defined at the household level and no household-aggregation is needed in this case.

Even if respondents specify the origin of the private transfers they received (e.g. from parents), the donors are not identified precisely and might

even not be included in the sample. However, the longitudinal dimension of the SOEP allows us to reconstructing family networks. More precisely, the survey is organized as follows. The SOEP started in 1984 with a initial wave being a representative sample of private households living in Germany. All sampled-household members of 16 and older are eligible for a personal interview. The SOEP follows these original sample members over time and tries to re-interview each of them on a yearly basis, regardless of geographical mobility (within the boundaries of Germany) or whether the household to which the individuals belong has dissolved (e.g. via death, divorce, someone leaving home). This rule also extends to respondents who entered a sampled-household after the first wave due to marriage, residential mobility or birth. As years go on, the children of the first wave reach age-eligibility and become panel members being followed and interviewed in their own right. If they move out and form their own household, they and their new household are still part of the sample and receive a new household identification number. Therefore, the SOEP distinguishes the current household identifier from the original household identifier. As a consequence, one can track each respondent in a given household at time t back to an originally-sampled household in the initial wave. It is worth mentioning that, because of refreshment samples, the calendar year of the initial wave for an originally-sampled member belonging to the initial sample may differ from the calendar year of the initial wave of an originally-sampled member belonging to the refreshment sample. Put differently, at time t, the number of years back to the initial interview is going to differ between those in the initial sample and those belonging to refreshment samples.

We take advantage of the follow-up of SOEP-households to reconstruct family networks. Specifically, we use the individual's variation in the current household identification number to identify existing family links between households. When a set of households originates from the same original household identifier, then we assume that this set of households forms f a family complete network wherein all family members are supposed to be connected each other with the same intensity. Because of the sample design of the SOEP, an individual belongs exclusively to one household which, in turn, belongs to one family.[9] As years go by and in the absence of panel attrition,

[9]Because of the small sampling probabilities, all observed unions occur between an original sampled individual (for whom the original household was sampled) and a new sampled individual (for whom the original household was not sampled). Hence, we do not observe the union of individuals (in a new household) coming from two different original sampled households. To have a household belonging to multiple families would require that some of the current household members originate from different households which were sampled in the SOEP.

the number of observable family links increases. However, observing family links will always be partial (even if the sampling period goes to infinity) as long as the sample does not correspond to the population. Indeed, we observe a family link for only 32.31% of the households in our sample. Conditionally on observing this family link, we can construct a vector of family characteristics e.g. by taking the mean of a given characteristic among all other households in the family. By construction, this vector of family characteristics will be observed only for a sub-sample (i.e. for households for which we observe at least one family link) and missing for the remaining sample.

3.4 A descriptive portrait of non-take-up in Germany

As our interest relies on the (non) participation of the eligible households, we restrict our sample to households simulated as being eligible (when $s = 0$). Our final sample is constituted of 6 018 households. Then, we observe four cases: the household receives private transfers and social assistance, the household receives only private transfers or social assistance and the household receives nothing. For each case, we report in Table 2 the mean and standard deviation of our variables. Particularly, we include private transfer, social assistance, the number of family links and a set of demographic variables, namely age, number of year of education, marital status (married, single, widowed, divorced, separated), number of children in the household, a dummy when living in East-Germany, a dummy indicating if the household head is not German and pre-income, that is the income before social assistance and private transfers.

Several features are worth mentioning. *First*, we can point out notable differences between households who receive private transfers and those who do not. Indeed, the household head is, in average, younger and more educated for households who receive private transfers compared to those who do not. The great majority are single and have less children than average. *Second*, households are less likely to receive private transfers when they live in East-Germany and when they are foreigners too. *Third*, as expected, households for which we observe family links are more likely to receive private transfers. *Finally*, both, the average value of the received benefit and the average value of the simulated benefit are lower when households receive private transfers. Moreover, simulated benefit reveal important variation when the private transfers are assumed to be fully observed ($s = 1$). This suggests that there is either an effective reduction of the entitlement benefit as households declare receiving private transfers or that they are wealthier than those who do not receive private transfers.

Among those who receive private transfers, we found non-negligible dif-

ferences between those who take-up and those who do not. Particularly, the takers are generally older with more children but less educated than those who rely only on private assistance. In addition, they are more likely to be divorced, to live in East-Germany or to be foreigner. As expected the decision to participate is strongly correlated with the value a household can claim. This correlation is mechanically amplified when the benefit is reduced because of private transfers. More interestingly, we point out that the average value of private transfers reduces when people take-up social assistance while the conditional probability of taking-up is lower when households receive private transfers. Indeed, households without private transfers take-up the benefit with a probability of $0.30/0.81 = 0.37$ whereas it is only $0.04/0.19 = 0.21$ for those who receive private transfers. This suggests a substitution between private and public transfers. We are particularly interested in determining whether this negative correlation between public and private transfers is due to the substitution of private transfers or instead to observed and unobserved differences. In other terms, we want to determine whether households who receive private transfers do not take-up social assistance because they are less needy or because of other contextual factors. If there is a substitution effect, then this may have important implications on poverty and inequality, and in fine on the effectiveness of anti-poverty policies.

4 Methodological issues

We consider the following linear utility model explaining welfare participation:

$$P_i^* = \alpha T_i + \tilde{X_i}'\beta + \epsilon_i, \tag{2}$$

where P_i^* is the (unobserved) utility to be on welfare for household i, T_i is the total amount of private transfers received, $\tilde{X_i}$ is a vector of observed characteristics and ϵ is the error term. $\tilde{X_i}$ might include entitled benefits, citizenship, but also variables related to social stigma, information and transaction costs.[10] The aim of our empirical strategy is to estimate the effect of private transfers on the use of public transfers i.e. we want to estimate the value of the parameter α. Particularly, if it is significant and negative, we can conclude that private transfers discourage the take-up of social assistance. On the opposite, if it is significantly positive, we can conclude that private transfers encourage the take-up of social assistance resulting into very different distributional implications.

[10] See Currie (2004) for a survey of the costs associated with take-up of welfare programs.

Table 2: Descriptive statistics of non-take-up

	Without private transfers			With private transfers		
	NTU	TU	Total	NTU	TU	Total
Nb of obs.	3 062	1 835	4 897	882	239	1 121
Frequency	0.51	0.30	0.81	0.15	0.04	0.19
Private transfers, received	0.00	0.00	0.00	6 694	1 682	5 625
	-	-	-	(585.46)	(141.75)	(465.63)
Social assistance, received	0.00	6 393	2 395	0.00	5 625	1 199
	-	(89.38)	(55.48)	-	(240.78)	(85.82)
Social assistance, simulated $s = 0$	6 736	10 030	7 970	6 871	10 240	7 589
	(85.86)	(128.40)	(75.60)	(160.13)	(359.53)	(153.05)
Social assistance, simulated $s = 1$	6 736	10 030	7 970	3 572	8 943	4 717
	(85.86)	(128.40)	(75.60)	(145.38)	(363.11)	(152.89)
Nb of observed links	0.61	0.77	0.67	1.10	1.18	1.12
	(0.02)	(0.03)	(0.02)	(0.04)	(0.08)	(0.03)
Age	49.57	44.47	47.66	34.57	37.25	35.14
	(0.24)	(0.28)	(0.19)	(0.42)	(0.69)	(0.36)
Education	11.68	11.01	11.43	13.06	11.77	12.79
	(0.04)	(0.05)	(0.03)	(0.08)	(0.15)	(0.07)
Marital, married	0.41	0.26	0.35	0.21	0.23	0.22
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Marital, single	0.23	0.34	0.27	0.58	0.47	0.55
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)
Marital, widowed	0.13	0.03	0.09	0.02	0.02	0.02
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Marital, divorced	0.19	0.31	0.23	0.13	0.25	0.15
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Marital, separated	0.04	0.05	0.04	0.06	0.03	0.06
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Nb of children	0.46	0.71	0.56	0.45	0.92	0.55
	(0.02)	(0.03)	(0.01)	(0.03)	(0.07)	(0.03)
East-Germany	0.31	0.46	0.37	0.24	0.44	0.29
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Foreigner	0.04	0.06	0.05	0.02	0.03	0.02
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)
Pre-income	19 144	9 003	15 344	10 705	9 952	10 544
	(193.65)	(179.85)	(155.31)	(568.05)	(509.52)	(459.95)

Pooled waves (2009-2011), 6 018 eligible households. NTU means non-take-up and TU take-up. Standard errors are indicated in parenthesis.

Assessing the impact of private transfers on welfare participation involves a range a methodological challenges. The most evident relates to the fact that we do not observe the marginal utility of participating P_i^*. We observe only whether the marginal utility of participating is positive if the household

participates or not when the marginal utility of participating is negative. Let P_i be a binary variable indicating if the household i takes-up social assistance $P_i = 1$ or not $P_i = 0$, then $P_i = 1$ if $P_i^* > 0$ and zero otherwise. Assuming that the error term ϵ follows a normal distribution, this leads to the well-known probit model.

4.1 Endogenous private transfers

A second methodological issue arises because of the endogenous nature of private transfers T_i. The literature on the crowding-out hypothesis abounds with loads of evidence[11] suggesting that private transfers and public transfers are jointly determined. On one side, (monetary) incentives to take-up social assistance are determined by the needs and resources of the household, and thus by private transfers. On the other side, private transfers can arise either for altruistic motivations or as a part of an informal exchange. Altruistic motivations of private transfers imply that the receipt of private transfers is positively correlated with the wealth of the donor while being negatively correlated with the wealth of the recipient. Thus, benefiting more from social assistance is likely to lower the likelihood of receiving altruistic private transfers. This is a conventional simultaneity bias. Let T_i^* be the latent propensity of receiving private transfers such as:

$$T_i^* = X_i'^T \pi + \epsilon_i^T, \tag{3}$$

where $X_i'^T$ is a vector of household characteristics and the error term ϵ_i^T follows a normal process. Using the same latent variable approach, if i's latent receipt propensity is higher than some critical threshold (normalized to 0), then the household receives private transfer. Let T_i be a binary indicator of receipt of private transfers for each household, then $T_i = 1$ if $T_i^* > 0$ and zero otherwise. Observe that here we do not include explicitly the effect of welfare participation P_i on the propensity to receive private transfers T_i^* as i) we are not interested in the causal determinants of private transfers per se, and ii) a reverse causation would imply that the unobservables ϵ^P and ϵ^T would be correlated. Therefore, we view the private transfer equation as a reduced form equation in which covariates also affecting welfare participation have been included and wherein reverse causation is captured by the correlation between the unobservables. Assuming that receipt of private transfers and take-up of social assistance follow a bivariate normal distribution $\Phi_2(\cdot)$ with correlation $\rho_{P,T}$, then the conditional probability that a household takes-up

[11] See, for example, Albarran and Attanasio (2002), Cox and Jimenez (1990), Cox and Jimenez (1992), and Jensen (2004).

social assistance is:

$$\text{Prob}(P = 1|T = 1, X^P, X^T) = \frac{\Phi_2(X'^P\beta + \alpha, X'^T\pi, \rho_{P,T})}{\Phi(X'^T\pi)}.$$

Clearly, a simultaneity bias might arises through the correlation between the unobservables $\rho_{P,T}$. Although, our model is qualitatively different from the bivariate probit model presented in Maddala (1983, p. 123), the endogenous nature of private transfers does not need special consideration in formulating the likelihood (Greene 2002, chap. 17).

In order to deal with this simultaneity problem, we use an instrumental variable approach providing exclusion restrictions that identify the parameters in the welfare equation. In doing so, we include variables in $X_i'^T$ which are not determinant of welfare participation. Precisely, the propensity to receive private transfers for a given household is instrumented by unexpected windfall transfers (e.g. lottery winnings, inheritance) received by its family network. As discussed before, both motivations of private transfers imply a positive correlation between the donor's wealth and the receipt of private transfers (Cox 1987). Other things being equal, if the wealth of the donor increases then the demand for informal goods and services increases while the demand for altruistic redistribution increases as well. Therefore, one can expect a household whose family had received windfall transfers to receive more private transfers without affecting directly its propensity to claim social assistance.

Although lottery winnings and inheritance transfers are certainly the most exogenous income shocks occurring in a natural setting, one cannot definitively exclude that they are not correlated with observed and unobserved household characteristics. As a consequence, we control for observed heterogeneity at the household level by introducing a set of household characteristics, namely: age and number of years of education of the household head, marital status, number of children in the household, living in East-Germany, being a foreigner, and household income before social assistance and private transfers. These factors can be reasonably thought as being determinants of private transfers (and welfare participation). For short, they may proxy for the permanent income of recipients as capital market imperfections may also play an important role in transfer behaviour (Cox 1990). With constant current resources, higher permanent income of potential recipients increases their desired consumption. If capital markets are imperfect, an increase in permanent income raises the probability of receiving private transfers (and to take-up social assistance). In addition, we control for observable heterogeneity at the family level by including the family average for each of these covariates (i.e. contextual effects). Specifically, for each household, we take

the average of a given variable among all households belonging to family f excluding its own household i. In doing so, we posit that family characteristics proxy for the demand for informal goods and services. We also include the mean distance between a household and his family as a proxy for altruistic motives of private transfers.[12] Summary statistics of the instruments and of this set of covariates are provided in Table 3. We obtain that 3% of households are related with households having received a windfall conditionally on observing at least one family link.

Table 3: **Family covariates:** summary statistics

	Mean	C.V.	Min	Max
Fam. windfall	0.03	597.21	0.00	1.00
Fam. eligibility	0.34	122.46	0.00	1.00
Fam. distance	74.42	192.33	0.00	855.00
Fam. age	45.31	28.10	19.00	90.00
Fam. education	12.21	19.50	7.00	18.00
Fam. marrital, married	0.42	101.17	0.00	1.00
Fam. marital, widowed	0.06	364.07	0.00	1.00
Fam. marital, divorced	0.18	186.58	0.00	1.00
Fam. marital, separated	0.05	394.55	0.00	1.00
Fam. no. children	0.43	166.23	0.00	6.00
Fam. East-Germany	0.40	120.37	0.00	1.00
Fam. Foreigner	0.02	667.87	0.00	1.00
Fam. pre-income	29 649	75.81	-4 761	370 428

Pooled waves (2009-2011), 2 529 eligible households with at least one observed family link.

4.2 Sample selection of the instruments

Because observability of family links relies on the sample design but also on the household course of life, there are reasons to suspect that the instrument is not missing at random. To fix ideas, consider estimating the substitution of private transfers using family's windfall transfers (lottery winnings, inheritance) as an instrument for one's household probability of receiving private transfers. Suppose that family's windfall transfers are allocated randomly among households and that household formation is only due to children moving out the parental home. Since, some costs are associated with household

[12]The geographic area (NUTS 2) in which a household live has been reported in the SOEP. The distance between two households is computed as the Euclidean distance (in km) between the centroids of the respective NUTS2 areas in which the two household live.

formation (e.g. housing costs), only a sub-sample of children will leave the parental home. In this case, family's windfall transfers would satisfy the conditional independence within the sub-sample of children who had left the parental home. But the instrument is missing or undefined for the sub-sample of individuals who had decided to stay within the parental home. The instrument is missing non-randomly if children leaving the parental home have a different level of unobservables affecting welfare participation than households whose children decided to stay at home.

Usually, researchers simply limit the analysis to the sub-sample where the instrument is non-missing. Obviously, this introduces a common sample selection bias wherein the instrumental sample is not representative of the population. Let F_i^* be the latent propensity of observing family links such as:

$$F_i^* = X_i'^F \nu + \epsilon_i^F, \tag{4}$$

where X_i^F is a vector of household characteristics and the error term ϵ_i^F follows a normal process. If i's latent observation propensity is lower than some critical threshold (normalized to 0), then its family link is not observed, and hence its instrument is not observed too. Let F_i be a binary variable indicating whether we observe family links in the SOEP $F_i = 1$ if $F_i^* > 0$ and zero otherwise. In the case of selection on the unobservables (ϵ^F, ϵ^T) and supposing that receipt of private transfers and the process of observing family links are modelled using a bivariate normal distribution with correlation $\rho_{T,F}$, then the conditional probability that a household receives private transfers is:

$$\text{Prob}(T = 1|F = 1, X^T, X^F) = \Phi\left(\frac{X'^T\pi + \rho_{T,F}X'^F\nu}{\sqrt{1-\rho_{T,F}^2}}\right).$$

Clearly, the bias arises because of the correlation $\rho_{T,F}$ between the unobservables. Following the seminal paper of Heckman (1979), this model has been first employed by Van de Ven and Van Praag (1981) in the case of the binary choice models. Remark that identification of the whole system requires that some household characteristics explaining the observation of family links X_i^F are excluded from the private transfers and welfare participation equations. In this attempt, we used the sub-sample membership as an instrument for the observation of family links. Indeed, the SOEP is a collection of sub-samples which have been constructed in order to improve the representativeness of the initial sample but also to enlarge the sample so to cover groups of special interest or for covering the former German Democratic Republic (GDR) after the reunification. An extensive description of the sample design of the SOEP is provided in Wagner et al. (2007). For example, sample A is the main initial sample covering the population of private households living in Federal

Republic of Germany (FRG) in 1984. Consequently, one can expect a sample A household to have a higher probability of observing family links than refreshment samples. Therefore, our identifying restriction assumes that the sample membership determines the observability of family links while being orthogonal to both private transfers received and take-up of social assistance. These instruments have been commonly used in the literature to deal with sample selection issues in survey data, see for instance Cappellari and Jenkins (2004) for panel attrition in the BHPS. Although this exclusion restriction has an *ad hoc* flavour from an economist perspective, it takes advantage of the random sampling of the survey providing a strong instrument.

4.3 Estimation strategy

To summarize, we consider the following model of welfare participation with endogenous private transfers and sample selection in which the exclusion restrictions are explicit:

$$P^*_{if} = \alpha T_{if} + \gamma b_{if} + X'_{if}\beta + F_i \bar{X}'_{(-i)f}\delta + \epsilon^P_{if} \quad (5)$$

$$T^*_{if} = \eta F_i \bar{W}_{(-i)f} + X'_{if}\pi + F_i \bar{X}'_{(-i)f}\kappa + \epsilon^T_{if} \quad (6)$$

$$F^*_i = \lambda S_i + X'_i \nu + \epsilon^F_i \quad (7)$$

$$P_{if} = \begin{cases} 1 & \text{if } P^*_{if} > 0 \\ 0 & \text{if } P^*_{if} \leq 0 \end{cases} \quad T_{if} = \begin{cases} 1 & \text{if } T^*_{if} > 0 \\ 0 & \text{if } T^*_{if} \leq 0 \end{cases} \quad F_i = \begin{cases} 1 & \text{if } F^*_i > 0 \\ 0 & \text{if } F^*_i \leq 0 \end{cases}$$

where the indexes i and f refer to households and families, respectively. The first equation describes the process governing the decision to take-up social assistance, the second equation describes the propensity to receive private transfers and the last equation indicates whether at least one family link is observed. The decision to take-up social assistance is determined by the amount of private transfers T_{if} received and the entitled benefit b_{if} which has been simulated with the assumption that private transfers are not taken into account to compute the entitled benefit ($s = 0$; we investigate the sensitivity of our results with respect to this assumption in the next section). The propensity that a given household receives private transfers is determined by family's receipt of windfall transfers $\bar{W}_{(-i)f}$. In both equations, we control for observed heterogeneity at the household and family levels (X_{if}, $\bar{X}_{(-i)f}$) where X_{if} includes age, number of years of education of the household head, marital status, number of children in the household, living in East-Germany, being a foreigner and household income before social assistance and private transfers while $\bar{X}_{(-i)f}$ includes average distance to the family and average values of the X_{if} of other households in the family (excluding the own household

i). These factors can reasonably be thought as being determinants of private transfers and welfare participation. For short, they may proxy for the permanent income of recipients as capital market imperfections may also play an important role in transfer behaviour. With current resources held constant, higher permanent income of potential recipients increases their desired consumption. If capital markets are imperfect, an increase in permanent income raises the probability of receiving private transfers and to take-up social assistance. Alternatively, they can be seen as proxy for the expected duration of the poverty spell. A smaller expected duration of the poverty spell reduces the present value of the benefit and thus the probability of participating. Last, the family contextual effects proxy for the demand for informal goods and services while the average distance to the family might proxy for altruistic motives of private transfers. Remark that we observe $\bar{X}_{(-i)f}$ and $\bar{W}_{(-i)f}$ only when $F_i = 1$ that is, when $F_i^* > 0$. The i's propensity to observe at least one family link F_i^* is determined by the household characteristics and by the sample membership S_i.

Assuming a multivariate normal distribution of the disturbances (ϵ^P, ϵ^T, ϵ^F) with correlation parameters $\rho_{P,T}$, $\rho_{P,F}$ and $\rho_{T,F}$ we have:

$$\begin{bmatrix} \epsilon^P \\ \epsilon^T \\ \epsilon^F \end{bmatrix} \sim \mathcal{N} \left(\begin{bmatrix} 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho_{P,T} & \rho_{P,F} \\ \rho_{P,T} & 1 & \rho_{T,F} \\ \rho_{P,F} & \rho_{T,F} & 1 \end{bmatrix} \right).$$

This is a trivariate probit model of welfare participation with endogenous private transfers and endogenous sample selection. Parameters of the system can be estimated by full information maximum likelihood (FIML). The maximum likelihood estimator is based on the full specification of the model. Remark that the likelihood function does not necessitate a particular attention as compared to the common trivariate probit model. Estimation is performed using the package `GJRM` in `R` based on a penalized likelihood approach proposed in Filippou et al. (2017). Because the full information approach relies strongly on the distributional specification, we relax those assumptions by estimating the model (5-7) using linear methods as specification checks. We take advantage of the linear setting to estimate fixed effect models as well. Although, these estimates go in the same direction than our baseline results, they clearly show the limits of the linear specification with our data.

In order to assess the exogeneity of private transfers and of observing family links, we report in the top panel of Table 4 estimates and confidence intervals for the correlation parameters. The correlation between unobservables affecting welfare participation and private transfer $\rho_{P,T}$ was positive and statistically significant, indicating a higher propensity to receive private

transfers among the participants compared to the non-participants. The correlation between unobservables affecting welfare participation and observation of family link $\rho_{P,F}$ was negative and statistically significant like the correlation between unobservables affecting private transfers and observation of family link $\rho_{T,F}$ suggesting a lower propensity to observe family links among those who participates or receive private transfers. Therefore, these tests on correlation of unobservables indicate that receipt of private transfers and sample selection of the instruments are endogenous justifying our methodology.

Particularly, our methodology provides two sources of identification: through the distributional assumptions of the model and through the exclusion restrictions.The system of simultaneous equations (5-7) is just-identified. Exclusion restrictions are provided by the simulated value of the benefit b_{if}, the family's income windfall $\bar{W}_{(-i)f}$ and the sample membership S_i. In order to test the validity of exclusion restrictions, we supposed that if the functional form were the sole identifying restriction, then one may treat the exclusion restrictions about the family's income windfall and sample membership status as over-identifying and testable. We report in the bottom panel of Table 4 test statistics and $p-$values for the Wald test. These estimates indicate that family's receipt of windfall transfers and the sample membership variable could be excluded from the welfare participation equation, with $p-$values for the Wald test being 0.339 and 0.232, respectively. Remark that the family's receipt of windfall transfers and the sample membership variables were also found to be significant determinants of private transfers and observation of family links with $p-$values being 0.022 and 0.000, respectively. Therefore, these tests confirm that identification of the model does not rely solely on the distributional assumptions but also on the exclusion restrictions provided by the instrumental variables.

Table 4: Estimates of model correlations and model test statistics

Correlations between unobservables affecting:	Estimate	95% C.I.
Welfare participation and private transfer $\rho_{P,T}$	0.588	$[0.429, 0.718]$
Welfare participation and family link $\rho_{P,F}$	-0.206	$[-0.289, -0.117]$
Private transfer and family link $\rho_{T,F}$	-0.358	$[-0.429, -0.259]$
Null hypotheses for Wald tests	Test statistic	$p-$value
Exclusion of family windfall from welfare participation equation	0.913	0.339
Exclusion of sample membership from welfare participation equation	1.428	0.232
Inclusion of family windfall in the private transfer equation	5.230	0.022
Inclusion of sample membership in the sample selection equation	1250.683	0.000

Pooled waves (2009-2011), 6 018 eligible households.

5 Empirical results

5.1 Receipt of private transfers and non-take-up

In this section, we present the main estimation results of model (5)-(7). Particularly, we provide in Table 5 estimation results for a series of nested specifications going from the naive probit model in column (1) to our baseline model in column (4) which has been used to build Table 4. More precisely, we estimate a probit model of welfare participation ignoring the endogeneity of private transfers (arising from simultaneity and from correlation between the unobservables) in the first column. In column (2), we take into account the potential endogeneity of private transfers arising from the simultaneity by estimating a model of welfare participation with endogenous private transfers described by the first two equations (5) and (6) of the model. We estimate this model by using a limited information approach. The first estimation step consists of probit estimation of the private transfer equation. At the second step, we insert the predicted values for the endogenous explanatory variables in the equation of interest which can be then estimated by a standard probit estimation. Remark that the estimated asymptotic covariance matrix must be corrected at this step. Estimation of both steps are reported in Table 5. This two-step procedure gives consistent estimators and appropriate asymptotic standard errors, see Rivers and Vuong (1988). However, it is potentially inefficient insofar as it does not account for the possible correlation between the unobservables determining welfare participation and private transfers. Then, we estimate this model by full information maximum likelihood in column (3) as proposed in Greene (1998) for instance. In doing so, we take into account the potential endogeneity of private transfers arising from unobservable correlated effects $\rho_{P,T}$. Column (4) presents estimates of our baseline model as described by equations (5-7). In doing so, we take into account the potential sample selection of the instrument. For clarity of Table 5, we do not report estimates of the sample selection equation. Therefore model column (4) is based on the sample of eligible households (6 018 households) while estimation of the models corresponding to columns (1)-(3) are based on the sample of eligible households for which at least one family connection has been observed (2 529 households). In every specification, we include household characteristics, family contextual effects, our set of instruments and year dummies. Private transfers are expressed in thousand euros and standard errors are given between parentheses. In addition, we assume that private transfers are not observed by the welfare agency ($s = 0$); we relax this assumption and investigate issues related to eligibility in the next section. Being single is used as the baseline factor for marital.

Table 5: Main results

	(1)	(2)		(3)		(4)†	
	Probit	IV probit		bi-probit		tri-probit	
	MLE	LIML		FIML		FIML	
Dep. variable	Welfare participation	Welfare participation	Private transfers	Welfare participation	Private transfers	Welfare participation	Private transfers
Constant	1.874***	−0.718	−1.377***	1.891***	−1.294***	1.813***	−0.619***
	(0.388)	(0.921)	(0.373)	(0.360)	(0.354)	(0.164)	(0.160)
Private transfers, receipt	−0.456***	−0.447***		−1.571***		−1.579***	
	(0.075)	(0.079)		(0.137)		(0.116)	
Household characteristics							
Entitled benefit	0.069***	0.069***		0.062***		0.070***	
	(0.007)	(0.006)		(0.006)		(0.004)	
Age	−0.006*	−0.095***	−0.039***	−0.019***	−0.042***	−0.015***	−0.029***
	(0.003)	(0.031)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)
Education	−0.045***	0.166**	0.092***	−0.004	0.095***	−0.053***	0.113***
	(0.016)	(0.077)	(0.015)	(0.016)	(0.015)	(0.011)	(0.009)
Marital, married	−0.423***	0.200	0.281***	−0.294***	0.323***	−0.366***	0.144**
	(0.103)	(0.245)	(0.104)	(0.098)	(0.104)	(0.059)	(0.067)
Marital, widowed	−0.650***	−1.220***	−0.248	−0.601***	−0.265	−0.742***	−0.152
	(0.163)	(0.268)	(0.212)	(0.153)	(0.218)	(0.092)	(0.124)
Marital, divorced	0.177*	0.703***	0.232**	0.208**	0.281***	0.174***	0.105
	(0.101)	(0.212)	(0.110)	(0.095)	(0.109)	(0.059)	(0.070)
Marital, separated	−0.187	0.776**	0.435***	−0.052	0.498***	−0.175*	0.254**
	(0.164)	(0.385)	(0.164)	(0.153)	(0.159)	(0.100)	(0.107)
No. children in HH	0.161***	−0.156	−0.139***	0.085**	−0.164***	0.140***	−0.035
	(0.040)	(0.118)	(0.040)	(0.039)	(0.040)	(0.024)	(0.026)
East-Germany	0.518***	0.220	−0.129	0.389***	−0.117	0.431***	−0.288***
	(0.103)	(0.149)	(0.100)	(0.099)	(0.099)	(0.050)	(0.055)
Foreigner	0.179	−0.287	−0.190	0.115	−0.203	0.241***	−0.087
	(0.238)	(0.330)	(0.318)	(0.220)	(0.292)	(0.091)	(0.116)
HH pre-income	−0.083***	−0.246***	−0.073***	−0.097***	−0.077***	−0.164***	−0.078***
	(0.015)	(0.060)	(0.014)	(0.014)	(0.014)	(0.010)	(0.009)
Family characteristics							
Fam. windfall			0.009***		0.011***		0.006**
			(0.004)		(0.003)		(0.003)
Fam. eligibility	0.295***	0.221**		0.239***		0.383***	
	(0.083)	(0.091)		(0.074)		(0.067)	
Fam. distance	0.000	0.002***	0.001***	0.001***	0.001***	0.001**	0.001***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fam. age	0.002	0.002	−0.000	0.001	−0.000	0.004	−0.007**
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
Fam. education	−0.085***	0.044	0.054***	−0.051***	0.048***	−0.025*	0.038***
	(0.016)	(0.050)	(0.015)	(0.016)	(0.015)	(0.013)	(0.013)
Fam. marital, married	−0.462***	−0.006	0.189	−0.327***	0.171	−0.452***	0.250**
	(0.115)	(0.207)	(0.123)	(0.111)	(0.122)	(0.111)	(0.114)
Fam. marital, widowed	−0.251	0.296	0.238	−0.161	0.257	−0.249	0.286
	(0.188)	(0.273)	(0.194)	(0.177)	(0.194)	(0.182)	(0.187)
Fam. marital, divorced	−0.146	0.217	0.158	−0.102	0.142	−0.150	0.167
	(0.121)	(0.177)	(0.132)	(0.114)	(0.130)	(0.116)	(0.125)
Fam. marital, separated	−0.279	0.197	0.205	−0.156	0.197	−0.216	0.211
	(0.203)	(0.281)	(0.200)	(0.187)	(0.194)	(0.179)	(0.176)
Fam. no. children	0.012	−0.291**	−0.141***	−0.028	−0.134**	−0.006	−0.168***
	(0.047)	(0.117)	(0.053)	(0.046)	(0.052)	(0.046)	(0.051)
Fam. East-Germany	0.084	−0.298*	−0.164	0.016	−0.156	0.047	−0.079
	(0.106)	(0.176)	(0.103)	(0.099)	(0.102)	(0.072)	(0.075)
Fam. Foreigner	0.914***	−0.316	−0.548	0.671***	−0.338	0.730***	−0.422
	(0.276)	(0.557)	(0.335)	(0.259)	(0.300)	(0.232)	(0.272)
Fam. pre-income	−0.039	0.147**	0.109***	−0.019	0.117***	0.047**	0.044**
	(0.024)	(0.061)	(0.034)	(0.024)	(0.031)	(0.018)	(0.020)
Marginal effect	-0.078	-0.109		-0.268		-0.240	
$\rho_{P,T}$	-	-		0.72		0.59	
				(0.56, 0.85)		(0.43, 0.73)	
AIC	2 566	2 557	2 430	4 973		15 006	
BIC	2 712	2 709	2 565	5 259		15 462	
No. obs.	2 529	2 529		2 529		6 018	

$p \in [0, 0.01]$ '***', $p \in [0.01, 0.05]$ '**', $p \in [0.05, 0.1]$ '*'

The sample is constituted of eligible households in 2009-2011 for which we observe at least one family tie. Standard errors are in parentheses. Marginal effects are computed as the average difference (for those who receive private transfers) between the predicted probability with actual private transfers and the predicted probability without any private transfer. † The coefficient of the observation equation are not provided in this Table. The estimated correlations with the event no observable link are 0.152 and 0.343 for welfare participation and private transfers, respectively. Both are statistically different from zero.

For every specification, we found a negative and significant effect of private transfers on the propensity to take-up social assistance. Our results clearly indicate that the main source of endogeneity arises because of unobservables affecting welfare participation and private transfers so as the estimated coefficient of private transfer doubles (from -0.447 to -1.571). The coefficient $\rho_{P,T}$ is positive and statistically significant in columns (3) and (4) of Table 5, suggesting a higher propensity to receive private transfers among the participants than the non-participants. In addition, we compute the marginal effect of private transfers for those who receive private transfers as the average difference between the predicted probability with actual private transfers and the predicted probability without any private transfer. Consequently, the marginal effect gives the average effect of private transfers on the probability that a household takes up social assistance when it receives private transfers. In our baseline specification in column (4), receiving private transfers lowers by 24% the probability of taking-up social assistance on average. Considering the other determinants of welfare participation and private transfers, several features are worth mentioning. As expected, the propensity to take-up social assistance increases with respect to the value of the entitled benefit but it decreases with the wealth of the household as the marginal utility of the benefit decreases. However, being older, more educated, married, separated or widowed (as compared to being single or divorced) are associated with a lower propensity to take-up social assistance whereas the number of children at home, living in East-Germany and being a foreigner are associated with a higher propensity to take-up social assistance. These estimates are rather close to comparable studies of non-take-up in Germany (e.g. Bruckmeier and Wiemers 2012). Estimated family contextual effects indicate that the mean income of the family, the proportion of foreigners and eligible households within the family are associated with a higher propensity to take-up whereas it lowers as the family education level and the proportion of married households in the family increase. These effects suggest that family does not affect one's decision to take-up social assistance only through monetary transfers but also indirectly through family transmission of social norms, aspirations or welfare culture (e.g. Bertrand et al. 2000, Dahl et al. 2014). Considering the propensity to receive private transfers, our estimates are consistent with previous results of the literature (e.g. Cox and Jakubson 1995) and with altruistic motives of private transfers as we find positive and significant coefficients for the wealth and windfall income of the family while we find a negative and significant coefficient for the wealth of the recipient. In addition, we found that the likelihood of receiving private transfers is negatively (positively) related to the age of the recipient (family) which is consistent with intergenerational transfers flowing from the

old to the young. The level of education of the recipient together with the education of the family are associated with a higher propensity to receive private transfers like being married or separated too.

In sum, our results describe two stories of social assistance wherein the determinants associated with receipt of public and private financial transfers are heterogeneous. Furthermore, we found evidences that private transfers reduces strongly incentives to take-up social assistance (at any commonly-used risk level) even when controlling for endogenous private transfers and sample selection. We found that the most dramatic bias arises because of positive correlation between unobservables affecting welfare participation and private transfers. However, these results have some limitations. Particularly, the full information maximum likelihood estimation relies strongly on the normality assumption of the error terms while regular tests on the shape of the distribution of residuals indicate that they are not normal. In addition, we assume that private transfers were not observed by the welfare agency while they might be, at least partially, observed reducing the value of the entitled benefit. We investigate these issues in the next section.

5.2 Sensitivity to measurement

In our baseline specification, we assume that private transfers were not observed by the welfare agency; that is, $s = 0$ in equation (1). However, the welfare agency might, at least partially, observe private transfers reducing the value of the entitled benefit and thus incentives to take-up social assistance. This measurement error is likely to bias upward the effect of private transfers since the substitution effect could capture both the genuine monetary substitution of private transfers and the indirect effect of private transfers lowering the value of the entitled benefit. In order to check whether our results were driven by such measurement issue, we consider the extreme scenario in which private transfers observed in the data are assumed to be perfectly observed by the welfare agency. In addition, they reduce the value of the entitled benefit by the same amount of private transfers received i.e. $s = 1$. This conservative scenario allows us to obtain a lower bound estimate, therefore still estimating a significant effect of private transfers strengthens our case. Furthermore, it is worth mentioning that this is not the actual entitled benefit per se which matters for the household decision to take-up social assistance since the value of the benefit is unknown by the household before claiming. Therefore, the household decision is based on the prospected value of the benefit and for that it is meaningful to consider the extreme cases $s = 0$ and $s = 1$. We report estimation results using the entitled benefit simulated in the case of $s = 1$ in (2) of Table 6. For the ease of comparing the results, we also include

Table 6: Sensitivity to eligibility

	(1) Baseline $s=0$	(2) Baseline $s=1$	(3) Exclude students	(4) Net transfers
Dep. variable	Welfare participation			
Constant	1.813***	1.758***	2.646***	1.774***
	(0.164)	(0.165)	(0.195)	(0.164)
Private transfers, receipt	−1.579***	−1.424***	−1.242***	−1.489***
	(0.116)	(0.117)	(0.153)	(0.108)
Household characteristics				
Entitled benefit	0.070***	0.079***	0.082***	0.079***
	(0.004)	(0.004)	(0.004)	(0.004)
Age	−0.015***	−0.016***	−0.026***	−0.016***
	(0.002)	(0.002)	(0.003)	(0.002)
Education	−0.053***	−0.043***	−0.050***	−0.041***
	(0.011)	(0.011)	(0.012)	(0.011)
Marital, married	−0.366***	−0.348***	−0.357***	−0.349***
	(0.059)	(0.060)	(0.063)	(0.059)
Marital, widowed	−0.742***	−0.730***	−0.722***	−0.739***
	(0.092)	(0.092)	(0.094)	(0.091)
Marital, divorced	0.174***	0.181***	0.154**	0.185***
	(0.059)	(0.059)	(0.062)	(0.059)
Marital, separated	−0.175*	−0.161	−0.270**	−0.166*
	(0.100)	(0.101)	(0.106)	(0.100)
No. children in HH	0.140***	0.133***	0.062**	0.132***
	(0.024)	(0.025)	(0.027)	(0.024)
East-Germany	0.431***	0.420***	0.451***	0.412***
	(0.050)	(0.051)	(0.054)	(0.050)
Foreigner	0.241***	0.240***	0.288***	0.241***
	(0.091)	(0.092)	(0.095)	(0.091)
HH pre-income	−0.164***	−0.173***	−0.207***	−0.175***
	(0.010)	(0.010)	(0.012)	(0.010)
Family characteristics				
Fam. eligibility	0.383***	0.371***	0.333***	0.370***
	(0.067)	(0.067)	(0.076)	(0.067)
Fam. distance	0.001**	0.000**	0.001**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Fam. age	0.004	0.005	0.004	0.005*
	(0.003)	(0.003)	(0.003)	(0.003)
Fam. education	−0.025*	−0.023*	−0.012	−0.020
	(0.013)	(0.013)	(0.015)	(0.013)
Fam. marital, married	−0.452***	−0.487***	−0.516***	−0.483***
	(0.111)	(0.112)	(0.122)	(0.111)
Fam. marital, widowed	−0.249	−0.247	−0.326	−0.266
	(0.182)	(0.183)	(0.200)	(0.182)
Fam. marital, divorced	−0.150	−0.186	−0.125	−0.220*
	(0.116)	(0.117)	(0.128)	(0.116)
Fam. marital, separated	−0.216	−0.254	−0.230	−0.248
	(0.179)	(0.180)	(0.195)	(0.178)
Fam. no. children	−0.006	0.000	0.050	−0.001
	(0.046)	(0.047)	(0.053)	(0.046)
Fam. East-Germany	0.047	0.045	0.041	0.023
	(0.072)	(0.072)	(0.080)	(0.072)
Fam. foreigner	0.730***	0.726***	0.804***	0.719***
	(0.232)	(0.234)	(0.257)	(0.232)
Fam. pre-income	0.047**	0.045**	0.030	0.042**
	(0.018)	(0.018)	(0.021)	(0.018)
Marginal effect	-0.240	-0.213	-0.192	-0.199
$\rho_{P,T}$	0.59	0.61	0.50	0.65
	(0.43, 0.73)	(0.46, 0.73)	(0.30, 0.67)	(0.49,0.77)
AIC	15 006	14 892	13 178	14 715
BIC	15 462	15 348	13 626	15 170
No. obs.	6 018	6 018	5 364	6 018

$p \in [0, 0.01]$ "***", $p \in [0.01, 0.05]$ "**", $p \in [0.05, 0.1]$ "*"

The sample is constituted of eligible households in 2009-2011 for which we observe at least one family tie. Standard errors are in parentheses. Marginal effects are computed as the average difference (for those who receive private transfers) between the predicted probability with actual private transfers and the predicted probability without any private transfer.

in column (1) our baseline estimates when $s = 0$ as provided in column (4) of Table 5. Clearly, we found a significant substitution effect even when the entitled benefit would have been reduced by private transfers. Remark here that we do not assume that private transfers react to a change in the observability of these transfers by the welfare agency. Instead we just ask, for a given quantity of private transfers, whether the negative correlation between private transfers and welfare participation is attributable to a reduction of the incentives to take-up provided by a reduction in the value of the entitled benefit. Taking into account private transfers in determining the value of the entitled benefit improves substantially the quality of the model (as expressed by the AIC and the BIC) suggesting that either the welfare agency actually takes into account private transfers or that households expect their transfers to lower the value of the benefit.

Another potential confounding factor could affect our results because of interactions between welfare programs. In particular, all students who start higher education are eligible to financial aid for students ("Berufsausbildungsförderungsgesetz", BAFöG). This benefit is means-tested and depends on the composition and income of the student's family. As BaföG is prioritized over social assistance, the presence of BaföG students might be a confounding factors knowing that private transfers are more concentrated among the young. Therefore, we investigate this issue by estimating our baseline model excluding households whom the household's head is younger than 25 years old. We report the results in column (3) of Table 6. We still observe a significant and negative effect of private transfers on welfare participation indicating that the situation of students was not driving our results. The last concern relies on the choice of the private transfer variable. Indeed, there are reasons suggesting that we should look at the net private transfers (private transfers received minus private transfer given). Thus, instead of considering only whether a household receives private transfers, we use the variable net recipient indicating whether a household receives more than he gives. The results for net transfers are reported in (4) of Table 6 and show that net private transfers substitutes to social assistance.

5.3 Specification checks

Because the full information approach relies strongly on the distributional specification, we relax those assumptions in this section by estimating the system of simultaneous equations (5-7) using the linear probability model. Estimations results are reported in Table 7. In the first column, we estimate a model of welfare participation with endogenous private transfers by two-stage least squares (2SLS). In doing so, we restrict our sample to households

for which we have observed at least one family link. In order to take into account the unobserved heterogeneity at the household level and thus potential sample selection of the instrumental variables, we estimate a fixed effect model with endogenous private transfers by within-household transformation in column (2) and by first-difference in column (3). Remark that usual tests confirm that family's windfall is a valid instruments being excluded from the welfare participation while being significant determinant of the receipt of private transfers even in a linear setting. In every specification, we consider private transfers as a continuous variable; that is, we consider the effect of one thousand more euros of private transfers on the propensity to take-up social assistance. In every case, we found a negative and significant effect of private transfers on welfare participation confirming our results even when controlling for household unobserved heterogeneity.

6 Counterfactual analysis of poverty

In this section, we address the question of quantifying the estimated public crowding-out effect. Particularly, if some households resign to take-up social assistance because private transfers substitute it, it is not clear whether they are better-off with private transfers instead of public transfers.

In order to answer this question, we process in two steps. First, we compare the predicted non-take-up rate under actual circumstances (actual non-take-up) with the predicted non-take-up rate without private transfers (counterfactual non-take-up). In this attempt, we use the estimates of our baseline model provided in column (4) of Table 5. Predicted probabilities of welfare participation lie in the continuous probabilistic space, then we use the actual mean predicted probability as an arbitrary cut-off point. Thus, households with predicted probabilities greater than the actual mean are assumed to participate and those below are assumed not to participate. For households with missing family links, we impute their missing family characteristics at the observed sample averages. Then, we compute the non-take-up rate as the share of eligible households who do not take-up. Our baseline model predicts that 54% of eligible households do not take-up social assistance under actual circumstances as compared to 66% which is actually observed in our data. However, without private transfers the non-take-up rate would be close to 44%. Therefore, about 19% of the non-take-up is explained by private transfers received.

In a second step, we determine whether the substitution of private transfers has been detrimental in terms of poverty. Let us consider the actual and counterfactual disposable income distributions of the whole popula-

tion Y^A and Y^C respectively. The actual distribution is defined as $Y_i^A = Y_i^0 + P_i^A \times b_i + T_i^R - T_i^G$ where Y_i^0 is the market income of household i (before public and private transfers), P_i^A is the predicted participation under actual circumstances, b_i is the value of the simulated benefit and T_i^R, T_i^G are private transfers received and given, respectively. Then, we consider the counterfactual distribution $Y^C = Y_i^0 + P_i^C \times b_i$ where there is no private transfer $T_i^R = T_i^G = 0$ and households react by claiming more social assistance i.e. $P_i^C > P_i^A$. In order to be as representative as possible, we include non-eligible households in the sample such that b_i equals zero when the household i is not eligible. We provide in Table 8 estimates of the take-up rate and a set of poverty measures for the actual and the counterfactual distributions. Particularly, we provide the non-take-up rate (NTU), the proportion of households below the poverty line (FGT_0), the average poverty gap (FGT_1) and a measure of inequality among the poor (FGT_2) for the sample of recipients of private transfers and the whole sample. These statistics are computed using the FGT_α index presented in Foster et al. (1984) for $\alpha = \{1, 2, 3\}$. The parameter α can be viewed as a measure of poverty aversion, it gives a greater emphasis on the poorest poor.

Results provided in Table 8 shows that the absence of private transfers would increase the take-up of social assistance by 60% for recipients of private transfers implying a reduction of 8% of the proportion of poor households among them. Most importantly, the intensity of poverty and inequality among the poor reduce by 23% and 25% as measured respectively by the FGT_1 and FGT_2. Mechanically, the evidence is mitigated as we consider the whole sample. At the scale of society, the absence of private transfers would increase the take-up of social assistance by 19% implying a reduction of only 3% of the proportion of poor households. Even if relatively few people actually receive private transfers, the crowding-out of social assistance has a sensible effect on the non-take-up rate and on the situation of the poor. Particularly, the average distance between disposable income and the poverty line decreases by 7% and inequality among the poor reduces by 10%.

We also provide the average public and private transfers received in the population to point out the variation in the cost of the welfare program. Indeed, as the number of households remains the same in both distributions, the average welfare benefit can be interpretated as a change in the total value spent by the welfare state. Remark that we ignore the financing of social assistance which could have behavioural and distributional effects on its own. Therefore, we have to understand these results as valid for the actual taxation scheme. It turns out that the welfare participation response in the absence of private transfers would have increased the welfare program costs by 22%. Results are more easily expressed in terms of elasticities. An

increase in 1% of the take-up rate would increase the cost of the welfare program by 1.13% and would decrease the proportion of poor households by 0.17%, the intensity of poverty and the inequality among the poor by 0.40% and 0.54% respectively.

Our results suggest that about 20% of the non-take-up between 2009 and 2011 in Germany was explained by private transfers. In addition, we find that the substitution of private transfers reduces substantially the welfare program cost. However, social assistance is more effective in alleviating the lot of the poorest among the poor than private transfers but at a greater cost. It is worth mentioning that this relative efficiency depends on the network of private transfers, see Bourles et al. (2017) for a study of altruism in networks.

7 Conclusion

Welfare participation has received an increased attention among economists and policy-makers in recent years as non-participation would apparently lower the effectiveness of public intervention. In this paper, we examined whether receipt of private transfers affects the propensity to take-up social assistance in Germany between 2009 and 2011. We exploited the follow-up of households in the SOEP to reconstruct family links and estimate a model of welfare participation that control for the endogenous nature of private transfers and for sample selection of the instruments. We find that 20% of the non-take-up is due to the substitution of private transfers. However, we find that social assistance is more effective than private transfers in alleviating poverty and its intensity.

The present paper contributes to the literature in several ways. First, we complement the literature on the non-take-up by showing that a substantial proportion of the non-take-up is attributable to the substitution of private transfers. Consequently, the non-take-up rate (i.e. the share of participants among the eligible people) must not be understood as an indicator of the inefficiency of social assistance since a higher non-take-up rate might simply indicates more private transfers. Second, we challenge the literature on the crowding-out effect by demonstrating that private transfers can, in turn, crowd-out social assistance. Indeed, economists have been used to investigate the anti-poverty effectiveness of public transfers taking private-transfer responses into account. For instance, Cox and Jakubson (1995) compare the actual poverty rate with what would have been poverty without public transfers taking into account private-transfer responses. The present paper considers the reverse question; that is, what would have been poverty without private transfers and taking into account take-up responses. We find that

social assistance would have been more effective in alleviating poverty and its intensity than private transfers actually did.

Because there might be missed potential unobserved correlated effects, we surely do not regard our results as definitive. As discussed in this paper, a non-negligible source of potential measurement errors could arise from simulation errors when assessing eligibility. We devoted a particular attention to this issue, demonstrating that our results are robust to different measures of eligibility. In addition, our results rely on the assumption that claimants do not declare private transfers to the welfare agency (deliberately or by error), but do it correctly in survey data. However, one cannot definitively exclude that private transfers observed in the SOEP might be subject to reporting errors as well. This would attenuate the estimated effect if errors were random or correlated with the take-up of social assistance. But, if under-reporting of private transfers is a serious issue in the SOEP, then our results would have over-estimated the effect of private transfers. In the light of the above analysis, modelling explicitly the presence of such discrepancies in the measurement of entitlement and of private transfers between the welfare agency and the SOEP could improve substantially our knowledge of non-participation, with obviously important consequences for understanding the efficiency of German social policy.

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Appendix

A Private transfers in the SOEP

Very few surveys contain precise information on private transfers. A notable exception is the German Socio-Economic Panel (SOEP) which is a longitudinal representative survey of households living in Germany provided by the DIW Berlin.[13] The SOEP waves cover the period 1984-2011. However, private transfers have been precisely reported since the wave 2009. Therefore, we restrict the data to the waves 2009-2011. The unit of observation is the household and they are interviewed on a yearly basis. We have 10 280, 9 328 and 10 900 households in 2009, 2010 and 2011 respectively.

Several types of private transfers have been reported by households sampled in the SOEP. The most accurate data we use on private transfers gave information about the origin of private transfers, particularly respondents were asked "*In the last year, have you personally given (received) payments or financial support from relatives or other people outside this household?*" The respondents are then asked to specify the value and the origin (destination) of these transfers (i.e. from parents/parents-in law, from children/in-law children, from spouse or divorced spouse, from other relatives and from unrelated persons). Clearly, this question covers only monetary transfers and we do not observe the purpose of these transfers. Specific questions are dedicated to transfers in kind and alimonies. Unexpected windfall transfers have also been reported, precisely respondents were asked "*Did you or another member of the household receive a large sum of money or other forms of wealth (car, house, etc.) as inheritance, gift, or lottery winnings last year? We refer to money or other forms of wealth worth more than 500 Euros.*" The respondents are then asked to specify the value of this unexpected windfall and whether it was due to a lottery win, inheritance, or gift.

Table 9 provides summary statistics of the private transfer variables. Among the 30 508 sampled households over 2009-2011, 10.20% of households declare having received payments or financial support from relatives or other people in the last year and the average value received, among them, is 4 039 euros. The value of private transfer received lies between 0 and 400 000 euros per year and its distribution is characterized by a high degree of inequality as it is indicated by the coefficient of variation. The great majority of private transfers come from parents or parents-in-law. Remark that data on private transfers given do not match perfectly those declared

[13] A detailed documentation of the SOEP data can be found in Wagner et al. (2007).

as received. This does not necessarily means that individuals tend to (under) over-report what they actually (receive) give. For instance, it could be the case that there are relatively more givers than recipients i.e. if multiple households tend to support simultaneously the same household. Concerning lottery winnings, inheritance and gifts, only 3.35% of the households had declared an unexpected windfall transfers in the last year and the average value received, among them, is 34 747 (maximum 2 500 000) euros. Most of these transfers are due to inheritance and important gifts while lottery winnings are relatively rare.

B A micro-simulation model for eligibility to social assistance

B.1 Overview of social assistance in Germany

Since the Hartz reforms (2003-2005), the main German social assistance program is the unemployment benefit II ("Arbeitslosengeld II", ALG II) codified in the book SGB II of the Social Code ("Sozialgesetzbuch"). Although it refers to unemployment, it is designed as an assistance program which guarantees a minimum income to cover basic needs. Specifically, the ALG II provides social assistance for employable persons between 15 and 65 years old who are not employed and not in receipt of unemployment insurance benefits. Hence, this benefit is means-tested with respect to income and wealth and does not depend on previous work history. The German welfare system and the Hartz reforms are extensively described in Bruckmeier and Wiemers (2012) and Steiner et al. (2012). The welfare system has not been modified deeply since the Hartz reforms in 2005. The micro-simulation model presented hereafter is based on a simplified procedure of the STSM-IAB model adapted for the GSOEP that is extensively described in Bruckmeier and Wiemers (2012).

B.2 Entitled benefit and eligibility status

The ALG II is the last resort safety net which guarantees a minimum income in order to cover the basic needs of the household. It targets employable (able to work) persons between 15 and 65 years old and it is means-tested with respect to income and wealth. This benefit is means-tested with respect to income and wealth and does not depend on previous work history. Basically, if the household income and wealth are lower than some predetermined thresholds, then the household is eligible to the ALG II benefit.

For testing income, the household needs are compared to the (adjusted) household income. The needs HN_j of household j are defined as:

$$HN_j = \sum_{i=1}^{n_j} (W_{ij} + AW_{ij})BR + HC_j,$$

where n_j is the number of household members, W_{ij} is the personal weight of member i in household j, AW_{ij} are weights for additional specific needs, BR is the basic rate (set at 374 EUR in 2011) and HC_j are the housing costs including rent and heating costs of household j. The personal weight of a member W_{ij} are defined respectively as: 90% of the basic rate for each adult over 25 able to work (including the head), 80% of the basic rate for each child between 15 and 25 able to work and 60% of the basic rate for each child less than 15 years old. The basic rate was set at 375 EUR per month. Hence, the household needs (excluding housing costs) of a couple with 2 children younger than 15 is $374 \times (0.9 \times 2 + 0.6 \times 2) = 1\ 122$. The amount of the benefit is defined as the difference between the household needs HN_j and the adjusted household incomes.

The adjusted household income HY_j is the disposable household income including labour and capital incomes, the tax paid, benefits excluding benefits and pensions that are not primarily supposed to cover basic needs (except social assistance, children's allowance and housing benefits), see Table 8 of Bruckmeier and Wiemers (2012). The entitlement of some other benefits (social assistance, children's allowance and housing benefits) are determined simultaneously and the take-up of ALG II excludes the household of taking-up those benefits since they are implicitly already included in the AGL II. Moreover, allowances are granted for labour earnings in order to increase incentives to work. For labour earnings greater than 1 200 EUR (1 500 EUR for households with children), the benefit reduction rate is 100%. Between 800 and 1200 EUR (1 500 EUR for households with children), the benefit is reduced by 90%, while it is reduced by 80% for labour earnings between 100 and 800 EUR. It is not reduced for labour earnings lower than 100 EUR. For example, for a employment income of 900 EUR, the adjusted household income is reduced of $900 - 0.8 \times (800 - 100) + 0.9 \times (900 - 800) = 250$ EUR.

If the adjusted household income HY_j is lower than the household needs HN_j, then the household is eligible. Otherwise, the entitlement is excluded. A similar test is performed on the household wealth: the cumulated assets of the household must be lower than a threshold, namely the wealth allowance. The wealth allowance WE_j depends on the age of the adults in the household:

$$WE_j = 750 + 3100 \times Nb\ Children + \text{Min}(9750; \text{Max}(150 \times age; 3100)).$$

The wealth test is passed if household financial assets are zero after accounting for all wealth allowances. If one of both of the tests are not passed by the household, all its members are assumed to be not eligible to ALG II.

To summarize, our micro-simulation algorithm applies the following procedure:

1. Select the sample of households for which the head is between 15 and 65 years old and able to work. Ability to work is hardly observable, hence we use the disability status as a proxy.

2. Compute the quantities of interest HN_j, HY_j, WE_j for every household j. This can be done easily since all the information needed is observed. However, housing costs are missing for some households, hence we impute them the median observed housing cost.

3. Perform the income and wealth tests, those who pass both tests are eligible.

4. Compare the simulated benefit with prioritized benefits, the household is eligible for the maximum benefit he is entitled to.

Remark that the household community has been assumed to be the household observed in the SOEP. Although they are theoretically two different notions, in practice they are very close. We have determined eligibility after having annualised incomes while the benefits are given on a monthly basis. We may miss to capture short term episodes of eligibility but we capture those who are chronically eligible.

Since 2005 there is an additional child benefit ("Kinderzuschlag") for parents who are not entitled to unemployment benefit II for themselves but their children are. The maximum amount of this transfer is 140 euros per month for children under 18 years who are living in the same household as their parents. Both, the lower and the upper income threshold for eligibility depend on the potential unemployment benefit II amount of the household. The lower income threshold is determined by the unemployment benefit II level of the adult members of the household, while the upper income threshold amounts to the total level of unemployment benefit II, including all children in the household. Income above this threshold is withdrawn at a rate of 70%.

B.3 Quality of simulation

In this section, we assess the quality of our micro-simulation model. We assume that private transfers are unobserved by the welfare agency ($s = 0$), although assuming that $s = 1$ gives similar results. We obtained 6 018

households in 2009-2011 and 65% (3 944) of them do not take-up social assistance. Among others, Bargain et al. (2012) have proposed to use the number of households taking-up while being simulated as non-eligible (type II error) as a quality measure. In our case, 433 (17%) households are found to be non-eligible participants which is a common level of error in the literature. Our micro-simulation results are consistent with the existing literature on non-take-up in Germany which provides non-take-up rates varying between 0.4 and 0.7, see Table 1 of Bruckmeier and Wiemers (2012). Of course, the comparability of these studies is limited due to different data sets and simulation approaches.

In order to assess further the quality of our simulation, we compare the observed benefit and the simulated benefit distributions (for participants) in figure 1 using a Quantile-Quantile plot. A Q-Q plot is a graphical representation of the quantiles of the observed benefits against the quantiles of the simulated benefits. The dashed 45 degree line represents the equality between both distributions. We over-estimate the benefits amounts. This is probably due to the yearly basis approximation of the simulation while it is actually based on monthly data. Specifically, households can be eligible (in mean) over a year while they actually face episodes of eligibility and of non-eligibility lowering incentives to undertake the cost of participating for each episode of eligibility.

Figure 1: Q-Q plot of observed and simulated benefits (euros/year)

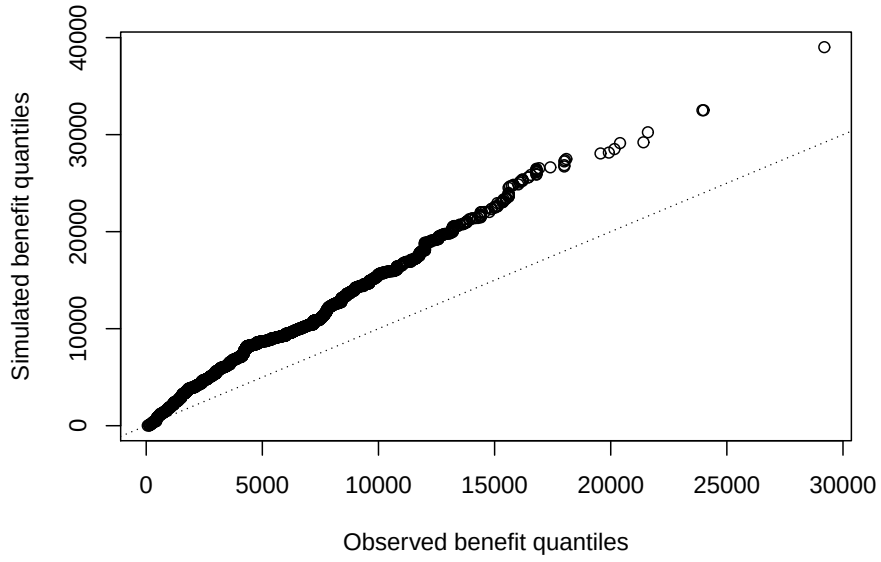

Table 7: Specifications checks

	(1) LPM 2SLS	(2) LPM - FE IV - W.E.	(3) LPM - FE IV - F.D.
Dep. variable	Welfare participation		
Constant	1.036^{***}		-0.002
	(0.107)		(0.013)
Private transfers, amount	-0.015^{***}	-0.012^{***}	-0.010^{***}
	(0.002)	(0.003)	(0.003)
Household characteristics			
Entitled benefit	0.021^{***}	0.008^{***}	0.007^{***}
	(0.002)	(0.002)	(0.002)
Age	-0.001	-0.005	0.003
	(0.001)	(0.012)	(0.011)
Education	-0.013^{***}	0.018^{*}	0.010
	(0.004)	(0.010)	(0.011)
Marital, married	-0.098^{***}	-0.024	-0.047
	(0.029)	(0.056)	(0.057)
Marital, widowed	-0.172^{***}	-0.035	-0.061
	(0.044)	(0.076)	(0.080)
Marital, divorced	0.066^{**}	0.017	-0.006
	(0.029)	(0.071)	(0.072)
Marital, separated	-0.054	0.023	0.008
	(0.047)	(0.074)	(0.076)
No. children in HH	0.052^{***}	0.020	0.025
	(0.011)	(0.023)	(0.024)
East-Germany	0.158^{***}	0.112	0.110
	(0.029)	(0.072)	(0.069)
Foreigner	0.061	0.011	0.008
	(0.069)	(0.175)	(0.168)
HH pre-income	-0.033^{***}	-0.010^{***}	-0.006^{*}
	(0.004)	(0.003)	(0.003)
Family characteristics			
Fam. eligibility	0.100^{***}	-0.018	-0.033
	(0.024)	(0.026)	(0.025)
Fam. distance	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Fam. age	0.001	0.002	0.002
	(0.001)	(0.002)	(0.002)
Fam. education	-0.022^{***}	0.003	0.004
	(0.004)	(0.006)	(0.007)
Fam. marital, married	-0.149^{***}	-0.071	-0.079
	(0.032)	(0.053)	(0.055)
Fam. marital, widowed	-0.067	-0.134	-0.107
	(0.054)	(0.087)	(0.087)
Fam. marital, divorced	-0.059^{*}	-0.027	-0.023
	(0.035)	(0.061)	(0.062)
Fam. marital, separated	-0.102^{*}	-0.136^{**}	-0.149^{**}
	(0.056)	(0.069)	(0.070)
Fam. no. children	0.008	-0.023	-0.015
	(0.013)	(0.026)	(0.026)
Fam. East-Germany	0.015	-0.071	-0.081
	(0.030)	(0.057)	(0.057)
Fam. foreigner	0.254^{***}	0.123	0.102
	(0.075)	(0.122)	(0.134)
Fam. pre-income	-0.013^{*}	-0.005	-0.005
	(0.007)	(0.005)	(0.005)
HH fixed effect		✓	✓
Year fixed effect		✓	✓
R^2	0.279	0.029	0.024
Adj. R^2	0.272	-1.394	0.015
No. obs.	2 529	6 018	2 467

$p \in [0, 0.01]$ "***", $p \in [0.01, 0.05]$ "**", $p \in [0.05, 0.1]$ "*"

The sample is constituted of eligible households in 2009-2011 for which we observe at least one family tie. Standard errors are in parentheses.

Table 8: Actual and counterfactual poverty

	Recipients of private transfers			All		
	Actual	Counterfactual	Variation	Actual	Counterfactual	Variation
Private trans.	4038.48	0.00	-100.00	412.21	0.00	-100.00
Public trans.	309.08	2178.84	604.94	879.03	1069.88	21.71
NTU	0.90	0.35	-61.54	0.54	0.44	-19.16
FGT_0	0.24	0.22	-8.30	0.14	0.13	-3.30
FGT_1	0.08	0.06	-22.92	0.03	0.03	-7.62
FGT_2	0.04	0.03	-25.40	0.01	0.01	-10.28

We use the square root equivalence scale for comparing households of different sizes. The poverty line is defined as 50% of the median disposable income (12 231 euros per year).

Table 9: **Private transfers:** summary statistics

		Among recipients/givers		
	Frequency	Mean	C.V.	Maximum
Private transfers, received	10.20	4 039	257	400 000
- From parents	7.79	4 080	273	400 000
- From children	0.42	1 894	124	14 000
- From spouse	0.63	4 270	98	20 400
- From relatives	2.39	1 796	258	51 700
- From others	0.66	2 487	315	99 999
Private transfers, given	21.48	4 644	200	291 500
- To parents	2.85	2 241	226	100 000
- To children	14.41	5 198	194	290 000
- To spouse	1.01	6 055	145	99 999
- To relatives	4.13	2 159	225	76 000
- To others	2.40	1 432	214	40 000
Windfall	3.35	34 747	315	2 500 000
- Winning	0.18	-	-	1.00
- Inheritance	1.53	-	-	1.00
- Gift	1.80	-	-	1.00

Pooled waves (2009-2011), 30 508 households.