

Bleninger, Sara

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German Socio-Economic Panel Study (SOEP)
DIW Berlin
Mohrenstrasse 58
10117 Berlin, Germany

Contact: Uta Rahmann | soeppapers@diw.de

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Sara Bleninger

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Abstract

Manski's partial identification allows less restrictive, therefore, more credible assumptions than the assumption of random treatment assignment to solve the evaluation problem. In this article the theory of partial identification is applied to the welfare effect of the euro cash changeover. When evaluating the impact of the euro cash changeover on individual welfare, Wunder et al. (2008) face the evaluation problem. Instead of arguing for a comparability of both treatment groups used (i.e. the British and the German Population), partial identification as a more robust technique is used for evaluating the effect of the euro cash changeover. Imposing less restrictive assumptions leaves out an answer about the direction of the welfare effect.

1 Introduction

The aim of policy analysis is to inspect the effect of the interesting policy. Here, the effect of a policy is the difference between the response to the policy and the response to no policy. As a person is either affected or not affected, both never simultaneously occur, the difference cannot be easily computed. The researcher faces the evaluation problem. One statistical solution to the evaluation problem is random treatment assignment. The effect can be computed by differentiating the outcomes of randomly selected groups. However, in policy research random treatment assignment is rare. Thus, scientists look for, or even artificially compose, a reference point, the placebo group. In doing so, there is no random treatment assignment and the effect of the policy cannot be computed by just subtracting the outcomes of the treatment groups. Without additional assumptions, both groups are incommensurable, which brings the evaluation problem back.

As the evaluation problem is a matter of identification rather than of the sampling process, it can only be solved by imposing assumptions. Ordinarily, researchers prefer to assume needed random treatment selection. In this article, assumption of random treatment selection is called Means Missing At Random (MMAR) to point out the analogy to the missing data problem. A formal definition of this assumption will be given later on.

Assumption MMAR leads to a point identified, i.e. an exact solution (Manski, 2009). Therefore, it is easily interpretable. This advantage of a precise estimation is gained with the imposition of a rigorous assumption. Although needed information can only be substituted by assumptions as the evaluation problem is a matter of identification (Manski, 1989, Pearl, 2000), other, perhaps more credible assumptions are possible. Seemingly, assumption MMAR is often chosen for the reason of its unambiguous and precise results, rather than for the reason of its credibility.

The lack of credibility gives reason to take weaker assumptions for policy analysis into account, and therefore a different perception on maintained assumptions is needed. The theory of partial identification achieves this. It is an innovative approach that provides not only a bandwidth of assumptions, but also brings back a discussion about their strength. As different strengths of assumptions lead to different strong identification of parameters it is called partial identification, a theory developed by Manski (2003). This is why assumptions are possible that do not yield accurate estimates, but an identification region which is more

credible. The uncertainty that is inherent in policy analysis because of missing information on counterfactuals is reflected by the imprecise estimation in terms of a computed interval. Manski (2003) calls this interval the identification region. Vansteelandt et al. (2006) call this ignorance region and they name ambiguity ignorance. Latest research combines the identification regions with the theory of random sets. (Beresteanu et al., 2012)

In this article, on the one hand it is shown that partial identification is applicable to complex policy evaluations as the welfare effect of the introduction of the euro, on the other hand the impact of assumptions on results of such an evaluation is pointed out. In the latter case, the presented analysis can be understood as a sensitivity study about the comparability of treatment and placebo group in a quasi-experiment. The practical example is a reanalysis of Wunder et al. (2008). In Wunder et al. (2008) the welfare effect of the euro cash changeover in Germany is inspected by employing the British as the control group. The comparability of the Germans and the British is checked by bearing partial identification on that example. It is shown, that less restrictive assumptions do not lead to the conclusion of a welfare effect of the euro.

In the next section a short review of the study of Wunder et al. (2008) is given. This section regards especially to the evaluation problem inherent to the analysis of the welfare effect of the euro cash changeover. Therefore, it serves as motivation of the theory of partial identification. Thereafter, Manski's theory of partial identification is applied to the welfare effect of the introduction of the euro. Section 4 informs about the used data of the German Socio-Economic Panel (SOEP) and the British Household Panel Survey (BHPS). Moreover, the estimation strategy is explained. In section 5 the estimation results of the welfare effect using different assumption are discussed. The last section gives a conclusion about partial identification of the welfare effect of the euro cash changeover.

2 Evaluating the Cash Changeover as an Quasi-Experiment: the Wunder et al. Study

A negative welfare effect of the euro cash changeover is hypothesized by Wunder et al. (2008). It is based on a perceived inflation that bears a negative welfare effect. After the introduction of the euro Germany was accompanied by such a

perceived inflation. Although official statistics did not give any evidence of a real inflation, the general public felt certain about money depreciation (Brachinger, 2006, Wunder et al., 2008, Parsley and Wei, 2008). This deviation of measured and perceived inflation after the introduction of the euro is reported for other countries of the European Monetary Union, too (Fluch and Stix, 2005, Dziuda and Mastrobuoni, 2009). Even though the inflation was just perceived, its effect on welfare does not need to be imaginary. Wunder et al. (2008) bring three arguments forward for a negative welfare effect of the perceived inflation. First, the overestimation of inflation causes an underestimation of purchasing power leading to a consumption that has a lower welfare than it would have without this misjudgment. This is why the misjudgment of prices reduces the "price system's allocative efficiency" (Brachinger, 2006). Second, the conversion from DM to euro leads to a nominal bisection of money. This illusion leads to a subjective reduction of income. The third argument is that the cash changeover entailed additional transaction costs to consumers. There are several other studies about the effects of the euro cash changeover, Gärling and Thøgersen (2007) gives a short review about main findings.

To check the negative welfare effect empirically Wunder et al. (2008) compared the German reaction to the cash changeover with the British reaction to no cash changeover. In doing so, they dissolved the underlying evaluation problem by constructing a missing group for comparison. Therefore, the British serve as a control group for the Germans.

On the basis of data from the German Socio-Economic Panel (SOEP) and the British Household Panel Survey (BHPS), Wunder et al. (2008) analyzed the effect of the cash changeover with a DID-approach, whereas satisfaction with income is used as an indicator of welfare. The idea behind this approach is that through differentiating twice control of parallel time and group effects is possible (Albouy, 2004, Blundell and Costa Dias, 2002). Additional covariates can be introduced to the model to control its impact on satisfaction with income. For the reason of different scales in the BHPS and the SOEP satisfaction with income is dichotomized into satisfied and not satisfied. Afterwards, Wunder et al. (2008) estimated a logit model, where the interaction effect of time and treatment is interpreted as the causal effect according to the DID-approach.

Wunder et al. (2008) estimated that the probability of British being satisfied with income increases from 53.4 to 54.7 percent, whereas in Germany it decreases

from 64.6 to 56.2 percent. Herewith Wunder et al. (2008) calculated a reduction of 9.7 percentage points in the predicted probabilities because of the introduction of the euro. Wunder et al. (2008) argue that for the reason of using the DID-approach these results can be solely attributed to the euro cash changeover.

As noted above, the evaluation problem is a matter of identification. To solve the identification problem the DID-approach employs two assumptions (Blundell and Costa Dias, 2002, Albouy, 2004): First, there has to be a common time trend across treatment groups also called parallel trend assumption. Second, within each treatment group there is no composition change. For the reason of these assumptions it is plausible that group specific effects and time trends can be differentiated away. However, are the assumptions themselves feasible? Especially, the assumption of a common time trend in both groups implies that changes of uncontrolled covariates over time and reactions to them are the same in both groups (i.e. curves are parallel). This can only be plausibly reasoned if groups are comparable which can only be guaranteed if treatment assignment is random, i.e. conditional on used covariates the British would react like the Germans and vice versa because the same shift in means is presumed which is assumption MMAR. Manski (2003) defines assumption MMAR as:

$$E[Y_t|C = b, X] = E[Y_t|C = g, X], t \in \{0, 1\} \quad (1)$$

where Y_t is the response to treatment t conditional on covariates X , and, C defines whether Y_t is observed in Britain ($C = b$) or in Germany ($C = g$). This is also the definition used in this paper. Assuming this, the unknown counterfactual outcomes can just be replaced with the observed outcome of the respective other group, which yields the treatment effect:

$$H_{MMAR}[\Delta E(Y|X)] = E[Y_1|C = g, X] - E[Y_0|C = b, X] \quad (2)$$

As both terms of equation (2) are determined in the sampling process, the estimation of the effect is just one point, which Manski (2003) calls point identification.

It may be justifiable to assume MMAR in a completely randomized experiment, where treatment selection indeed has nothing to do with specific characteristics of the treated. However, it can be questioned that the exposition to either treatment is random when evaluating the welfare effect. Is it credible to assume

that the membership of one country is random? Assumption MMAR cannot be proven empirically because counterfactuals are not observable (Manski, 2003). Moreover, comparability of both countries cannot be guaranteed. Assumption MMAR yields an unambiguous point identified result which depends on a strong assumption.

To account for country specific characteristics, Wunder et al. (2008) used macroeconomic variables and a random intercept on the regional level. As this allows only different intercepts, Wunder et al. (2008) controlled for a parallel trend of both countries, but different slopes and different shapes of the curves are not allowed. Again reactions in both groups to uncontrolled influences are forced to be the same because curves need to be parallel and can be absorbed by differentiating over time.

Assuming that via differences a common time trend and a fully matched group-specific effect is compensated, is equivalent to assuming that the counterfactual outcomes may be replaced by the outcomes of the respective treatment conditional on covariates. Even though used covariates, especially the macroeconomic ones, control for a lot, it cannot be assured that reactions to all other influences are the same in both countries and can be absorbed by a common time trend.

It has to be doubted that picking assumption MMAR is the right decision, because of incredibility of direct comparability of the German and the British population. Wunder et al. (2008) used a strong assumption to face the evaluation problem. In addition, they do not check up on other assumptions which are less restrictive. This gives cause for concern about the influence of the assumption MMAR. Is the identified negative welfare effect determined by the underlying assumption, or is it robust if other less strong assumptions are imposed? To answer this question, the impact of the euro cash changeover is reanalyzed by imposing different assumptions. When relaxing assumption MMAR in the way done in this article, no evidence for a negative welfare effect emerges.

3 Partial Identification of the Effect of the Cash Changeover

The evaluation problem can be rearranged as a missing data problem: As treatments are mutually exclusive, only the outcome Y of the realized treatment rule

but nothing else is observable. This is the bad news. The good news is that the responses that were realized under the actual treatment rule are known. Therefore, there is partial knowledge about the treatment effect and only parts remain undetermined by data.

The effect $\Delta E(Y|X)$ of a treatment t given the covariates X is defined as the difference of the mean outcome Y_1 when the treatment euro cash changeover had been conducted, and the mean outcome Y_0 when the treatment no cash changeover (or a placebo) had been realized. (Rubin, 1974) As the impact of the cash changeover in Britain ($C = b$) and in Germany ($C = g$) is observed:

$$\Delta E(Y|C = c, X) = E[Y_1|C = c, X] - E[Y_0|C = c, X], \quad c \in \{b, g\} \quad (3)$$

For both countries the counterfactual outcomes cannot be observed. The second term for the German data and first term for the British data remain undetermined.

As the responses are just partially known, the effect itself is uncertain. Rather than defining this uncertainty away using assumptions, a representation of it is more appropriate. Partial identification can handle this problem (Manski, 2003).

Instead of assuming that the British can serve as a control for the Germans (MMAR) and therefore equating treatment and country affiliation, the conditional expected outcomes are inspected more precisely. Using the law of iterated expectations the responses to the two treatments can be amplified by country affiliation (Manski, 2003):

$$\begin{aligned} E[Y_t|X] &= EE[Y_t|C, X] \\ &= E[Y_t|C = g, X]P(C = g|X) + E[Y_t|C = b, X]P(C = b|X), \quad t \in \{0, 1\} \end{aligned} \quad (4)$$

Counterfactuals remain undetermined. $E[Y_1|C = b, X]$ as well as $E[Y_0|C = g, X]$ are not realized. The advantage of this representation is that the known parts and the unknown counterfactuals can be distinguished.

Instead of replacing counterfactuals by the observable response of the respective country, the unknown parts can be restricted to the set Γ_Y being the support of Y . In this sense, Γ_Y is the set of all possible outcomes of Y . On a worst case view, the unknown parts of the outcomes $E[Y_1|C = b, X]$ and $E[Y_0|C = g, X]$ can be restricted to Γ_Y as the expectation of a variable can never exceed the support of the variable. This is the case if nothing more is known than the support

of Y . Then the unknown parts can take every value $\gamma \in \Gamma_Y$. For the reason of γ taking different values of the set Γ_Y , a set of possible outcomes for each treatment is defined. This set is called the *identification region* $H(\cdot)$ (Manski, 2003).

If the support is one dimensional (i.e. $Y \in [y_l, y_u]$) the sets of possible outcomes called identification regions are:

$$\begin{aligned} H [E [Y_0|X]] &= [y_l P(C = g|X) + E [Y_0|C = b, X] P(C = b|X), \\ & y_u P(C = g|X) + E [Y_0|C = b, X] P(C = b|X)] \end{aligned} \quad (5)$$

$$\begin{aligned} H [E [Y_1|X]] &= [E [Y_1|C = g, X] P(C = g|X) + y_l P(C = b|X), \\ & E [Y_1|C = g, X] P(C = g|X) + y_u P(C = b|X)] \end{aligned} \quad (6)$$

The worst case view is evident. In this perspective the counterfactual outcomes take the lowest value possible y_l or the highest value possible y_u . These identification regions use the empirical evidence alone because no additional assumption is needed other than the support of Y and the regularity assumptions to guarantee the existence of expectations. These regions are rather wide but more credible. Moreover, they can be narrowed by restricting the feasible support through assumptions. They restrict the feasible set of outcomes to $\Gamma_Y^{as} \subseteq \Gamma_Y$ and therefore constrain the identification region (Manski, 2003, Vansteelandt et al., 2006).

To simplify analysis, the same support is supposed for both treatments which has to be kept in mind when estimating the responses to the euro cash changeover. Moreover, if the support Γ_Y shrinks to a subset $\Gamma_Y^s \subseteq \Gamma_Y$, the identification region shrinks accordingly (Manski, 2003). For analyzing the welfare effect of the euro cash changeover this is done by estimating bounds for two different supports: the whole bandwidth $\Gamma_Y = [-6, 6]$ and a restricted support $\Gamma_Y = [-3, 3]$ when assuming that satisfaction with income doesn't vary that much just in between two years.

Equations 5 and 6 can be used to construct bounds for the treatment effect. First, the upper bound of the treatment effect is the widest range of the two treatment groups. Therefore, the upper bound of the verum group and the lower bound of the placebo group are differentiated. Second, the lower bound of the treatment effect is the difference of the lower bound of the verum group to the

upper bound of the placebo group, as this is the smallest range (Manski, 1989). Then the identification region for the treatment effect using the empirical evidence alone is (Manski, 1990):

$$\begin{aligned}
H[\Delta E(Y|X)] = & \\
& [E[Y_1|C = g, X]P(C = g|X) + y_tP(C = b|X) \\
& - y_uP(C = g|X) + E[Y_0|C = b, X]P(C = b|X), \\
& E[Y_1|C = g, X]P(C = g|X) + y_uP(C = b|X) \\
& - y_tP(C = g|X) + E[Y_0|C = b, X]P(C = b|X)], \tag{7}
\end{aligned}$$

whereas the same support is feasible for Germans and British to assure comparability of the two countries. As these bounds are computed using the worst case view that the unknown parts of our responses can be everywhere in our support, the identification region covers all credible outcomes (Manski, 2003). If the uncertainty evoked by the sample character of the data (i.e. the uncertainty of estimation) is ignored, it is certain that the true effect is somewhere in the computed identification region. In fact, these identification regions using the empirical evidence alone are rather wide, but there is no doubt about the location of the true effect being in it. The imprecise result is very trustworthy.

The bounds for the treatment responses and for the treatment effect can be narrowed through imposing assumptions. These restrict the feasible set of outcomes to $\Gamma_Y^{qs} \subseteq \Gamma_Y$, which narrows the bounds of the identification regions (Manski, 2003). The assumption of mean independence of outcomes and instruments (MI) is introduced.

Following, it is assumed that the outcome Y is mean independent of the instrumental variable V (MI). Manski's definition of MI is (Manski, 2003):

$$E(Y|V, X) = E(Y|X) \tag{8}$$

Assumption MI yields this identification region for the group that was not exposed

to the cash changeover:

$$\begin{aligned}
H_{MI} [E [Y_0|X]] = & [\max_{v \in V} y_l P(C = g|V = v, X) \\
& + E [Y_0|C = b, V = v, X] P(C = b|V = v, X), \\
& \min_{v \in V} y_u P(C = g|V = v, X) \\
& + E [Y_0|C = b, V = v, X] P(C = b|V = v, X)] \quad (9)
\end{aligned}$$

The identification region of the group that had to bear the introduction of the euro can be constructed equivalently. Later on marital status is employed as instrument. Using the two identification regions of the responses under assumption MI, the identification region of the effect of the introduction of the euro under assumption MI can be constructed in the same way as under empirical evidence alone.

The identification power of assumption MI depends on the used instruments. If the implied instrument point identifies the outcome in one subpopulation and the intersection is nonempty, the intersection of course also shrinks to one point. MI has no identifying power if the instrument and the treatment are statistically independent (Manski, 2003). Assumption MI can be falsified. MI always defines a non-empty identification region if it holds (Manski, 2003). Therefore, if the identification region is empty, assumption MI does not hold. Despite its falsifiability, most researcher avoid assumption MI because of its ambiguous result (i.e. an identification region rather than a point).

4 Estimation Strategy and Data

The same data are available that were employed in the Wunder et al. (2008) study: Data on an individual level are provided by the SOEP and the BHPS. Both are annually repeated surveys, based on households. All household members older than 16 years of age were asked a detailed series of questions based on their living conditions (Wagner et al., 2007, Haisken-DeNew and Frick, 2005). The 2001 and 2003 surveys which are the years surrounding the introduction of the euro are taken into account.

As in the Wunder et al. (2008) study, both samples (i.e. the SOEP and the BHPS) can be combined, too. Comparability is guaranteed by using the Cross-National Equivalent Files. Except satisfaction with household income, all vari-

ables are defined equivalently in both datasets. Wunder et al. (2008) dichotomize the variable satisfaction with household income, for the reason of different scales in BHPS and SOEP. But herewith, a lot of information about the changes in the level of satisfaction is lost. In the SOEP the scale of satisfaction with income has 11 stages and the BHPS has 7. To keep ordinal information just four levels are merged to two on each extreme in the SOEP, which creates a 7 stage scale. Beside preserving ordinal level of measurement this has little loss in precision because of low allocation in extremes.

The interesting outcome is the change in satisfaction with income from 2001 to 2003 which is the difference in satisfaction with income. There are two reasons for using the difference in satisfaction levels as outcome: First, it is intuitively the response to the treatment corrected for the baseline level given in 2001. Second, beneath some computational advantages the computed effect is also a DID as the outcomes are differentiated twice. Through this, a control of constant time and group effects is included.

The following individual variables are controlled: marital and employment status, sex, age, logarithm of the equivalent income (post taxes), education measured on three stages, and a dummy for bad health status. These variables are included for the year 2001. For the reason of using a change variable as a response changes of the exogenous variables from 2001 to 2003 are taken into account.

Data on a regional level come from Eurostat and the Croner-Reward Group. The regional classification is on NUTS1-level¹. The growth rate, the unemployment rate or the inflation rate are not used, but their changes from 2001 to 2003 are taken into account, because in any estimated model the absolute regional covariates are not significant. This shows that the levels are controlled by the differentiated response variable. In Wunder et al. (2008) the regional variables are significant in levels as their response variable is the dichotomized satisfaction with income and not the change in satisfaction. Moreover, using all three macro-economic covariates creates a fatal problem when estimating the conditional probabilities. For the reason of a disjunct support, estimation of conditional probabilities makes no sense, as it becomes either zero or one. Therefore, it seems appropriate to remove the regional variables by levels and just consider their differences.

¹For a further description see http://epp.eurostat.ec.europa.eu/portal/page/portal/nuts_nomenclature/introduction

For estimation of each bound, estimated expectations and probabilities that are conditioned on covariates $X = x$ and on treatment $T = t$ are required. The latter can be interpreted as conditional expectations of a binary variable, too. The simplest way to estimate the conditional expectations is to compute a regression. All points lying on an estimated regression line (i.e. the predicted values) are those conditional expectations needed (Hastie and Tibshirani, 1986, Manski, 1989).

There are two requirements for the estimation of the regression. First, assumptions should not be reintroduced through the back door. Any functional form of a regression produces such an assumption. The underlying functional form is decisive for a causal inference. Semiparametric regression solves this problem by providing the possibility to display data without determining the functional form, but it still allows predictions to be made from the smoothed function itself (Hastie and Tibshirani, 1987). In this study, penalized spline smoothing is used as a semiparametric regression technique, as there are metric variables that have to be smoothed and nominal variables that cannot be smoothed. The trade-off between smoothness and an accurate fit to the data is controlled by the number of knots which partition data into intervals and the smoothing parameter. (Hastie and Tibshirani, 1986, Eilers and Marx, 1996)

The second requirement applies especially to the case of this study. According to Wunder et al. (2008), there are variables of two levels that need to be controlled. On the one hand data on an individual level and on the other hand some variables on the regional level have to be taken into account. To face the two levels, a random intercept on the regional level is introduced and a mixed model is estimated. (Verbeke and Molengherghs, 2000). The additive model of spline smoothing needs to be combined with the mixed model. To face this, the mixed model representation of an additive model is appropriate (Fahrmeir et al., 2007).

For assumption MMAR and for using empirical evidence alone these conditional expectations are estimated:

$$E(\Delta satisfaction|T = t, C = c, X) = \beta_0^{T=t} + s_1^{T=t}(income) + s_2^{T=t}(age) + s_3^{T=t}(\Delta income) + \beta_1^{T=t}X_{micro} + \beta_2^{T=t}X_{macro} + \eta_r^{T=t} + \epsilon_i^{T=t}, (t, c) \in \{(0, b), (1, g)\}$$
(10)

where the superscript $T = t$ refers to the two treatments. As the changes in satisfaction are defined as response, the effect of the experiment is a DID, too. Conditioning on the subgroups by the instrumental variable V is achieved by splitting the datasets according to these groups and estimating only on the basis of the subgroup. The estimated parameters for the two subgroups are used to predict values on the complete dataset. By this, the responses are estimated using the structure of the complete dataset. For each estimated response only information of the subgroups is used as these are the only data taken into account for estimation of the parameters.

As the conditional probabilities $P(C = b|X)$ and $P(C = g|X)$ cannot be estimated as a linear model, the mixed smoothed model needs to be generalized. The resulting model class for prediction of conditional probabilities is the class of generalized additive models (GAM). For estimating probabilities of a binary response the natural link function (i.e. the logit) is chosen. The estimation of the conditional probabilities can easily be derived by using the inverse logit (Hastie and Tibshirani, 1986). The following logit model is estimated:

$$\log\left(\frac{P(C = g|X)}{1 - P(C = g|X)}\right) = \beta_0 + s_1(\text{income}) + s_2(\text{age}) + s_3(\Delta\text{income}) + \beta_1 X_{\text{micro}} + \beta_2 X_{\text{macro}} + \eta_r + \epsilon_i \quad (11)$$

$$P(C = b|X) = 1 - P(C = g|X) \quad (12)$$

Again, predictions for the complete dataset are used to compute bounds using the empirical evidence alone. The structure of joint covariate distribution, and the information of the dataset is kept doing this.

It seems appropriate to use marital status as an instrumental variable when employing assumption MI, defined in equation (8) for the following reasons:² First, there is the technical reason that no significant effect of any marital status on the change in satisfaction with income can be found in any estimated regression (see Appendix A). This reasons to believe in the independence of change in

²Also other less credible instruments were taken into account. If assumption MI is feasible and the instruments have identifying power, the results are comparable to the results given.

satisfaction with income and marital status. Moreover, the significant parameters in the logit model gives hope to some identification power of the instrument. Second, for all processes that intermediate the impact of marital status on personal wellbeing is controlled. These processes are "elevating financial resources, fostering better physical health, and providing greater emotional support"(Stack and Eshleman, 1998). As income on a household level and changes in this income are considered in the estimations the first mechanism is already covered. For the same reason the second process is controlled. Because the response is satisfaction with income rather than life satisfaction, an influence through the mechanism "providing greater emotional support" is not plausible. Third, marital status does not change a lot in the short run. As the response is change in satisfaction with income in two years, time invariant effects of marital status are just differentiated away. Therefore, assumption MI using marital status as an instrument is feasible. For the estimation using assumption MI the equations are slightly different. To estimate the bounds using assumption MI, the expectations are computed in every subgroup defined by marital status. In doing so, the condition on marital status is rendered. For every one of the subsamples bounds on empirical evidence alone are computed. The upper bound under assumption MI is the minimum of the upper bounds in the subgroups. The lower bound is the maximum of the lower bounds in the subgroups.

5 Results

As in this analysis slightly modified data had to be used, a literal comparison of the results to that of the Wunder et al. (2008) study is not possible. With respect to the response which is the change in satisfaction from 2001 to 2003, a complete case analysis is necessary. The response can only be computed if satisfaction with income is answered in both years because it is the difference between the two. However, a similar conclusion should be gained as in the study of Wunder et al. (2008), when imposing assumption MMAR. If underlying assumptions are diminished the same conclusion of a negative welfare effect should be drawn. Otherwise, it reasons an impact of underlying assumptions in the conclusion of a negative welfare effect. Assumption MMAR, as it is equivalent to a direct comparison in the DID-approach, serves as benchmark for the comparability of the two data settings.

Table 1: Estimation Results using different assumptions

point identified estimation					
Means		response	euro	-0.257	
Missing			no euro	0.0434	
at Random		effect		-0.301	
partial identified estimations				bounds	
				lower	upper
Empirical	$y \in [-3, 3]$	response	euro	-0.944	0.637
Evidence			no euro	-2.198	2.222
Alone		effect		-3.165	2.835
Empirical	$y \in [-6, 6]$	response	euro	-1.734	0.637
Evidence			no euro	-4.408	4.432
Alone		effect		-6.165	5.835
Mean	$y \in [-3, 3]$	response	euro	-0.708	0.417
Independence			no euro	-2.017	1.920
		effect		-2.628	2.434

Source: SOEP 2001 and 2003. BHPS 2001 and 2003. Eurostat. Croner-Reward Group. Estimated with R Development Core Team (2010) using Wood (2004), Wood (2008) and Wood (2006)

First, the responses in both countries (i.e. equation 10) are estimated. The results of the semiparametric estimation for the German subgroup and the British subgroup are given in the appendix. In all estimated models the regional variables on absolute values as well as the random intercepts that were introduced for the region are not statistical significant. For this reason, they can be removed. In table 1 the estimated bounds and effects are reported.

As assumption MMAR states that the British react like the Germans would have reacted if the Germans did not have the euro, the British can then be treated as a usual placebo group. Therefore, the difference of the conditional expectations of British and Germans can be computed. In doing so, the treatment effect is in turn a DID because the response itself is the difference between 2001 and 2003.

As reported in table 1, the mean change in satisfaction from 2001 to 2003 in Germany is clearly negative, whereas in Britain the response is nearly zero. Assuming MMAR the result is a significant negative effect of -0.301 (Welch test, $t=64.79$, $df=10575$). This evident negative welfare effect under assumption MMAR corresponds to the results of Wunder et al. (2008). They report a reduction of 9.7 percentage points for the probability of being satisfied. The results of this analysis using less restrictive assumptions can be compared to the Wunder et al. (2008) study. Therefore, they can be used for the sensitivity analysis.

Additionally, figure 1 delivers a closer look at the effect by drawing the conditional effect against a covariate. Age is chosen as an appropriate covariate to

show the welfare effect of the euro-introduction. Moreover, the development of the response seems interesting itself and easily interpretable. It is evident that

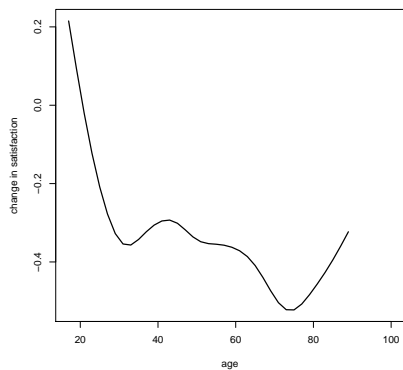


Figure 1: welfare effect against age under assumption MMAR

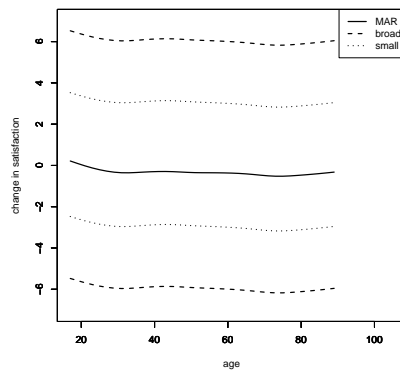


Figure 2: welfare effect against age under empirical evidence alone

younger people do not have such a negative effect as older people have. Ignoring the small ups and downs of the middle aged, an interpretation of figure 1 is that the older people get the more they suffer from the cash changeover. This corresponds to the findings of Dziuda and Mastrobuoni (2009). However, it is worth to keep an eye on the scales of the estimated effect. Nowhere is a very strong effect, denoting that the estimated negative welfare effect is not very intense. When comparing this to the following figures, different scales have to be kept in mind.

It can be doubted that the change in satisfaction of the British is the same as the reaction of the Germans without the introduction of the euro. There are several reasons: Different mindsets, different political systems, different economies and so on. The DID-approach and additional regional variables control for such things as long group specific effects are constant over time and the trends in both countries are parallel (i.e. changes due to time and other uncontrolled variables are the same). However, mentioned reasons could also cause different slopes over time in both countries. Moreover, assumption MMAR cannot be proven. The comparability needed for arguing for random treatment selection is questionable. Using assumption MMAR does not seem appropriate.

The assumption MMAR point identifies the effect of the euro cash changeover. In contrast, the empirical evidence alone provides the broadest interval possible. This reflects that using just the support of the response as an assumption is the least restrictive possibility. To point out that the support of the response can also be interpreted as an assumption, identification regions are computed for two

different supports. First, the level of satisfaction is allowed to change over the whole range. This denotes that a person can be fully satisfied in one year and in the next fully dissatisfied. Then the response ranges from -6.0 to 6.0 as these are the minimum and the maximum of the response. However, it can be questioned whether satisfaction with income really can change that extremely. If it can be assumed that satisfaction is on a specific level and would not completely differ next year, it is possible to constrain the support of possible changes. Therefore, the solutions can range from -3.0 to 3.0 for the change in satisfaction in a second computation. Every other number between -6.0 to 6.0 would be allowed, too. The estimations of these two identification regions for the overall effect are shown in rows four and five in table 1, and for visualization the identification regions are plotted against age in figure 2.

There are two things evident, the first is, the identification region of the welfare effect with the more unrestricted support reflects this by its broader width. The more the support is restricted, the thinner the estimated interval is for the welfare effect. It can be shown (Manski, 1990) that using the empirical evidence alone, one can bind the effect on the half of its support. Second, both identification regions include the zero. As the unknown welfare effect can only be thought between the bounds but not on a special value, it is not possible to conclude a direction of the effect of the cash changeover anymore. Moreover, this provides reasons to doubt the estimated negative welfare effect under MMAR because there is no indication for it when relaxing this assumption. The result while employing MMAR may be caused by the assumption itself and not by the data.

To visualize this again, the two identification regions are plotted against age in graph 2. It has to be noted that these two identification regions are rather wide. Using the empirical evidence alone is the most relaxed assumption possible. Therefore, it can be concluded that the real effect is part of this region, without the need of any unexaminable assumption than the range of possible responses. It is quite certain that the true effect is between these bounds when ignoring the uncertainty of estimation. In the middle is the curve of the estimation using assumption MMAR. It has to be pointed out that, first, this line is not exactly in the middle between upper and lower bound of the estimation using the empirical evidence alone, and that, second, it is not parallel to the other lines. This is why the bounds under empirical evidence are not constructed as a confidence interval from which it clearly has to be distinguished. A confidence interval reflects the uncertainty produced by the sampling process. The identification region con-

tains every effect or outcome that is feasible under the maintained assumption. It therefore corresponds to the uncertainty evoked by the identification problem, and it cannot be solved by extending the sample.

As using the range of the support is a very weak assumption, which naturally generates rather large identification regions, it seems reasonable to introduce a third assumption: MI. Marital status measured with three categories is employed as the instrumental variable. The support of the response has to be specified. For the given reasons it is bound on the interval $[-3, 3]$. In row 6 of table 1 the results of the estimation of the welfare effect using assumption MI are shown.

Assumption MI still does not point identify the welfare effect of the cash changeover, but it has identifying power. This is realized by shrinking the identification region of both responses and equivalently of the effect. The bandwidth of the identification region is diminished compared to the bounds using the empirical evidence alone. Furthermore assumption MI can be proven as argued in section 3: If assumption MI is not true it is not possible to compute any identification region (Manski, 2003). As reasonable regions are estimated based on this assumption, assumption MI is not falsified.

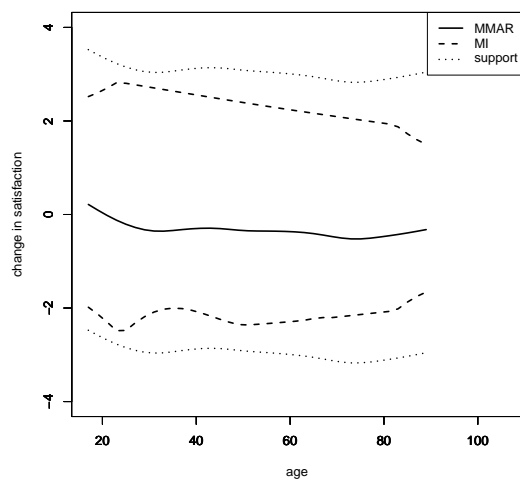


Figure 3: welfare effect against age under assumption MI

The magnitude of the identifying power of assumption MI is evident when looking at the bounds plotted against the age. This is done in figure 3. The shrinkage that is possible through the additional information brought in by assumption MI is evident. The bounds using MI are clearly tighter than using the

empirical evidence alone. In regards to contents, assumption MI gives no reason for a negative welfare effect because both positive and negative effects are still included.

The evidence for the negative welfare effect of the Wunder et al. (2008) study disappears, if only the support or if mean independence of outcome and instrument is assumed. Although it is not possible to decide for a direction of the welfare effect of the euro cash changeover, the estimated bounds under empirical evidence alone and under assumption MI are not meaningless, as they are more credible. From a positive point of view, it shows that it is not necessary to assume strong, but incredible prerequisites for estimating the effect of a policy. The estimated identification regions are more credible than the point identified result under assumption MMAR. The incredibility of assumption MMAR is still inherent in the point identified, negative welfare effect. There is a trade-off between accuracy of estimation and credibility of assumptions maintained.

6 Conclusion

This paper reanalyses the study of Wunder et al. (2008) using partial identification. When relaxing the assumption of comparability of British and Germans it is not possible to decide about the direction of the welfare effect of the euro cash changeover. Whether the effect is positive or negative, either is possible. This gives no reason to doubt a negative welfare effect that was found by Wunder et al. (2008), but it also gives no reason to believe in the negative welfare effect. The negative welfare effect that is shown by Wunder et al. (2008) do not withstand the robustness check by two less restrictive assumptions. Using the empirical evidence alone, a rather broad identification region is estimated. This is no surprise as it is the weakest assumption possible. Also, assumption MI, which has identifying power and can be proven, cannot give reason for a positive or a negative effect. The negative welfare effect is not robust against relaxing assumptions in the way that had been considered here.

The trade-off between the credibility of imposed assumptions and the unambiguousness of results is evident: Less restrictive, but more credible assumptions yield broader identification regions making conclusions less precise, but the identification problem inherent to policy analysis has not to be assumed away. Moreover, the theory of partial identification allows a discussion and a representation

of the strength of assumptions needed to face the evaluation problem.

It is shown that partial identification is applicable to complex analyses. When evaluating the welfare effect, additional covariates have to be controlled. This is done by employing a semiparametric regression technique. Supplementary, it is possible to include random effects to account for group or time effects.

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A Estimation Results of GAMs

Table 2: Estimation Results

	BHPS		SOEP		Probabilities	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
(Intercept)	-1.134 [◇]	0.582	-0.494	0.360	-1.263 e+03***	2.745 e+01
female20011	0.007	0.039	-0.001	0.024	-1.277 e-01**	4.031 e-0.2
badh20011	0.093	0.092	-0.087*	0.041	8.898 e-01***	1.287 e-01
married20011	-0.0118	0.062	0.011	0.041	1.588 e-01**	5.985 e-02
separated20011	0.107	0.156	0.113	0.099	2.044 e+0***	9.440 e-02
divorced20011	-0.086	0.086	0.056	0.057	-9.478 e-01***	7.096 e-02
widowed20011	-0.049	0.101	0.032	0.065	2.008e e+00***	9.907 e-02
selfempl20011	0.098	0.107	-0.022	0.074	1.906 e+00***	1.357 e-01
employee20011	0.102	0.076	0.009	0.051	8.040 e-01***	8.776 e-02
jobless20011	0.205	0.232	0.015	0.085	4.734 e+00***	2.175 e-01
pensioner20011	0.083	0.107	0.086	0.068	2.317e - 01 [◇]	1.193 e-01
ausbild20011	0.014	0.198	-0.147	0.100	3.511e e+00***	1.784 e-01
educ_low20011	0.065	0.043	0.059 [◇]	0.031	2.433 e-01***	5.052 e-02
educ_high20011	0.097	0.063	0.025	0.031	-1.653 e+00***	4.908 e-02
srh_change0	-0.327**	0.123	-0.244***	0.059	-1.693 e-01	1.417 e-01
srh_change1	-0.511***	0.148	-0.581***	0.073	-9.448 e-01***	1.589 e-01
selfempl_change0	0.496**	0.178	0.014	0.132	9.965e -01***	1.783 e-01
selfempl_change1	0.784**	0.251	0.024	0.193	-1.184 e+00***	2.258 e-01
employee_change0	0.360***	0.100	0.213**	0.070	2.096 e+00***	9.345 e-02
employee_change1	0.934***	0.162	0.502***	0.116	1.785 e+00***	1.541 e-01
jobless_change0	-0.177	0.266	-0.250*	0.106	5.067 e+00***	2.331 e-01
jobless_change1	-0.475	0.320	-0.815**	0.148	9.817 e+00***	2.882 e-01
pensioner_change0	0.092	0.169	0.322**	0.118	1.270 e+00***	1.640 e-01
pensioner_change1	0.200	0.221	0.486	0.151	2.467 e+00***	2.247 e-01
ausbild_change0	0.147	0.230	-0.096	0.119	3.226 e+00***	1.765 e-01
ausbild_change1	-0.032	0.397	-0.219	0.184	7.274 e+00***	2.732 e-01
marital_change0	0.459***	0.133	0.312***	0.085	-1.583 e+00***	1.363 e-01
marital_change1	0.474**	0.173	0.624***	0.115	2.497 e+00***	1.546 e-01

Source: SOEP 2001 and 2003. BHPS 2001 and 2003. Eurostat. Croner-Reward Group. Estimated with R Development Core Team (2010) using Wood (2004), Wood (2008) and Wood (2008)

Note: Significance: [◇]<0.1; *<0.01; **<0.001; ***=0