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Content

Abstract

Panel survey participation can bring about unintended changes in respondents' behavior and/or reporting of behavior. Using administrative data linked to a large panel survey, we analyze changes in respondents' labor market behavior. We estimate the causal effect of panel participation on the take-up of federal labor market programs using instrumental variables. Results show that panel survey participation leads to a decrease in respondents' take-up of these measures. These results suggest that panel survey participation not only affects the reporting of behavior, as previous studies have demonstrated, but can also alter respondents' actual behavior.

Zusammenfassung

Die wiederholte Teilnahme an Längsschnittstudien kann zu unbeabsichtigten Verhaltensänderungen und/oder Änderungen im Antwortverhalten der Teilnehmer führen. Um solchen Verhaltensänderungen nachzugehen, haben wir Umfragedaten der Längsschnittstudie PASS mit administrativen Daten verknüpft und schätzen mittels Instrumentenvariablen den kausalen Effekt der wiederholten Umfrageteilnahme auf die Teilnahme an Maßnahmen der aktiven Arbeitsmarktpolitik. Die Ergebnisse deuten darauf hin, dass Umfrageteilnehmer aufgrund der (mehrmaligen) Teilnahme an der Befragung an weniger Maßnahmen der aktiven Arbeitsmarktpolitik teilnehmen. Diese Resultate verdeutlichen, dass die wiederholte Teilnahme an Längsschnittbefragungen sich nicht nur auf das Antwortverhalten der Teilnehmer auswirken kann, sondern auch auf deren tatsächliches Verhalten.

JEL-Klassifikation: C83, J64

Keywords: Administrative Data, Longitudinal Data, Panel Conditioning, Instrumental Variable, Treatment Effects

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1 Background

Panel surveys are a key resource for researchers and policy makers who seek to understand dynamic processes, such as movements in and out of the labor market. Yet such surveys are also vulnerable to the critique that participation can distort respondents' behavior and/or responses, making the collected data unrepresentative of the larger population. This phenomenon is referred to as *panel conditioning* (Halpern-Manners/Warren/Torche 2017). Although concerns about panel conditioning first arose in the 1940s (Lazarsfeld 1940), researchers from many disciplines still rely on panel data for causal analysis. In this study, we will test whether repeated participation in the large-scale German panel study “Labor Market and Social Security” alters respondents' labor market behavior. We think of participation in the first three waves of the panel survey as a treatment and the panel conditioning effect as a treatment effect. The outcome variable of interest is take-up of federal labor market programs. That is, we use techniques of causal analysis to study whether panel survey participation makes respondents more or less likely to take part in the labor market programs.

Our study faces two major methodological challenges. First, we need to disentangle the effect of survey participation on changes-in-behavior from changes-in-reporting. Administrative labor market data, which are independent of respondents' reporting, allow us to study changes in actual behavior. Second, we need to eliminate the confounding effects of survey nonresponse and attrition from the estimates of panel conditioning. We use an instrumental variable approach and select a second sample of persons who were eligible for selection into the panel survey but were not selected. Thus, our data consist of two random subsamples – one selected for the survey, the other one not. Instrumenting the actual participation in several waves of the survey, i.e. the treatment, with the (random) offer to participate in the survey, we hope to eliminate bias due to nonresponse and attrition and estimate the causal effect of panel survey participation on respondents' labor market behavior.

Previous studies of behavioral changes due to panel survey participation are rare and most suffer from design flaws. Many do not disentangle changes-in-reporting from changes-in-behavior due to data limitations; others do not untangle panel conditioning effects from other effects such as attrition. We contribute to this sparse literature and tackle the methodological challenges that previous studies have encountered. We are not aware of any previous study that has analyzed changes in labor market behavior due to panel conditioning.

Before we turn to the data, methods and results of our study, we discuss the two different forms of panel conditioning (changes-in-behavior and changes-in-reporting) in more detail and explain why we expect panel participation to alter survey respondents' labor market behavior.

1.1 Changes-in-behavior panel conditioning

If participation in a panel survey induces changes-in-behavior, then the survey's sample becomes less representative of the population over time, and estimates based on the data will be biased (Yan/Eckman 2012). The classic finding of this type of panel conditioning, from the field of political science, is that participation in a pre-election survey increases voter turnout in upcoming elections (Clausen 1969; Kraut/McConahay 1973; Yalch 1976; Traugott/Katosh 1979; Granberg/Holmberg 1992). However, not all studies have detected this effect (Smith/Gerber/Orlich 2003). Panel participation can also affect other types of behavior: water treatment product use and purchases of health insurance (Zwane et al. 2011); purchases of automotive services (Borle et al. 2007), automobiles (Morwitz/Johnson/Schmittlein 1993; Chandon/Morwitz/Reinartz 2005) and computers (Morwitz/Johnson/Schmittlein 1993); saving for retirement (Crossley et al. forthcoming); and cheating in exams (Spangenberg/Obermiller 1996). For an extensive review of relevant studies in consumer behavior and marketing research, see Dholakia (2010).

The literature proposes several theoretical explanations for changes-in-behavior panel conditioning (Warren/Halpern-Manners 2012). The cognitive stimulus theory best explains why we expect changes in behavior due to panel conditioning to arise in our labor market survey data. This theory holds that repeatedly being asked the same questions causes respondents to become more aware of the topic of the survey, raises their consciousness of the issues, and motivates them to engage in the behavior under study (Sturgis/Allum/Brunton-Smith 2009; Zwane et al. 2011; Warren/Halpern-Manners 2012). This approach likely explains panel conditioning in studies of voting behavior: a pre-election interview may stimulate interest and participation in the election and thereby increase voter turnout among respondents (Clausen 1969). Based on this theory of cognitive stimulation, we hypothesize that repeatedly answering questions in a panel survey about whether or not one has participated in active labor market policies (ALMP) increases the likelihood that respondents will participate in those measures. ALMP are programs administered by the government that aim to reduce unemployment and increase participation in the labor market (Crépon/van den Berg 2016): they may include measures such as job application trainings or continuing education courses. Taking part in a survey that includes several questions about such programs may stimulate respondents to think more about the ALMPs, enhance their awareness of them and thereby increase the likelihood that respondents will participate in these ALMPs. In this way panel participation could change some respondents' behavior. Furthermore, we expect that the effects of participation in the survey will increase with each wave. That is, the size of the effect of panel survey participation on respondents' labor market behavior will increase with each wave.

1.2 Changes-in-reporting panel conditioning

Panel conditioning can also induce changes in how respondents report their behavior, which we refer to as changes-in-reporting (Waterton/Lievesley 1989; Sturgis/Allum/Brunton-Smith 2009; Cantor 2010; Warren/Halpern-Manners 2012). If respondents become more comfortable with and more trusting of the interviewing process over panel waves, their reporting of behavior may become more accurate (for a detailed review of theoretical explanations for changes-in-reporting panel conditioning, see, for example, Warren/Halpern-Manners 2012). In this way, panel participants may become better respondents through repeated participation in the survey. It is also possible that respondents learn from prior participation how the questionnaire is structured and then falsify their answers to reduce the length of the survey (van der Zouwen/van Tilburg 2001; Yan/Eckman 2012; Warren/Halpern-Manners 2012). As a result, panel participants may become worse respondents over time.

Compared to the sparse research on changes-in-behavior, many more studies have analyzed these changes-in-reporting. Panel conditioning in labor market outcomes was found to lead to systematic underestimation of unemployment in the Current Population Survey (Hansen, et al. 1955; Bailar 1975, 1989; Shack-Marquez 1986; Solon 1986; Shockey 1988; Halpern-Manners/Warren 2012). In contrast, conditioning effects were not detected in the Survey of Income and Program Participation (Pennell/Lepkowski 1992). Other reports, such as home alteration and repairs expenditures (Neter/Waksberg 1964), travel behaviors (Mathiowetz/Lair 1994; Meurs/van Wissen/Visser 1989), the reporting of illicit behavior (Halpern-Manners/Warren/Torche 2014) or substance abuse (Williams/Block/Fitzsimons 2006; Torche/Warren/Halpern-Manners 2012), vaccination behavior (Battaglia/Zell/Ching 1996), knowledge of contraception methods (Coombs 1973) and reporting of exercising (Williams/Block/Fitzsimons 2006) are also affected by changes-in-reporting panel conditioning. Other studies find panel conditioning effects only in knowledge questions, but not in attitudes or self-reported behaviors (Clinton 2001; Dennis 2001; Toepoel/Das/van Soest 2009; Das/Toepoel/van Soest 2011; Peter/Valkenburg 2012; Axinn/Jennings/Couper 2015; Struminskaya 2016).

Both forms of panel conditioning can occur at the same time (Halpern-Manners/Warren 2012; Yan/Eckman 2012): for example, respondents of a panel survey on recycling and the environment may over-report their recycling behavior due to social desirability bias in early waves of the survey but report more accurately in later waves as they become more comfortable with the interview process. Yet, at the same time, repeatedly asking respondents about recycling and environmental issues could increase respondents' awareness of the importance of recycling and lead to changes in their behavior due to increased environmental consciousness (the cognitive stimulation hypothesis). Researchers need to be aware of the various ways that panel conditioning can occur and how it can bias inference: working with data affected by panel conditioning, researchers risk mischaracterizing the existence, magnitude and correlates of changes across survey waves in respondents' attitudes and behaviors, which are the main estimates made from panel data (Clinton 2001; Halpern-Manners/Warren/Torche 2017). In addition, as mentioned above, the conclusions drawn from the panel participants may not generalize to the larger population.

1.3 Methodological challenges

The review of previous research on panel conditioning above demonstrates that the literature is full of contradictory findings. The confusion in the literature is likely due to two major methodological challenges in studying panel conditioning. The first is that disentangling changes-in-behavior from changes-in-reporting is difficult or impossible to do without validation records for the answers given in the survey (Waterton/Lievesley 1989; van der Zouwen/van Tilburg 2001). Indeed, most of the studies of changes-in-reporting mentioned above do not distinguish between the two types of panel conditioning (for exceptions see Pennell and Lepkowski (1992); van der Zouwen and van Tilburg (2001); Duan et al. (2007); Halpern-Manners and Warren (2012)). To isolate changes-in-behavior panel conditioning, we need data that are unaffected by respondents' reporting, such as administrative process data. Using such data, researchers can study changes-in-behavior by, for example, comparing respondents' behavior in the administrative data with the behavior of those who were not interviewed. This is the approach we follow in our study. If one was interested in changes-in-reporting, comparisons of respondents' survey answers with administrative records may be used (e.g. Yan/Eckman 2012). If only survey data are available, one may compare experienced respondents with novel respondents in panel studies with rotating designs to study changes-in-reporting (Halpern-Manners/Warren 2012).

The second major challenge to estimating panel conditioning error is elimination of confounding sources of error in panel studies (Williams/Mallows 1970; van der Zouwen/van Tilburg 2001; Sturgis/Allum/Brunton-Smith 2009; Das/Toepoel/van Soest 2011; Halpern-Manners/Warren 2012). The most common confounding source of error is panel attrition. For example, almost all studies of panel conditioning in the Current Population Survey (CPS) do not distinguish between attrition and conditioning (Halpern-Manners/Warren 2012). Yet, some researchers, using other data sets, attempt to control for the effects of attrition by conditioning on covariates related to attrition or to exclude attrition by comparing only those who did not attrit (Shack-Marquez 1986; Pennell/Lepkowski 1992; Dennis 2001; Nancarrow/Cartwright 2007; Toepoel/Das/van Soest 2009; Das/Toepoel/van Soest 2011; Halpern-Manners/Warren 2012). Two other potential sources of error that can easily be mistaken for panel conditioning are interviewer effects and mode effects. Van der Zouwen and van Tilburg (2001), for example, show that changes in reported network size over time are due to interviewer changes between two waves of a panel survey and not panel conditioning. Similarly, Halpern-Manners and Warren (2012) warn that changes over time may also result from a change of the data collection mode: the CPS, for example, uses in person interviews in the first and telephone interviews in subsequent rounds.

We note that only one study of changes-in-behavior does not suffer from the problems discussed above. Crossley et al., (forthcoming) employs a quasi-experimental design to identify a survey participation effect in a large-scale panel survey in the social sciences. Analyzing administrative wealth data from respondents of a Dutch online panel, Crossley et al. find that being interviewed about saving behavior for retirement has, on average, a negative effect on respondents' future saving behavior. Although the authors estimate the effect of participation in a survey module fielded only once, their results lend support to the hypothesis that (repeated) survey participation can alter respondents' behavior. Moreover, their identification strategy, which uses an instrumental variable approach, can be applied to the estimation of panel conditioning effects, and that is the approach we take in this study.

Exploiting linked survey and administrative data, we address the methodological challenges mentioned above in our analysis of changes-in-behavior panel conditioning in the panel study "Labor Market and Social Security" (PASS). In the following sections, we explain why our approach combining linked survey and administrative data with instrumental variable techniques is an ideal method for estimating the causal effect of repeated participation in the survey on respondents' labor market behavior.

2 Data

Combining survey responses and administrative records, we address the two major methodological challenges that have plagued previous studies of panel conditioning. Because the administrative records are not affected by respondents' survey responses, we can isolate changes-in-behavior conditioning. Furthermore, since the administrative records do not suffer from interviewer or mode effects, we eliminate the confounding effects of these error sources. The records also provide a rich source of variables to use to adjust for panel participation: The original sample for the panel survey was selected from the administrative data and we select a second random sample as a control group. With appropriate methods, we can remove the effects of nonresponse and attrition from the panel conditioning estimates.

2.1 Panel "Labor Market and Social Security"

PASS is a large-scale yearly panel survey conducted by the Institute for Employment Research on labor market topics. It consists of a household interview and additional individual interviews with all household members who are at least 15 years old. The main topic of the survey is the pathways into and out of unemployment benefits receipt type II (long-term benefits due to unemployment, disability or employment that does not reach a minimum standard of living), dynamics of the material and social situation of benefit recipients, changes in recipients' behavior and attitudes over time, and interactions between recipients and the benefit providing agencies (Trappmann et al. 2013).

The PASS sample consists of two subsamples.[1] The *recipient subsample* (n=42,150) is a representative sample of all unemployment benefit units in Germany drawn from a register maintained by the Federal Employment Agency. Selection was made based on the most recent available administrative records in July 2006. Unemployment benefit units are in most cases identical to households (see Trappmann et al. 2013), but most of the variables on the register concern the individual who filed the benefit claim. For sake of simplicity, we will refer to this register as a register of individual benefit recipients. This register is part of the administrative data we describe below.

The second subsample is a *general population sample* of households, selected from a commercial data set of addresses in Germany. We do not use this subsample in our analysis and do not describe it further (see Trappmann et al. 2013 for details).

Both samples were drawn using a multi-stage sampling design (Schnell 2007; Rudolph/Trappmann 2007). In the first stage, 300 postcode areas were drawn as primary sampling units. In the second stage, unemployment benefit units in the selected postcode areas were drawn from the administrative register of unemployment benefit recipients. (For details on the second stage sampling of the general population sample see Schnell 2007; Rudolph/Trappmann 2007; Trappmann et al. 2013.) Since the recipient sample was drawn from the administrative data, respondents and nonrespondents can easily be re-identified in the administrative records. We discuss linkage between the survey and administrative data in more detail below.

We consider data from the first three waves of PASS, collected on an annual basis between winter 2006 and spring 2009. In each of these waves, respondents were asked the same set of questions about their participation in ALMPs (see Appendix for the text of the questions). These questions may provide a cognitive stimulus that will increase respondents' awareness and take-up of the programs, and for this reason we expect to see changes-in-behavior panel conditioning.

Before we proceed, we need to make an important restriction to the analysis sample. The information in the administrative data refers to the individual who filed the benefit claim (see above). Yet, the PASS survey collects data from all individuals living in the household. Given that we have administrative records only for the originally sampled recipient, we discard all other household respondents and retain only those who were themselves sampled from the register, that is, those who applied for unemployment benefits.

Household response rates are 35 percent in Wave 1, 51 percent in Wave 2, and 65 percent in Wave 3 (all response rates conditional on participation in previous waves, Christoph et al. 2008; Gebhardt et al. 2009; Berg et al. 2010) .

1 We do not consider refreshment samples that were introduced at several waves of PASS (see Trappmann et al. 2013)

Table 1
Number of respondents and number of successfully linked respondents per wave

	Wave 1	Wave 2	Wave 3	Response in any wave
Realized interviews with sampled recipient	6,698	3,344	3,466	6,738
Successfully linked to administrative records	5,465 (81.6%)	2,979 (89.1%)	3,106 (89.6%)	5,489 (81.5%)

Source: Own calculations based on PASS survey data (waves one, two and three) and administrative data (IEB) as described in Section 2.2. Sample restrictions apply.

2.2 Administrative data and linkage

The administrative data we use to investigate changes-in-behavior conditioning are the 'Integrated Employment Biographies' (IEB), which include records of all employment spells subject to social security, unemployment benefit receipt, participation in ALMP, or spells of job search (Jacobebbinghaus/Seth 2007). These records can be aggregated to the person level and contain histories of employment, unemployment, job search, and benefit receipt, as well as records of ALMP participation. Although the administrative data are not free of error, their overall quality is very high, and they are used by the German government to calculate pension claims, administer benefit claims and make payments (Jacobebbinghaus/Seth 2007; Köhler/Thomsen 2009; Kreuter/Müller/Trappmann 2010). In general, data on benefits, ALMP and job search are of the highest quality, because they are generated by activities of the Federal Employment Agency itself (Jacobebbinghaus/Seth 2007). A variety of socio-demographic variables such as gender, date of birth, citizenship, education and place of residence are also included in the data set.

Because the PASS recipient sample is selected from the same data source, we can easily identify most PASS respondents in the IEB. Table 1 shows the number of respondents per wave and the number of respondents successfully identified in the administrative records. Overall, we are able to identify and obtain full information on 5,489 respondents, i.e. 82 percent of the PASS respondent group (last column of Table 1). The linkage rate of the respondents is in line with other research using the same data (e.g. Kreuter/Müller/Trappmann 2010).

Most of the respondents whom we cannot identify in the administrative data are those who did not consent to linkage of their survey and administrative data (74 percent of the unlinked respondents). The remaining respondents cannot be found in the administrative records for no obvious reason. Missing data due to these two reasons may bias our estimates of panel conditioning. Beste (2011) and Sakshaug and Kreuter

(2012), however, shows that there is little selection bias due to respondents not granting consent to linkage or not having administrative records. The identification rate of survey respondents in the administrative data in this study is high, but missing linkage may still pose a threat to the validity of our results.

Identifying respondents in the administrative data is only the first step, however: to get an unbiased estimate of the effect of repeated survey participation on ALMP take-up, we need to control for the confounding effects of nonresponse and panel attrition.

3 Methods

We regard participation in the first three waves of PASS as a treatment, and the panel conditioning effect as a treatment effect: we are interested in estimating how receiving the treatment (participating in three waves of the PASS survey) changes respondents' behavior. The three-time PASS respondents form the treatment group. To estimate the treatment effect, we selected a second random sample from the IEB administrative data set (n = 42,150) to form the control group. This sample consists of unemployment benefit recipients who were eligible for the first wave of the PASS survey but were not selected in the first wave nor in any later waves. Thus, our data consist of two random samples, one selected for the survey, i.e. the respondents and nonrespondents of PASS, the other one, the control group, not.

In formal terms, $Z \in [0,1]$ defines an indicator of whether one was *assigned* to treatment $(Z = 1)$ or control $(Z = 0)$, i.e. selected for the survey or not. Moreover, $D \in [0,1]$ denotes the treatment indicator, i.e. whether one actually *participated* in the survey $(D = 1)$ or not $(D = 0)$. Y is the take-up of ALMP, the outcome of interest. If everyone complied with the treatment status assigned, then $D = Z$ and the treatment effect would simply be the difference between the average outcomes of the treated persons $(Y|D = 1)$ and the control group $(Y|D = 0)$.

However, as shown in Section 2, many persons selected for the survey did not participate: $Z = 1$, but $D = 0$. These persons are *non-compliers*. Nonresponse and attrition in surveys have many causes and can lead to bias or endogenous selection into treatment (Groves/Cialdini/Couper 1992; Groves/Singer/Corning 2000; Abadie 2003; Kreuter/Müller/Trappmann 2010). For example, people who agree to participate in three waves of a survey may be more compliant and thus more likely to participate in ALMP, even without the treatment of the survey (e.g. Zabel 1998; Rizzo/Kalton/Brick 1996; Lepkowski/Couper 2002). In other words, survey respondents are different from nonrespondents in ways that may bias our analysis of panel conditioning.

One simplistic method to address the problem of noncompliance with treatment assignment is to estimate an intention-to-treat (ITT) effect. In this approach, rather than comparing individuals by different treatment status, we compare those assigned to different treatments, conditional on a vector of predetermined covariates X:

$$ITT = E[Y|Z = 1, X] - E[Y|Z = 0, X \quad (1)$$

Since Z was randomly assigned, the ITT estimates the causal effect of the offer of treatment. However, due to noncompliance with the treatment status assigned (the take-up rate of the treatment ($E(D|Z = 1)$) across all waves is about 13 percent), the effect estimated via Equation 1 will be too small relative to the average causal effect on the treated (Angrist/Pischke 2009: Ch. 4). A more powerful class of methods uses the randomization of Z in an indirect way to overcome the bias due to noncompliance and to estimate the treatment effect.

3.1 Identification by instrumental variables

The instrumental variable (IV) approach is based on the following idea: if an instrument Z is available that induces exogenous variation in the treatment variable D, then instrumenting D with Z allows us to estimate the treatment effect of D (Imbens/Angrist 1994; Angrist/Imbens/Rubin 1996; Abadie 2003).

Following the potential outcomes framework (Rubin 1974, 1977), we define two potential outcomes: Y_1 is the outcome that occurs when a case receives treatment (participates in three waves) and Y_0 is the outcome without treatment. Obviously, we observe only $Y = DY_1 + (1 - D)Y_0$ for a given individual, i.e. either Y_1 or Y_0. Furthermore, let D_z represent the potential treatment status given $Z = z$. If, for a given case, $D_1 = 0$, then that case does not participate when selected; $D_1 = 1$ means that a case would participate when selected. Analogous to the potential outcomes setup, we observe only $D = ZD_1 + (1 - Z)D_0$, but never both potential treatments for any individual.

Following Angrist, Imbens and Rubin (1996), we divide the population into four groups:

- *Compliers*: $D_1 > D_0$ or equivalently $D_0 = 0$ and $D_1 = 1$.
- *Always-takers*: $D_1 = D_0 = 1$.
- *Never-takers:* $D_1 = D_0 = 0$.
- *Defiers*: $D_1 < D_0$ or equivalently $D_0 = 1$ and $D_1 = 0$.

In our framework, the group of survey respondents are the compliers: They were assigned to take the treatment, i.e. participate in the survey, and complied with the treatment assignment. There are no always-takers, i.e. people who take the treatment irrespective of their treatment assignment status, since participation in the survey is only possible for cases selected for participation. However, we do have never-takers, people who do not participate in the survey when they are selected (and also when they are not selected). Non-compliance in our setup is one-sided. Said another way, the probability that a case assigned to control does not take the treatment equals one ($Pr(D_0 = 0) = 1$). There are no defiers for the same reason.

Angrist et al. (1996) show that instrumenting D with Z estimates the local average treatment effect (LATE) for compliers under certain assumptions that we discuss in the next section. Moreover, since there are no always-takers and no defiers, the group of compliers and the group of treated are identical, and the LATE for compliers equals the average treatment effect on the treated (ATT).

3.2 Identification assumptions

To state the assumptions needed for the instrumental variable approach, we need to include Z in the definition of potential outcomes. Let Y_{zd} represent the potential outcome if $Z = z$ and $D = d$, and let X be a vector of known characteristics. Then, with the following nonparametric assumptions, we can use IV techniques to estimate the LATE for compliers.

(i) Independence of the instrument: conditional on X, the random vector $(Y_{00}, Y_{01}, Y_{10}, Y_{11}, D_0, D_1)$ is independent of Z.

(ii) Exclusion of the instrument: $Pr(Y_{1d} = Y_{0d}|X) = 1$ for $d \in \{0,1\}$

(iii) First stage: $0 < Pr(Z = 1|X) < 1$ and $Pr(D_1 = 1|X) > Pr(D_0 = 1|X)$

(iv) Monotonicity: $Pr(D_1 \geq D_0|X) = 1$

We discuss and test each assumption in turn.

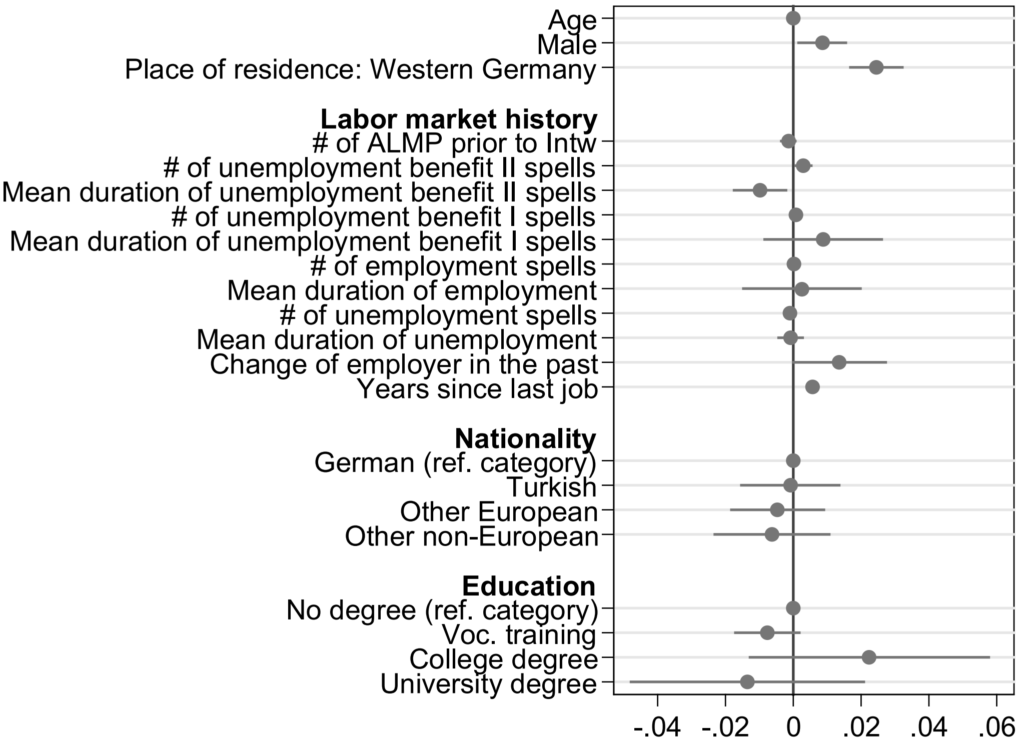
Assumption (i) means that treatment assignment Z is ignorable or as good as randomly assigned, conditional on X. This assumption seems justified in our study. The cases selected for the PASS survey and the control data set are equal size random samples of people registered as unemployment benefit recipients in the IEB at the date of sample selection of PASS. Nevertheless, we check whether treatment assignment is ignorable (conditional on X). The covariates X we use are all derived from the administrative data.[2] To avoid including variables in X that are affected by treatment or the anticipation of treatment (Caliendo/Kopeinig 2008; Stuart 2010), we use in X only those spells in the IEB data that ended before the first day of survey fieldwork (December 11th, 2006).

Figure 1 shows results of an OLS regression of covariates X on the treatment assignment indicator Z. Some of the variables have a significant impact on Z, i.e. differ between the treatment and the control group. The group assigned to treatment, i.e. participation in the survey, contains more males (by about one percentage point) and more people living in Western Germany (by about two percentage points) than the group assigned to control. Regarding labor market characteristics, people assigned to treatment have, on average, received unemployment benefit II more often (about 0.003 spells), but with a shorter duration (about 0.010 years or 3.7 days). Moreover, the time since their last job is marginally greater (about 0.006 years or 2.2 days). The

2 About 38 % values of the education variable (see Figure 1) were missing and were imputed following Fitzenberger, Osikominu and Völter (2005).

F-statistic, although small, supports this finding: we cannot reject the null hypothesis that all coefficients are zero ($F_{20,84279} = 11.97$). Overall, however, associations between X and Z are very weak and most covariates do not differ significantly between the group assigned to treatment and the group assigned to control.

Figure 1
Coefficients from OLS regression of covariates X on treatment assignment indicator Z. All covariates measured prior to start of the survey.



Note: Marginal effects from a logistic regression model accounting for the binary nature of Z produce similar results. Unemployment benefit II: longer-term benefits due to unemployment, disability or employment that does not reach a minimum standard of living. Unemployment benefit I: short-term unemployment benefits due to job loss.

Source: Own calculations based on administrative data (IEB) as described in Section 2.2.

There are three possible explanations for the small differences detected in Figure 1. First, the sampling used in PASS was based on the multistage sampling design described in Section 2.1, while the control group was selected as a simple random sample from the entire country. The differences we observe above may be due to unique characteristics of the population in the 300 areas selected for the survey. Second, the administrative records of unemployment benefit recipients are usually revised after three months to cover recipients whose benefit claims were still under examination at the time of the first reporting. Before revision, the administrative records miss about six percent of all recipients (Rudolph/Trappmann 2007). Thus the records may have changed slightly between the selections of the two samples. Third, differences between respondents granting consent to linkage and respondents not granting consent as well as missing administrative records (Section 2.2) may be another reason for the observed differences between the treatment assigned and the control assigned group. For these reasons, we detect small differences between the two samples, but we are confident that after controlling for these small differences in order to meet Assumption (i), no other uncontrolled differences between the two groups will exist.

Assumption (ii) states that variation in Z, i.e. assignment to treatment or control, does not influence potential outcomes except through D, i.e. response or nonresponse. Moreover, the assumption allows us to define potential outcomes in terms of D alone, i.e. $Y_0 = Y_{00} = Y_{10}$ and $Y_1 = Y_{01} = Y_{11}$. Taken together, (i) and (ii) guarantee that the only effect Z has on Y is through D, that is, that being selected for the survey does not affect participation in ALMP except through taking part in the PASS survey.

However, we need to discuss these two assumptions in more detail. We would like to estimate the effect of repeated participation in PASS on the take-up of ALMPs. Thus, as defined above, D refers to *repeated* participation in the survey. However, some persons participate in one or two waves and then drop out of the survey. These households are selected ($Z = 1$) but not treated ($D = 0$), because we have defined D as participation in all three waves. If participation in just one or two waves is enough to change behavior (as suggested by Crossley et al., forthcoming), then we have a violation of the assumptions: Z can affect Y, even when $D = 0$. Thus, we need to redefine our treatment to satisfy the assumptions. Instead of participation in the first three waves of PASS, we define the treatment as participation in *any* of the first three waves of PASS.[3] This modification to the definition of treatment strengthens our argument that, once we control for the small differences between the group selected for survey participation and the control group of unselected recipients, assignment to survey participation, Z, has no effect other than through actual participation in any wave of the survey, D, and thus that Assumptions (i) and (ii) hold.

Assumption (iii) states that D and Z must be correlated, conditional on X. In addition, the support of X conditional on $Z = 1$ must coincide with the support of X conditional on $Z = 0$. Since 5,489 people (about thirteen percent) responded in at least one of the first three waves of PASS, and no non-selected cases participated, this assumption appears to be satisfied. In addition, tests indicate that we do not need to worry about weak identification (F-statistic from an IV regression with a linear first stage including the covariates of Figure 1: $F_{1,84278} = 6357.10$; partial R-squared of the treatment indicator 0.07; Stock/Yogo 2005; Angrist/Pischke 2009: Ch. 4).

Assumption (iv), i.e. monotonicity, holds trivially because only people selected for the survey could participate in the survey.

3 It may be possible that being selected for the survey, receiving the advance letter announcing the survey, and perhaps receiving a few contact attempts could influence ALMP behavior without survey participation, but we believe the chances of that happening are very small. We are not aware of any literature showing that survey contact attempts and advance materials change behavior.

In addition to the four assumptions discussed above, we need to assume the stable unit treatment value assumption (SUTVA) (Rubin 1978; Angrist/Imbens/Rubin 1996). It implies that potential outcomes for each person are unrelated to the treatment status of others. All unemployment benefit recipients were independently selected from the administrative data and we exclude all people who were interviewed because they belong to a selected household. Thus, we do not see any reason why this assumption would not hold.

Having discussed the assumptions in detail, we next turn to estimation of the LATE or, in the terminology of Abadie (2003), the local average response function (LARF).

3.3 Estimation

Define the object of interest, the local average response function for compliers (LARF) as $E[Y|X,D,D_1 > D_0]$. Suppose $E[Y|X,D,D_1 > D_0] = h(D,X;\theta)$, where $h(D,X;\theta) = \alpha D + x'\beta$ is a linear parameterization for the LARF with the parameter vector $(\theta = \alpha;\beta)$. Furthermore, define $\kappa = 1 - \frac{D(1-Z)}{Pr(Z=0|X)} - \frac{(1-D)Z}{Pr(Z=1|X)}$. Using κ as a weighting scheme, the joint distribution of (Y,D,X) is identifiable for compliers using the IV idea described above and Theorem 3.1 of Abadie (2003: 236-237). Note that $\frac{D(1-Z)}{Pr(Z=0|X)} = 0$ since non-compliance is one-sided in our case. Using these results, Abadie (2003: 239) proposes the following least squares estimator for continuous outcomes:

$$(\hat{\alpha},\hat{\beta}) = \arg\min_{(\alpha,\beta)\in\Theta} \frac{1}{n}\sum_{i=1}^{n} \kappa_i (y_i - \alpha d_i - x'_i\beta)^2 \tag{2}$$

where we estimate κ with a probit model, i.e. $Pr(Z = 1|X) = \Phi(\mathrm{x}'\gamma)$.

When the outcome is binary, a probit transformation of the linear part of (2) can be used. The estimator is then defined as:

$$(\hat{\alpha},\hat{\beta}) = \arg\min_{(\alpha,\beta)\in\Theta} \frac{1}{n}\sum_{i=1}^{n} \kappa_i (y_i - \Phi(\alpha d_i - x'_i\beta))^2 \tag{3}$$

where $\Phi(.)$ is the cumulative distribution function of a standard normal distribution (Abadie 2003: 239). In both cases, variances are estimated according to Theorem 4.2 of Abadie (2003: 242).

We next define the outcome Y, participation in ALMP after participation in the survey, in more detail.

3.4 Outcomes

We consider two different variables related to ALMP participation as outcomes. The first is a simple indicator of whether a person participated in any ALMP (1) or did not (0). The second is the number of ALMP taken-up. If panel conditioning has led to changes in behavior, we should see that respondents are more likely to participate in ALMP and participate in more programs. In building these outcome variables, we consider only those spells that started after the first day of fieldwork (because the survey cannot affect ALMP spells before it started) and those that occurred before January 31st, 2010, the day before fieldwork of wave four started (because later spells may have been influenced by later waves of the survey and in this study we consider only the first three waves). Furthermore, we consider both outcomes at three different periods in time. The first period begins after the first day of field work of Wave 1 and ends just before the beginning of Wave 2. The second period begins after Wave 2 and ends before Wave 3 and the third, finally, after Wave 3 and before Wave 4. However, we must then also define the treatment accordingly. Table 2, showing possible response patterns, defines the treatment indicator for each outcome period.

Estimating the treatment effect for each of these treatments separately allows us to study whether panel conditioning effects get stronger over time, i.e. whether the effect size increases with each additional wave. However, our approach may underestimate the effects of *repeated* participation in the survey as the definitions of the treatments after waves two and three also include people who responded in only one wave.

For each outcome, we first estimate the ITT described in (1). Second, we estimate the LARF with the estimator defined in (2) for the number of ALMP participations and the estimator defined in (3) for the binary participation indicator. Both LARF estimators are implemented in the LARF package in R (An/ Wang 2016).

Table 2
Treatment variable definitions and response patterns

Outcome period	Treatment	Response to survey: Wave 1	Wave 2	Wave 3	N	Total
After wave one & before wave two	$D = 1$	x			5,465	5,465
After wave two & before wave three	$D = 1$	x	x		2,961	5,483
		x			2,504	
			x		18	
After wave three & before wave four	$D = 1$	x	x	x	2,268	5,489
		x	x		693	
		x		x	821	
			x	x	11	
		x			1,683	
			x		7	
				x	6	

Source: Own calculations based on administrative data (IEB) as described in Section 2.2.

As a falsification test, we also create the same two outcome variables for earlier spells of ALMP, those that ended before the first day of fieldwork. If the assumptions discussed above are met and the model is correctly specified, we should not find a significant treatment effect on pre-treatment outcomes, because the survey cannot affect ALMP spells that occurred before the survey started. This approach is similar to the preprogram tests suggested by Heckman and Hotz (1989).

In sum, with the methods discussed here, we are able to demonstrate that our models and methods estimate the causal effects of repeated participation in PASS on the take-up of ALMP. The results allow us to answer our research question: whether panel conditioning leads to changes in respondent labor market behavior.

4 Results

In a first step, we present descriptive statistics of the two outcome variables. Then, we present the results of the ITT analysis and our main results, the estimated treatment effects of participation in several waves of the survey. At each step, we also check the results from the falsification test: if our models are working well, they should not detect any treatment effect before Wave 1 (that is, before treatment began).

4.1 Descriptive statistics

Table 3 shows descriptive statistics of the two outcome variables at four different periods in time. The first set of rows show that the two outcomes do not differ much between the group selected for the survey ($Z = 1$) and the control group of unselected recipients ($Z = 0$) before the start of the survey. This result is expected, given the random selection process. The last two columns, however, show that respondents to the survey ($D = 1|Z = 1$), i.e. selected cases who will respond to at least one wave of the survey, participate more often in ALMP and in more ALMP than nonrespondents ($D = 0|Z = 1$). These results support the claim that actual participation in the survey, i.e. take-up of the treatment, is highly selective, which inspired our use of the instrumental variable approach in the first place.

Table 3
Descriptive statistics of the outcome variables

Outcomes		Mean (Std. dev.) Selected ($Z = 1$)	Not selected ($Z = 0$)	Respondents† ($D = 1\|Z = 1$)	Nonrespondents ($D = 0\|Z = 1$)
Before wave one	Number of ALMP participations	1.64 (2.02)	1.69 (2.02)	2.05 (2.17)	1.58 (1.99)
	ALMP participation (Yes=1)	0.61 (0.49)	0.62 (0.49)	0.71 (0.45)	0.60 (0.49)
	N	42,150	42,150	5,489	36,661
After wave one & before wave two	Number of ALMP participations	0.32 (0.68)	0.34 (0.71)	0.40 (0.73)	0.31 (0.68)
	ALMP participation (Yes=1)	0.23 (0.42)	0.24 (0.43)	0.29 (0.45)	0.22 (0.41)
	N	42,150	42,150	5,465	36,685
After wave two & before wave three	Number of ALMP participations	0.63 (1.09)	0.66 (1.12)	0.77 (1.14)	0.61 (1.07)
	ALMP participation (Yes=1)	0.34 (0.48)	0.36 (0.48)	0.42 (0.49)	0.33 (0.47)
	N	42,150	42,150	5,483	36,667
After wave three & before wave four	Number of ALMP participations	0.93 (1.44)	0.98 (1.49)	1.12 (1.54)	0.90 (1.43)
	ALMP participation (Yes=1)	0.42 (0.49)	0.44 (0.50)	0.49 (0.50)	0.41 (0.49)
	N	42,150	42,150	5,489	36,661

† See Table 2 for the definition of respondents in each wave.
Source: Own calculations based on administrative data (IEB) as described in Section 2.2.

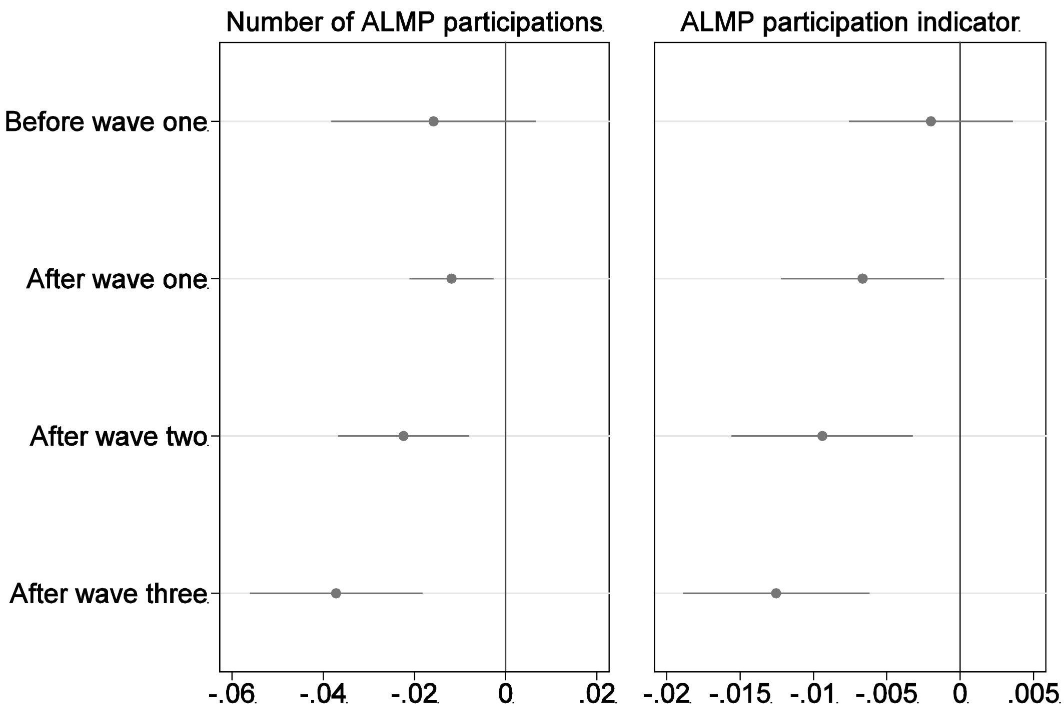
4.2 Treatment effects

Figure 2 shows the results of the intention-to-treat analysis, i.e. the differences in the outcomes between the group assigned to survey participation and the group assigned to control. We include *all* independent variables shown in Figure 1 as covariates in our ITT analysis, for two reasons. First, to ensure that the ITT effect is not biased by the small differences between the group assigned to treatment and the group assigned to control. Second, because including covariates that do not differ between $Z = 1$ and $Z = 0$ can reduce some of the variability in the outcome variable and thereby increase the precision of the estimates (Angrist/Pischke 2009: Ch. 4).

Before we discuss the ITT effects *after* participation in the survey, we first check that there are no significant differences between the selected and unselected cases w.r.t. the two ALMP outcomes measured *prior* to the start of the survey, i.e. we run the falsification test described in Section 3.4. The first row in Figure 2 shows that there are no significant differences in the number of ALMP (left panel) nor for the ALMP participation indicator (right panel). This finding supports the claim that assignment to treatment or control was in fact random, conditional on X (assumption (i)).

After the first wave (the second row of Figure 2), the ITT becomes negative and significant for both outcomes, though small in size. Unemployment benefit recipients selected for the survey participate in about 0.01 ALMP less than the control group (left panel of Figure 2), controlling for X. In addition, take-up of ALMP is less than one percentage point smaller among the selected cases (right panel of Figure 2). Both effects become stronger after waves two and three (rows three and four of Figure 2). These results are evidence that participation in the PASS survey leads to a small decrease in ALMP participation, and the effect becomes stronger over the three waves. However, note that the ITT effects underestimate the average causal effect on the treated, due to noncompliance with the treatment assignment status. Using the LARF estimator described in Section 3, we can get a clearer picture of the actual effect size of (repeated) survey participation.

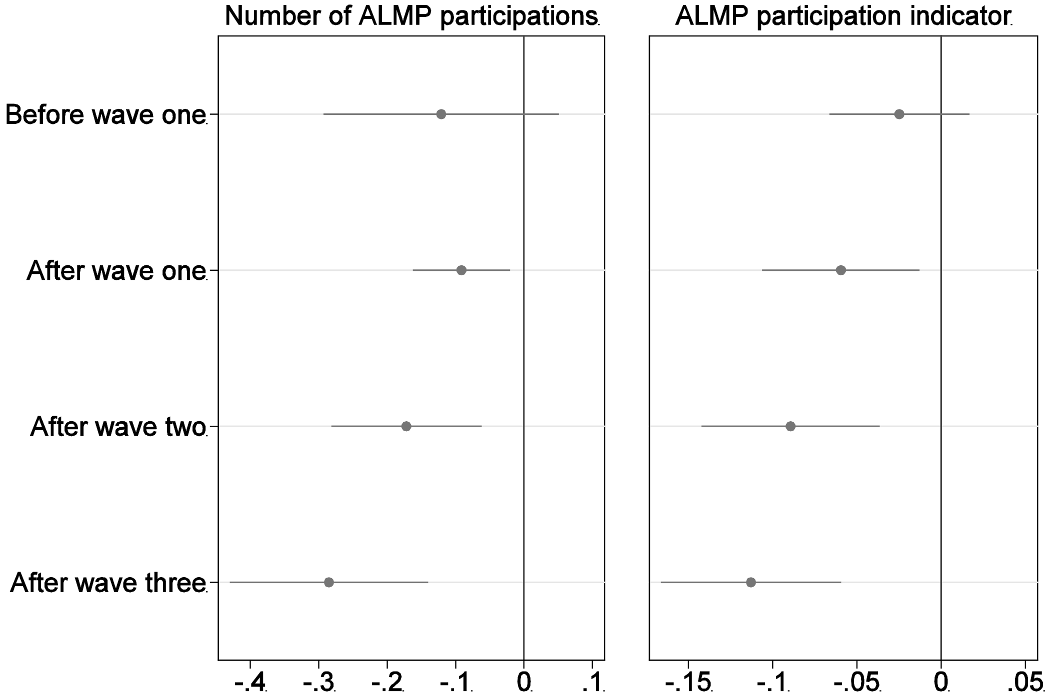
Figure 2
Intention-to-treat analysis controlling for covariates X (see Figure 1). Point estimates with 95% confidence intervals. N=84,300.



Source: Own calculations based on administrative data (IEB) as described in Section 2.2.

Figure 3 shows the main results of our study, the estimates from the IV models. In each of these models, we control for the independent variables shown in Figure 1 in order to justify assumptions (i) and (ii) and to reduce some of the variability in the outcome variable and thereby increase the precision of the estimates (Angrist/Pischke 2009: Ch. 4). Note that once we include covariates in the estimation, the LARF estimator described in Section 3 still equals the ATT under one-sided noncompliance. The ATT, however, is then specific to these covariates.

Figure 3
Local average response function controlling for covariates X (see Figure 1). Point estimates with 95% confidence intervals. N=84,300.



Source: Own calculations based on administrative data (IEB) as described in Section 2.2.

Again, before turning to the outcomes after survey participation, we first look at the take-up of ALMP *before* the start of the survey to assess the model specification with a falsification test. If the model is correctly specified and if the assumptions discussed in Section 3 hold, then there should be no significant effect on ALMP participation prior to treatment. The first row of Figure 3 reports the results of this analysis. We do not find a significant treatment effect for the number of ALMP (left panel) or for the ALMP participation indicator (right panel). These results give us even greater confidence that our IV model is correctly specified and the assumptions discussed in Section 3 are met and thus that we can estimate the causal effect of survey participation on respondents' take-up of ALMP.

Turning to the other rows in Figure 3, the effect of participation in the survey on ALMP take-up after the start of the survey, we find a significant negative causal effect of participation in the first wave of the survey on respondents' take-up of ALMP (second row). Respondents participate in about 0.09 fewer programs (left panel) and participation decreases the probability of taking-up an ALMP by 0.05 percentage points. After participation in Wave 2, the magnitude of both effects increases to about -0.17 programs and -0.08 percentage points, respectively. Participating in Wave 3 leads to a further increase in effect size to about -0.27 and -0.11, respectively.

These results are strong evidence for the presence of panel conditioning effects in PASS. However, the results are unexpected: We hypothesized that repeatedly responding to questions about ALMP should increase participation in ALMP, due to cognitive stimulus. Yet, survey participation seems to *decrease* the number of programs people participate in and to decrease the likelihood to participate in ALMP. Nevertheless, it appears that repeated participation in PASS changes respondents' actual behavior, i.e. respondents are less likely to participate in ALMP, and participate in fewer programs, than similar persons who are not exposed to the survey. Moreover, these effects seem to increase with the number of survey waves, though the differences between the coefficients are not significant except for the increase from wave one to wave three. Taking all the results and the falsification test together, we find strong evidence of changes-in-behavior panel conditioning in the PASS survey, though not in the hypothesized direction.

5 Discussion

In this study, we have regarded selection and participation in the first three waves of a large panel survey on labor market behavior as a treatment and have used techniques of causal analysis to estimate changes in respondent behavior due to panel conditioning as a treatment effect. The results revealed that respondents of the first three waves are less likely to participate in ALMP, and to participate in fewer programs, than a group of eligible but unselected unemployment benefit recipients. Moreover, we found that the effects increase with each additional wave.

Because many people selected for the survey did not respond, our analysis accounts for noncompliance with the treatment assigned. We relied on an instrumental variable approach and instrumented the actual participation in the survey with the random assignment of people to the survey. In addition, we discussed the assumptions necessary to identify the local average response function with this instrument in detail and addressed potential concerns about model misspecification or violated assumptions with a falsification test.

The cognitive stimulus theory offers a plausible explanation as to why panel participation can change respondents' behavior. We hypothesized that asking people repeatedly about a specific behavior works as a stimulus, which increases respondents' awareness and motivates them to take up this behavior. In line with theory, we found that respondents' labor market behavior is changed by (repeated) participation in the survey. Interestingly, each additional exposure to the treatment, i.e. each additional wave, seems to intensify this effect. Yet, in contrast to our hypothesis, respondents participate in fewer measures and are less likely to participate. Moreover, we likely underestimated the effects of *repeated* participation in the survey as the definitions of the treatments after waves two and three also include people who responded in one wave only.

Do these results prove the cognitive stimulus theory wrong? Probably not. This theory still offers a plausible explanation of *why* repeatedly answering the same questions in a panel survey might alter respondents' behavior. Other mechanisms also offer explanations of our effect. ALMP are measures designed to facilitate reintegration of unemployment benefit recipients into the labor market. Yet, their effectiveness is often up for debate (Crépon/van den Berg 2016). Repeatedly answering questions about ALMP may stimulate respondents to reflect on their reintegration strategy into the labor market. This might lead them to turn to other methods to improve their employment situation, instead of taking-up ALMP with questionable benefits. Thus, panel survey participation might decrease ALMP participation. Altogether, more research needs to be done regarding the theoretical mechanisms driving panel conditioning.

Although analyzing additional labor market outcomes such as job search behavior is possible with the data of this study, it would go beyond the scope of this paper. Future work should expand our results to such outcomes for which administrative data are available. If respondents reflect on their strategies for reintegration into the labor market as mentioned above, there is reason to think that participation in a survey like PASS could as well affect the time unemployed persons need to find a new job. We would also like to see our results replicated with other data sets and variables. However, we note that in scenarios where external validation records are not available, clear distinction between the two forms of panel conditioning will be difficult. Unfortunately, in the majority of scenarios, researchers will not have external validation data at hand.

We find strong evidence that panel conditioning not only affects reporting, as suggested by some researchers, but also behavior itself. Our approach disentangles changes-in-behavior from changes-in-reporting. Using the administrative data, which are independent of respondents' reporting of behavior, we can conclude that the effects we found represent actual changes in labor market behavior. In addition, appropriate methods and data are needed to unveil panel conditioning effects and disentangle them from other confounding sources of error. This analysis was possible with the PASS data set because of its connection to a large administrative data set. The administrative data allowed us to address the two challenges to the estimation of panel conditioning effects. Our use of an instrumental variable approach, and thorough discussion of its assumptions, let us control for nonresponse and attrition, which in the past has been a major challenge to the study of panel conditioning. Furthermore, these effects cannot be due to interviewer behavior, mode effects, or bias due to incorrect self-reporting thanks to the administrative data. Yet, we note that incomplete linkage of respondents of the survey to their administrative data poses a potential threat to the validity of our results.

Our results suggest that the PASS recipient sample is no longer representative of all recipients in Germany (at least in terms of ALMP participation), because participation in the survey over the waves has changed respondents' behavior. As a consequence, inference made from PASS data may be biased if it includes ALMP participation either as a dependent or independent variable. For example, assessments using the PASS data of whether ALMP programs help the unemployed find a new job, an important public policy question, may apply only to PASS respondents and not to the larger recipient population, because the respondents have been changed by the survey.

We note that the participation in federal labor market programs is a rather specific form of (labor market) behavior, and we cannot generalize these findings to other behaviors of interest in panel studies. Yet, with our example, we hope to raise researchers' attention to the fact that repeated participation in panel surveys can change respondents' behavior, a fact that is often not acknowledged by researchers working with panel data.

The possibility that panel data such as PASS is biased due to changes-in-behavior and/or changes-in-reporting panel conditioning has been acknowledged for a long time. However, so far, panel conditioning has been primarily studied by researchers from survey methodology or survey research, and the majority of work has been published in corresponding journals. Yet, as panel data and panel methods have become more popular in recent years with social scientists and economists as tools to uncover causal effects, applied researchers need to be aware that such data can come with new sources of error. Other panel-specific sources of error, such as attrition, have been widely acknowledged by researchers and are addressed, for example, by weighting methods or by introducing refreshment samples. Panel conditioning, by contrast, is often ignored. Our results suggest this strategy is unwise, because panel conditioning can have strong effects on substantively important variables.

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Appendix

Official English version of PASS survey questions asking for ALMP participation (PASS, 2006, 2007, 2008).

Wave 1

Employment agencies and [fill in type of authority responsible for administration of unemployment benefit II] have various possibilities to support you in finding a vocational training position or a job. Now we would like to learn about your experiences. Have you since January 2005 participated in a program financed or promoted by the job center ("Arbeitsamt") or [name of authority responsible for unemployment benefit II], which was to improve your prospects for finding a job or a vocational training position like application training measures for instance, or in a program that offered you a job opportunity like a one-euro job for example? Please also think of programs that were of short duration, maybe of a few days only.

Wave 2

Have you participated in at least one of the following programs, measures or courses, which was promoted or financed by the job center ("Arbeitsamt") or [fill in type of authority responsible for administration of unemployment benefit II] since January 2006? Please also consider programs which were only of short duration.

Wave 3

Have you January 2007 participated in at least one of the following programs, measures or courses which was financed or promoted by the job center ("Arbeitsamt") or employment agency? Please also consider programs which were only of short duration.

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