
Perte d'emploi, famille et redistribution

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Résumé

Cette étude quantifie les conséquences de la perte d'emploi sur de multiples dimensions de la structure familiale, du revenu individuel et du ménage, et des trajectoires sur le marché du travail. La croissance économique, qui exige la réallocation des travailleurs entre différents emplois (Aghion and Howitt, 1994) expose les salariés au risque d'être licenciés. Cette préoccupation est à l'origine des divers systèmes de sécurité et d'assurance sociale qui caractérisent les économies modernes. Cependant, ces systèmes d'assurance collective formels ne sont pas le seul canal par lequel les travailleurs peuvent atténuer ce risque. En effet, les familles peuvent offrir une bonne assurance contre ce risque (i) en diluant le risque individuel grâce à la mise en commun des ressources, et donc des chocs individuels de tous les membres du ménage, et (ii) en modifiant leurs allocations de temps et d'effort pour s'adapter à la nouvelle structure de l'avantage comparatif au sein de la famille. Ce deuxième mécanisme est généralement appelé l'effet de travailleur supplémentaire (Lundberg, 1985).

Cependant, si pour le salarié licencié, les mécanismes d'assurance familiale augmentent la valeur de la vie dans une famille lorsqu'il est licencié, l'inverse est vrai pour le partenaire non licencié. Ces effets négatifs peuvent encore être renforcés si les couples subissent un coût psychologique lorsque la nouvelle répartition du temps et des efforts est perçue comme contraire aux normes traditionnelles de genre. Compte tenu des rôles habituels des hommes et des femmes dans les pays occidentaux, cela peut être particulièrement le cas lorsque le déplacement d'emploi frappe les hommes.

En se basant sur la méthodologie développée par Jacobson, LaLonde, and Sullivan (1993) et étendue par Halla, Schmieder, and Weber (2020), nous construisons un échantillon de travailleurs licenciés qui sont affectés par une perte d'emploi vraisemblablement exogène, et nous

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les comparons à des travailleurs très similaires qui ne sont pas affectés par un tel choc. Ces épisodes de licenciement sont repérés à partir des flux de sortie des établissements dans les Déclarations Annuelles de Données Sociales (DADS) exhaustives. Salariés licenciés et non-licenciés sont ensuite comparés à partir de l'ensemble des informations chaînées dans l'Échantillon Démographique Permanent (EDP), en particulier bulletins de naissance et données socio-fiscales. Du point de vue méthodologique, nous développons un cadre permettant de corriger le biais à la baisse sur l'effet du licenciement sur les revenus et l'offre de travail du conjoint, qui résulte de ce que l'EDP ne permet d'observer que la trajectoire professionnelle d'un seul des deux conjoints.

Le licenciement n'a pratiquement aucun effet sur la structure familiale : la probabilité de vivre dans une relation hétérosexuelle cohabitante ne change pas en conséquence de la perte d'emploi, et les décisions des travailleurs en matière de fécondité ne semblent pas réagir au choc. Nous ne trouvons pas de preuve d'un effet de travailleur supplémentaire : les revenus et l'offre de travail des conjoints des travailleurs licenciés n'augmentent pas en réponse à la baisse soudaine des revenus de leurs partenaires. En d'autres termes, si les familles fournissent une assurance contre le risque de revenu qui résulte du licenciement, c'est uniquement par la mise en commun des ressources plutôt que par des ajustements collectifs dans l'allocation du temps et des efforts. Enfin, alors que l'assurance chômage semble assurer les travailleurs de manière assez efficace contre le choc d'emploi de courte durée, elle a beaucoup moins d'importance à long terme lorsque la plupart des travailleurs ont réussi à trouver un nouvel emploi. Les travailleurs semblent partiellement assurés grâce à un complexe qui comprend le travail indépendant, les marchés de capitaux et la progressivité du système d'impôts et de transferts. Cependant, l'ampleur du choc à long terme, même après la prise en compte de ces ressources, reste considérable.

Abstract

We leverage French longitudinal data issued from multiple administrative registers to investigate how job loss affects couple and family structure, spouses' labor supply and lastly all components that combine into household's disposable income. Our difference-in-difference estimates imply close to no effect of these large income and employment shocks on couple formation and dissolution, and fertility decisions. Spouses do not seem to adjust their labor supply in response to their partners' job loss. In the short run, unemployment insurance divides the magnitude of the income shock by a factor 2 to 3. By contrast, it provides very little insurance against the permanent component of the shock against which households are partially insured at best. These results hold regardless of the gender of the laid-off worker.

1 Introduction

The reallocation of workers across jobs, especially due to innovation-related productivity shocks, has been emphasized as a necessary condition for economic growth (Aghion and Howitt, 1994). However, the collective benefits of the reallocation may be deemed unacceptable if this reallocation results in a costly externality for the restricted subset of workers who are actually hit by job displacement. This concern, either conscious or not, serves as the basis for the various safety nets and social insurance systems that characterize modern economies.

On top of these collectively organized insurance systems, workers may also mitigate the related income risk at a more micro level, thanks to their own families. Indeed, as long as the within family correlation in a earnings risk remains small, families may offer good insurance against individual idiosyncratic risk (i) by diluting the individual risk thanks to the pooling of resources, and thus of all household members' individual shocks, and (ii) by limiting the welfare implications of the said risk through the within-family reallocation of time and effort based on the new structure of within-family comparative advantage (Becker, 1981) that emerges as a

consequence of a shock on the return on time spent on the labor market by one family member. This mechanism is generally coined as the added worker effect (Lundberg, 1985), and the optimal taxation of couples depends on whether or not this channel is active (Kleven, Kreiner, and Saez, 2009).

Even though job displacement emphasizes the insurance role played by families, its effect on family structure, especially in the context of heterosexual couples, remains theoretically ambiguous. Indeed, while for the displaced worker, due to the family insurance mechanisms, the value of belonging to a family increases due to job displacement, the opposite is true for the non-displaced partner. Moreover, these adverse effects may be further enhanced if couples incur a psychological cost when the new allocation of time and effort is perceived to conflict traditional gender norms (Akerlof and Kranton, 2000). Given the usual gender roles in Western countries, this might particularly be the case when job displacement hit men, as it may affect their status as breadwinners.

In this paper, we investigate this issue in the French context, taking advantage of a combination of administrative records that span over multiple dimensions of family structure, individual and household income, and labor market trajectories. Building on the methodology inaugurated by Jacobson, LaLonde, and Sullivan (1993) and extended by Halla, Schmieder, and Weber (2020), we elaborate a sample of displaced workers which are affected by a plausibly exogenous job separation, and compare them to very similar workers who are not affected by such a shock to investigate (i) whether job displacement disrupts couples and families, or decreases the odd of single workers finding a new partner; (ii) whether spouses provide insurance against the related income risk by increasing their labor supply in response to job displacement; and (iii) how well families, safety nets and capital markets insure workers against the income risk that results from job displacement.

We find that job displacement has close to no effect on average on family structure: the probability of living in a cohabiting heterosexual relationship does not change as a consequence of job separation, and workers' fertility decisions do not seem to react to the shock. We do not find evidence of an added worker effect: displaced workers' spouses' earnings and labor supply do not increase in response to the sudden decrease in their partners' earnings. In other words, if families provide insurance against the income risk that results from job displacement, it is only through the pooling of resources rather than from collective adjustments in the allocation of time and effort. Lastly, whereas unemployment insurance seems to insure workers quite effectively against the short-lived employment shock, it matters much less in the long run when most workers' have managed to find a new job. Workers seem partially insured at best against the long-run income loss, through a bundle that involves self-employment, capital markets and the progressivity of the taxes and transfers system. Even so, the magnitude of the long-run shock even after these resources are taken into account remains sizable.

These results contribute to three related strands of the literature. First, several papers have investigated the impact of job displacement on family structure, for instance in terms of the divorce probability (Charles and Stephens, 2004; Eliason, 2012) or in terms of fertility decisions (Huttunen and Kellokumpu, 2016). With respect to the evidence on divorce, our investigation is slightly broader because we focus both on the probability of maintaining a cohabiting heterosexual relationship after job displacement for already formed couples and on the probability of finding a mate for single workers, i.e. both on couple dissolution and couple formation. Consistent with the American evidence, but in contrast with the Swedish case, we do not find that job displacement induced by downsizing and plant closure affects this probability. This evidence also diverges from multi-country investigations that suggest that at the individual level, male job loss correlates with higher rates of divorce (Solaz et al., 2020), and recent results on the fall of the marriage value of young men as a result of their falling relative earnings, which are identified from more macro shocks (Autor, Dorn, and Hanson, 2019).

We also find close to no effect of job displacement on fertility decisions, with the exception

of a slight decrease in men’s fertility, which contrasts the Finnish evidence. Because they are informative about the effect of income shocks on fertility decisions conditional on workers’ time-constant heterogeneity, these null effects also speak to methodological issues regarding the effect of children on labor supply and labor earnings. Specifically, they suggest that differences in the timing of children’s birth are unlikely to stem from unobserved shocks on parents’ earnings, so that event-study and difference-in-difference approach yield credible results (Kleven, Landais, and Sogaard, 2019).

Second, we contribute to the literature that has investigated within-family time and effort reallocation in response to individual income shock, which has long focused on the measurement of the added worker effect, i.e. married women’s tendency to increase their labor supply when their spouse is hit by a negative income shock (Lundberg, 1985). Recent contributions include Halla, Schmieder, and Weber (2020) who investigate married woman’s labor supply decisions in response to their husbands’ job displacement in Austria; they find evidence of a small added worker effect, which amounts to a 1 percentage point increase in the employment rate. While our estimates are not significantly different from 0, they are compatible with such tiny increases, even in a context in which the baseline female employment rate is much higher.¹ With respect to this paper, we extend the methodology to investigate men’s response to their female partners’ job displacement, and find similar results. By contrast, other recent studies, more structural on their approach, tend to find sizable spousal labor supply responses to individual wage shocks (Blundell, Pistaferri, and Saporta-Eksten, 2016). More broadly, our results suggest that couples react very little to shocks that affect the within-family comparative advantage (Becker, 1981): otherwise, job displacement that decreases the returns on time and effort spent by one family member on the labor market, keeping her home-production productivity constant, should induce the reallocation of time and effort of her spouse towards the labor market.

A key element to explain these differences in family insurance mechanisms may be related to the collective insurance offered both by unemployment insurance and the progressive taxes and transfers system, if the latter crowd out the former (Cullen and Gruber, 2000; Autor et al., 2019). Our third contribution thus relates to the role played by these safety nets. Indeed, we show that in the short run, unemployment benefits provide quite effective insurance against the income shock that results from job displacement: its magnitude would be two to three times larger absent unemployment insurance. Yet unemployment insurance offers much less coverage against the long-run component of the shock, which is not driven by employment. Workers seem partially insured at best against the long-run component of the shock, through a bundle that involves not only self-employment and capital markets, but also the progressivity of the taxes and transfers system, which is consistent with recent results based on Canadian data (Stepner, 2019).

The remainder of the paper is organized as follows. Next section presents the data and describes the constitution of our sample of displaced and control workers. Section 3 details our empirical framework. Section 4 presents the results and Section 5 concludes.

2 Data

Our analysis relies on two French administrative datasets. Firstly, we rely on the *Déclaration Annuelles de Données Sociales* (DADS), which consist in comprehensive payroll tax registers that cover the universe of French salaried employees, to identify sharp decreases in plant size that we interpret as mass layoffs.² Secondly, we use these mass layoffs to identify the conse-

¹The employment rate for male displaced workers’ spouses is about 46% before job displacement in Austria, against more than 80% in the French setting.

²Mass layoffs are strictly regulated under French law. In Appendix A, we detail the most salient features of this regulation. In practice and due to data limitations, our empirical approach may depart from the precise, legal definition of a mass layoff.

quences of exogenous job separations in the *Échantillon Démographique Permanent* (permanent demographic sample, EDP), a combination of administrative registers that links payroll tax data with tax returns and birth registers, thanks to a common identifier based on a Social security number.³

2.1 Payroll tax registers

Our labor market data are drawn from the *Déclarations Annuelles de Données Sociales* (DADS). By law,⁴ French employers have to fill in a DADS form for every employee subject to payroll taxes. The form contains detailed information about days paid, hours paid, occupation, industry, gross and net wages, other job characteristics (beginning, duration and end of a period of employment and part-time employment), employer characteristics (size and location) and individual characteristics (age, gender and municipality of residence). These forms are gathered in a linked employer-employee database: plants are identified by the Siret, a 14-digits plant identifier, and individuals are identified by a anonymized identifier based on the NIR, a Social security number.⁵

2.2 Permanent Demographic Sample

The *Échantillon Démographique Permanent* (permanent demographic sample, EDP) gathers administrative information from birth registers, tax returns, firm records and from the file of welfare benefits recipients, for a representative sample of the French population including all individuals born one of the first four days of April, July, October or between January 2nd and 5th. The EDP follows the professional, financial, residential and family situation of these "EDP individuals" each year between 2010 and 2016. On top of this core sample, for each year, the EDP features information on each person listed in the same dwelling.⁶

Key to our empirical approach is the fact that the EDP data include an extract of the DADS data. This allows us to link these data to the comprehensive payroll tax data, and specifically to determine treatment and control groups based on plant-level in- and outflows that can be observed in the comprehensive DADS registers.⁷ We then consider the consequences of these exogenous employment shocks in terms of family structure and income, based on income tax returns and birth registers that form part of the EDP data.

2.2.1 Tax returns

Household structure The EDP data provide information based on income tax returns. This information is available at (i) the individual level for some income sources, especially labor earnings or unemployment benefits, and (ii) at the household level for all other income sources, including capital income or social benefits. In other words, the structure of the data assumes perfect pooling of capital income within the household.

³In practice, tax returns are linked with other datasets based on variables with high identifying power (name, surname, gender, date of birth, birth location).

⁴The absence of DADS as well as incorrect or missing answers are punished with fines.

⁵In practice, this anonymized identifier can only be used to track individuals over two consecutive year of the data.

⁶Precisely, information is available for each member of a tax household listed in this dwelling, including in particular young adults living apart from their parents but still registered on their income tax return and excluding people registered in another tax household (for instance a student from another family who is renting a room there). Individuals living in collective housing and homeless people are excluded from the EDP.

⁷In practice, the plant-level identifier is not available in the extract of the DADS that is included in the EDP data. Instead, we link the two datasets based on individual and job-level variables with high identifying power. The accuracy of this statistical matching approach is over 98%.

This household level is not defined by fiscal law: it is a statistical concept which the definition is managed by Insee during the recollection of tax returns data. The definition of this household level is based on the pairing of income tax returns and housing tax returns. In the context of this particular paper, this approach is highly desirable because it allows us to treat cohabiting and married couples the exact same way, even though the former fill in separate income tax returns while the latter fill in joint income tax returns as married couples are taxed jointly. This is particularly important in a context in which marriage is increasingly delayed or abandoned in favor of civil unions and cohabiting relationships: in 2014, slightly more than one cohabiting heterosexual couples in three had married during the first 5 years of co-residency ([Costemalle, 2015](#)).

On top of delineating households, Insee provides variables that describe the type of household at stake. These variables are based on (i) the family information that appears in the tax returns, and (ii) the age and gender of the members of the household. Indeed, especially in the case of unmarried couples, tax returns alone are not sufficient to assess the type of relationship that may exist between cohabiting adults. Specifically, two different-sex cohabiting adults are considered to be in a heterosexual cohabiting relationship if either (i) they appear as married, have signed a civil union contract or declared themselves to be a couple in their housing tax return; or (ii) they live together and have an age difference of 15 years or less. Based on age differences, Insee then assesses whether other inhabitants of the household are likely to be children of a couple or of one of the older inhabitants; this is relatively easy in the case of minor children who appear as such on income tax returns, since they allow their parents to benefit from a tax rebate, but can get complicated for older children. For this reason, and also due to a left-censoring issue, we base ourselves on birth registers when investigating fertility decisions.

In the end, we rely on this imputed information in two ways. Firstly, all our estimates are conditional on household type as measured before job displacement. This household type is a categorical variable with 5 categories: single individuals, childless couples, single-parent families, couples with children and lastly a remaining category which gathers all other situations. Secondly, we create a dummy variable that equals 1 if a displaced (control) worker is either the main registrant of the household or the spouse of the main registrant of the household, and 0 otherwise, which allows us to observe couple formation and dissolution in response to job displacement. This approach is unsuited for same-sex couples, which is why, on top of sample size considerations, we restrict ourselves to the study of heterosexual relationships.

Income variables The income variables extracted from the income tax returns are available at both the individual and the household level. Our analysis relies on both levels. Firstly, we consider the individual income data related to displaced workers or their spouses. This individual income sums wage income,⁸ unemployment benefits, self-employed earnings, pensions and alimonies. Throughout the paper, we aggregate the last three components together. Our sample excludes workers that would be eligible for a retirement pension after job loss. Additionally, the payment of alimonies is triggered by couple dissolution, that we do not find to increase in response to job displacement. As a result, the effect of job displacement on this component corresponds to adjustments through self-employment. These income variables mix up all margins of labor supply, i.e. whether to work or not, but also how many hours, for which kind of employer, the level of effort etc. Most of these margins cannot be disentangled with the data. However, we can get a sense of the extensive margin of employment by considering three dummy variables: (i) having positive wage income (salaried employment); (ii) having positive

⁸The wage income concept in terms of the income tax differs from the wage income concept in terms of the payroll tax. For instance, sick leave and maternity leave benefits are part of the wage income in terms of the income tax, but are excluded in terms of the payroll tax. These differences may to some extent explain differences between our wage income estimates and those of other recent papers that rely on the DADS data, such as [Brandily, Hémet, and Malgouyres \(2020\)](#).

wage income or positive self-employed earnings (employment); (iii) having positive wage income or positive self-employed earnings or positive unemployment benefits (participation).

Secondly, we rely on the household-level income data. These data allow us to consider additional margins of adjustment that cannot be observed in the individual data. Specifically, our focus is on household disposable income, which equals the sum of all individual incomes described *supra* plus capital income (excluding capital gains) and family and welfare benefits, less housing, property and income taxes. This allows us to consider not only the role played by unemployment benefits in providing insurance against these large income and employment shocks, but also whether households are able to effectively insure themselves thanks to the capital market – which would show up in the capital income component – and the insurance value of the progressive taxes and transfers system.

The income data is sometimes missing: empirically, for each year of the data about 8% of displaced and control workers cannot be observed in the income tax returns. In practice, our estimates are conditional on being observed in the income tax returns data, but not necessarily continuously so. In Appendix E, we assess the robustness of our results with respect to this choice by (i) displaying estimates of the effect of job displacement on the probability of being present in the income tax returns data; and (ii) by replicating our analysis on a sample of workers who are continuously observed in the data. The sample inclusion rate decreases very little due to job displacement. However, our results turn out to be immune to this issue.

Our data include negative incomes, as self-employed workers are allowed to report losses in their income tax returns. These negative incomes are quite uncommon; however, we choose to drop observations related to households for which negative incomes are observed even once. Our approach investigates income measured in levels, as opposed for instance to the logarithm of income. This allows us to include individuals or households whose earnings equal 0. However, this could also make our estimates less robust with respects to outliers in the right tail. For this reason, we winsorize all earnings and income variables at the 99th percentile level.

2.2.2 Birth registers

Our analysis also relies on birth registers which form part of the EDP data. Births are registered by an individual who was present at the time of birth, usually the father, but in some cases a doctor or a midwife. These registers allow us to follow the fertility decisions of EDP individuals, since they cover children of EDP individuals. Specifically, children born to EDP individuals may be tracked from 1968 when their EDP parents are born on October, and 2004 for the others. This creates a left-censoring pattern in the data. For this reason, we focus on the number of children born each year around the employment shock, as opposed to the overall number of children born to EDP parents,⁹ which would incorporate unobserved past fertility decisions.

Birth registers in the EDP incorporate three distinct types of events: usual childbirths, still births and adoptions. For the sake of this particular paper, we choose to consider the first two types of events, that will capture (potential) parents' decision to have children, regardless of the outcome. The criteria according to which still births are registered varied over time. Since 2008, still births are registered on the basis of a medical certificate of childbirth (delivery). However, the still birth rate is sufficiently low for this particular choice not to affect our results.¹⁰

2.3 Job displacement identification and sample construction

We identify exogenous shocks on the employment relationship of an individual based on in- and outflows measured at the plant-level in the DADS data. Let T denote a year between 2011 and 2017. We begin by selecting private-sector plants (i) with more than 10 workers on January, 1st

⁹The former being the derivative of the latter.

¹⁰The still birth rate is about 1% since this law change.

$T - 1$; (ii) of which at least 25% of workers present on January, 1st $T - 1$ left the plant during year $T - 1$; and (iii) of which the number of workers decreased by at least 25% between January, 1st $T - 1$ and December, 31st T . Condition (iii) ensures that we are focusing on large decreases in the workforce of a particular plant, as opposed to plants in which the turn-over is extensive. To ensure that these events do indeed correspond to mass layoffs, as opposed to mere changes in plant-identifier or a spin-off, we then track workers who left the plant during year $T - 1$ and recover their employers during year T . We impose that no more than 25% of the workers who left the plant work in the same plant during year T ; this kind of restriction can be found for instance in [Gathmann, Helm, and Schönberg \(2020\)](#). Lastly, when a plant matches these criteria for multiple years, we only keep the first event.

Armed with this collection of plant identifiers affected by mass layoffs, we turn to the individual-level DADS data that form part of the EDP sample. We consider an individual to be affected by a mass layoff if (i) she works in an affected plant and leaves it at some point between one year before and one year after the plant-level event, (ii) her tenure within that plant by the time she leaves is at least 3 years and (iii) she is between 25 and 50 years old by the time she leaves the plant. Condition (ii) ensures that we do not consider temporary workers for which job separation cannot be regarded as a shock; it further implies that the separations upon which we focus do not correspond to young workers' gradual entrance into the workforce, that usually involves multiple job transitions ([Topel and Ward, 1992](#)). Condition (iii) ensures that we are not dealing with individuals for which retirement is an option, so as to simplify the interpretation of our results.

Our empirical analysis requires that we compare these *displaced* workers with a set of *control* workers to quantify the impact of these employment shocks. To determine the set of control workers, we consider workers (i) who work in the same detailed (5-digits) industry and meet the same tenure requirement in their own plant at the time of the (counterfactual) job separation and (ii) have never worked in one of the affected plants. A similar individual may appear as a relevant control for multiple events; in such a case, we sample the timing of her counterfactual shock among all those for which she appears as a possible control with a uniform probability. To ensure that we have sufficiently large sample sizes and to support the assumptions upon which our empirical analysis rests, we drop detailed industry \times timing of the shock cells (i) that contain 5 controls individuals or less, or (ii) in which more than half of the individuals are treated. Condition (i) sustains the credibility of the common support assumption that motivates our reweighting approach. Condition (ii) rules out industry-level shocks: affected plants cannot gather the vast majority of the workforce in their industry. This makes it less likely that other plants of the same industry are affected by their hiring and laying-off decisions. It also makes it more plausible that control workers' job opportunities are left unaffected by the shock, which is crucial for the implicit Stable Unit Value Treatment Assumption upon which our empirical framework rests.

Due to data limitations and the fact that we allow for some anticipation in the effect of job displacement, we only focus on individuals that are affected by a (counterfactual) shock between 2012 and 2016. This leaves us with over 315,000 individuals who we observe between 2010 and 2016 in the income tax returns data, and between 2004 and 2016 in the birth registers.

2.4 Summary statistics

Figure 1 displays the distribution of the sizes of the affected plants, compared to that from which control workers originate. These sizes are measured as the number of workers on January, 1st, one year before the (counterfactual) plant-level event, and the distribution are weighted by the number of workers observed in our sample. Many workers in our sample originate from relatively small plants. However, quite reassuringly, this is also the case in our control group. This suggests that our delineation of the displaced group based on observed plant-level outflows

does not lead us to focus on spurious shocks that mostly stem from small sample sizes.

Figure 2 displays the distribution of the intensity of the plant-level shock in our displaced group. This intensity is measured by one minus the size of the plant on December, 31st one year after the shock, relative to this size on January, 1st almost two years before. Strikingly, over half of our displaced worker group were affected plant closure events.

Table 1 compares displaced and control workers, separately by gender, in terms of their ages, their occupation before the (counterfactual) shock and their family structure before the said shock. Regardless of gender and job displacement exposure, the average age in our sample is about 37. Even though there are some small differences in terms of occupation or family structure, the differences between displaced and control workers with respect to these variables, as measured two years before the (counterfactual) shock remain limited. Most workers live with a partner: two years before they were displaced, over 7 out of 10 displaced workers lived with a partner, almost always with a different-sex partner. The proportion is very close when it comes to our control group of non-displaced workers. Similarly, over 60% of displaced workers lived with children, and the same goes on for control workers.

Table 2 compares workers in terms of their labor earnings as observed in the payroll tax data, as well as their hours and days worked, to get a sense of their labor force attachment. All these variables are collected two years before the (counterfactual) job loss, so as to represent the baseline levels from which workers will evolve due to job separation. Control and displaced workers appear quite comparable along these dimensions. Prior to job displacement, workers earned about €20,000 for women and €25,000 for men. Their days worked (which include paid vacations and week-ends) amount to 340 days a year, which suggest a substantial labor force attachment, since in the DADS data an entire year of employment corresponds to 360 day. Lastly, when (i) dividing by the average days worked and (ii) comparing it to the baseline level for a full-time job in the DADS data (1,820 hours a year), the average hours worked imply that women work about 90% of a full-time equivalent in average, whereas men are almost always on a full-time basis.

We then compare displaced and control workers two years before the shock in terms of their income, as measured in the income tax returns data. We begin by workers' own individual earnings, as displayed in Table 3. Differences between displaced and control workers remain limited in that matter. Before displacement, displaced workers earn about €22,000 for women and €27,000 for men. These earnings mostly result from wage income, as opposed to other sources of income. Wage income observed in the income tax returns is slightly higher than wage income as observed in the payroll tax data (see Table 2); this is mostly due to the fact that the latter is more restrictive, whereas the former includes sources of earnings that are not considered as wages in terms of the payroll tax, e.g. sick leave benefits or maternal leave benefits. We check that consistent with our framework, almost every worker is observed in employment in the income tax returns data: the employment rate is very close to 100%, which speaks in favor of the consistency of income tax returns data with payroll tax data filled by employers.

We replicate this exercise, this time considering displaced and control workers' spouses' earnings before the (counterfactual) shock, thus restricting to the 70% of workers who live with a partner before the said job separation. Table 4 displays our results. Here again, differences between displaced and control workers are not very large. Consistent with typical gender inequality, displaced women's male partners earn much more than displaced men's female partners. Here again, earnings are driven by wage income as opposed to other sources of earnings. The participation and employment rates for women's male partners are 95%, whereas they are between 80% and 85% for men's female partners. These rates match quite closely the participation rate for workers aged 25 to 49 in France during the relevant time period. Due to the gender gap in participation and employment, there is less room for positive labor supply decisions at the extensive margin for men's female partner than for women's male partners, which induces most papers devoted to the added worker effect to focus on displaced male workers' female spouses.

Lastly, we consider income at the household level, as measured two years before the (counterfactual) job separation. Table 5 displays our comparison. Here again, differences across treatment groups are quite small. The average household disposable income ranges between €45,000 and €47,000 before job separation. This disposable income mostly results from wage income: other sources of earned and non-earned income matter much less. The average household pays between €7,000 and €8,000 a year in taxes (which sums income, housing and property taxes). We then consider the position of households in the equivalent income distribution, that is the distribution of disposable income divided by the number of consumption units in the household. Our sample overrepresents the middle of the distribution, and underrepresents the bottom of the distribution.

3 Empirical analysis

Our empirical analysis leverages differences in the evolution of family structure and labor outcomes of treated and control over time, after conditioning on relevant observables. To this end, we rely on a reweighted difference-in-difference approach developed by Abadie (2005). This section details this framework, and presents a way to quantify the implications of a violation of the parallel trends assumption upon which it relies when it comes to spousal labor supply.

3.1 Identifying the consequences of job displacement

Let $Y_{i,t}$ denote a variable that describes individual i 's family structure or income at time t , which is measured relative to the year during which her (counterfactual) employment shock occurs. Let D_i be a dummy variable that equals 1 if individual i belongs to the set of displaced workers, and 0 otherwise, in which case she belongs to the control group. Let C_i denote her cohort, which corresponds to the year during which her (counterfactual) job displacement takes place. Lastly, let X_i be a vector of time-constant observable variables, which includes: the industry in which worker i worked before the (counterfactual) employment shock, her 1-digit occupational group, her labor market experience and her tenure within the firm by the time she is displaced, her year of birth, and her family structure and the income quintile to which she belongs two years before the shock. In all what follows, all expectations and probabilities are taken conditional on gender; we keep this conditioning implicit throughout this subsection.

We define $Y_{i,t}(d)$ to be the potential outcome of individual i at time t , depending on whether she has ($d = 1$) or she has not ($d = 0$) been displaced. We are interested in the causal effect of job displacement on Y , i.e. (functionals of) the distribution $Y_{i,t}(1) - Y_{i,t}(0)$. Specifically, we consider the cohort-specific average treatment effect on the treated:

$$CATT(c, t) = \mathbb{E}[Y_{i,t}(1) - Y_{i,t}(0) \mid C_i = c, D_i = 1] \quad (1)$$

Our identification strategy is based on three assumptions:

Assumption 1 (Common support). *For all c , for all x :*

$$0 < \mathbb{P}(D_i = 1 \mid C_i = c, X_i = x) < 1 \quad (2)$$

Assumption 2 (Parallel trends in baseline outcome). *For all c , for all x , for all t, t' :*

$$\begin{aligned} & \mathbb{E}[Y_{i,t'}(0) - Y_{i,t}(0) \mid C_i = c, X_i = x, D_i = 1] \\ &= \mathbb{E}[Y_{i,t'}(0) - Y_{i,t}(0) \mid C_i = c, X_i = x, D_i = 0] \end{aligned} \quad (3)$$

Assumption 3 (Limited anticipation). *For all c , for all x , for all t , if $t < -1$, then:*

$$\mathbb{E}[Y_{i,t}(1) - Y_{i,t}(0) \mid C_i = c, X_i = x, D_i = 1] = 0 \quad (4)$$

Assumption 1 states that every worker affected by an employment shock has at least one non-affected surrogate worker with the exact same observable characteristics. Assumption 2 states that absent the employment shock, affected workers' average outcomes would have evolved the same as their non-affected surrogates' average outcomes. Lastly, Assumption 3 states that the average effect of job displacement equals 0 up until two years before the shock. The effect is not constrained to 0 one year before the actual job separation, so as to allow workers and household to anticipate these shocks that are not perfectly unforeseeable.

Under these assumptions, provided that individuals can be observed sufficiently long after job displacement, cohort-specific ATTs can be identified from the data:

Proposition 1 (Difference-in-difference estimand). *Let $\{\underline{T}, \underline{T} + 1, \dots, \bar{T} - 1, \bar{T}\}$ denote the set of years that can be observed in the data. For c and t such that $\underline{T} + 1 < c < \bar{T} + 1$ and $\underline{T} - 1 < c + t < \bar{T} + 1$, $CATT(c, t)$ can be identified from the data and:*

$$\begin{aligned} CATT(c, t) &= \mathbb{E}[Y_{i,t} \mid C_i = c, D_i = 1] \\ &\quad - \mathbb{E}[Y_{i,c-2} \mid C_i = c, D_i = 1] \\ &\quad - \mathbb{E}[\pi(c, X_i)Y_{i,t} \mid C_i = c, D_1 = 0] \\ &\quad + \mathbb{E}[\pi(c, X_i)Y_{i,c-2} \mid C_i = c, D_1 = 0] \end{aligned} \quad (5)$$

where $\pi(c, x) = \frac{\mathbb{P}(D_i=1 \mid C_i=c, X_i=x)}{1 - \mathbb{P}(D_i=1 \mid C_i=c, X_i=x)} \frac{1 - \mathbb{P}(D_i=1 \mid C_i=c)}{\mathbb{P}(D_i=1 \mid C_i=c)}$

In other words, it is possible to reweight the data so that the distribution of covariates in the control group matches that of the treated group. Because the parallel trends holds conditional on these covariates, as long as both the pre-treatment period and the post-treatment period are observed, it is possible to implement usual difference-in-difference techniques on the reweighted data. Our summary statistics show that displaced and control workers are very similar before the shock: our reweighted difference-in-difference approach makes them even more comparable so as to achieve a credible identification of the causal effect of job displacement.

Proposition 1 allows us to identify cohort-specific ATTs. Our aim is to aggregate them across cohorts to gain insights into the dynamic effects of job displacement. To do so, for each t , let $\mathcal{C}(t)$ denote the subset of cohorts for which $CATT(c, t)$ can be identified from the data.¹¹ We consider the aggregate average treatment effect on the treated:

$$\Delta(t) = \mathbb{E}[Y_{i,t}(1) - Y_{i,t}(0) \mid D_i = 1, C_i \in \mathcal{C}(t)] \quad (6)$$

This quantity represents the average difference between the realized outcomes and the counterfactual situation in which workers would not have been displaced, t years after the job separation took place, for workers affected by shocks of which the consequences can be inferred from the data. Because the data only cover a restricted time-period, this set of workers varies depending on t . By the law of iterated expectations:

$$\Delta(t) = \sum_{c \in \mathcal{C}(t)} \mathbb{P}(C_i = c \mid c \in \mathcal{C}(t)) CATT(c, t) \quad (7)$$

By Proposition 1, as long as $\mathcal{C}(t)$ is non-empty, it is therefore possible to express $\Delta(t)$ in terms of quantities that are all identified from the data. The non-emptiness condition is equivalent to $t \in \{\underline{T} - \bar{T}, \underline{T} - \bar{T} + 1, \dots, \bar{T} - \underline{T} - 3, \bar{T} - \underline{T} - 2\}$. With $\underline{T} = 2010$ and $\bar{T} = 2016$ we are therefore able to cover dynamic effects that span from six years before job displacement to four years after.

Combined with Proposition 1, Equation 7 suggests a very simple choice of plug-in estimator, in which we substitute expectations and probabilities with their empirical analogues. In practice,

¹¹Specifically, $\mathcal{C}(t) = \{\underline{T} + 2, \underline{T} + 3, \dots, \bar{T} - 1, \bar{T}\} \cap \{\underline{T} - t, \underline{T} - t + 1, \dots, \bar{T} - t - 1, \bar{T} - t\}$

we base our reweighting on conditional probabilities drawn from a probit model estimated by maximum likelihood. Under standard regularity and integrability assumption, this estimator is asymptotically normal. To conduct inference, we rely on a bootstrap approach, clustered at the level of the plant at which workers were working immediately before the (counterfactual) employment shock hit them. This choice is based on the premise that this is the level at which shocks are assigned (Abadie et al., 2017), given that we rely on in- and outflows measured at the plant-level to identify these shocks.

3.2 Quantifying the discouraged worker bias

A pitfall of our approach when it comes to spousal labor supply decisions is that couples may be affected by positively correlated shocks that can lead to violations of our conditional parallel trends assumption. Specifically, the EDP sampling plan is defined *at the individual level*. Consistently, this is also the level at which we determine our treated and control groups. If individuals are affected by both shocks to their own employment status, and shocks to their spouse’s employment status, and if these shocks are positively correlated, the problem is thus that, for individuals of which we do not observe the employment status in the DADS data, we are likely to interpret as the consequences of a shock to the employment status of their spouse what actually results from shocks to their own employment status. This positive correlation between employment shocks of spouses has been referred to as the *discouraged worker effect*, as opposed to the added worker effect. It is especially pronounced in settings in which spouses actually evolve in the same labor market, which is plausible for instance given that 10% to 20% of couples meet at work (Bozon and Rault, 2012; Rosenfeld and Thomas, 2012), and would result in downward bias in our estimation of the effect of the labor supply effect of a shock on one’s spouse employment status. In other words, we would conflate changes in spousal labor supply with changes in the labor demand for the work of displaced workers’ spouses.

To quantify the bias that results from this violation, in Appendix B, we develop a simple model of within-couple correlated employment shocks that allows us to approximate the consequences of this departure from our identifying assumptions. Under additional assumptions that (i) the effect a shock on a worker’s employment status on her labor outcomes dominates the effect of a shock on her spouse’s employment status; (ii) the within couple correlation of employment shocks remains limited; (iii) the direct effects of a shock on one’s own employment status do not depend on whether one’s spouse is affected by a shock or not; (iv) the only channel that generates the within couple correlation in employment shock is that spouses work in the same firm (detailed industry); and (v) the probability of receiving an employment shock does not depend on whether or not one works in the same firm (detailed industry) as her partner, we show that the bias in women’s (men’s) reaction to a shock on their male (female) partner’s employment status is well approximated by the direct effect of a shock on women’s employment status on women’s outcomes, multiplied by the share of couples that work in the same firm (detailed industry). The first quantity is a direct byproduct of our identification strategy. A reasonable estimate of the second term can be obtained from the French Labor Force Survey.

4 Results

4.1 Couple formation and dissolution

Figure 3 displays our estimates of changes in displaced workers’ probability of living in a heterosexual cohabiting relationship, relative to the (reweighted) control group, over time relative to job separation, and separately by gender, for workers who lived with a spouse two years before (counterfactual) job displacement. Table 1 shows that this group gathers about three quarters

of our sample. Under Assumptions 1 to 3, these quantities can be interpreted as the average dynamic causal effect of job displacement on couple dissolution.

Firstly, we find almost no difference in the evolution of the probability of being in a heterosexual cohabiting relationship before job displacement: the difference in trends before job separation is close to 0. In other words, displaced and control workers evolve in parallel in that matter. This fact strengthens the credibility of our empirical framework, as our model predicts that the causal effect of job displacement is 0 up until two years before the separation actually takes place. While this is not sufficient to assess the plausibility of our identifying assumptions *after* job displacement takes place, it does suggest that a causal interpretation of these quantities is possible.

Secondly, our estimates regarding the post-job separation period do not suggest that job separation disrupts couples. Indeed, our estimates remain precise and centered on the 0 line when it comes to displaced women, which suggests that the probability of being in a heterosexual relationship does not decrease due to job separation. When it comes to displaced men, our estimates are compatible with a very small decrease over time due to job displacement. The magnitude of this decrease, about 2.5 percentage points four years after an exogenous job separation is very small, and we can reject decreases larger than 5 percentage points over the same time period. Furthermore, the width of our confidence intervals is still compatible with null effects.

We then replicate this exercise, this time restricting ourselves to workers *who did not live with a spouse two years before job displacement*. This group gathers about a quarter of our sample. Under Assumptions 1 to 3, the estimated quantities would correspond to the dynamic effect of job displacement on couple formation, i.e. whether job displacement increases or decreases the probability of finding a mate for those who did not have one. Figure 4 displays our results.

Here again, we find very limited differences between displaced and control workers in terms of the evolution of their outcomes before the shock. In other words, displaced and control workers evolve in parallel over time before job displacement occurs, which helps sustain the validity of our identifying assumptions. After job displacement, the probability of living in a heterosexual cohabiting relationship seems to increase for women, but not for men. However, the magnitude of our estimate for women – 6 percentage points 4 years after job displacement – remains small, and our confidence intervals are still compatible with null effects. Lastly, we do not find formerly single men to change their probability of living in a cohabiting heterosexual relationship due to job displacement.

4.2 Fertility decisions

Figure 5 plots changes in the number of children born to displaced workers over time relative to job loss and relative to the (reweighted) control group, separately by gender. Here again, we find that displaced and control workers evolve in parallel over time *before* job separation, which supports the credibility of the identifying assumptions under which these estimates have a causal interpretation. We do not find any change past job separation, which suggests that job displacement does not lead to changes in fertility decisions.

4.3 Labor supply, taxes and transfers

We now turn to the consequences of job displacement on earnings, labor supply and transfers, first focusing on the displaced worker, then on her spouse's labor supply, and lastly on the household as whole, which will allow us to assess the role played by family insurance and the taxes and transfers system in insuring workers against exogenous shocks to the employment relationship.

4.3.1 Own earnings and labor supply

Figure 6 displays our estimates of changes in displaced workers' individual earnings and labor supply, as observed in the tax returns data, over time and relative to the (reweighted) control group. Displaced and control workers' earnings and labor supply evolve roughly in parallel before the shock.

Under our identifying assumptions, our estimates imply that, consistent with the intuition, job displacement results in a steep drop in workers' wage income. In the short run, the magnitude of this drop is about €5,000 for women and €6,300 for men. This short run transitory wage income shock is likely tied to the drop in employment: the yearly employment rate, as measured in the tax returns data by delineating the group of workers with positive labor earnings, drop by 7 (for women) to 8.5 (for men) percentage points.

However, in the longer run, the employment effect of job displacement fades out, which suggests that most displaced workers manage to find new jobs on the labor market at the same rate as their control counterpart. By contrast, while the magnitude negative wage income shock decreases, it remains notable, corresponding to 10% to 15% of the baseline pre-shock wage income (see Table 3). This suggests that this long run earnings drop is not driven by the displaced workers' inability to find new jobs, but rather by other channels such as the dissolution of valuable employer-employee matches (Lachowska, Mas, and Woodbury, 2020). In Appendix C, we replicate this analysis, relying on payroll tax data issued from the DADS instead of tax returns, to assess the robustness of these results. Our estimates are similar in the long run, and differences in the short run are likely the result of differences between the conceptual measures of outcomes in payroll tax data and income tax returns.

While the short run drop in wage income is very steep, job displacement does not result in similarly massive losses in individual earnings. The reason for this is that, consistent with the rationale, unemployment benefits limit the loss in earnings. Specifically, the insurance role of unemployment benefits allow workers to move from a €5,000 to a €1,800 short-run loss when it comes to women, and from a €6,300 to a €3,100 short-run loss for men. In other words, unemployment insurance divides the short-run earnings loss by a factor 2 to 2.8. Other sources of individual earnings, which consist of self-employed earnings, pensions and alimony matter much less in the short run. This is not surprising given that (i) workers in our sample are too young to be eligible for retirement pensions, (ii) moving from a salaried job to self-employed employment takes some time and (iii) alimonies are tied to couple dissolution, which we did not found to be triggered by job displacement at least in the short run.

This is no longer true in the longer run, which is not surprising given that the negative employment effects fade out over time, which will mechanically decrease the relevance of unemployment insurance. Four years after job displacement, we can no longer reject the null hypothesis that the overall individual earnings loss is the same as the wage income loss. Our baseline estimates would imply that other sources of income decrease the wage income loss by about 25% for women and 33% for men. Even so, while job displacement does not result in long-run differences in the employment rate, it does cause long-run earnings loss, of which the magnitude is about the same as the short-run earnings loss. This would implies that, *in terms of their individual earnings*, while workers are well insured against transitory employment shock thanks to unemployment insurance, there are much less insured against permanent wage shocks.

4.3.2 Spousal earnings and labor supply

Figure 7 displays our raw estimates of changes in displaced workers' spouses' earnings and labor supply, over time and relative to control workers' spouses. This estimation can only be performed when the displaced worker is observed to have a different-sex spouse.¹² Given that we do not find

¹²Displaced workers' individual loss are broadly the same regardless of whether they are with spouse or not. In Appendix D we replicate Figure 6 while conditioning on being observed with a spouse. Our

job displacement to change the probability of living in a cohabiting heterosexual relationship (Figures 3 and 4), this should not generate spurious patterns due to sample selection.¹³ We find very little differences between displaced and control workers' spouses' trends in earnings and labor supply. Specifically, we cannot reject the null hypothesis that displaced and control workers' spouses' earnings and labor supply evolve in parallel over time both before and after job displacement.

Discouraged worker effect As noted in Section 3, these results might be biased due to the within-couple correlation in employment shocks. To circumvent this issue, we showed that under simple assumptions regarding the structure of these within-couple correlations, and reasonable approximations, it is actually possible to implement a correction for the resulting downward bias in spousal labor supply. Specifically, we showed that this bias is well approximated by the product of the direct effect of one's own displacement on one's own labor supply and earnings by the share of couples who work for the same employer.

Based on the French Labor Force Survey (*Enquête Emploi en Continu*), we estimate that between 2010 and 2012, about 5% (3%) of men (women) who lived in a cohabiting relationship with a woman (man) aged between 25 and 50 who held a job as a salaried employee worked in the same plant as her (him). The corresponding shares when it comes to working in the same industry are 8% and 6%.

We combine these shares with our estimates of the direct effect of job displacement to correct the bias that results from the within-couple correlation of employment shocks in our investigation of the added-worker effect. Figures 8 and 9 display our results. Because the share of couples for which employment shocks are correlated is actually quite low, these results are very similar to those obtained without this correction. In other words, the added worker effect is not active: displaced workers' spouses do not increase their labor supply to compensate for the income loss that results from job displacement.

4.3.3 Total household income

Figure 10 displays changes in all the layers that range from wage income to disposable income for displaced workers' households, over time and relative to control workers' households. All components of disposable income evolve roughly in parallel between the displaced workers' group and the control group, which makes our identifying assumption reasonable. Crucially, income tax returns allow us to go through all the layers of both the taxes and transfers system, and earnings sources that combine into household's disposable income. As a result, we are able to get a sense of which sources of income provide household some kind of insurance against the income risk that job displacement generates.

Consistent with the rationale and our previous results, households experience sharp drops in wage income due to job displacement in the short run. The magnitude of these wage income losses coincides with the sum of the own wage income reaction and that of the spouse (which is close to 0). Once again as the individual-level data suggests, unemployment insurance provides effective insurance in the short run: unemployment benefits decrease the short-run wage income loss by more than 60%. Other components seem to matter much less in the short run, which may stem from a variety of frictions.

In the longer run, unemployment benefits seem to matter much less. This is consistent with our previous results, and the fact that the employment effect of job displacement is only short-lived, which mechanically limits the relevance of unemployment insurance. While our estimates

estimates appear extremely similar to our baseline results.

¹³To assess the robustness of our conclusions with respect to this concern, in Appendix E.3, we replicate our analysis on a balanced panel of workers who are continuously observed with a spouse. Our results are very similar to our baseline estimates, which confirms that this sample selection is not an issue here.

get noisy over time because we rely on a more restricted subsample, they still suggest that (i) job displacement results in a persistent shock on households' disposable income, which remains approximately constant over time, by contrast with the wage income component; and (ii) that households are partially insured at best against the long run wage income risk through a bundle that involves not only unemployment insurance, but also in a somewhat similar extent self-employment, capital income, and lastly the taxes and transfers system. This partial insurance does not seem to be active when it comes to displaced women. Overall, when compared with the pre-job loss baseline, this persistent disposable income loss amounts to a 3 to 6% decrease in household income, regardless of whether we consider the short-run or the long-run effects. Even though the data show that households are only partially insured against such shocks, the contrast is striking with the short-run individual wage income loss, which amounts to a 25% drop.

4.4 Selection into job displacement

Job displacement is not randomly assigned to workers: it results from decisions that may be endogenous with respect to the outcomes we investigate. Firstly, with the exception of plant closures, displaced workers are a subset of the workforce of the firm, which is likely to be chosen at least partly based on their contribution to the firm's product. For instance, [Seim \(2019\)](#) shows that within Swedish firms, workers who display high cognitive and noncognitive skills are less likely to be displaced than their counterparts with poorer skills. Secondly, when it comes to mass layoffs and even more plant closures, the assignment of job displacement is actually dependent on workers' past labor supply decisions: those with the best outside options may leave the firm *before* the decision to dismiss part of the workforce is made, solely based on their knowledge of the economic shock that triggers the layoff decision. The first channel is particularly salient in the French case: by law, employers are bound to either resort to criteria defined by industry- or firm-level collective agreements, or to define their own criteria, that always have to include: (i) workers' families (especially when it comes to single parents); (ii) workers' tenure within the firm; (iii) workers' employment prospects (especially when it comes to workers with disabilities or older workers); (iv) and lastly workers' productivity (see [Appendix A](#)).

To assess the robustness of our results with respect to these issues, we consider several quantitative exercises. Firstly, we replicate our framework while restricting the treatment group to the subset of workers who were affected by plant closures. These workers constitute over half of our baseline treatment group (see [Figure 2](#)). Because all workers of the firm are displaced, here the selection into displacement should be less active than it is otherwise. [Appendix F.1](#) displays our results, which are all consistent with our baseline estimates.

Secondly, within affected plants, we compare displaced and non-displaced workers. To do so, we restrict ourselves to plants that are affected by a mass layoff, but not by plant closure: otherwise the non-displaced group is empty. We replicate all our summary statistics tables to get a sense of how comparable displaced and non-displaced workers are in the first place. [Appendix F.2](#) displays our results. Overall, while there exist some differences between displaced and non-displaced workers that could be indicative of workers' families being taken into account in the displacement decision – displaced workers being slightly more likely to be single, and displaced men's female partners being more likely to hold a job than their counterparts – these differences remain limited and are not suggestive of a very strong correlation between observable family-related characteristics and job displacement.

Lastly, we turn back to the comprehensive payroll tax data to get a sense of how likely workers are to leave firms *before* the layoff decision is made. To do so, we plot, over time and separately for each cohort defined by the timing of job displacement (i) the average size of the plant from which workers were displaced, as measured by the number of salaried employees on January, 1st, and (ii) the strength of the outflows from these plants, as measured by the share of

workers who no longer work there on December, 31st year T , among those who did on January, 1st year $T - 1$. Figure 11 displays our results. Plant size does not diminish gradually over time before the plant-level shock, except perhaps the very last year before the shock. Similarly, outflows appear extremely stable over time before the shock, except one year before the shock. Importantly, this slight anticipation should not affect our results, given that we include in our treated group workers who left the plant either one year after or one year before the plant-level shock.

5 Conclusion

In this paper, we quantify the impact of job displacement on workers' family structure, on their spouses' labor supply and lastly on all components that combine into the disposable income as measured at the household level, so as to get a sense of the cost of labor reallocation as paid by workers. Relying on a combination of French administrative registers that span over multiple dimensions of family structure and income, we find that (i) job displacement does not seem to disrupt couples, and does not trigger negative fertility decisions in the short run; (ii) the added worker effect does not seem to prevail in France as displaced workers' partners do not increase their labor supply in response to the shock; (iii) unemployment insurance seems to provide effective insurance against the short-run income risk that results from job displacement; but (iv) in the long run, unemployment insurance does not seem to matter, as workers are partially insured at best against the long run component, through a bundle that involves self-employment, capital markets and the progressivity of the taxes and transfers system. Importantly, these results depend very little on the gender of the displaced workers.

These empirical results come with a puzzle. First, that households are only partially insured against this risk suggests that they are somewhat credit-constrained or face incomplete markets; otherwise, it would be possible for them to borrow in order to purchase assets of which the returns are negatively correlated with their unemployment risk to get effective insurance against this risk. However, these frictions should make the added worker effect more salient (Lundberg, 1985), which is not observed in the data. An explanation could be that traditional gender norms bias the allocation of time and effort away from the optimal response to these income and employment shocks, but this channel would imply gender-asymmetrical response to one's spouse job loss, which is again not observed in the data. In the short run, unemployment insurance seem to offer good enough insurance to crowd out spousal labor supply, as Cullen and Gruber (2000) would suggest. However, this explanation is no longer viable when it comes to the long run. Solving this puzzle opens avenues for future research.

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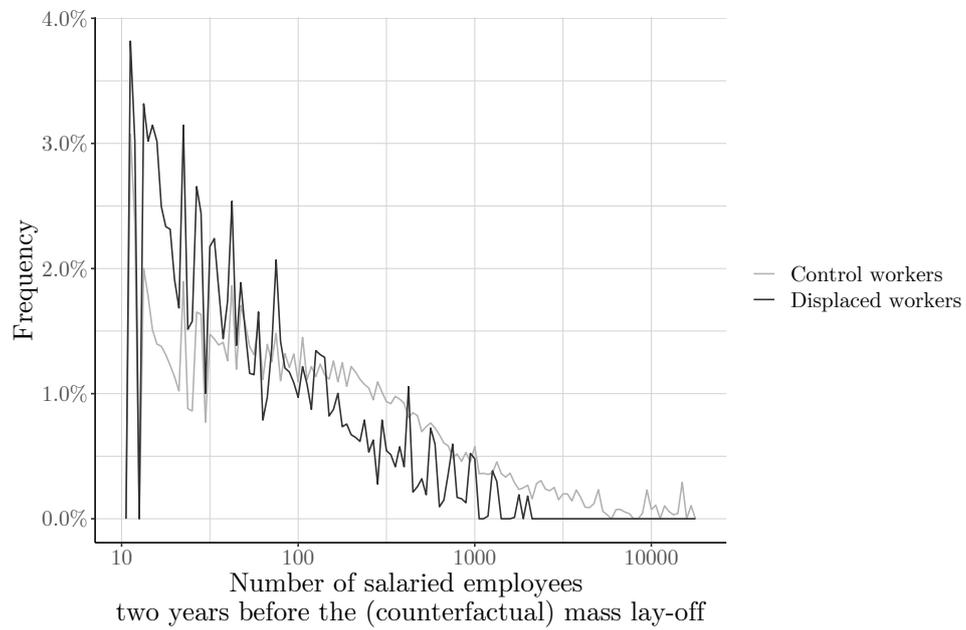
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Figures

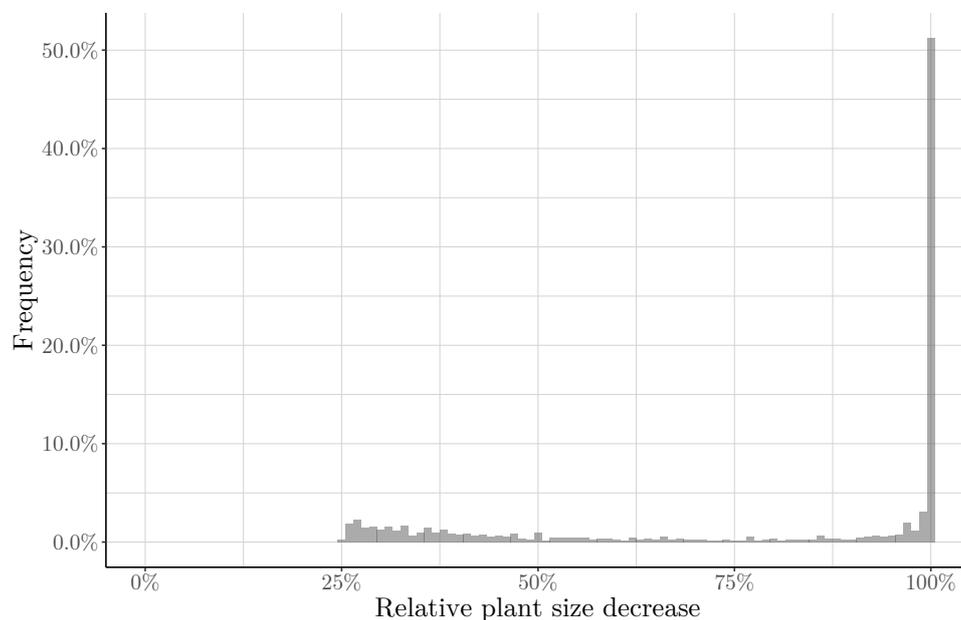
Figure 1: Plant size before mass layoff



Plant size is measured by the number of salaried employees on January, 1st, one year before the mass layoff.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

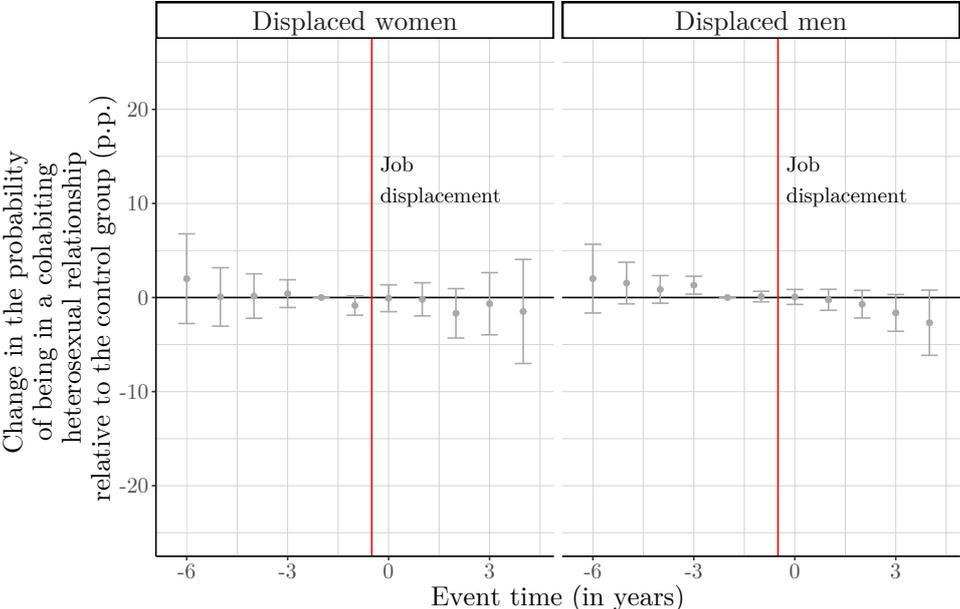
Figure 2: Mass layoff intensity



The mass layoff intensity is 1 minus the number of salaried employees on December, 31st the year of the event, divided by the number of salaried employees on January, 1st the year before.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

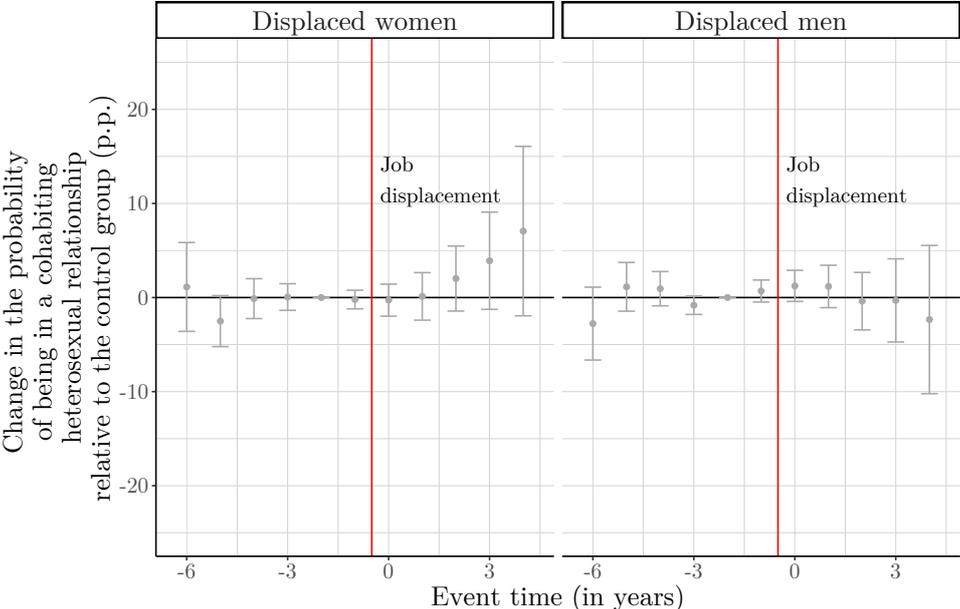
Figure 3: Difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, for those who were before displacement



Rewighted difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

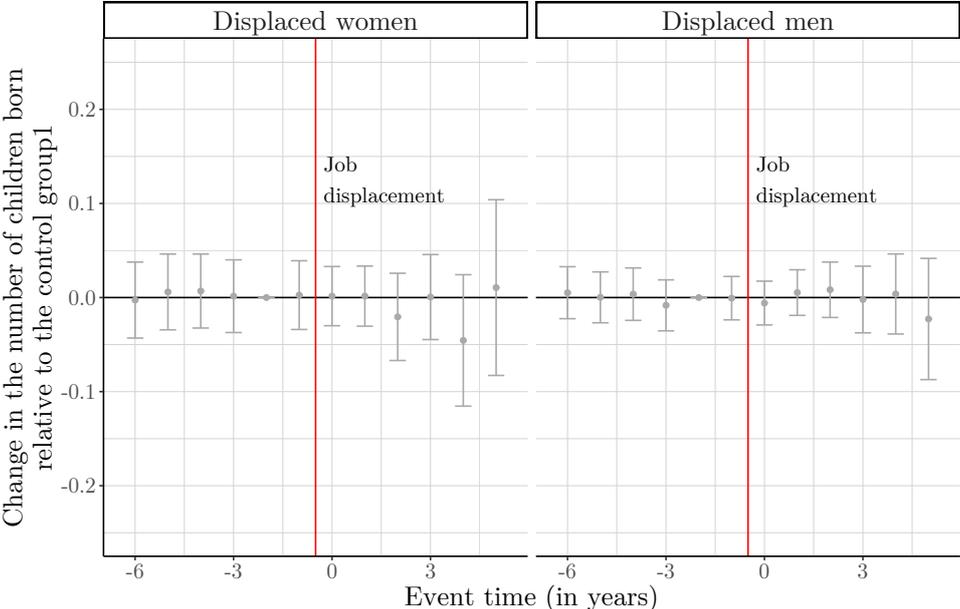
Figure 4: Difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, for those who were not before displacement



Rewighted difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

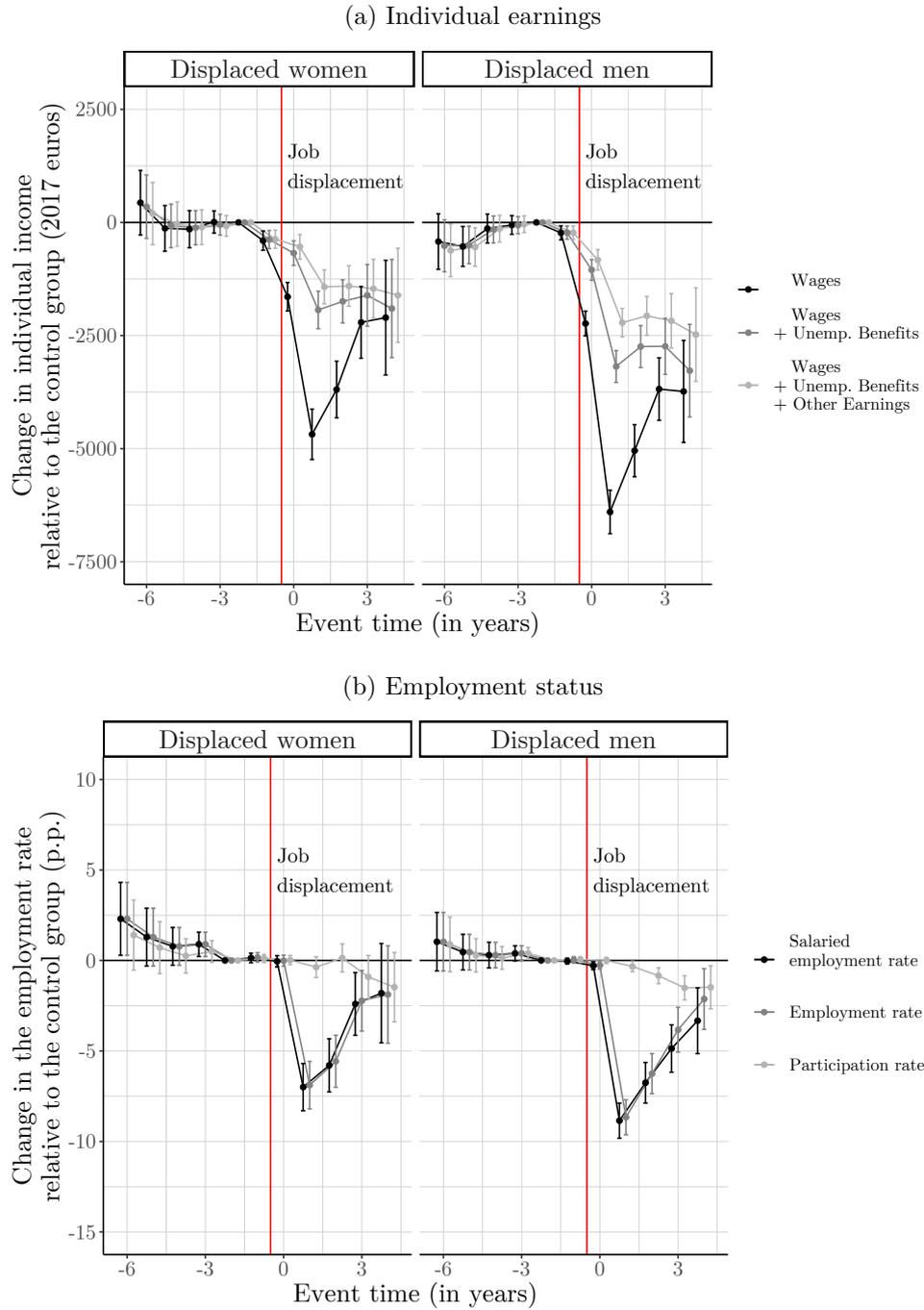
Figure 5: Difference-in-difference estimates of the impact of job displacement on the number of children born to workers



Rewighted difference-in-difference estimates of the impact of job displacement on the yearly number of children born to a worker, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

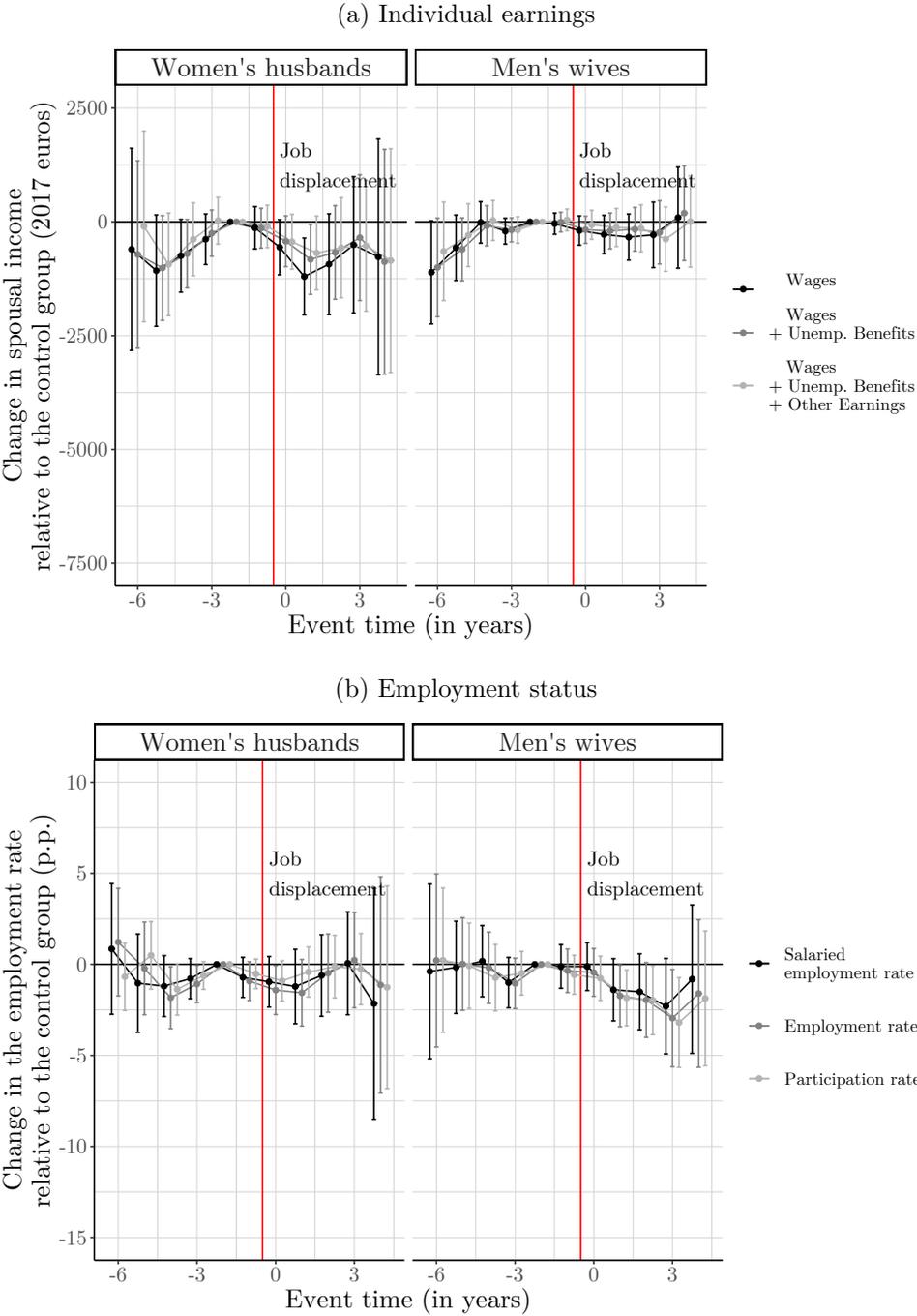
Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure 6: Difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status



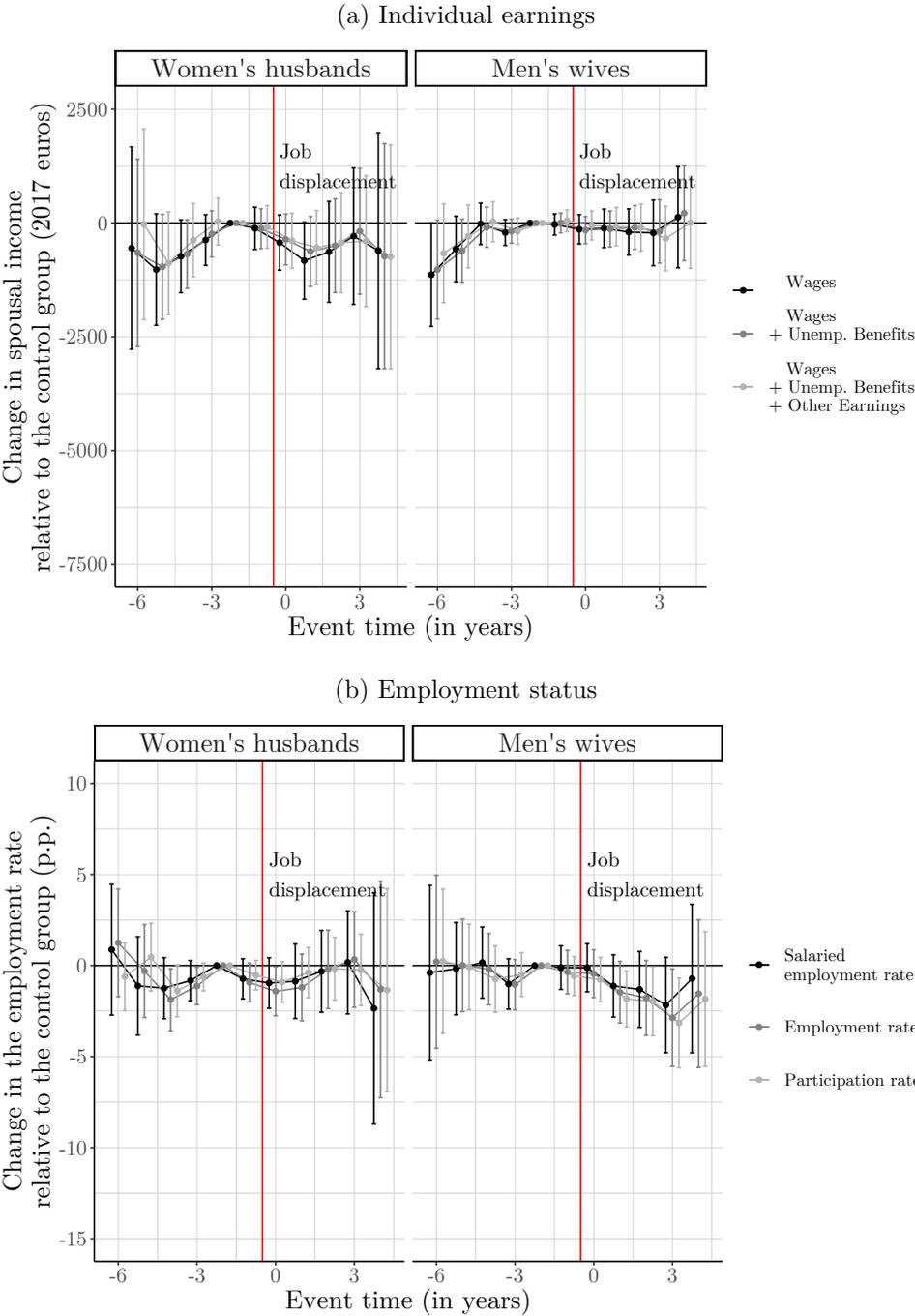
Rewighted difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure 7: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status



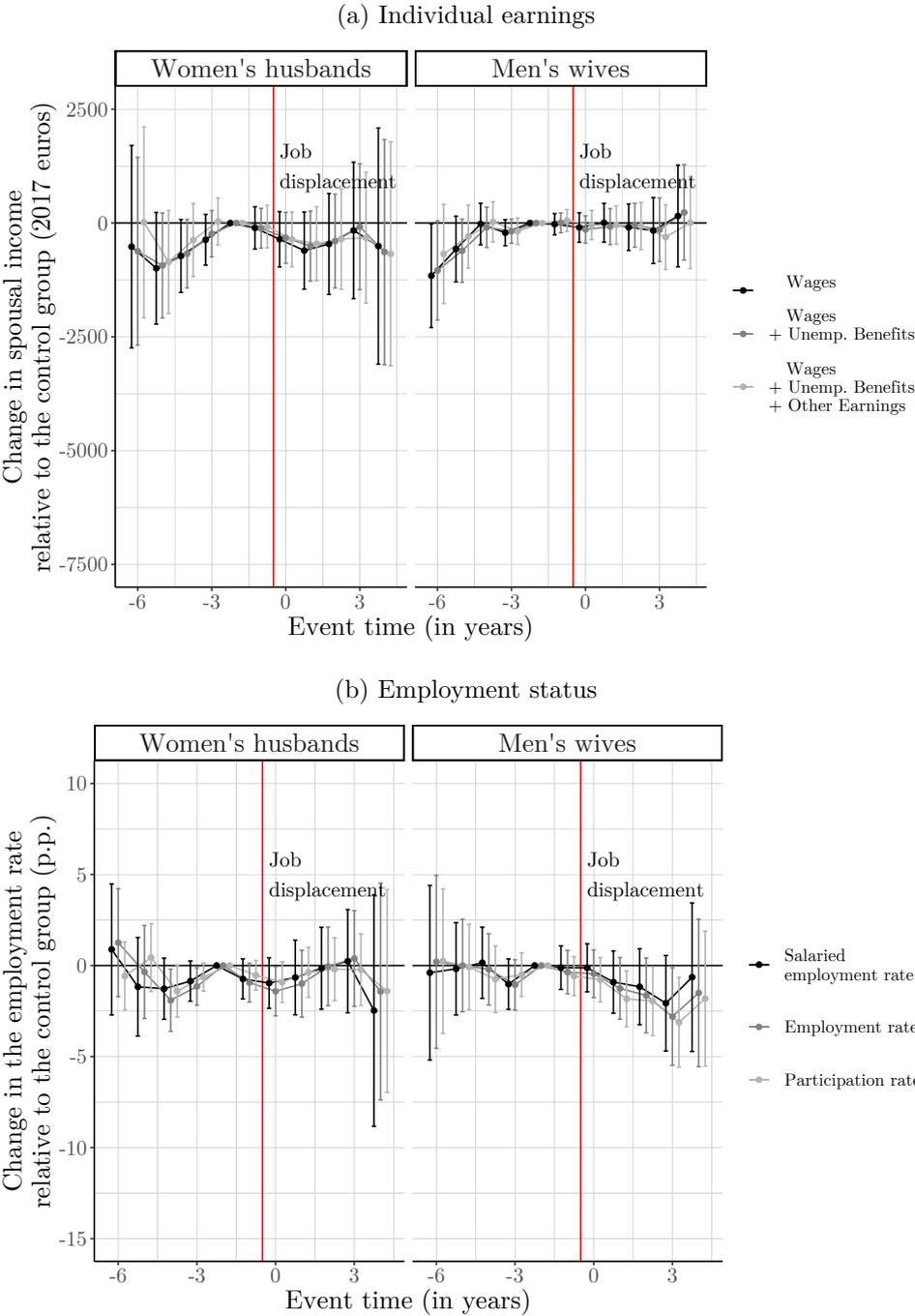
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure 8: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the plant level



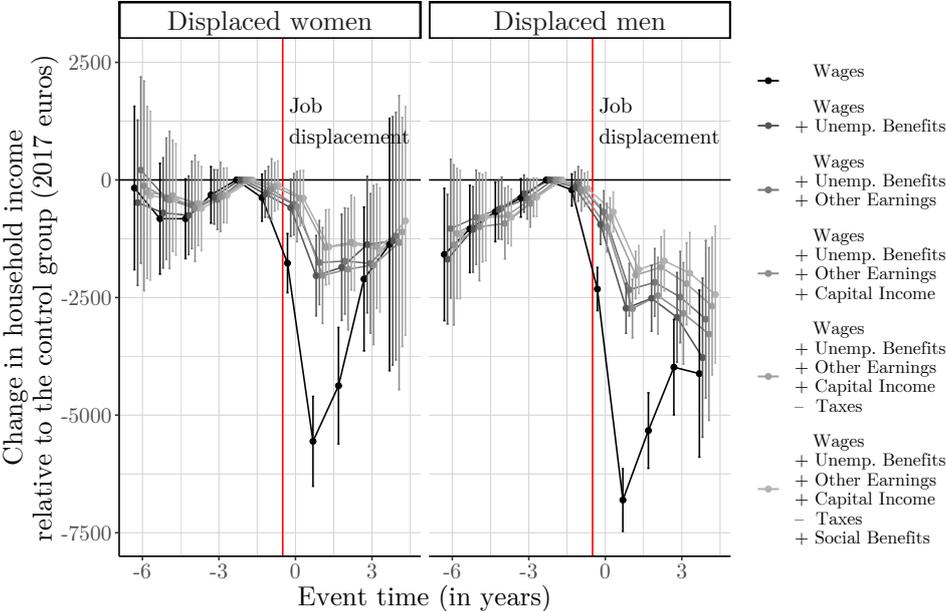
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure 9: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the industry level



Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

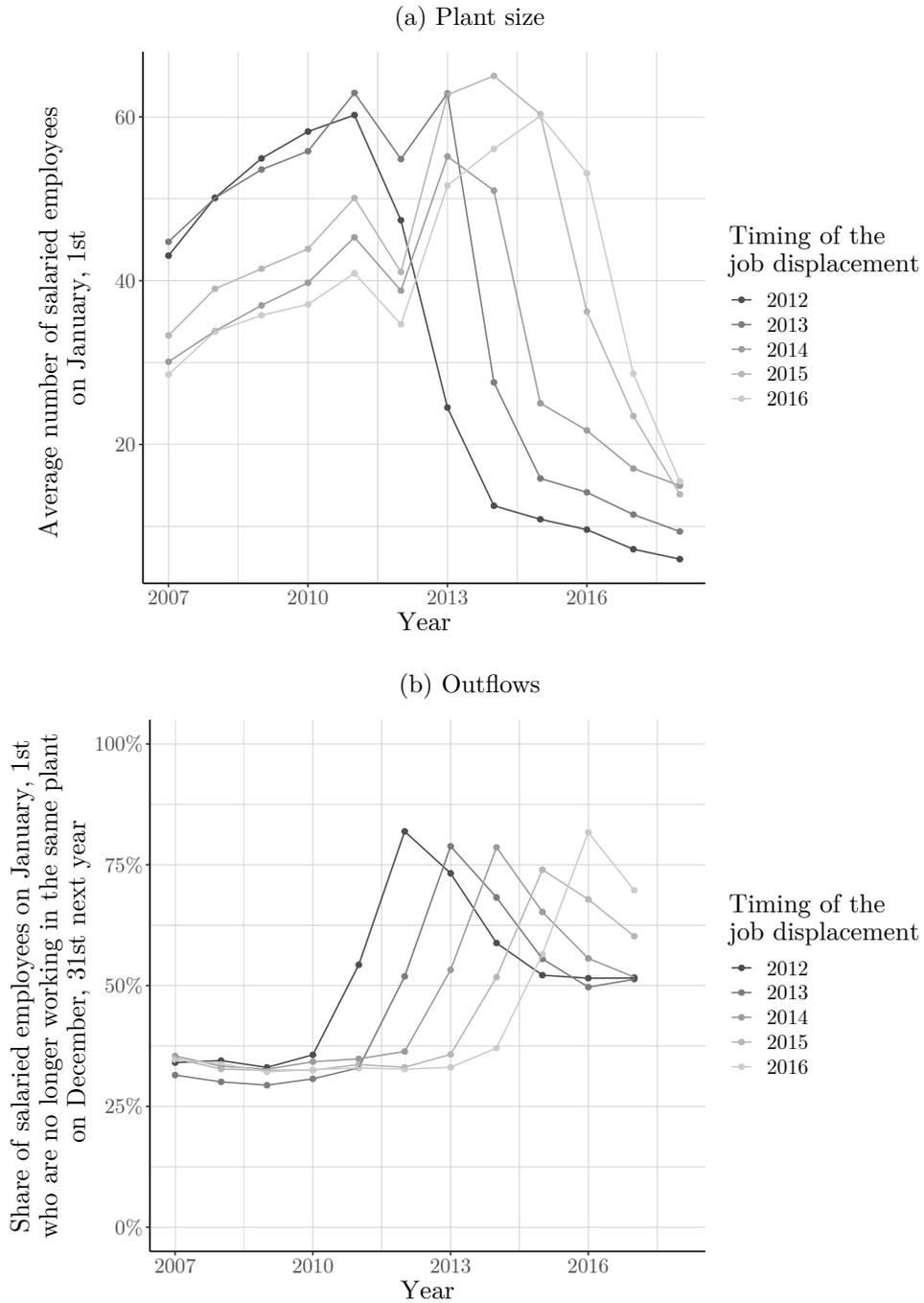
Figure 10: Difference-in-difference estimates of the impact of job displacement on workers' households' overall income



Rewighted difference-in-difference estimates of the impact of job displacement on workers' households' overall income, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure 11: Plant size and outflows over time



Plant size is measured by the number of salaried employees who work at a plant on January, 1st. Outflows correspond to the share of salaried employees who no longer work at the same plant on December, 31st almost two years later.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Tables

Table 1: Summary statistics: occupation and family structure

Gender	Women		Men	
	Control	Displaced	Control	Displaced
<i># Individuals</i>	76,322	3,251	106,348	6,124
<i>a. Age*</i>				
Mean	37.5	37.0	37.8	37.7
St.D.	7.6	7.3	7.6	7.3
<i>b. Occupational shares (in %)**</i>				
Managers and professionals	16.0	19.8	21.2	21.0
Intermediate occupations	21.6	16.8	20.5	17.4
Non-manual workers	48.7	48.2	13.9	15.7
Manual workers	13.7	15.2	44.3	46.0
<i>c. Family structure (in %)**</i>				
Single	11.6	14.6	15.0	14.7
Childless couple	15.2	15.6	14.4	14.1
Single parent	11.5	11.8	5.8	5.7
Couple with children	55.8	50.9	58.5	58.2
Other families	5.8	7.1	6.2	7.3
<i>d. Number of children**</i>				
Mean	1.2	1.1	1.2	1.2
St.D.	1.1	1.2	1.2	1.3
<i>e. Yearly number of childbirths**</i>				
Mean	0.2	0.2	0.2	0.2
St.D.	0.5	0.4	0.5	0.4

* At the time of the (counterfactual job displacement). ** As observed two years before job displacement.
Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table 2: Summary statistics: displaced workers' salaried earnings and labor supply two years before separation (payroll tax data)

Gender	Women		Men	
	Control	Displaced	Control	Displaced
<i>a. Wages (in 2017€)</i>				
Mean	19,900	20,400	26,100	25,200
St.D.	12,100	13,300	14,900	15,600
<i>b. Days worked</i>				
Mean	340	340	340	340
St.D.	70	70	50	60
<i>c. Hours worked</i>				
Mean	1,560	1,580	1,770	1,760
St.D.	500	520	400	500

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table 3: Summary statistics: displaced workers' earnings and labor supply two years before separation (income tax returns)

Gender	Women		Men	
	Control	Displaced	Control	Displaced
<i>a. Individual earnings (in 2017€)</i>				
Mean	22,000	22,500	27,900	27,100
St.D.	12,300	13,400	15,100	15,700
<i>b. Wages (in 2017€)</i>				
Mean	21,200	21,800	27,300	26,400
St.D.	12,400	13,400	15,200	15,700
<i>c. Unemployment benefits (in 2017€)</i>				
Mean	400	400	300	400
St.D.	1,600	1,800	1,500	1,800
<i>d. Other earnings (in 2017€)</i>				
Mean	400	300	300	300
St.D.	2,200	1,700	2,300	2,000
<i>e. Employment and participation rates (in %)</i>				
Employment	99.6	99.4	99.6	99.6
Participation	99.6	99.5	99.7	99.7

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table 4: Summary statistics: displaced workers' spouses' earnings and labor supply two years before separation (income tax returns)

Gender	Women's male spouse		Men's female spouse	
	Control	Displaced	Control	Displaced
<i>a. Individual earnings (in 2017€)</i>				
Mean	30,800	30,900	17,700	16,400
St.D.	19,000	19,600	14,200	14,400
<i>b. Wages (in 2017€)</i>				
Mean	28,000	28,300	16,100	15,100
St.D.	18,700	19,700	13,900	14,200
<i>c. Unemployment benefits (in 2017€)</i>				
Mean	600	600	600	600
St.D.	2,400	2,300	2,100	2,200
<i>d. Other earnings (in 2017€)</i>				
Mean	2,200	2,000	1000	700
St.D.	9,900	8,600	6,200	4,400
<i>e. Employment and participation rates (in %)</i>				
Employment	95.0	95.1	84.0	80.5
Participation	96.2	96.0	85.7	82.6

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table 5: Summary statistics: displaced workers' household income two years before separation (income tax returns)

Gender	Women		Men	
	Control	Displaced	Control	Displaced
<i>a. Disposable income (in 2017€)</i>				
Mean	46,400	45,900	45,800	45,100
St.D.	24,500	25,700	23,300	24,000
<i>b. Wages (in 2017€)</i>				
Mean	42,000	41,900	41,900	40,700
St.D.	25,600	27,400	24,000	25,100
<i>b. Unemployment benefits (in 2017€)</i>				
Mean	1,100	1,200	1,000	1,200
St.D.	3,100	3,300	2,900	3,200
<i>c. Other earnings (in 2017€)</i>				
Mean	3,700	3,500	3,200	3,400
St.D.	10,600	9,200	9,200	8,200
<i>d. Capital income (in 2017€)</i>				
Mean	4,600	4,400	4,400	4,100
St.D.	8,100	7,800	7,300	7,800
<i>e. Taxes (in 2017€)</i>				
Mean	-7,500	-7,600	-7,300	-7,100
St.D.	7,400	7,900	6,800	7,200
<i>f. Social benefits (in 2017€)</i>				
Mean	2,600	2,500	2,500	2,700
St.D.	3,700	3,600	3,800	4,000
<i>g. Position in the equivalent income distribution (in %)</i>				
First quintile	9.1	9.8	9.5	12.7
Second quintile	20.6	21.7	19.6	21.3
Third quintile	24.1	22.1	24.6	23.0
Fourth quintile	24.7	23.2	25.8	23.1
Fifth quintile	21.4	23.2	20.5	19.9

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

A Institutional background

In France, mass layoffs for economic reasons (i.e. layoff of at least two employees) are tightly regulated.

Duration. A single mass layoff is defined as the dismissal of two or more employees over 30 days. The time lapse between the notification of the layoff and the termination of the contract depends on the tenure of the worker: one month for workers with a tenure between 6 months and two years, two months for those with a tenure of more than two years.¹⁴ A single mass layoff can spread over three months and even more if, before being notified of the layoff, the worker had scheduled paid vacations during the period of notice.

Order of dismissals If a collective agreement applicable to the firm specifies the criteria determining the order of dismissals, then they are binding on the employer. Otherwise, the employer must define them, after consulting the *Comité Social et Économique* (CSE). In this case, the employer is bound to take into account all of the following elements: (i) employees' family responsibilities, in particular single parenthood; (ii) employees' tenure in the plant or firm; (iii) any situation making re-employment especially difficult, in particular that of elderly or disabled employees; and lastly (iv) skills assessed by occupation (e.g. speed and versatility in carrying out tasks). Other criteria may be added to this list, and the employer may give preference to one of these criteria, but this is only possible provided that other criteria are taken into account. Additionally, these criteria have to be taken into account even when the stake is an individual dismissal for economic reasons.

Lastly, anti-discrimination laws prohibit that certain criteria be used for the decisions, and it is also not possible to dismiss an employee solely because she works part-time in preference to a full-time employee performing the same type of tasks in the company.

Employment protection program. Firms with at least 50 employees and firing at least 10 employees over 30 days have to implement a *Plan de Sauvegarde de l'Emploi (PSE)*. This employment protection program includes several measures to support the reemployment of dismissed employees (internal or external redeployment, formations,...) When the firm implements a PSE, severance payments are exempted from income taxes.

Compensation. First, upon termination of a permanent contract¹⁵ for economic reasons (mass layoff or firm dissolution), workers with tenure 8 months or more are entitled to severance payments. These severance payments are bound to a minimum which depends on their gross

¹⁴Below 6 months, the length of the notice is either set by collective agreements or depends on practices in the company or the industry.

¹⁵*Contrat à Durée Indéterminée (CDI)*

monthly reference wage¹⁶ w according to the following formula:

$$\frac{1}{4}w \min \left\{ \frac{M}{12}, 10 \right\} + \frac{1}{3}w \max \left\{ \frac{M}{12} - 10, 0 \right\}$$

Firm- or industry-level agreements may set higher minimas. These severance payments are the same for collective contractual termination as they are for standard layoffs. On top of these payments, workers may be eligible to compensations for periods of notice and paid vacations they could not take advantage of before termination of their contract. These periods are compensated at a wage rate and these payments are considered as wages by the fiscal administration.

Legal severance payments as well as those set by collective agreements are fully exempted from income taxes and partly exempted from social contributions. Supra-legal payments are partly exempted from income taxes (except in the case of a PSE where they are fully exempted) and are not exempted from social contributions.

Firms below 1,000 employees (or firms going into receivership) have to offer a *Contrat de Sécurisation Professionnelle (CSP)* to dismissed workers.¹⁷ Upon their acceptance, these workers are entitled to a one-year special unemployment compensation called the *Allocation de Sécurisation Professionnelle (ASP)*. This compensation is the same as regular unemployment insurance benefits for workers with tenure less than one year, i.e. 57% of the previous wage in the most standard case. For workers with tenure over one year, this compensation amounts to 75% of their previous wage. Should she fail to find a new job during this time span, an ASP receiver is entitled to regular UI benefits for a potential duration equal to her initial entitlement duration upon job loss, minus one year.

Firms with over 1,000 employees have to offer a *Congé de reclassement*, which provides training and job search support. The length of this leave is between 4 and 12 months (24 months in case of a professional retraining). The employer pays the full wage during the period of notice and then at least 65% of the reference wage, with a lower limit equal to 85% of the French minimum wage (SMIC). This compensation is taxed as a labor income.

Finally, in order to cut labor costs, firms may also arrange early terminations of fixed-term contracts. At the end of a fixed-term contract, employees receive an allowance equal to at least 10% of the total gross income they earned during this contract. However, during a mass layoff, fixed-term contracts may be terminated before the end of the contract. When this decision is initiated by the employer, the employee receives severance payments equal to the sum of earnings she would have perceived until the end of this contract. During the trial period, the fixed-term contract can be terminated without compensation.

¹⁶This reference wage is defined as the higher value between average gross wage computed either the over the past year or over the past three months.

¹⁷There is no CSP when the employer and the employee sign an agreement for a contractual termination.

B Discouraged worker effect

Our approach has so far dealt with the identification of shocks on the employment status of one worker on her and her spouse's outcomes. As explained in Subsection 3.2, the issue with this framework is that the spouse is likely directly affected by employment shocks that are positively correlated with the shocks of which we aim at identifying the consequences. In this Appendix, we develop a very simple model of couples exposed to correlated employment shocks to get a plausible approximation of the size of the resulting bias.

We consider a population of heterosexual couples, indexed by i , which are composed of a female and a male partner $f(i)$ and $m(i)$. Each partner (i) has outcomes $Y_{f(i)}$ and $Y_{m(i)}$ respectively; and (ii) is exposed to employment shocks that are represented by dummy variables $D_{f(i)}$ and $D_{m(i)}$.

We frame the model in terms of potential outcomes. By contrast with our initial framework, in which potential outcomes are made to depend only on the observed partner's employment shocks, here these potential reactions, denoted as $Y_{f(i)}(d_{f(i)}, d_{m(i)})$ and $Y_{m(i)}(d_{f(i)}, d_{m(i)})$ may depend jointly on both partners' employment shocks. In order to lighten the exposition of the model, we make the following simplifying assumption:

Assumption B.1 (Exogeneity). *Couples' potential outcomes are mean independent of couples employment shocks: for all g in $\{f, m\}$, for all $(d_{f(i)}, d_{m(i)})$ and $(d'_{f(i)}, d'_{m(i)})$ in $\{0, 1\}^2$:*

$$\mathbb{E}[Y_{g(i)}(d_{f(i)}, d_{m(i)}) \mid D_{f(i)} = d'_{f(i)}, D_{m(i)} = d'_{m(i)}] = \mathbb{E}[Y_{g(i)}(d_{f(i)}, d_{m(i)})] \quad (8)$$

This assumption states that employment shocks can be treated as though they are random and uncorrelated with couples potential reaction to them. With respect to our conditional difference-in-difference design, this assumption is a massive simplification; it basically holds (i) after conditioning on observables, and differencing with respect to the pre-shock period outcomes; and (ii) assuming that average treatment effects are the same for treated and control groups. The differencing and conditioning remains implicit in our one-period model so as to keep the notations handy.

For any gender g in $\{f, m\}$ and any two vectors $(d_{f(i)}, d_{m(i)})$ and $(d'_{f(i)}, d'_{m(i)})$, we define the average joint treatment effect:

$$\Delta_{(d_{f(i)}, d_{m(i)}) \rightarrow (d'_{f(i)}, d'_{m(i)})}^g = \mathbb{E}[Y_{g(i)}(d'_{f(i)}, d'_{m(i)}) - Y_{g(i)}(d_{f(i)}, d_{m(i)})] \quad (9)$$

This quantity represents the average effect, for partners of gender g , of moving from the $(d_{f(i)}, d_{m(i)})$ situation to the $(d'_{f(i)}, d'_{m(i)})$ situation.

For g and g' in $\{f, m\}$, we also define the average partial treatment effect:

$$\Delta^{(g, g')} = \mathbb{E}[Y_g(i)(d_{g'(i)} = 1, D_{-g'(i)}) - Y_g(i)(d_{g'(i)} = 0, D_{-g'(i)})] \quad (10)$$

This quantity represents the average effects for partners of gender g 's outcomes of a shock on partners of gender g' 's employment relationship, *without changes in the shock that affects the other partner's status*.

Armed with these definitions and our exogeneity assumption, we are now able to show that the within couple correlation in employment shocks does indeed generate bias in our estimates, even when employment shocks are exogenous.

Proposition B.1. *Under Assumption B.1, the difference in expected outcomes the g' -treated and the g' -control group does not identify the partial treatment effect, unless (i) the other partner's employment shocks have no effect on the outcome, or (ii) there is no within couple correlation in employment shocks.*

Proof. Without loss of generality, we assume $g' = f$, i.e. that we are investigating the consequences of a shock on female employment:

$$\begin{aligned}
& \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 1] - \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 0] \\
= & \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 1, D_{m(i)} = 1] \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) \\
& + \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 1, D_{m(i)} = 0] \{1 - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1)\} \\
& - \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 0, D_{m(i)} = 1] \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0) \\
& - \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 0, D_{m(i)} = 0] \{1 - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0)\} \\
\stackrel{AB.1}{=} & \mathbb{E}[Y_{g(i)}(1, 1)] \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) \\
& + \mathbb{E}[Y_{g(i)}(1, 0)] \{1 - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1)\} \\
& - \mathbb{E}[Y_{g(i)}(0, 1)] \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0) \\
& - \mathbb{E}[Y_{g(i)}(0, 0)] \{1 - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0)\} \\
= & \mathbb{E}[Y_{g(i)}(1, 1) - Y_{g(i)}(0, 1)] \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) \\
& + \mathbb{E}[Y_{g(i)}(1, 0) - Y_{g(i)}(0, 0)] \{1 - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1)\} \\
& + \mathbb{E}[Y_{g(i)}(0, 1) - Y_{g(i)}(0, 0)] \{\mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0)\} \\
= & \Delta^{(g,f)} \\
& + \Delta_{(0,0) \rightarrow (0,1)}^g \{\mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0)\} \tag{11}
\end{aligned}$$

where the first equality follows from the law of iterated expectations, the third one from the linearity of the expectation operator and the last one from the law of iterated expectations and the definition of the average partial and joint treatment effects.

If shocks on the employment status of men have no effect on $Y_{g(i)}$, then $\Delta_{(0,0) \rightarrow (0,1)}^g = 0$. If there is no within-couple correlation in employment shocks, then $\mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) = \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0)$. Thus in either cases, $\mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 1] - \mathbb{E}[Y_{g(i)} \mid D_{f(i)} = 0] = \Delta^{(g,f)}$. Otherwise, the bias corresponds to the remaining term in Equation 11. \square

The remainder of this Appendix is devoted to the general case, that is the one in which employment shocks are assumed to be correlated within couple. Proposition B.1 states that our naive approach that simply compares couples of which partners of gender g are affected by a shock to couples of which the same partners are not affected by a shock is biased due to the fact that (i) the other partners in the treated couples are more likely to be affected by an employment shock (assuming the correlation is positive), and (ii) this additional shock changes

couples' labor supply decisions. Our aim is to offer a reasonable approximation for the size of the bias.

In order to do so, we make several simplifying assumptions:

Assumption B.2 (Direct effects). *Direct effects are much larger than indirect effects, i.e.:*

$$\begin{cases} \Delta_{(0,0) \rightarrow (0,1)}^m, \Delta_{(1,0) \rightarrow (1,1)}^m \gg \Delta_{(0,0) \rightarrow (1,0)}^m \\ \Delta_{(0,0) \rightarrow (1,0)}^f, \Delta_{(0,1) \rightarrow (1,1)}^f \gg \Delta_{(0,0) \rightarrow (0,1)}^f \end{cases} \quad (12)$$

Assumption B.3 (Limited correlation). *The within couple correlation in employment shocks is limited, i.e. for all g in $\{m, f\}$;*

$$\mathbb{P}(D_{-g(i)} = 1 \mid D_{g(i)} = 1) - \mathbb{P}(D_{-g(i)} = 1 \mid D_{g(i)} = 0) \ll 1 \quad (13)$$

Assumption B.2 implies that a worker's labor outcomes reacts much more to whether she is affected by an employment shock than to whether her partner is. Assumption B.3 states that most couples evolve in somewhat isolated labor markets, so that most of the time a worker being hit by a shock does not imply that her spouse is too.

As implied by Assumption B.2, we make the distinction between direct effects, i.e. the impact of one's own employment shock on one's own labor outcomes, and indirect effects, i.e. the impact of a shock on one's spouse employment relationship. When considering direct shocks under these new assumptions, the size of the bias as it appears in Equation 11 is small before $\Delta^{g,g}$. As a result, our approach offers a good approximation of the direct effect of employment shocks.

The same does not hold when it comes to indirect effects: indeed, this time the bias results from the multiplication of large direct effects by the within couple correlation, which can no longer be assumed to be small before $\Delta^{g,-g}$. To get a plausible approximation for the size of the bias, we introduce additional assumptions:

Assumption B.4 (Limited treatment effect heterogeneity). *Direct effects depend very little on whether the spouse is affected by a shock:*

$$\begin{cases} \Delta_{(0,0) \rightarrow (0,1)}^m \simeq \Delta_{(1,0) \rightarrow (1,1)}^m \\ \Delta_{(0,0) \rightarrow (1,0)}^f \simeq \Delta_{(0,1) \rightarrow (1,1)}^f \end{cases} \quad (14)$$

Assumption B.4 implies in particular that our estimates of the aggregate direct effect $\Delta^{(g,g)}$ is a reasonable approximation for the first component of the bias as it appears in Equation 11.

To approximate the second component of the bias, we now assume that we are provided with a certain partition of couples that predicts perfectly the within couple correlation in employment shocks. Specifically, we assume that this partition corresponds to the distinction between couple in which both partners work in the same firm (detailed industry), as opposed to partners who work in different firms (detailed industry).

Assumption B.5 (Predicted correlation). *There exists an observable subset of couples \mathcal{J} such that (i) if i belongs to \mathcal{J} , then spouses' employment shocks are perfectly correlated: $D_{f(i)} = D_{m(i)}$; and (ii) otherwise spouses' employment shocks are independent: $D_{f(i)} \perp\!\!\!\perp D_{m(i)}$.*

Assumption B.6 (Limited treatment probability heterogeneity). *The probability than one spouse is affected by an employment shock depends very little on whether shocks are correlated or not between spouses: for all g in $\{f, m\}$*

$$\mathbb{P}(D_{g(i)} = 1 \mid i \in \mathcal{J}) \simeq \mathbb{P}(D_{g(i)} = 1 \mid i \notin \mathcal{J}) \quad (15)$$

These assumptions provide us with approximations of the second component of the bias.

Proposition B.2 (Bias approximation). *Under Assumptions B.1 to B.6, the bias that results from correlated employment shocks between spouses in the estimation of indirect effects is approximately equal to the direct effect multiplied by the share of couples that belong to \mathcal{J} .*

Proof. We assume without loss of generality that we focus on the impact of a female employment shock on men's labor outcomes. That $\Delta_{(0,0) \rightarrow (0,1)}^m$ is well approximated by $\Delta^{(m,m)}$ is a direct consequence of Assumption B.4 combined with the law of iterated expectations. This gives us a good estimate of the first term of the bias.

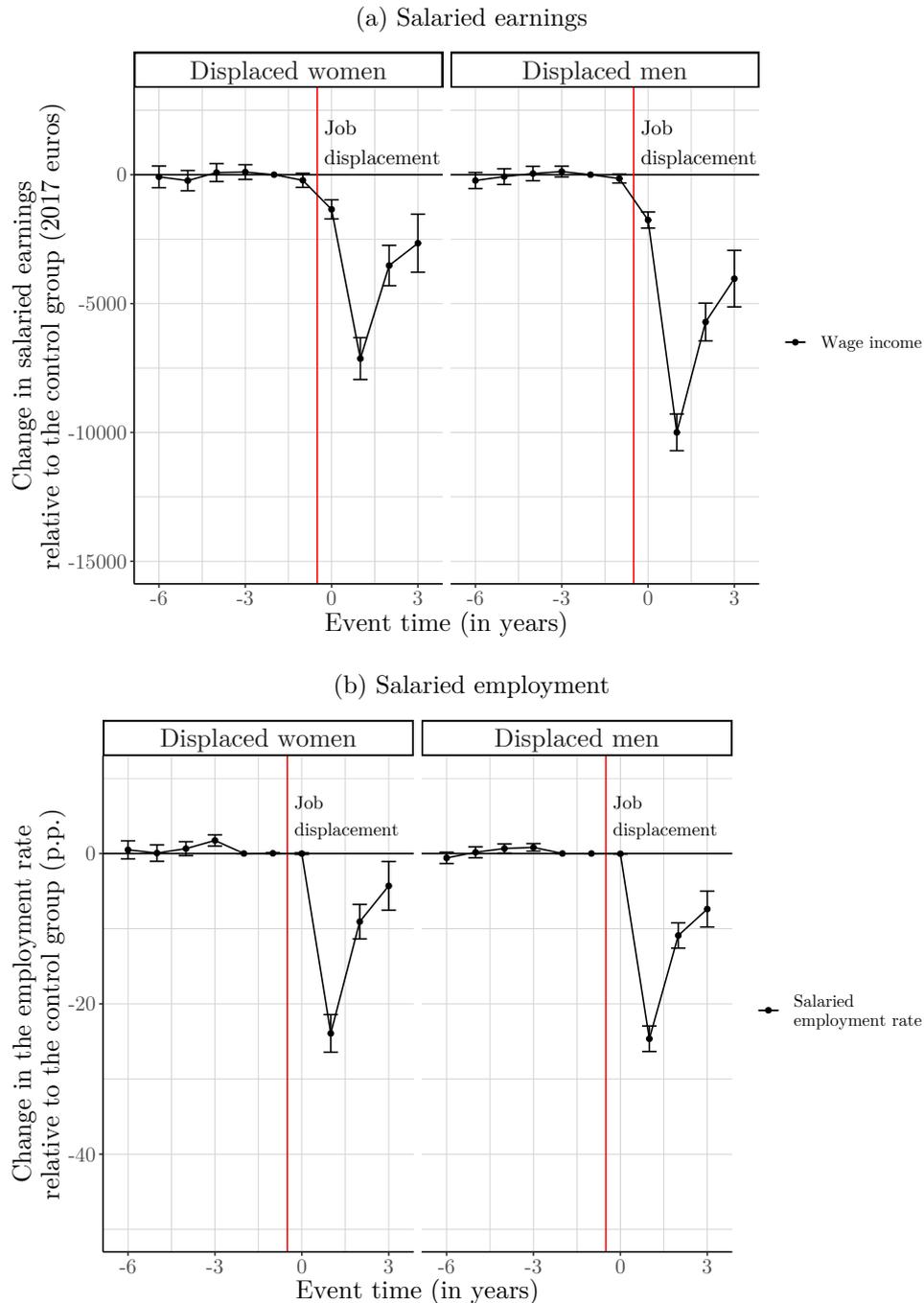
The second component of the bias writes:

$$\begin{aligned} & \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1) - \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0) \\ = & \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 1) \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1, i \in \mathcal{J}) \\ & + \{1 - \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 1)\} \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 1, i \notin \mathcal{J}) \\ & - \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 0) \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0, i \in \mathcal{J}) \\ & - \{1 - \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 0)\} \mathbb{P}(D_{m(i)} = 1 \mid D_{f(i)} = 0, i \notin \mathcal{J}) \\ \stackrel{AB.5}{=} & \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 1) \\ & + \{1 - \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 1)\} \mathbb{P}(D_{m(i)} = 1 \mid i \notin \mathcal{J}) \\ & - \{1 - \mathbb{P}(i \in \mathcal{J} \mid D_{f(i)} = 0)\} \mathbb{P}(D_{m(i)} = 1 \mid i \notin \mathcal{J}) \\ \stackrel{AB.6}{=} & \mathbb{P}(i \in \mathcal{J}) \end{aligned} \quad (16)$$

where the first equality simply follows from the law of iterated expectations. \square

C Earnings and employment effects in the DADS panel

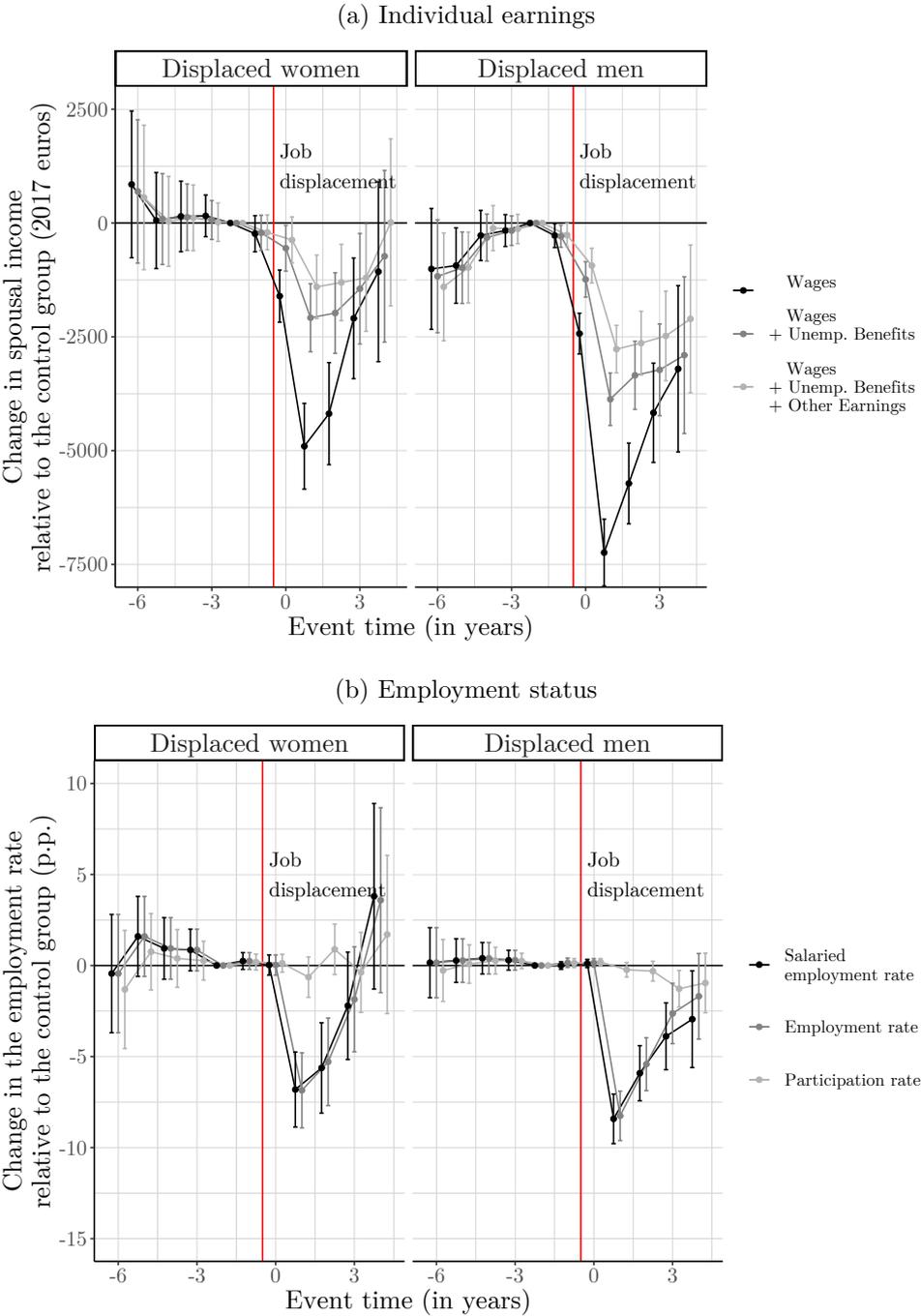
Figure C.1: Difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status



Rewighted difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

**D Individual-level income loss for displaced workers
with spouses**

Figure D.1: Difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status (workers in a cohabiting heterosexual relationship)

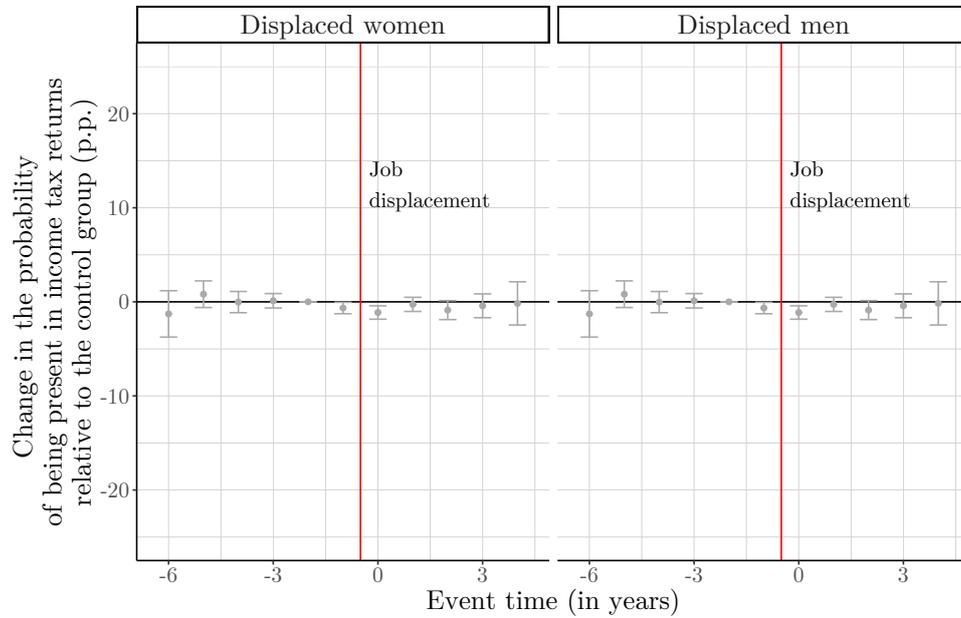


Rewighted difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

E Sample selection

E.1 Impact of job displacement on sample inclusion

Figure E.1: Difference-in-difference estimates of the impact of job displacement on workers' presence in the income tax returns data

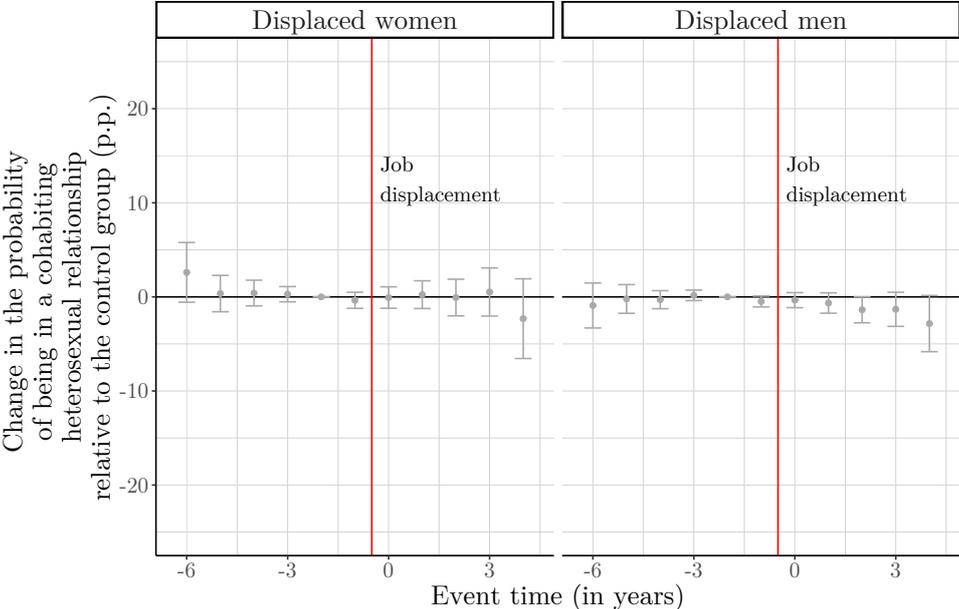


Rewighted difference-in-difference estimates of the impact of job displacement on workers' presence in income tax returns, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

E.2 Replication on the balanced panel

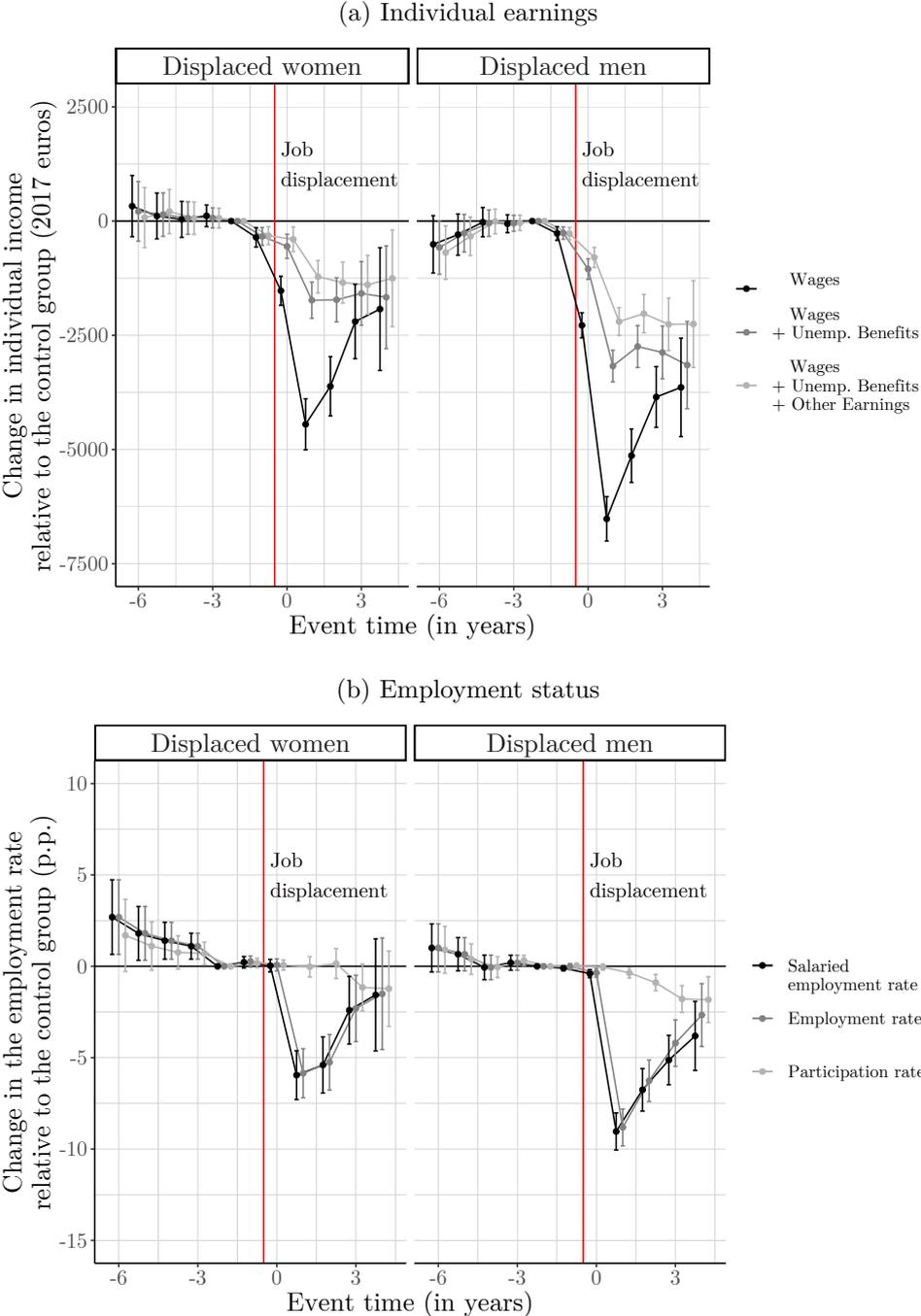
Figure E.2: Difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship (balanced panel)



Rewighted difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

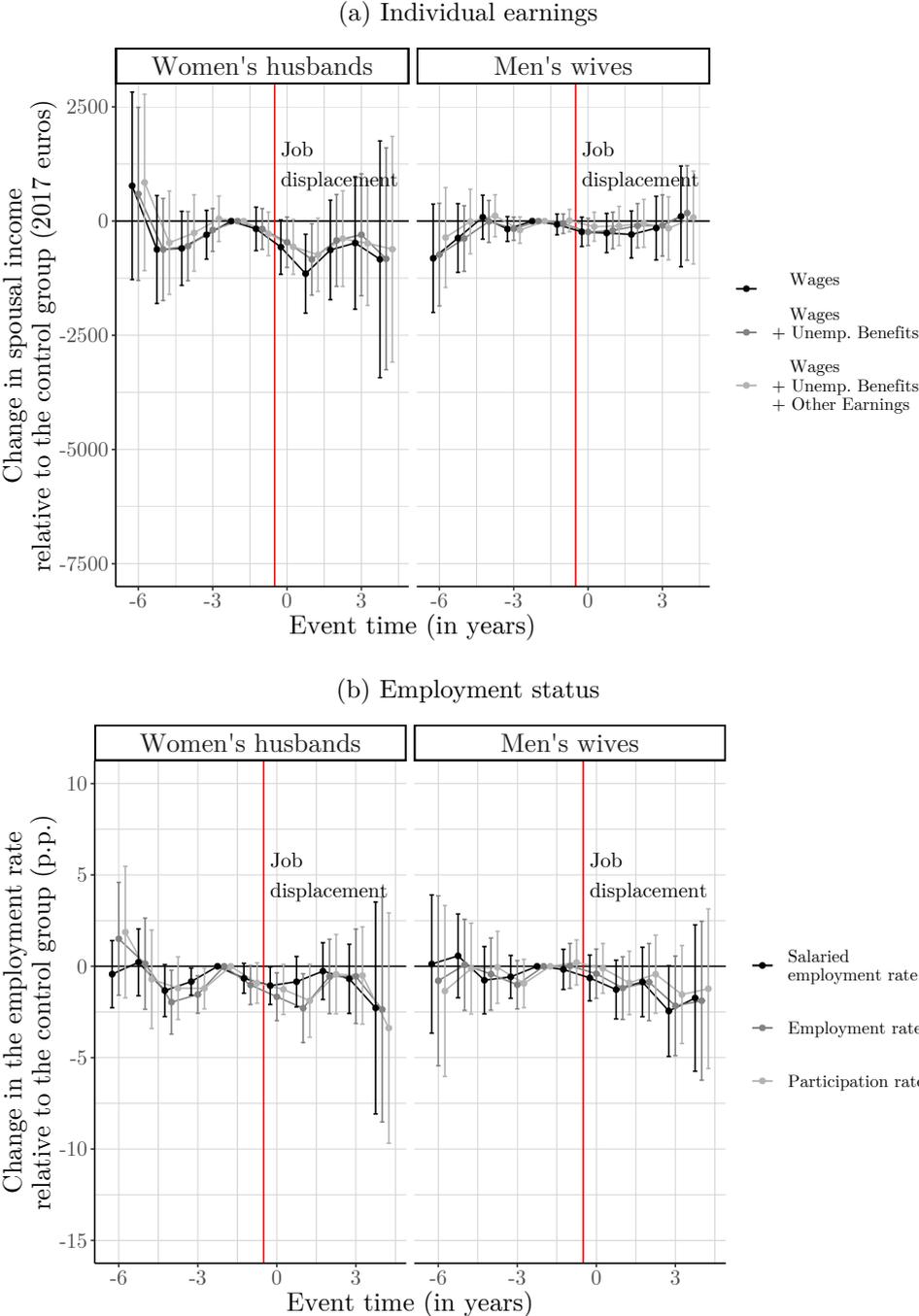
Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.3: Difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status (balanced panel)



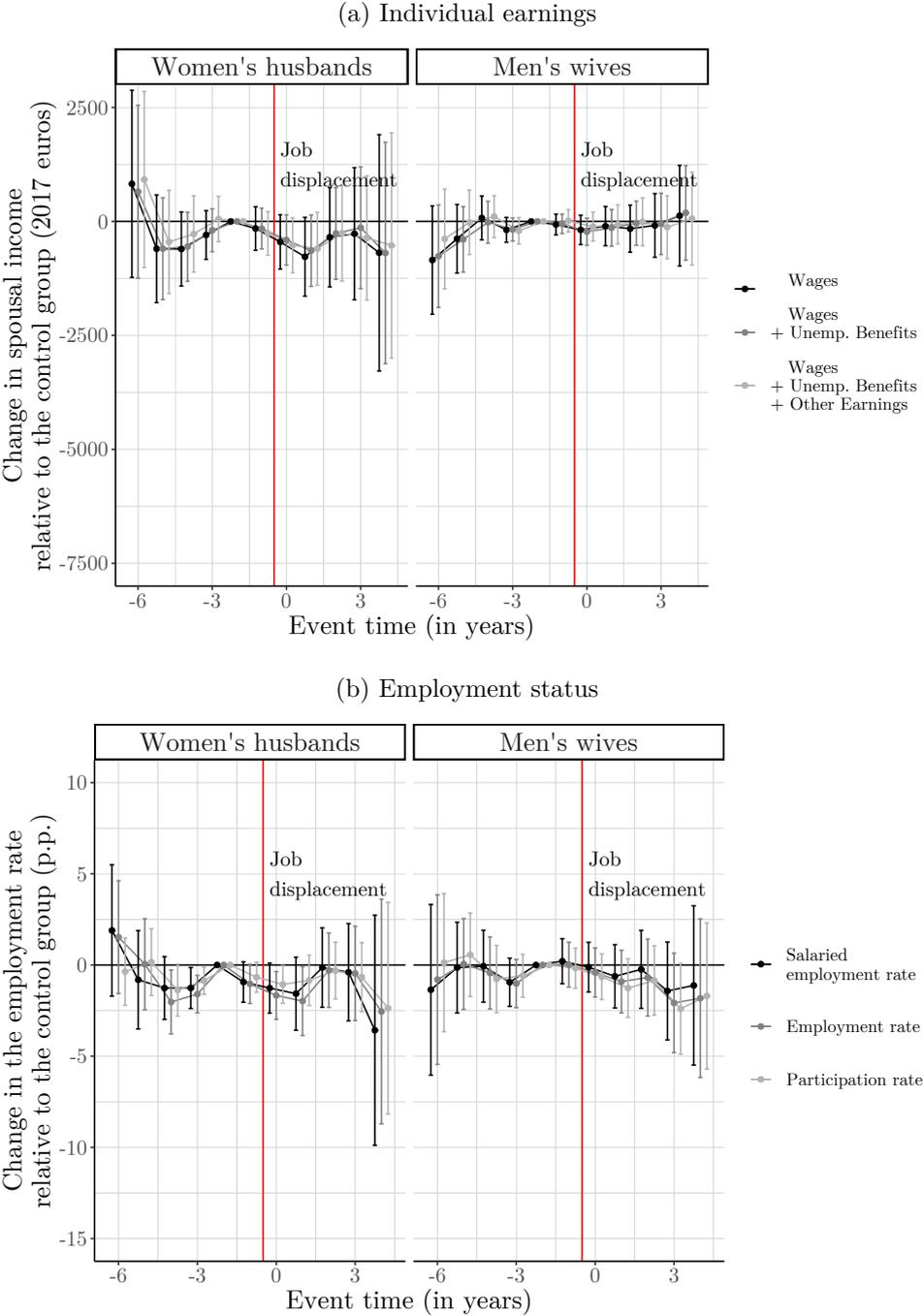
Rewighted difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.4: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status (balanced panel)



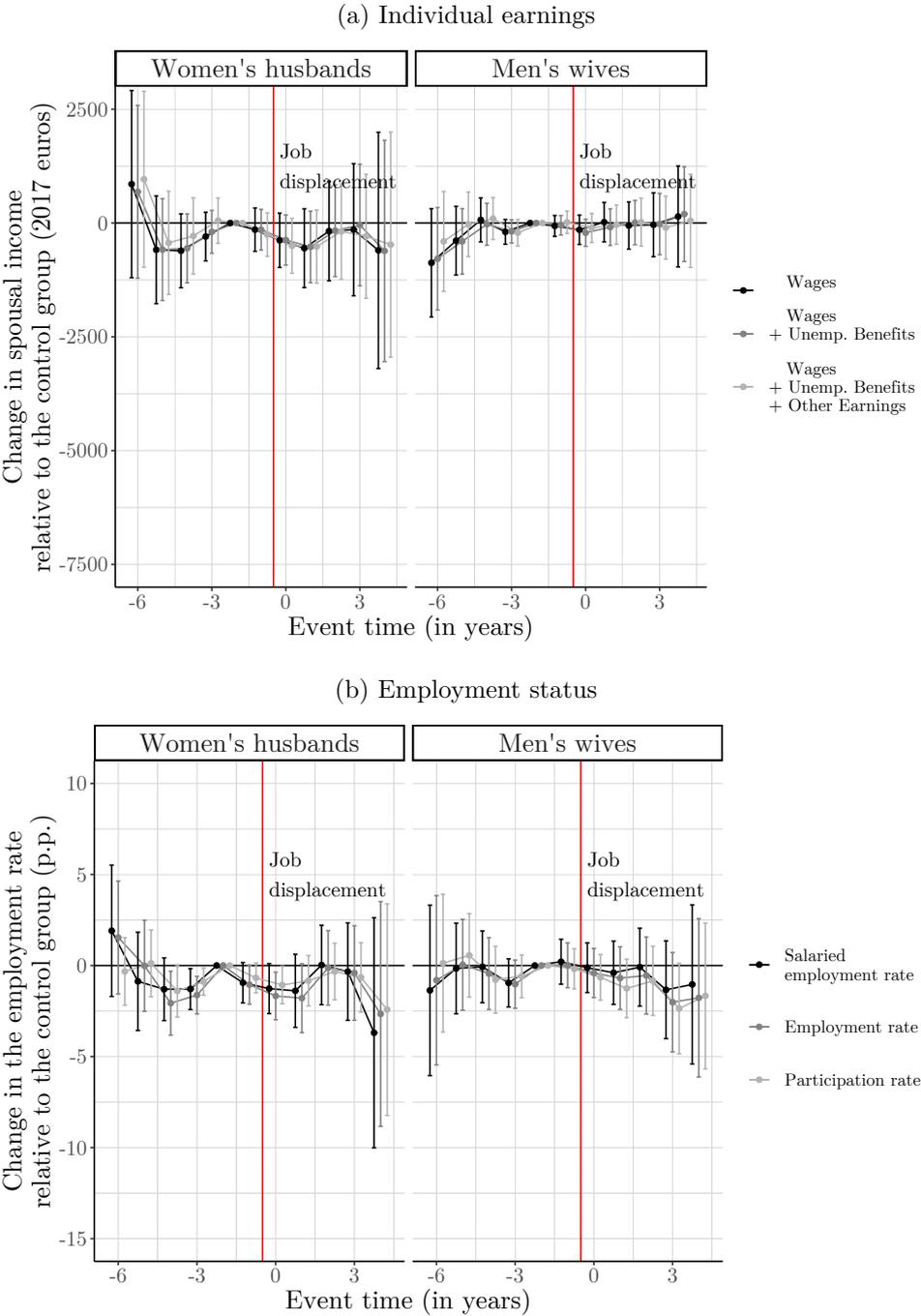
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.5: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the plant level (balanced panel)



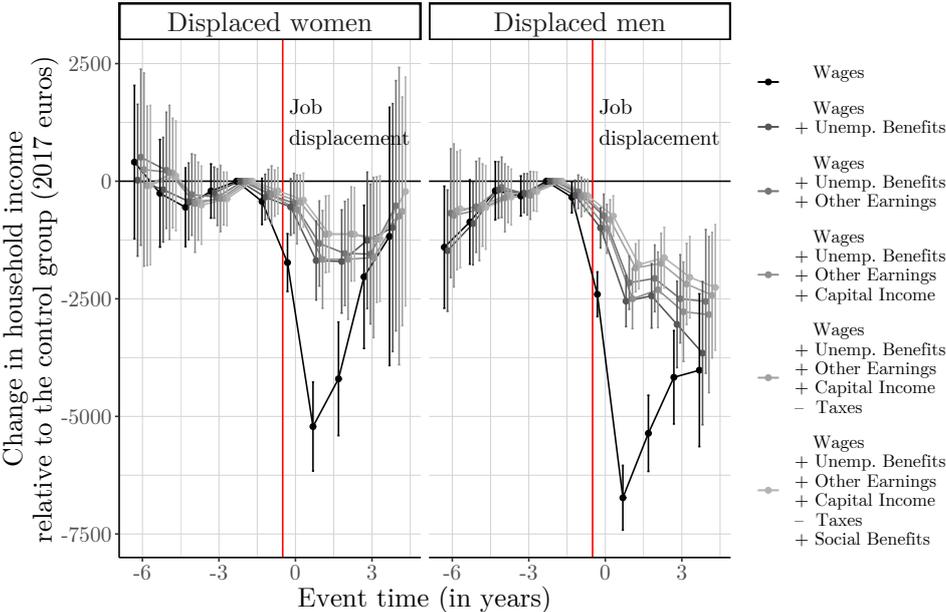
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.6: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the industry level (balanced panel)



Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.7: Difference-in-difference estimates of the impact of job displacement on workers' households' overall income (balanced panel)

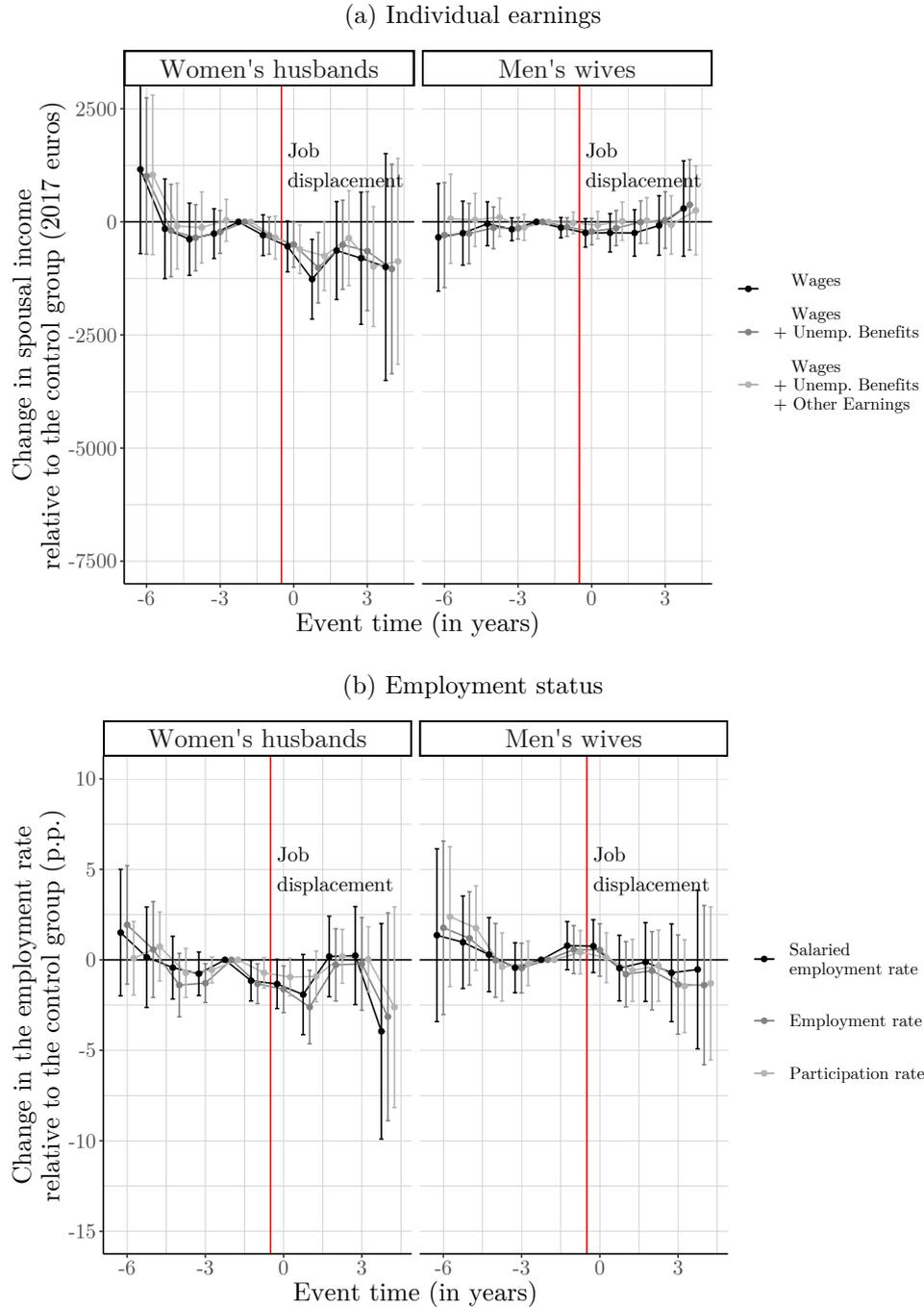


Rewighted difference-in-difference estimates of the impact of job displacement on workers' households' overall income, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

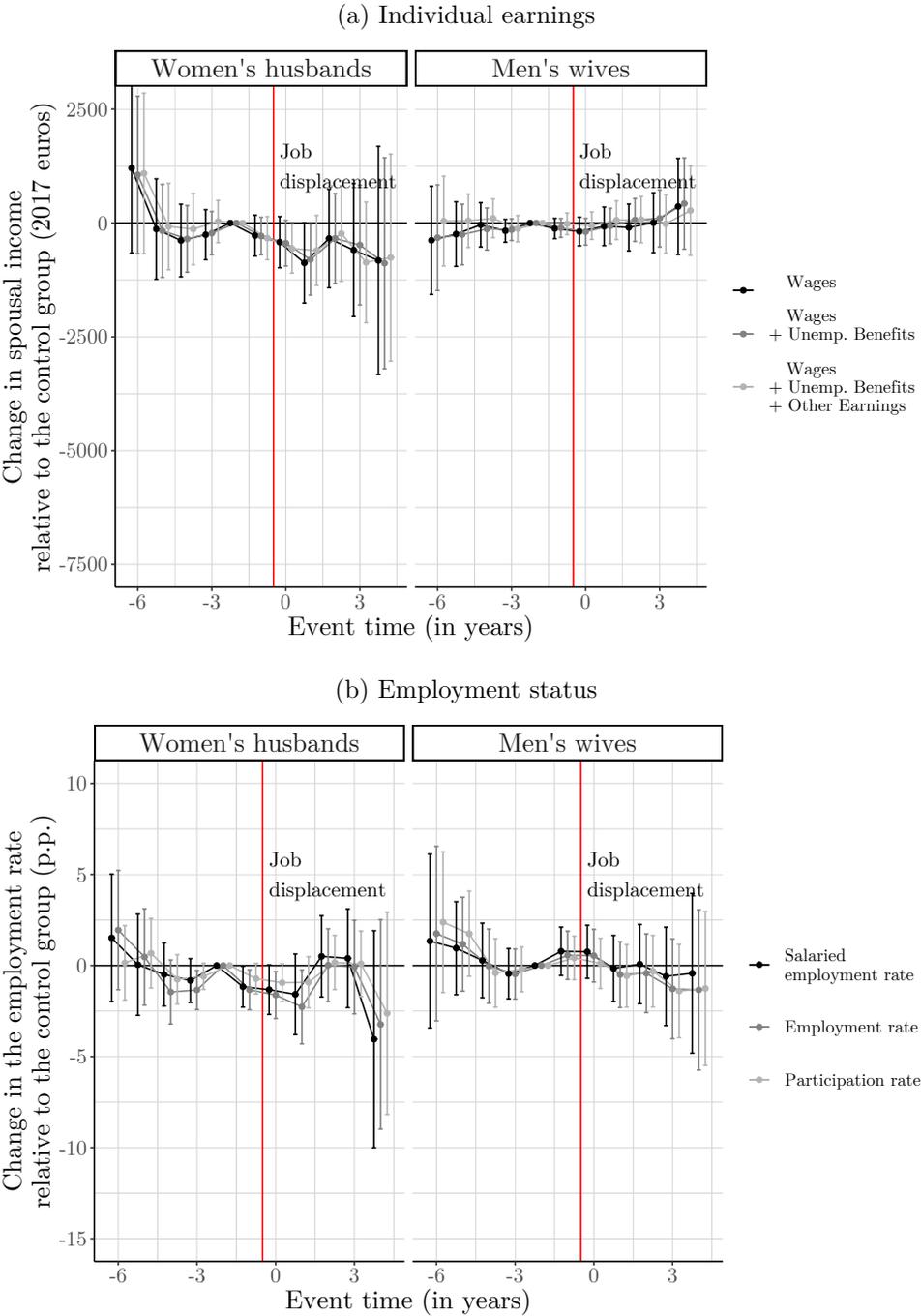
E.3 Replication on the balanced panel of spouses

Figure E.8: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status (balanced panel of spouses)



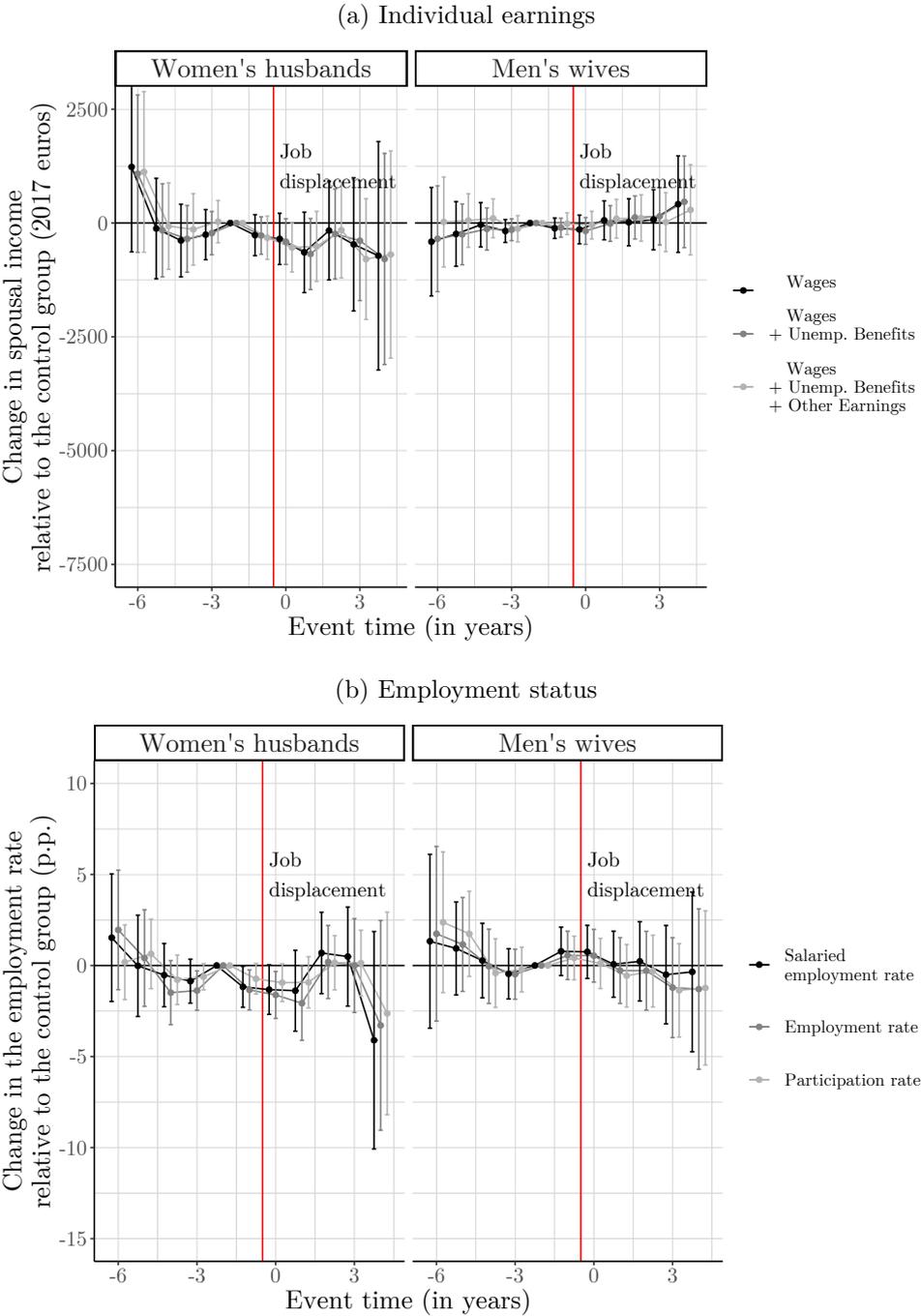
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.9: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the plant level (balanced panel)



Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure E.10: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the industry level (balanced panel)

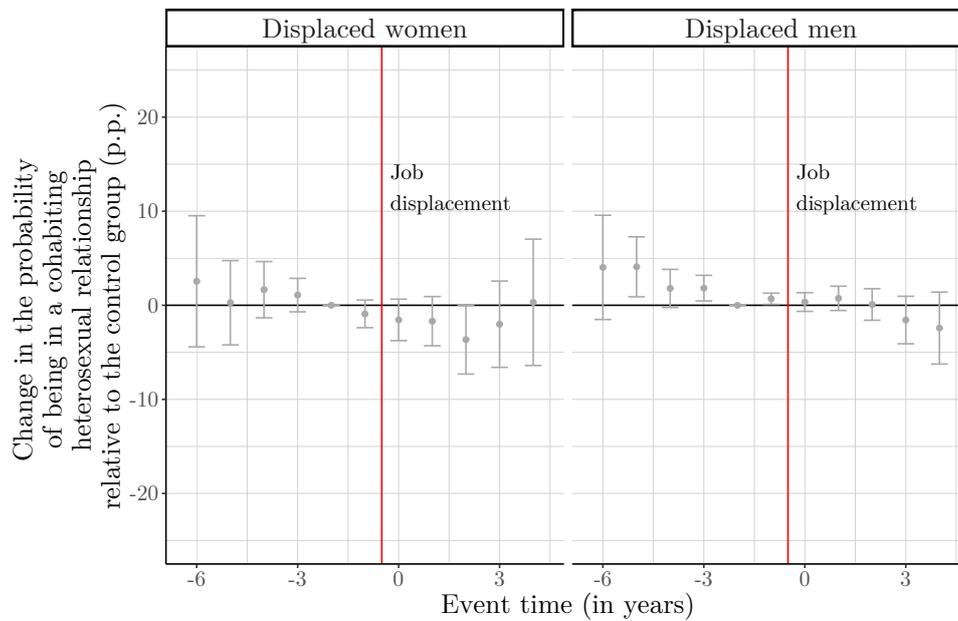


Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

F Selection into job displacement

F.1 Restriction to plant-closure events

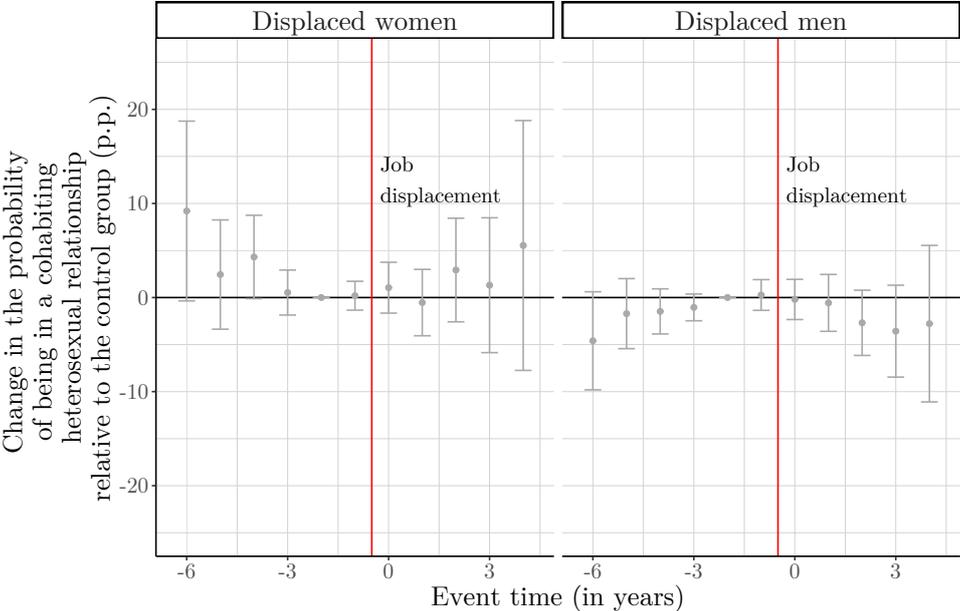
Figure F.1: Difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, for those who were before displacement (plant-closure events only)



Rewighted difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

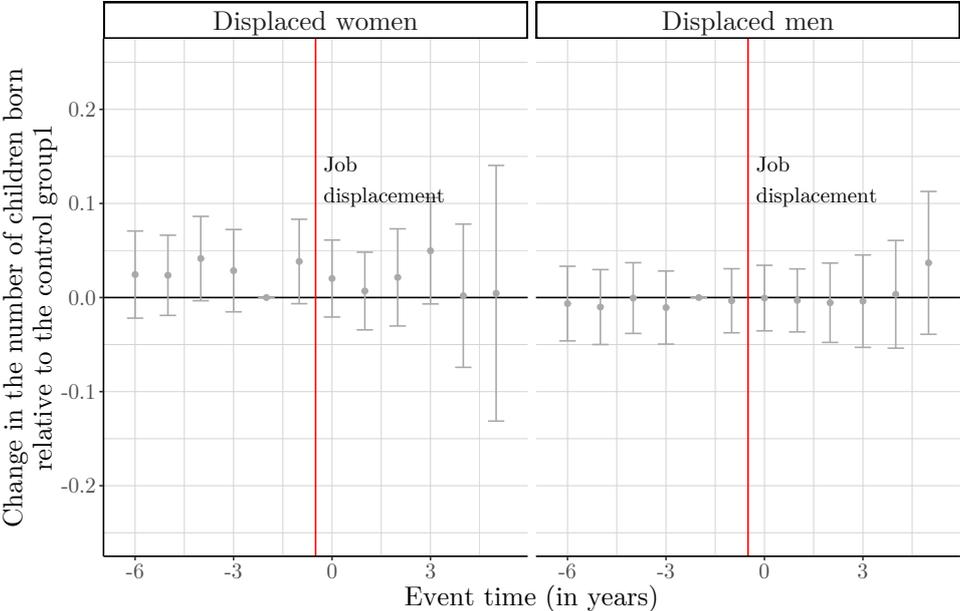
Figure F.2: Difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, for those who were not before displacement (plant-closure events only)



Rewighted difference-in-difference estimates of the impact of job displacement on the probability of being in a heterosexual cohabiting relationship, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

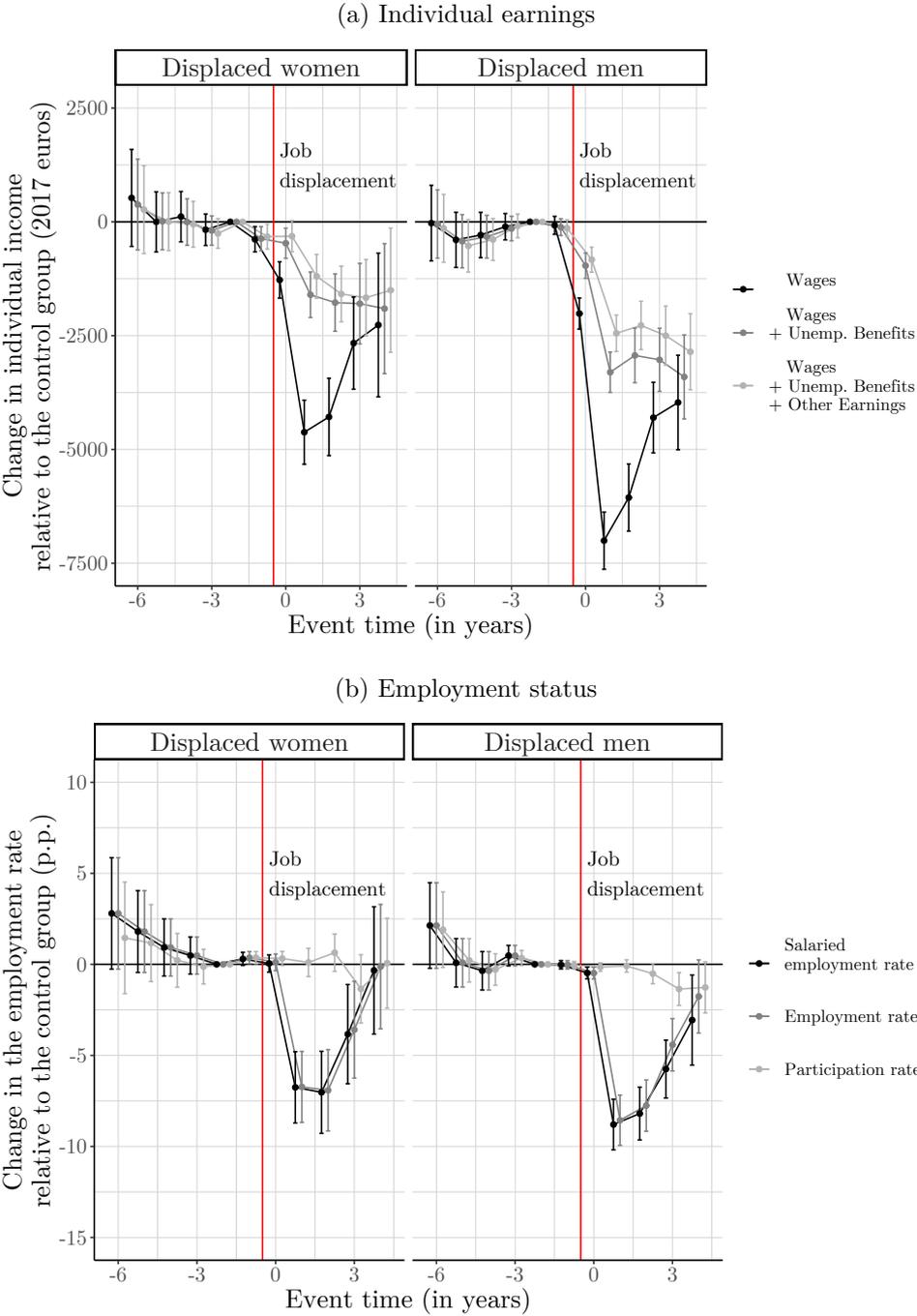
Figure F.3: Difference-in-difference estimates of the impact of job displacement on the number of children born to workers (plant-closure events only)



Rewighted difference-in-difference estimates of the impact of job displacement on the yearly number of children born to a worker, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

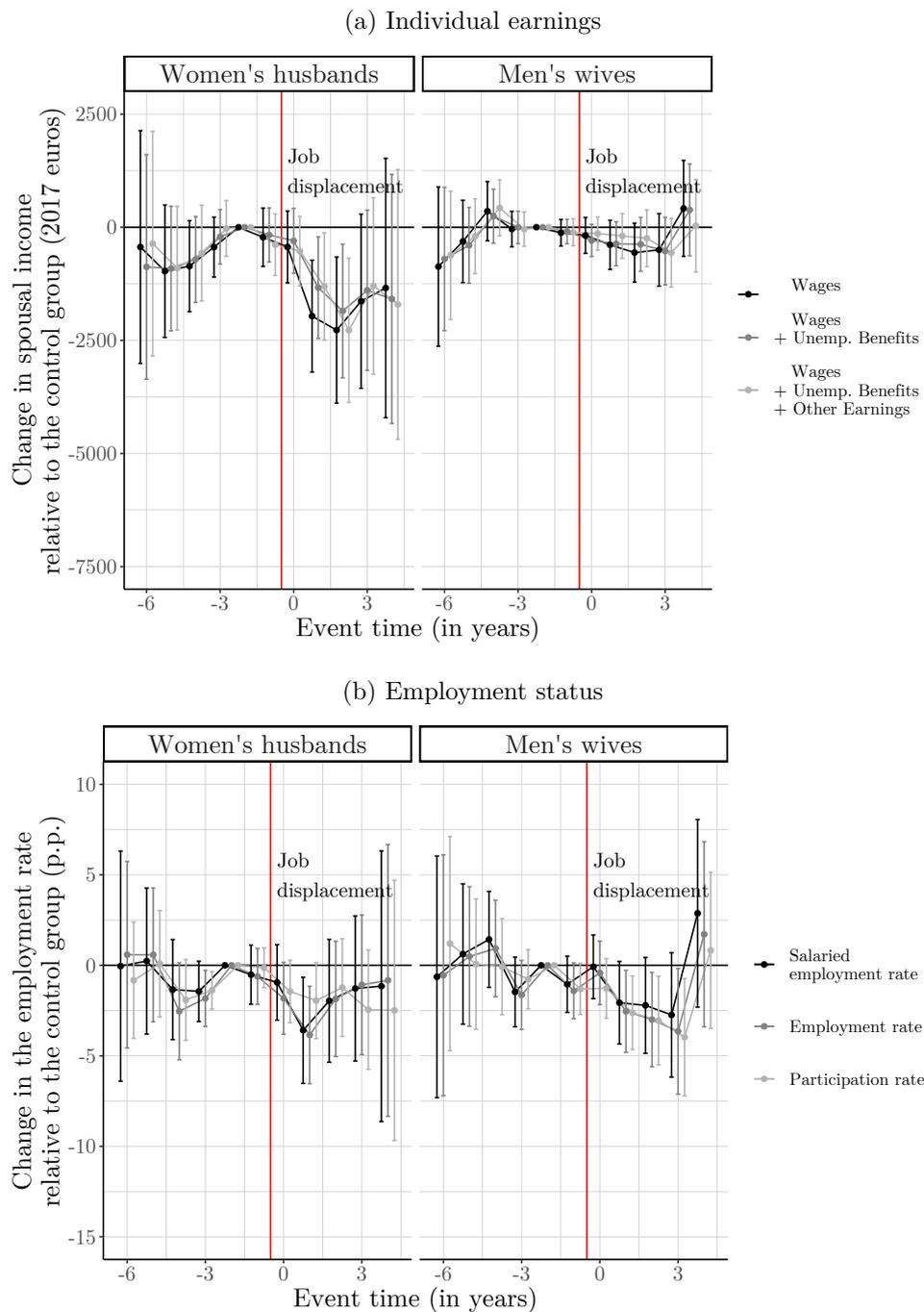
Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure F.4: Difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status (plant-closure events only)



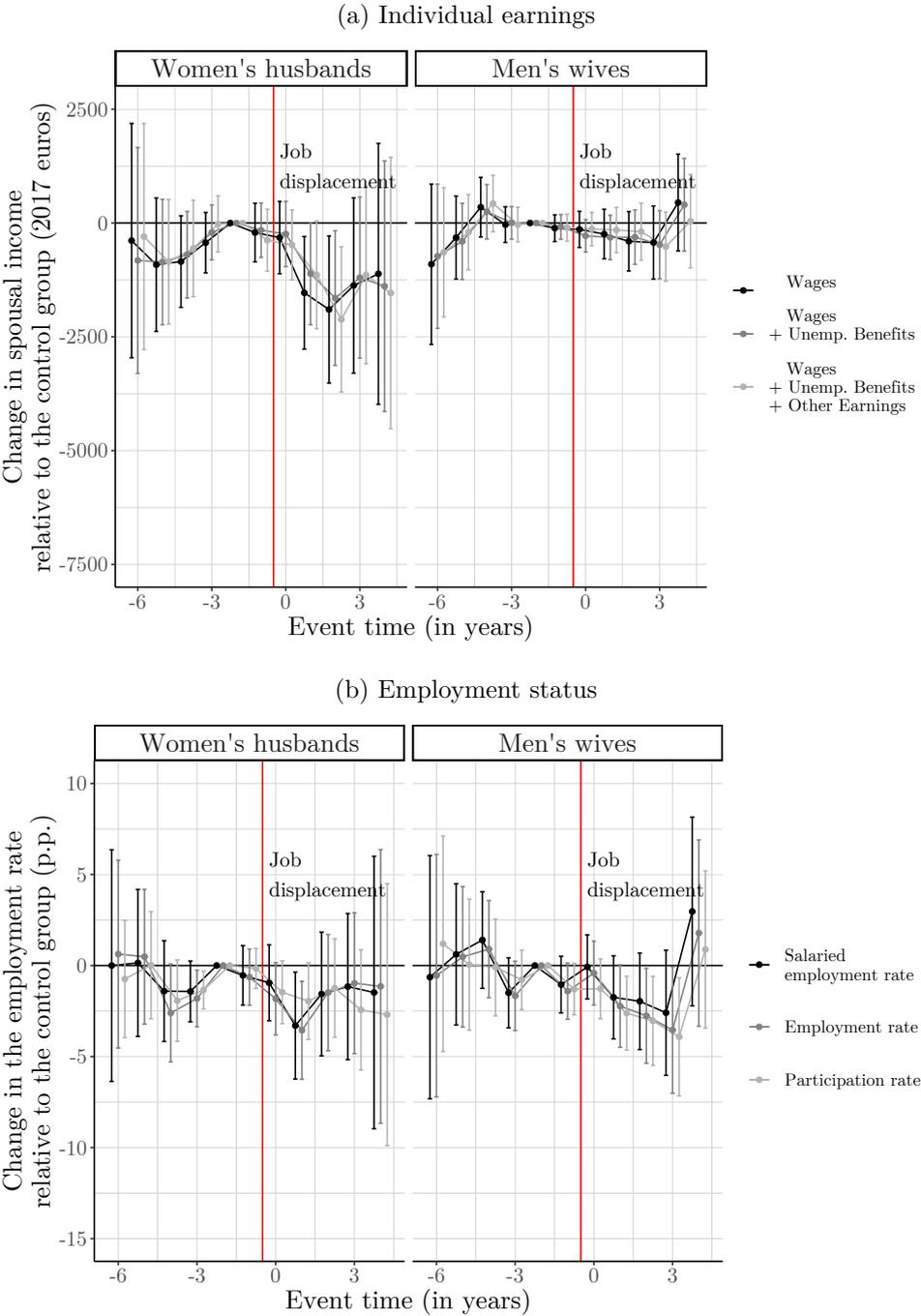
Rewighted difference-in-difference estimates of the impact of job displacement on workers' own individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure F.5: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status (plant-closure events only)



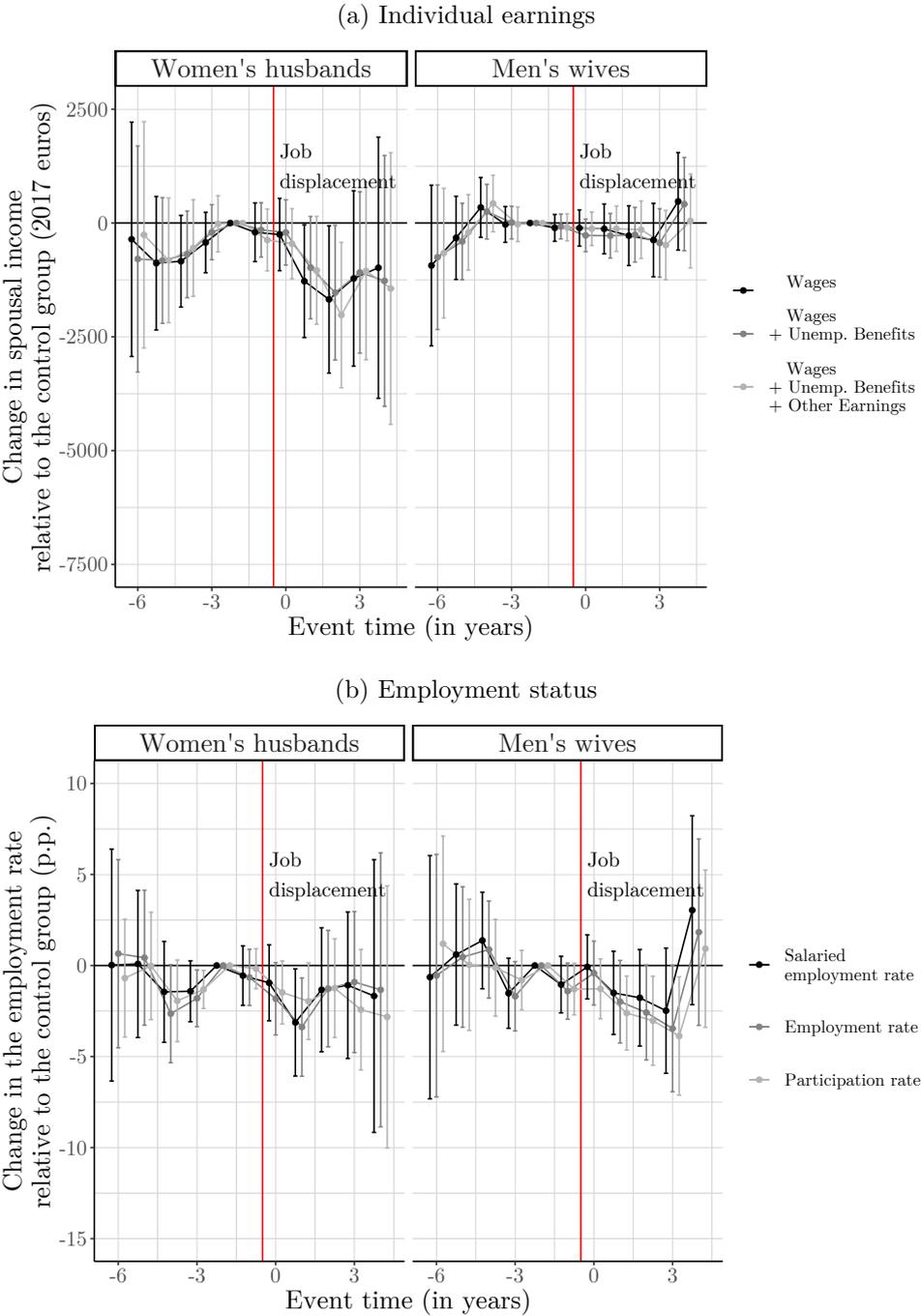
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure F.6: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the plant level (plant-closure events only)



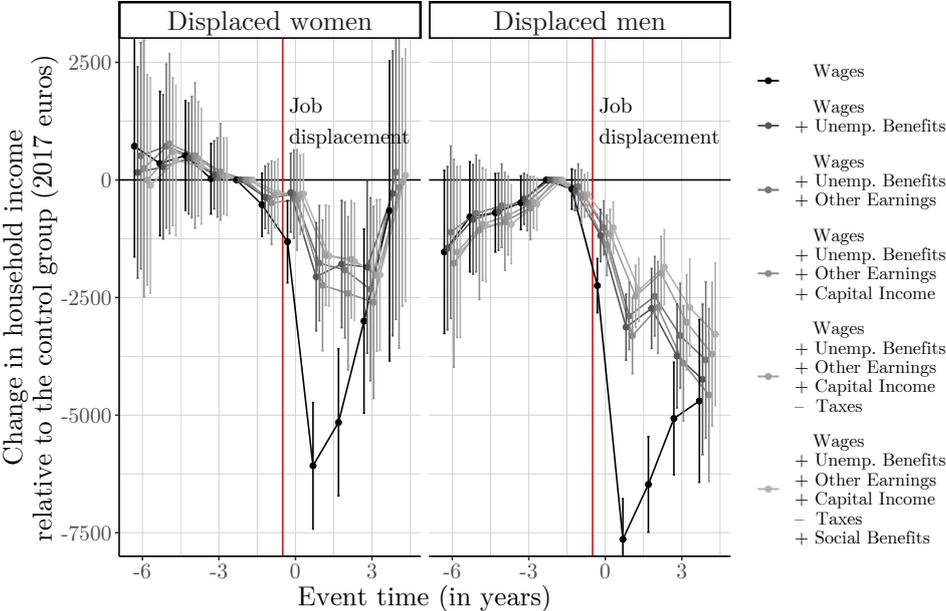
Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure F.7: Difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, with the discouraged worker effect correction at the industry level (plant-closure events only)



Rewighted difference-in-difference estimates of the impact of job displacement on workers' spouses' individual earnings and employment status, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications. *Source.* Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Figure F.8: Difference-in-difference estimates of the impact of job displacement on workers' households' overall income (plant-closure events only)



Rewighted difference-in-difference estimates of the impact of job displacement on workers' households' overall income, by gender of the displaced worker and time since job displacement. Standard errors are clustered at the plant level and estimated by bootstrap with 200 replications.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

F.2 Comparison with non-displaced workers of downsized plants

Table F.1: Summary statistics: occupation and family structure

Gender	Women		Men	
	Non-displaced	Displaced	Non-displaced	Displaced
<i># Individuals</i>	578	1,702	1,068	3,001
<i>a. Age*</i>				
Mean	36.6	36.3	37.5	37.4
St.D.	7.0	7.2	7.1	7.2
<i>b. Occupational shares (in %)**</i>				
Managers and professionals	18.5	23.8	16.7	25.1
Intermediate occupations	15.4	17.3	17.7	17.8
Non-manual workers	52.2	45.9	15.3	17.1
Manual workers	13.8	12.9	50.4	40.1
<i>c. Family structure (in %)**</i>				
Single	12.5	16.6	14.0	15.8
Childless couple	18.2	16.0	13.9	14.7
Single parent	8.5	11.9	5.9	5.7
Couple with children	55.5	48.1	58.9	57.1
Other families	5.4	7.4	7.4	6.7
<i>d. Number of children**</i>				
Mean	1.2	1.0	1.2	1.2
St.D.	1.1	1.1	1.2	1.2
<i>e. Yearly number of childbirths**</i>				
Mean	0.2	0.2	0.2	0.2
St.D.	0.4	0.5	0.5	0.5

* At the time of the (counterfactual job displacement). ** As observed two years before job displacement.

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table F.2: Summary statistics: displaced workers' salaried earnings and labor supply two years before separation (payroll tax data)

Gender	Women		Men	
	Non-displaced	Displaced	Non-displaced	Displaced
<i>a. Wages (in 2017€)</i>				
Mean	19,900	20,400	26,100	25,200
St.D.	12,100	13,300	14,900	15,600
<i>b. Days worked</i>				
Mean	340	340	340	340
St.D.	70	70	50	60
<i>c. Hours worked</i>				
Mean	1,560	1,580	1,770	1,760
St.D.	500	520	400	500

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table F.3: Summary statistics: displaced workers' earnings and labor supply two years before separation (income tax returns)

Gender	Women		Men	
	Non-displaced	Displaced	Non-displaced	Displaced
<i>a. Individual earnings (in 2017€)</i>				
Mean	23,500	23,600	25,900	28,700
St.D.	14,600	14,700	14,400	17,400
<i>b. Wages (in 2017€)</i>				
Mean	22,400	22,700	25,000	28,000
St.D.	14,300	14,700	14,00	17,500
<i>c. Unemployment benefits (in 2017€)</i>				
Mean	600	500	600	500
St.D.	2,100	1,900	2,200	1,900
<i>d. Other earnings (in 2017€)</i>				
Mean	400	300	200	300
St.D.	3,500	1,900	2,000	1,900
<i>e. Employment and participation rates (in %)</i>				
Employment	98.3	99.5	98.7	99.6
Participation	98.3	99.6	99.4	99.6

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table F.4: Summary statistics: displaced workers' spouses' earnings and labor supply two years before separation (income tax returns)

Gender	Women's male spouse		Men's female spouse	
	Non-displaced	Displaced	Non-displaced	Displaced
<i>a. Individual earnings (in 2017€)</i>				
Mean	33,000	32,500	15,200	17,800
St.D.	21,000	19,900	12,700	15,200
<i>b. Wages (in 2017€)</i>				
Mean	29,100	30,300	13,700	16,300
St.D.	21,400	20,300	12,700	15,200
<i>c. Unemployment benefits (in 2017€)</i>				
Mean	600	500	600	600
St.D.	2,400	2,200	2,000	2,200
<i>d. Other earnings (in 2017€)</i>				
Mean	3,300	1,600	800	800
St.D.	12,000	7,000	4,900	5,000
<i>e. Employment and participation rates (in %)</i>				
Employment	95.0	95.5	77.2	82.0
Participation	96.2	96.2	79.1	83.9

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.

Table F.5: Summary statistics: displaced workers' household income two years before separation (income tax returns)

Gender	Women		Men	
	Non-displaced	Displaced	Non-displaced	Displaced
<i>a. Disposable income (in 2017€)</i>				
Mean	46,400	46,900	45,800	46,700
St.D.	24,500	27,200	23,300	25,500
<i>b. Wages (in 2017€)</i>				
Mean	42,000	43,400	41,900	42,800
St.D.	25,600	29,200	24,000	26,800
<i>b. Unemployment benefits (in 2017€)</i>				
Mean	1,100	1,300	1,000	1,200
St.D.	3,100	3,500	2,900	3,100
<i>c. Other earnings (in 2017€)</i>				
Mean	3,700	3,400	3,200	3,400
St.D.	10,600	8,400	9,200	8,600
<i>d. Capital income (in 2017€)</i>				
Mean	4,600	4,500	4,400	4,500
St.D.	8,100	8,200	7,300	7,500
<i>e. Taxes (in 2017€)</i>				
Mean	-7,500	-8,100	-7,300	-7,800
St.D.	7,00	8,500	6,800	7,800
<i>f. Social benefits (in 2017€)</i>				
Mean	2,600	2,400	2,500	2,500
St.D.	3,700	3,500	3,800	3,800
<i>g. Position in the equivalent income distribution (in %)</i>				
First quintile	8.0	9.5	13.4	11.9
Second quintile	17.5	20.7	21.8	19.2
Third quintile	25.3	20.0	24.3	21.1
Fourth quintile	25.4	23.4	23.7	23.8
Fifth quintile	23.9	26.4	16.9	24.0

Source. Insee, DADS comprehensive files; Insee and Ministry of Finance (DGFIP), EDP sample.