Income or leisure? On the hidden benefits of (un)employment

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ARTICLE INFO

ABSTRACT

Do unemployed people benefit from more free time, while consumption is the sole motive for employed people to accept a life with less available time? Does this apply equally to men and women? To inform ongoing policy debates on how to address the problem of unemployment, we provide a comprehensive discussion of the traditionally assumed trade-off between income and leisure in labor supply decisions, which has been contested for a variety of reasons. Using rich German panel data, we compare non-employed individuals after plant closures with employed individuals regarding their subjective well-being obtained from time use and income. We find that increased non-working time through unemployment translates into higher free-time satisfaction, while also improving satisfaction with family life, as a possible hidden benefit of being unemployed when having more time for home production. Meanwhile, there is a strong decline in satisfaction with household income, especially among unemployed men, which cannot be explained solely by a lower level of income. We inspect the role of social norms in this context and argue that individuals obtain identity utility through having a job that ensures being self-sufficient, as a hidden benefit of employment. Based on studying unemployed workers transitions into retirement and a separately conducted survey experiment, we provide strong evidence on the importance of the social norm of earning a living from work. Our experiment also shows that home production is a way of mitigating the pressure to comply with this norm, which is true for men and women alike.

1. Introduction

Most social sciences emphasize the importance of work for the human being as a more meaningful and socially valuable activity than leisure (e.g., Jahoda 1981; Blustein 2013; Budd 2019). Notably, however, economists have traditionally upheld a very different view of the labor market, describing a trade-off between work and leisure. According to standard microeconomics, work itself yields a disutility and must be compensated with income, as the main motive for individuals to supply work. On the other hand, additional leisure is a clear benefit of being jobless, which, by assumption, creates the trade-off that people are facing in the labor market: either, one supplies hours of work for income or abstains from the labor market to enjoy leisure. This notion has been the basis for many labor market concepts and policy changes designed to tackle the problem of unemployment, ranging from economic reforms in the US and the UK in the 1980s and 1990s (e.g., Dolowitz 2002), to the 'Hartz' reforms of the German labor market (e.g., Krebs and Scheffel 2013),

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as well as to current policy debates.[1] Given the high policy relevance and contradictory views among experts inside and outside the academic profession, the aim of this article is to empirically answer the question of whether unemployment indeed reduces the utility from income, while increasing the utility from leisure.

Both the assumptions that additional leisure time is beneficial for the unemployed and that reduced income is the main negative aspect of unemployment are questionable. First, work hours are at historic lows in many industrialized countries (Huberman and Minns 2007; Giattino et al., 2020), allowing people to enjoy leisure activities despite having a job. Additional free time might thus be of little value to them, providing no additional utility when they become unemployed. Empirical support for this comes from 'the economics of happiness' (for reviews, see, e.g., Frey and Stutzer 2010; Weimann et al. 2015; Nikolova and Graham 2021). In this literature, unemployment is shown to be a major negative shock severely reducing people's happiness above and beyond the loss of consumption opportunities (e.g., Clark and Oswald 1994; Kassenboehmer and Haisken De-New 2009), with a magnitude much higher than that of other life events, such as marriage, divorce or birth of a child (Clark et al. 2008).[2] Second, another important finding of this literature challenges the notion of income as a key factor in labor supply decisions: unemployment reduces life satisfaction substantially, even when income changes are accounted for (Winkelmann and Winkelmann 1998; Knabe and Rätzel 2011). As a result, the happiness literature characterizes the contribution of the income loss to the overall well-being effect of unemployment as small. Furthermore, the usually assumed trade-off between income and leisure often ignores the role of gender, which seems questionable in light of persistent gender inequalities in labor markets and growing evidence on the role of heterogeneous preferences and constraints in this context (e.g., Booth and van Ours 2008; 2009; 2013; Bertrand et al. 2015; Cook et al. 2021; Bolotnyy and Emanuel 2022). We conclude that there are good reasons to question the relevance of commonly assumed determinants of labor supply used in economic models.

To ascertain how much income and leisure actually matter in the utility from working and hence labor supply decisions, we employ several empirical approaches that have not been used so far for this purpose. Our empirical investigation is based on theoretical considerations in which we augment utility as a function of consumption and leisure with an identity utility component (Akerlof and Kranton 2000). We understand paid market work as a means to achieve another objective besides consumption, by allowing individuals to be self-sufficient: Individuals obtain identity utility if they are able to comply with the social norm not to live off others. An important implication is that the source of income matters. Self-earned market income (employment) generates higher overall utility than the same level of income from financial assistance by the government (unemployment). This could be a particular issue for men if they feel a need to conform to the traditional role as breadwinner. We argue that direct measures of the utility from leisure and income, the latter capturing the roles of both consumption and the source of income, help reveal the significance of these factors in people's lives when employed or not.

Our first empirical investigation makes therefore use of specific satisfaction data about how satisfied people are with their household income and their free time, depending on their employment status. We thereby give individuals in both employment and in non-employment a voice to inform us as researchers about the utility that they derive from these components of well-being. Importantly, despite the enormous amount of work on the well-being of the unemployed across disciplines, both pieces of information on income satisfaction and free-time satisfaction have not been used so far for our research objectives. We obtain them from a rich longitudinal dataset, namely the German Socio-Economic Panel (SOEP) study. This annual investigation into the lives of thousands of individuals is designed to be representative for Germany, the EU's largest labor market, which facilitates studying potential gender differences in the significance of income and leisure for people's well-being.[3] As further features, the SOEP includes information on actual income levels and daily time allocation, in addition to subjective assessments that capture how people perceive these factors in their lives. This allows us to draw a comprehensive picture of the consequences of unemployment. Of special interest for the empirical identification are exogenously triggered employment transitions, for which we focus on plant closures reported by SOEP participants. Following previous research studying plant closures to yield credible evidence on the implications of unemployment, we leverage the rich survey data and employ a matching approach (e.g., Marcus 2013; 2014).

We find that individuals entering non-employment obtain a substantial increase of self-assessed satisfaction with free time,

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[1] As an example of the focus on monetary incentives in this context, Austria's Minister of Labor, Martin Kocher, spoke out against an increase of unemployment benefits in an interview with *Der Standard* in January 2021. He referred to the 'well-known truism' that such a policy would prevent unemployed workers from taking up a job. This policy example relates to the ongoing academic discussion about the notion of voluntary vs. involuntary unemployment in economics (see, e.g., Christiano et al. 2021).

[2] While economists have increasingly contributed to this research, there is an even larger body of work from other disciplines such as sociology and psychology on the well-being of the unemployed, starting with Jahoda et al. (1933). For reviews, see McKee-Ryan et al. (2005); Luhmann et al. (2012) and Gedikli et al. (2023). According to meta-analyses by Paul and Moser 2009, the share of people with mental health problems is twice as high among the unemployed compared to the employed. Note that psychologists measure well-being more broadly than economists, for instance by including several indicators of subjective well-being, mental health and physical health. For studies focusing on the impact of unemployment on health-related outcomes, see, e.g., Eliason and Storrie (2009); Sullivan and von Wachter (2009); Böckerman and Ilmakunnas (2009); Schmitz (2011); Drydakis (2015); Cygan-Rehm et al. (2017); Vodopivec et al. (2021). Apart from these research strands, there is also a large body of work by economists on the monetary effects of unemployment. For recent studies on the consequences of job loss for earnings, see, e.g., Farber (2017); De Fraja et al. (2021) and Schmieder et al. (2023).

[3] In a recent contribution to the policy debate on the role of gender in the labor market, Ilieva and Wrohlich (2022) use SOEP data to document that the gender gap in employment rates has reduced markedly from roughly 40 to 25 percentage points between 1984 and 2020 in Germany. The part-time employment gap has largely remained the same, as the rate of part-time work is still 40 percentage points higher among females than among males. The gender pay gap has closed by one third, although the trend has been much less clear in more recent years.

compared to those workers who experience no plant closure and stay employed. Interestingly, our time use analysis reveals that the loss of work is accompanied by only a relatively small increase in time spent on enjoyable leisure activities, such as hobbies. Instead, the unemployed spend more of their extra time on 'useful' leisure activities, such as childcare, which we aggregate under the label of home production. Arguably, this could improve their situation in non-work-related areas of life, as a hidden benefit of unemployment. In line with this conjecture, we find a substantial positive effect of becoming unemployed on satisfaction with family life, as an indicator of family-related well-being.[4] Interestingly, we do not see gender differences in satisfaction outcomes that could indicate gender-specific benefits from having more time for family and household activities.

At the same time, we find that unemployment leads to tremendous dissatisfaction with household income. While our analysis of mechanisms indicates that the premium in free-time satisfaction directly results from having more time, this is different for income satisfaction where the reduction in actual income levels fails to explain most of the negative impact of unemployment. The conclusion that monetary effects alone are unable to explain the severe dissatisfaction with income does not change when we additionally consider the implications of being jobless for permanent income, as part of our additional analyses. Instead, our results point to the idea that the source of income matters, as unemployed individuals lose a hidden benefit of employment in the form of identity utility from complying with the norm not to live off others. As an important part of our study, we also examine the role of gender in this context to learn more about social norms in the labor market. We find that the dissatisfaction with income is more pronounced among unemployed men, which points to traditional gender roles.

Finally, we compare the income satisfaction of unemployed and employed people who retire, based on an approach by Hetschko et al. (2014), providing indirect evidence on social norms. We find a positive retirement effect on satisfaction with household income among formerly unemployed men and women that cannot be explained by rather negligible changes in their actual income levels. This conforms to the idea of a non-monetary effect when unemployed people leave working age, so that identity utility is restored.

Similar to the previous research, our SOEP-based analysis provides indirect evidence on the importance of the social norm not to live off others and hence identity for the well-being effects of (un)employment. By means of a second empirical investigation, we complement the analyses of household panel data with a survey experiment that allows us to directly identify the strength of the social norm. Based on a sample of more than 1000 German online participants, we measure how socially appropriate the broader population deems the job take-up behavior of a hypothetical person who became involuntarily unemployed due to a plant closure and who can reject or accept a job offer. In order to minimize the role of monetary considerations in the evaluation of the hypothetical job offer, the description of the scenario makes it clear that taking up employment hardly affects the actual level of available income. However, it does affect whether the person lives of transfers from others (staying unemployed) or not (taking up employment). Building upon the approach to measuring social norms proposed by Krupka and Weber (2013), we incentivize the participants of our experiment to report their expectations of what most participants deem appropriate for an unemployed person's job take-up behavior in order to elicit the social norm not to live off others.

As a first dimension in our 2 × 2 × 2 experimental design, we randomly vary the labor supply decision itself to identify a possible gap in the social appropriateness of accepting versus rejecting the job offer. By doing so, we assess the relevance of the social norm against living off others as a potential explanation for the income satisfaction effects of unemployment. Second, the time use in unemployment of the hypothetical decision-taker varies randomly: It takes the form of either leisure enjoyment or home production. This informs us about the extent to which society appreciates the productive use of free time by unemployed people, referring to the hidden benefit of unemployment revealed in our panel analysis. As a result, home production could be a way of reducing the social pressure to supply market work. Third, we vary the gender of the decision-taker in each case to find out about gender-specific social-norm effects and hence a possible role of the traditional male breadwinner model in the impacts of unemployment on income satisfaction.

Our experiment provides evidence for a strong social norm against living off others. While people consider it socially appropriate to accept a job that promises no significant benefit other than independence from public transfers, refusing such a job offer is seen as socially inappropriate. The gap in the appropriateness between these two decisions reflects the social pressure to work for a living, which can be reduced if the unemployed engage in home production. Engaging in seemingly productive non-market work appears to mitigate the perceived inappropriateness of refusing to supply market work. The gender of the hypothetical decision-taker also matters for the strength of the social norm, although our results do not consistently support the concept of the traditional breadwinner model. While men are more strongly expected than women to supply market work if they spend their time on pure leisure in unemployment, this gender difference disappears in the home production treatment. Here, both male and female workers experience similar relief from the pressure to supply market work. Furthermore, when being asked about their own choices in regard to the trade-off between earned income and leisure, the participants confirm all of our results based on the social appropriateness, except that females report taking up the job more often than males, which is also true in the home production treatment. Overall, our findings point to a convergence of gender-related norms in relation to market work and non-market work.

Our paper contributes to the literature in the following ways. First, we introduce a behavioral perspective into the discussion of labor supply decisions, which has so far focused mainly on the role of standard economic incentives to encourage unemployed people to take up a job. By providing evidence on hidden non-monetary benefits of both employment and unemployment, we confirm a need to expand the understanding of individual choices in the labor market and put forward identity as a potentially important motive. Second, we contribute to the research on subjective well-being by reconciling some of the key findings in this literature with the

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[4] Recent research based on SOEP data shows that satisfaction with family life is a meaningful predictor of future domestic events, such as childbirths, and can hence be seen as an economically relevant indicator of the well-being obtained from this life domain (Chadi and Hetschko 2021).

theoretical understanding of labor supply in economics. To provide causal evidence on the social norm to earn a living from work, we go beyond indirect measures usually preferred in happiness research by introducing an experimental approach that directly identifies the level of social pressure to comply with the norm. As a third contribution, we add to the research in behavioral economics by providing a policy-relevant application of a methodological concept used in experimental research to identify social norms. In the process, we show how social pressure could translate into behavior, as we link the evidence on social appropriateness perceived by individuals with their own hypothetical labor market choices. Fourth, we contribute to the research on gender inequalities in the labor market by highlighting the role of social norms in this context. While some of our results are consistent with traditional gender roles, the evidence also points to societal trends towards more gender equality when it comes to norms in the labor market.

Apart from our contributions to ongoing research, the results of our investigation also yield policy implications which we discuss towards the end of the paper in the concluding Section 6. Beforehand, in Section 5, we present the survey experiment. The results of the panel analysis are reported in Section 4, while the data and the methods that we use are described in Section 3. To begin with, we continue our discussion of theoretical considerations in the subsequent Section 2, thereby establishing the basis for our empirical agenda.

2. Theoretical considerations

In the following, we discuss the theoretical approach to labor supply in economics in light of various areas of research. In particular, we attempt to reconcile the standard microeconomic model of labor supply with conflicting findings from research on subjective well-being by augmenting the textbook notion of utility from working, comprising the two determinants of consumption and leisure, with an identity utility component. Following time use studies, we consider leisure in a broad sense, as the best possible allocation of time devoted to non-work activities (e.g., Graff Zivin and Neidell 2014). Besides purely enjoyable leisure activities (leisure in a narrow sense), this may include useful activities, such as further education or home production (e.g., Gronau 1977; Rupert et al. 2000; Burda and Hamermesh 2010).

The starting point for our discussion is that previous research suggests that the individual costs of unemployment are largely non-monetary in nature. In a recent study, Hussam et al. (2022) show experimentally that there is a significant non-monetary benefit of employment, which complements the above-mentioned insights from happiness research, contradicting the notion that income from labor is the primary reason why people work. Also the supposedly positive role of leisure for utility appears questionable if unemployment causes substantial non-monetary costs instead of the gains suggested by the traditional labor supply model (Rätzel 2012; Bryson and MacKerron 2017). This has inspired some economists to consider ways to resolve the potential conflict between theory and empirics, for example by analyzing long-run income effects in a macroeconomic framework (Bayer and Jüssen 2015).

That being said, there could be another strong non-monetary impact reducing overall utility levels so much that the benefit of unemployment in terms of leisure utility is real, but hidden. In fact, other findings in the well-being literature point to a potential increase of utility from leisure in connection with joblessness. Research on momentary happiness shows that most leisure activities, including home production, are more pleasurable than work-related activities (e.g., Kahneman et al. 2004a; Krueger and Mueller 2012; Bryson and MacKerron 2017). As a result, the unemployed enjoy overall more pleasant weekdays than the employed (Knabe et al. 2010). In line with this, recent research shows that watching television, as quantitatively the most important leisure activity for many people, can affect life satisfaction positively (Chadi and Hoffmann 2021). It would also seem that worker well-being can benefit from reductions of work hours (e.g., Lepinteur 2019).

In a similar fashion, analyses of people's time use reveal that employed individuals spend much time not working while at work (Burda et al. 2020). This indicates some form of cost associated with working and, hence, benefits from leisure time. There may be additional benefits from being unemployed, as freed-up time can be reallocated to useful activities, such as home production. In the process, unemployment might resolve work-family conflicts that originate from time scarcity (e.g., Berg et al. 2003; Michel et al. 2011). The idea of increased home production among the unemployed has largely been overlooked in the empirical literature (see Hennecke and Pape 2022 for a recent exception).

If being out-of-work increases the utility from leisure time, which non-monetary component in overall utility levels may over-compensate this positive influence in the life of the jobless? The literature on the psychological effects of unemployment has looked at a variety of possible candidates in this context. For example, Jahoda (1981) discusses a potential lack of structure in the lives of the unemployed as well as the loss of social contacts outside the nuclear family. As another candidate to explain why people may suffer severely when being out of work, the research on subjective well-being has paid much attention to the concept of a social norm to work which the unemployed fail to meet.[5] In pioneering studies, Clark (2003) as well as Stutzer and Lalive (2004) show that jobless people report higher well-being in regions where the social norm to work is relatively weak. A crucial aspect of this social norm is the idea that one should provide for oneself and one's family through the fruits of one's own labor rather than subsist on the assistance of others (Chadi 2012; 2014; Elster 1989; Hetschko et al. 2020; Stavrova et al. 2011). Accordingly, the norm to work essentially proscribes living off others, which could have gender-specific implications. Indeed, unemployed men suffer more from unemployment than women in regions where the unemployment rate is comparatively low, presumably because the local norm to work is particularly strong (e.g., Clark 2003; Howley and Knight 2022).

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[5] The psychological literature is, by and large, supportive of the reasons proposed by Jahoda (1981) to explain the misery of the unemployed. However, the evidence for a role of identity in this context is mixed (Paul and Batinic 2010). In contrast, the economics literature focuses on non-compliance with social norms, which, as we argue, is strongly related to identity from a theoretical standpoint, while other reasons are deemed less important (Hetschko et al. 2021; Hussam et al. 2022).

In order to theoretically consider the social work norm as an explanation for differences in well-being between unemployed and employed individuals, we follow Hetschko et al. (2014) who propose introducing the concept of identity utility by Akerlof and Kranton (2000) into the debate about the well-being effects of unemployment. The identity utility model incorporates the social psychology theories of social identity and self-categorization (e.g., Tajfel and Turner 1986) into economic reasoning. In what follows, we go beyond previous work by showing that these ideas can be incorporated in an extended model of labor supply.

According to the basic identity utility framework by Akerlof and Kranton (2000), the utility (V_j) of individual j can be split into a standard component (u_j) and an identity utility component (i_j). The standard component u_j conforms to the standard model of labor supply. It positively depends on consumption C_j, which is an increasing function of the exogenously given wage rate, w, and work hours, H_j. H_j equals $T - L_j$, with T being the time budget and L_j denoting hours of leisure ($T > 0$, $T \geq L_j \geq 0$). u_j moreover increases with leisure. Identity utility i_j depends on the social group (also referred to as 'identity') the person aims to belong to (K_j). Hetschko et al. (2014) argue that people of working age aspire to be part of the group of able-bodied members of society. We consider the norm to make a living from work the relevant prescription (P) of this social group, which is beyond the individual's control. Other determinants of identity utility are the person's actions (observable outcomes), whereby we focus on earning money from work (paid work hours), wH_j, as a means to meet P and exogenously given characteristics (ε_j).[6] A question we address empirically is whether these hours of work need to be spent on market work or alternatively could be spent on home production to ensure norm compliance. In summary, overall utility can be illustrated as

$$V_j\left(u_j\left(C_j\left(w,H_j\right),L_j\left(H_j\right)\right),i_j\left(K_j,P,\varepsilon_j,wH_j\right)\right), \tag{1}$$

whereby u_j is suppressed in what follows as it depends on C_j and L_j only.

Psychologically, the preferred image of the person in society and hence identity utility is the higher the better the fit between one's intended status (in our case, meeting P to belong to K_j) and one's actual situation (in our case, the hours worked H_j). Of great relevance to our discussion is, hence, P, which represents the strength of the prescription to work for people belonging to the working-age group and, as we assume for illustrative purposes, varies between 0 (does not apply at all) and 1 (fully applies).

Importantly, the degree to which people are expected to meet P may vary dependent on their individual characteristics ε_j, i.e., $P(\varepsilon_j)$, so that P impacts i differently across individuals j. Similar to the group differences discussed by Schöb (2013), we suspect that the gender of j may determine the prescription to work for a living, if men are more strongly expected to comply than women in line with traditional gender roles. Some studies point to the importance of the employment status of the partner in this context, discussing whether men feel a particularly strong pressure to work if their female partner is employed (e.g., Clark 2003; Knabe et al. 2016; Nikolova and Ayhan 2019). In addition, young workers who have just entered the labor market and can rely on their parents' income may be expected less to comply with the norm than the middle-aged. Finally, high levels of wealth may excuse people from doing paid work to some extent, as they are self-sufficient anyway.

Considering the time budget as constraint and neglecting K_j because it is the only social category under consideration, we reformulate (1) as

$$V_j\left(C_j\left(wH_j\right),T-H_j,i_j\left(w,Y_{\text{minself}},H_j,P(\varepsilon_j)\right)\right) \tag{2}$$

with

$$i = \begin{cases} Y_{\text{minself}} - P(\varepsilon_j)\left(Y_{\text{minself}} - wH_j\right) \text{if} Y_{\text{minself}} \geq wH_j \\ Y_{\text{minself}} \text{if} Y_{\text{minself}} < wH_j \end{cases}. \tag{3}$$

Y_{minself} represents the exogenously given minimum level of self-earned income that makes the person independent of any transfers. Including Y_{minself} in this particular way ensures that people who are not subject to the social norm against living off others ($P(\varepsilon_j) = 0$) enjoy maximum identity utility irrespective of their level of work hours.

Now consider the total utility effect of unemployment, which we assume is an absolute drop of H_j, regardless of the person's previous working time, so that $dH_j < 0$. Note that unemployment is neither a partial nor a marginal change of consumption or leisure. As unemployment prevents people from earning income, we focus on the first upper piece of (3), where Y_{minself} exceeds wH_j, when determining the total derivative

$$dV_j = \underbrace{\frac{\partial V_j}{\partial c_j}\frac{\partial c_j}{\partial H_j}dH_j}_{(I)\leq 0} + \underbrace{\frac{\partial V_j}{\partial L_j}\frac{\partial L_j}{\partial H_j}dH_j}_{(II)\geq 0} + \underbrace{\frac{\partial V_j}{\partial i_j}wP(\varepsilon_j)dH_j}_{(III)\leq 0} \tag{4}$$

In the following empirical analyses, we aim to isolate the different parts of the absolute change in overall utility, *(I)*, *(II)* and *(III)*, rather than measure the total dV. In the process, we assume that the utility value of hours of leisure and the utility value of income can be separately measured. This is inspired by the so-called domain satisfaction approach (van Praag et al. 2003) which assumes that

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[6] Unlike our model, the original framework by Akerlof and Kranton (2000) considers actions of others as arguments of u (e.g., externalities) and i. With regard to i, social exclusion may be important in our context, which could be reflected by the actions of others in the identity utility function. For instance, there could be a cost society imposes on working-age people who deviate from the norm to make a living from work and hence may experience exclusion from social activities. Kunze and Suppa (2017) show indeed that jobless people avoid social interaction.

overall utility can be compartmentalized into domain-specific utilities and that these may be measured by means of domain-specific satisfaction indicators.

The change in satisfaction with household income approximates the sum of the absolute changes in utilities from consumption (*I*) and identity (*III*), both of which reduce in unemployment as they positively depend on work hours H_j. In line with previous work, we argue that measures of income satisfaction capture aspects besides consumption, such as comparisons (Clark et al. 2009) or expectations (Powdthavee 2012), and, in our case, the identity utility from how income is obtained (self-earned or not).

As doing paid market work helps to increase income for consumption and to meet the norm not to live off others, the effect of becoming unemployed on income satisfaction is expected to be unambiguously negative. This should still be the case if changes of income and hence of the utility from consumption (i.e., *I*) are controlled for. Evidence in support of this comes from Powdthavee (2012) who conducts a broad analysis of unemployment and the satisfaction with various life domains.[7] In line with the notion that (*III*) depends on $P(\varepsilon_j)$, we also consider gender heterogeneity in the effect of unemployment on income satisfaction. In light of traditional gender roles, a man of working-age might lose identity utility in particular, while an unemployed woman of working age may experience less pressure to comply with the norm to work. To find out more about this, we test if the effect of unemployment on income satisfaction is more negative for men than for women, especially when controlling for (*I*).

In a similar way, we aim to clarify whether non-employment increases utility from leisure time. To measure the change in the utility from leisure (*II*), we make use of data on people's satisfaction with free time.[8] We expect that unemployment releases time which is redirected towards leisure enjoyment, but also towards time spent with the family and household chores, all of which could benefit people's overall satisfaction with free time.

Note that our model focuses on the idea of obtaining identity utility through market income, but it does not explicitly consider alternative identities related to the use of free time. This might be too strict from an empirical perspective, given that non-market work could offer avenues to establish one's identity, especially home production. One may argue that this matters especially to females, since household chores are performed to a larger extent by women than by men (e.g., Jessen et al. 2022) due to persistent gender roles regarding work and family. Thus, more time for home production in unemployment might yield different free-time satisfaction effects, depending on whether or not someone perceives household activities as part of their identity (Grogan and Koka 2013). We address this in our following empirical analyses by comparing unemployment-related changes in time use and satisfaction with free time between women and men, and we also measure the possible benefits of providing home production to comply with beliefs regarding socially appropriate behaviors in our survey experiment below (Section 5).

3. Methodology of the panel analysis

3.1. Data

Our empirical analyses are based upon 17 waves (1999–2015) of German Socio-economic Panel (SOEP) data (see Wagner et al. 2007). We make use of SOEP version 32.1 (10.5684/soep.v32.1). Several thousand working-age individuals took part annually in these SOEP waves and provided an enormous amount of information on their socio-demographic characteristics, biographies, activities and well-being. An important feature of the dataset for our study is its panel structure, which allows us to follow the same workers over several years.

At the beginning of the SOEP questionnaire, information is gathered on the satisfaction with life domains. To analyze satisfaction with the level and source of income, we use the item: *'How satisfied are you with your household income?'* The data also comprise an overall measure of satisfaction with free time: *'How satisfied are you with your free time?'* Respondents indicate their satisfaction levels on an eleven-point scale from zero (*'completely dissatisfied'*) to ten (*'completely satisfied'*). We use these satisfaction scores to establish continuous outcome variables, which is common practice in the happiness research field (see, e.g., Ferrer-i-Carbonell 2013) with its focus on life satisfaction (or: 'general satisfaction').[9]

Information on exogenously triggered job losses is another important feature of the SOEP for our objective to identify the effects of entering unemployment. If a job has ended since the last interview, SOEP participants are asked about the specific reason (*'How did that job end?'*). The list of possible reasons includes, for example, dismissal, resignation, and mutually agreed termination. Consistent with related studies (see our discussion below), our empirical strategy relies on the category *'My place of work or office closed'* (hereinafter referred to as 'plant closure'). In most of these cases, large groups of workers lose their jobs and the individual characteristics of a particular worker are typically not the cause of subsequent unemployment.[10]

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[7] Note that only a few studies discuss satisfaction with household income, such as D'Ambrosio and Frick (2012) using SOEP data, none of which provide a deeper analysis of exogenously triggered unemployment.

[8] Powdthavee (2012) documents based on the British Household Panel Survey (BHPS) that satisfaction with the *amount* of free time increases when workers enter unemployment, while their satisfaction with the *use* of free time shows no increase. In contrast to the SOEP, the BHPS does not contain a global assessment of free-time satisfaction. Note that only a few empirical studies analysing data on people's satisfaction with their free time exist thus far, such as Gimenez-Nadal and Sevilla-Sanz (2011).

[9] We obtain the same qualitative results when applying ordered probit to consider the ordinal nature of the satisfaction outcome variables.

[10] Note that the response category of job termination due to plant closure has been part of the SOEP in every year since 2001. As we consider information on individuals two years prior to the event of a plant closure, as discussed below, we base our analysis upon data starting with the SOEP wave of 1999.

The SOEP also includes a time use module that allows us to examine people's activities when employed or not. The module does not include all possible activities, but it sufficiently addresses activities we are interested in, namely work and different forms of spending free time.[11] First, people indicate how many hours they spend doing their jobs, including commuting time. This provides us with a broad definition of working time. Second, respondents also indicate the time they spend on enjoyable leisure activities, such as hobbies (leisure in a narrow sense). Third, we add up hours invested in errands, housework, care for children or other persons, and repairs, including gardening, and define the sum as home production.

To compare people's income situation when in work and out of work, we consider information from the SOEP household survey that inquires the total net income per month. Respondents add up their household income after taxes from work and other sources, as well as any social security benefits. To establish equivalent household income levels based on the new OECD scale, we divide this income (at 2011 prices) by the weighted sum of persons living in the same household. While the first person gets a weight of 1, any additional person older than 14 years gets a weight of 0.5, children up to the age of 14 years get a weight of 0.3.

Finally, we use data on socio-demographic characteristics, job characteristics, including individual-level labor earnings, and indicators of health, such as doctor visits. These variables serve as covariates in our matching-based identification strategy, which we describe next.

3.2. Identification strategy

Our empirical strategy builds upon previous research that aims at establishing causal effects of unemployment by exploiting plant closures as an exogenous trigger of job loss. Starting with the seminal study based on SOEP data by Kassenboehmer and Haisken De-New (2009), the idea of focusing on cases of workers who lose their job for a reason beyond their control is commonly used in the literature on subjective well-being (for a recent review, see Suppa 2021). To investigate the effects of becoming unemployed on our main outcome variables, we define workers who experience exogenously triggered job losses via plant closure as our 'treatment group' (in experimental terminology) and compare these treated workers with a 'control group' of workers who do not experience a plant closure and stay employed. We conduct the following four further steps to identify effects on workers who involuntarily become unemployed in the wake of a plant closure.

First, we routinely control for individual-fixed effects by leveraging the longitudinal nature of the data. Since the seminal work on methodology in happiness economics by Ferrer-i-Carbonell and Frijters (2004), this step is seen as crucial for identification purposes and considered to be far more relevant than the cardinality assumption underlying linear regression techniques. In the main analysis, we therefore employ a difference-in-differences (DiD) approach where we calculate the change in the outcome variable (e.g., free-time satisfaction) between treatment and control group at the individual level over time. By focusing on changes instead of levels, we ensure that our findings cannot be driven by time-invariant between-person variation, as we implicitly control for individually fixed characteristics.

Second, we choose a time window of three survey years (instead of two, before and after job loss) to avoid that our results are confounded by the circumstances immediately preceding the trigger incident of a plant closure.[12] Hence, we calculate the changes in the outcomes of the treated from the second-last SOEP interview before the plant closure ($t = -2$) to the first interview afterwards ($t = 0$). In the year in between ($t = -1$), they are still employed. After losing the job due to plant closure, the treatment group is non-employed at $t = 0$, which according to our definition includes individuals who either have filed for unemployment or not. Thereby, we also consider cases where individuals after their job loss focus on the role of the homemaker as well as cases where the actual amount of unemployment benefits is seen as not substantial enough to initiate the process of filing for unemployment. While we use the terms jobless, non-employed and unemployed interchangeably in the following, we exclude (early) retirees from our definition and hence from the sample. With respect to the observations in the control group, we measure the same two-year difference for people who are employed over at least three SOEP interviews in a row ($t = -2, t = -1, t = 0$), while we exclude cases of employer changes between $t = -1$ and $t = 0$ from the sample.

Third, we increase the comparability of treatment and control group further by imposing additional sample restrictions regarding age and employment: For one, all workers in the sample are adults of working age (18–60 years). In addition, if employed, individuals are observed only in dependent employment, either full-time or part-time, which excludes apprentices and self-employed people. For the sample restriction concerning employment, we require that both control group and treatment group meet these conditions at $t = -2$ and $t = -1$, while at $t = 0$ only workers in the control group are employed.[13]

Fourth, we apply a matching technique to account for remaining differences between treatment and control group, considering that

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[11] Studies that are based on the Day Reconstruction Method (Kahneman et al. 2004a; 2004b) routinely consider more than ten activities (see, e.g., Knabe et al. 2010). In their app-based study, Bryson and MacKerron (2017) are able to analyze almost 40 different activities. Given these large numbers of possible activities, which are sometimes hard to separate from each other, some of the research on time use relies on datasets in which activities are aggregated under certain categories (see, e.g., Gimenez-Nadal and Sevilla 2012; Dittrich and Mey 2015).

[12] For example, in Germany, employers making staff redundant must observe periods of notice of up to three months before layoff, which could already affect the workers before they lose their jobs. By choosing the second-last interview as a reference point instead of the last interview before the job loss, we follow related work analyzing plant closures using SOEP data (e.g., Marcus 2013; 2014; Chadi and Hetschko 2018; Hetschko and Preuss 2019; Chadi and Goerke 2023). For a recent study on possible lead effects prior to plant closures, see Wunder and Zeydanli (2021).

[13] Numbers of observations are moreover restricted by missing data as soon as we consider additional information in our analyses. We report the sample size underlying each analysis in the respective notes below tables and figures.

the likelihood of experiencing a plant closure could still vary across different groups of workers even after imposing our sample restrictions. Therefore, we exploit the information available in the SOEP on socio-demographics, job characteristics as well as health in order to perform entropy balancing (EB, Hainmueller 2012). In the course of this procedure, we also consider region effects (federal state) and time effects (survey year), as well as condition on the pre-treatment levels of time use and our main outcome variables. By balancing on the means and standard deviations of the covariates, EB guarantees high matching quality as measured by statistical similarity of treatment and control group (for applied studies, see, e.g., Hetschko et al. 2019; Everding and Marcus 2020; Friehe and Marcus 2021). We prefer this non-parametric procedure over other matching techniques for which the researcher needs to select covariates depending on whether they contribute to statistically equal groups or not (for an overview of matching techniques, see Caliendo and Kopeinig 2008).

Table A1 in Appendix A provides descriptive statistics of the covariates that underlie the matching process producing our main results.[14] The table shows how these characteristics differ between the treatment group, the initial control group and the reweighted control group after EB has been applied. Treated and control observations are fairly similar before balancing, except for earnings and job characteristics that relate to firm-specific risk of plant closure (industry, company size, occupational status). The two groups perfectly align afterwards.

3.3. Common trend analysis

Before presenting our main results, we analyze a sample without matching weights to examine the similarity of satisfaction trends between control group (continuously employed) and treatment group (not employed after job loss due to plant closure). To be included in this analysis, we require respondents to take part in six consecutive survey waves. For the treatment group, this means that individuals are observed in each year from the interview five years before they report being out of work in the first interview after plant closure. To keep the sample size as large as possible, observations are not excluded yet because of missing values on covariates.

Based on our visualization in Fig. A1, we do not find any notable differences in trends of free-time satisfaction and income satisfaction before workers in the treatment group enter unemployment. The fact that satisfaction trends are relatively volatile for the treated may be due to their sample size being much smaller than that of the control group of continuously employed workers. Overall levels of satisfaction also differ between the controls and the treated who generally report being less satisfied. While this does not affect our affirmative conclusion regarding the common trend assumption, our analyses account for these differences throughout, as they rely solely on within-person changes. We conclude that there is supportive evidence of common trends even before applying the matching procedure, which we employ in our subsequent main analysis to further improve the comparability of treatment and control group.

4. Results of the panel data analysis

4.1. Main results for free-time satisfaction and income satisfaction

We use a simple DiD model to establish our main results. The dependent variable in our model is $\Delta Satisfaction_{i,t=-2 \to t=0}$, which reflects the mean satisfaction difference from $t = -2$ to $t = 0$. The main independent variable $Unemployment_{i,t=-1 \to t=0}$ identifies treated and control group individuals i. To estimate differences in satisfaction changes between the two time points, we run an OLS regression that allows us to control for our set of pre-treatment covariates $X_{i,t=-2}^{'}$, as measured at $t = -2$ (see Table A1). We thereby complement our model to reduce the unexplained variation left to the error term e_i:

$$\Delta Satisfaction_{i,t=-2 \to t=0} = \alpha Unemployment_{i,t=-1 \to t=0} + \beta X_{i,t=-2}^{'} + e_i \tag{5}$$

The pre-treatment covariates are also used to conduct matching via EB, which implies that the observations of the control group are assigned weights that ensure statistically identical groups. In our main specification, we combine in one regression analysis both the adjustment of the control group through entropy balancing weights and the use of pre-treatment information as control variables, which maximizes the precision of our estimates.

Table 1 presents the main findings from our analyses of satisfaction with free time (Panel A) and satisfaction with household income (Panel B). Across specifications, the effect of becoming unemployed on satisfaction with free time is strongly positive (between +0.7 and +0.8, or 11 %–13 % of the treatment group's initial level). In addition, satisfaction with household income decreases tremendously by about –1.8, which amounts to approximately 30 % of the average income satisfaction the treated reported two years before entering non-employment. As an alternative way of illustrating the importance of these effects, we estimate the change in the probabilities of being satisfied with free time respectively household income. For this purpose, we use binary median-split variables indicating whether or not the respondent is 'satisfied' by reporting a level of satisfaction that is above the median (which is 7 in both cases). The probability of belonging to that group of people satisfied with their free time increases by 15 percentage points in unemployment. At the same time, becoming unemployed makes it 23 percentage points less likely to score above the median of income satisfaction.

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[14] Note that our results do not change qualitatively when we use more parsimonious sets of covariates. Notably, this is also true when we rerun our main analyses without conditioning on our main pre-treatment outcomes variables, as can be seen in Table A2. This robustness check refers to the discussion in the literature about the role of pre-treatment outcomes in matching or regression models (e.g., Chabé-Ferret 2017), which has also been considered in some studies on happiness (see, e.g., Vendrik 2013; Kaiser 2020; O'Connor 2020).

Table 1
DiD effects on satisfaction with free time and satisfaction with household income.

Panel (A) Satisfaction with free time				
	(1) mean	(2) OLS	(3) EB	(4) EB & OLS
Unemployed	0.807*** (0.251)	0.748*** (0.206)	0.728*** (0.251)	0.737*** (0.134)
Observations R²	63,801 0.001	63,801 0.255	63,801 0.025	63,801 0.447
Panel (B) Satisfaction with household income				
	(1) mean	(2) OLS	(3) EB	(4) EB & OLS
Unemployed	−1.792*** (0.220)	−1.778*** (0.188)	−1.781*** (0.221)	−1.773*** (0.139)
Observations R²	63,801 0.003	63,801 0.253	63,801 0.149	63,801 0.467

Source. SOEP 1999–2015.
Note. The table presents estimates of the difference-in-difference effect of unemployment due to plant closure on the two-year change in income satisfaction and free-time satisfaction from $t=-2$ to $t=0$. Specifications (1) and (2) rely on the initial control group, while population weights are applied to the whole sample. Specifications (3) and (4) are based on a reweighted control group. Reweighting is performed using entropy balancing. The characteristics used for reweighting are listed in Table A1 (except for the year and the federal state). Table A1 provides descriptive statistics on these characteristics measured at $t=-2$, including income satisfaction and free-time satisfaction. 'OLS' controls include the same set of characteristics as covariates (Specifications 2, 4). The sample consists of 194 treated observations and 63,607 control group observations. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

The different ways of considering possible dissimilarities between individuals in treatment and control group barely change the effect sizes, despite the rich information taken into account. This suggests that the trigger event of a plant closure is as good as random or, if not, that at least the observed differences between affected and unaffected individuals are irrelevant for the effects in our selection of outcome variables. Accordingly, one could argue that the role of non-observed factors may be negligible as well.

We also reproduce the finding from the literature that unemployment significantly decreases life satisfaction using our DiD approach (see Table A3). This indicates that satisfaction with income reflects more aspects important for general satisfaction than satisfaction with free-time, at least in the context of unemployment and notwithstanding other domain satisfactions.

Appendix B entails an in-depth discussion concerning the validity and robustness of our main findings based on further analyses. We start this discussion by focusing on possible sample selectivity prior to experiencing the event of a plant closure. To this end, we make use of additional variables to control for workers' expectations regarding future job loss as well as related labor market events based on a subsample of SOEP waves where the corresponding information is available. Moreover, based on monthly data, sample selectivity in the wake of losing work is inspected. What is more, we vary sample restrictions (e.g., regarding work hours, multiple observations of the same person) and we conduct OLS fixed-effects analyses, as an alternative to matched DiD estimations. In addition, we provide a comprehensive investigation of potential measurement issues that could be related to social desirability bias, building upon previous research (e.g., Conti and Pudney 2011; Kassenboehmer and Haisken-De New 2012; Chadi 2013), as well as attrition bias (e.g., Chadi 2019; 2021). From all these additional analyses and discussions, we infer that our main results are valid and robust.

Furthermore, we use the panel data to investigate the link between subjective perceptions and future labor market behavior by testing whether income satisfaction and free-time satisfaction are predictors of accepting a job offer.[15] This informs us about the practical relevance of well-being measures and constitutes an attempt to reconcile the happiness approach to measure the utility from consumption and leisure with the microeconomic concept of choice-based utility. Using different estimation methods, we find that non-employed workers who are less satisfied with their income than others also take up a new job more swiftly. The methodology and results of this analysis may be found in the separate Appendix C.

4.2. Gender differences

Gender has been shown in some previous studies to be a potentially important factor in shaping the well-being effect of unemployment, as discussed in Section 2. Accordingly, especially male workers may be expected to comply with the norm not to live off others, implying that the negative income satisfaction effect documented in the previous subsection for the average worker should be more

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[15] To our knowledge, no other study has yet provided evidence on the importance of either income satisfaction or free-time satisfaction for job take-up. Previous research on subjective data and labor market entries have looked at (dis)satisfaction with life in general as potential motivation to take up employment, yielding mixed evidence (e.g., Krause 2013; Gielen and Van Ours 2014; Mavridis 2015; Rose and Stavrova 2019). Note that recent experimental findings show that identity concerns matter for labor supply (Oh 2023), which could be a reason why income dissatisfaction may serve as a predictor of job take-up in our analysis.

pronounced in males than in females. One may also argue that the effect on free-time satisfaction varies across genders, if, for instance, the time spent on home production and hobbies, or the gain of leisure utility from these activities, differs between men and women.[16]

Fig. 1 documents the results when we rerun our main analyses for gender subsamples. Note that entropy balancing is repeated, accordingly. We find a significantly stronger treatment effect on income satisfaction in the male subsample based on our main specification. As the estimate in the female subsample is outside the confidence interval of the male estimate, and vice versa, we are able to reject the null for the difference between the two estimates. This aligns with the idea that men of working age may face pressure to conform to a traditional breadwinner identity, increasing the urgency to comply with the social norm to make a living from work. On the contrary, the free-time satisfaction effects are not significantly different between men and women, even though men seem to benefit somewhat more. It appears that the benefits from a gain of leisure time when entering unemployment are not gender-specific.

4.3. The role of changes in time use and income

To better understand the mechanisms underlying our main findings based on subjective data, we examine the role of objective changes in people's lives when considering available information on their time use allocation and on their income situation. We start with a graphical inspection of how time use and different forms of income evolve in our treatment group of workers when they become non-employed following a plant closure.

The upper part of Fig. 2 shows how workers change their allocation of time when they switch employment status. Comparing non-employment ($t = 0$) and employment two years before ($t = –2$), we find that the time spent on both home production and pure leisure activities increases significantly while working time drops to almost zero. According to the available data, the loss of working time (-8.2 h) is larger than the combined increase of time spent on home production (+2.2 h) and pure leisure activities (+1.3 h).[17] When we separately investigate gender differences in this context (see Fig. A2), we find that both men and women spend significantly more time on home production and pure leisure, although the increase of the latter is rather small among women. Since entering non-employment releases relatively more time for men to be spent on activities other than work, they are able to reduce the home production gap to women, reaching 79% of non-employed women's hours of home production. This seems substantial given that the same women spent almost twice as much time on home production as treated men when both were employed at $t = –2$.[18]

The lower part of Fig. 2 shows that the reduction of working time when entering non-employment is accompanied by a decline of monthly equivalent income of 357 euros from $t = -2$ to $t = 0$. Individual net labor earnings (i.e., self-earned income) vanish completely. Instead, the non-employed receive transfers of about 818 euros per month.[19] Regarding gender differences, we find that men receive more income via transfers compared to women (see Fig. A2). While this aligns with the observation of a substantial gender pay gap and the fact that unemployment benefits depend on prior earnings, we also find that the loss of household income is more pronounced for men, despite the difference in the level of benefits. Prior to non-employment, equivalent household income levels are similar across genders.

Next, we connect the observed changes in people's time use and income with our main findings on free-time satisfaction and income satisfaction. In this context, related research on life satisfaction isolates the non-monetary costs of unemployment from the effects of objective changes in people's lives by controlling for income in regression analyses (e.g., Knabe and Rätzel 2011). To find out if our main findings are attributable to changes in income and available non-working time, we expand our DiD model with covariates reflecting changes in people's lives as follows:

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[16] Apart from gender, age might be another demographic characteristic that could modify the implications of entering unemployment. However, evidence from additional analyses does not clearly support this idea. Our findings do not change if we are more restrictive at the lower bound of the working-age definition (25 years instead of 18 years) where many people might still be in education. The same is true if we are less restrictive at the upper bound and extend our age restriction from 60 years to 65 years. In a separate analysis, we observe that older workers aged above the median (of 45 years) suffer somewhat more than younger workers in terms of income satisfaction and benefit slightly less in terms of free-time satisfaction. These differences are however not robustly statistically significant across specifications.

[17] We also observe that the unemployed spend 0.3 hours more on further education. Recall that several time-consuming activities are not included in the SOEP time use questionnaire. For instance, newly unemployed people invest a substantial amount of time in job search (Krueger and Mueller 2010), while employed people get comparatively little sleep (Knabe et al. 2010). Another factor that might contribute to the unexplained gap in time use are travel times. While commuting is explicitly mentioned as part of the work item in the SOEP time use module, there is no item to capture the time spent on other forms of travelling which could increase in unemployment (e.g., job interviews). Note that the limited detail of the time use module does not concern the regression analysis of this section, where we define non-working time in the broadest possible way, in line with our theoretical considerations (Section 2).

[18] Using partner information in the household survey, we are able to look into spillover effects of unemployment on the partner's time use. Interestingly, the increase of home production documented here for the unemployed is not mirrored by a decline of the partner's hours devoted to home production, regardless of gender.

[19] In our institutional context, displaced workers are eligible for public unemployment insurance benefits (UIB, 'Arbeitslosengeld'), which usually replace about 60% of former net labor earnings. UIB amount to 84% of all transfers in our treatment group. The remainder mainly consists of social benefits ('Arbeitslosengeld II', 'Sozialhilfe') plus additional housing benefits ('Wohngeld'), which both are granted to the household as a whole. Social benefits supplement household income unless it reaches the legally defined minimum subsistence level (including UIB). This takes place regardless of the person's employment status, which explains why we observe a small number of workers receiving transfers while being employed. As can be seen in Fig. A2, these transfers are concentrated on women, potentially due to the fact that labor earnings are higher for men.
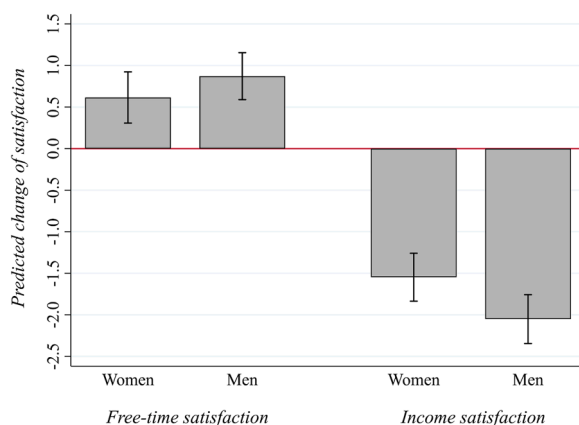
Fig. 1. Gender-specific effects of unemployment.

Source. SOEP 1999–2015.

Note. The figure presents gender-specific estimates of the difference-in-difference effect of unemployment due to plant closure on the two-year change in free-time satisfaction and income satisfaction from t=-2 to $t = 0$ based on the main specification (4) in Table 1. Entropy balancing was repeated for the gender subsamples. The female sample consists of 92 treated observations and 30,311 control group observations. The male sample consists of 102 treated observations and 33,296 control group observations. Population-weighted pre-treatment satisfaction levels (measured at t=-2) for females and males are: 6.52 and 6.43 (satisfaction with free time) as well as 6.07 and 6.06 (satisfaction with household income). Whiskers denote 95 % confidence intervals.
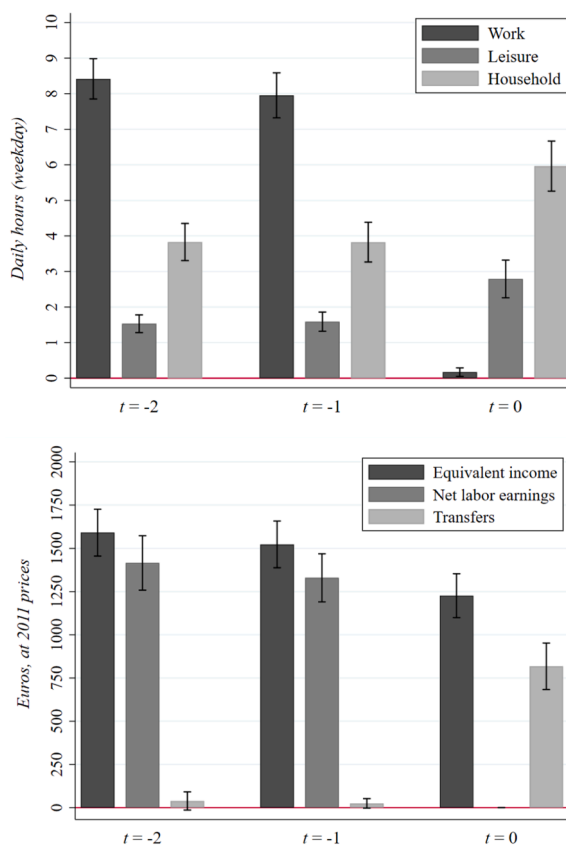
Fig. 2. Time use and income before and after becoming unemployed.

Source. SOEP 1999–2015.

Note. Whiskers denote 95 % confidence intervals. The underlying sample consists of 194 workers whose job termination follows a plant closure between $t = -1$ to $t = 0$. Results are population-weighted.

$$\Delta Satisfaction_{i,t=-2\rightarrow t=0} = \alpha Unemployment_{i,t=-1\rightarrow t=0} + \gamma \Delta \log\left(Objective_{i,t=-2\rightarrow t=0}\right) + \beta X^{'}_{i,t=-2} + e_i \tag{6}$$

For free-time satisfaction, we add the objective change of non-working time (24 h minus daily working time) from $t = -2$ to $t = 0$ to the model ($\Delta \log\left(Objective_{i,t=-2\rightarrow t=0}\right)$). To account for decreasing marginal utility in leisure, these changes are calculated as the differences of the natural logarithms of the two variables. In the same way, we consider the change in equivalent household income from $t = -2$ to $t = 0$ when analyzing income satisfaction.

Table 2 shows that the effect of unemployment found for free-time satisfaction (Panel A) not only becomes smaller, but also statistically insignificant, if the change of non-working time is controlled for. It appears that actual changes in time use are not only significant predictors of people's subjective assessments, they also account for the positive effect of unemployment on the satisfaction with free time. Likewise, the objective measure of changes in equivalent income is significantly related to changes in subjectively assessed income satisfaction (Panel B); however, changes in actual income only partially explain the impact of unemployment on income satisfaction. While the size of the coefficient of entering non-employment decreases somewhat once the former is included, the effect is still enormous and highly significant. In line with our theoretical considerations (Section 2), this could point to the loss of identity utility when workers violate the social norm not to live off others.[20]

Along the lines of the previous subsection, we also analyze if the roles of objective changes accompanying the transition from employment to unemployment matter differently for the well-being of males and females. Panel A of Table 2 shows similar results for satisfaction with free time, which can be explained by the observed similarity in changes of time allocation across genders. Considering the change of income in Panel B seems to reduce the effect of unemployment on women's income satisfaction somewhat more (coefficient changes by 0.398 points) compared to men (0.279). This is interesting to note in light of the gender gap in earnings, and may point to gender-related heterogeneity when it comes to the non-monetary benefits of employment. That said, we also note that this gender difference in how much the effect changes is statistically insignificant.[21]

4.4. Further evidence on the hidden benefits of (un)employment

4.4.1. Implications for family life

Before we discuss the idea of possible hidden benefits of employment to understand our results for income satisfaction in the following SubSection 4.4.2, we further examine to what extent unemployment is beneficial for private life. While unemployment increases satisfaction with free time, our above time use analysis shows that leisure in a narrow sense is increasing relatively little, pointing to an alternative benefit from having more time available. Spending more time on domestic activities could impact family life positively, for example by resolving work-family conflicts. Examples could be spending time with children or doing housework together with the partner, which some respondents might experience as rather enjoyable in unemployment as time is no longer scarce. At the same time these are forms of home production and hence leisure in a broader sense.

To examine this idea, we leverage information on satisfaction '*with family life*' as another outcome that is based on assessments using the eleven-point scale from zero ('completely dissatisfied') to ten ('completely satisfied'). To determine the effect of unemployment, we employ the identification strategy that underlies our main analyses and run again a DiD regression based on a treatment group of individuals who report plant closure as reason for recent job termination and a control group of continuously employed workers. Accordingly, we estimate Eq. (5) again using the change of family-life satisfaction from $t = -2$ to $t = 0$ as dependent variable. Beforehand, we reweight the control group via entropy balancing, which we apply with regard to the means of the characteristics in Table A1 and the pre-treatment level of family-life satisfaction. Note that this analysis has to rely on a smaller sample size (82 treated observations and 35,554 controls), as compared to our main analyses, given that satisfaction with family life has been a part of the SOEP questionnaire since 2006 only.

Table 3 presents the results for the DiD effect based on regression analyses where we vary the use of covariates as well as matching via entropy balancing. The results show that people experience an increase of satisfaction with family life when they become

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[20] In light of the idea that the relationship between satisfaction and income might be more concave than implied by the log function (see Layard et al. 2008), we check our functional form assumption by using income categories instead (e.g., deciles). This does not change our conclusion. In a separate discussion (which can be found at the end of Appendix B), we also consider another alternative interpretation of our finding, according to which possible long-term income changes (Stevens 1997; Huttunen et al. 2011; Braga 2018) could explain the vast dissatisfaction among unemployed people (Knabe and Rätzel 2011; Bayer and Jüssen 2015). Based on various additional analyses, however, we conclude that changes in permanent income cannot fully explain the loss of income satisfaction in the wake of unemployment.

[21] In further analyses, we inspect the role of part-time employment and find that the results for income satisfaction effects in women strongly depend on whether they worked full-time before unemployment or not. The loss of income satisfaction does not differ between formerly full-time working men and women (about -2 scale points). Controlling for income explains roughly half of that effect in full-time women, but only 14% of the effect in full-time men, which is significantly different. Regarding free-time satisfaction, we find for both formerly full-time working men and women that the effect of unemployment is strong (close to $+1$ on the scale) and becomes statistically insignificant if the change of non-working time is controlled for. In another set of analyses, we inspect the role of partnership status. The results do not differ qualitatively from those reported before when we focus only on partnered men and women. Nevertheless, we are a bit cautious here with our conclusions, given that the size of the treatment group shrinks markedly in such additional analyses based on subgroups.

Table 2

The role of changes in time use and income.

Panel A) Free time satisfaction						
	Full sample		Women		Men	
	Spec. 4 in Table 1	+ Objective change	Spec. 4 in Table 1	+ Objective change	Spec. 4 in Table 1	+ Objective change
Unemployed	0.737***	0.103	0.615***	−0.202	0.871***	0.068
	(0.134)	(0.239)	(0.157)	(0.219)	(0.144)	(0.319)
Change of log(non-working time)		1.462***		2.393***		1.588***
		(0.491)		(0.515)		(0.575)
Observations	63,801	63,801	30,403	30,403	33,398	33,398
R²	0.447	0.451	0.562	0.571	0.446	0.449

Panel B) Income satisfaction						
	Full sample		Women		Men	
	Spec. 4 in Table 1	+ Objective change	Spec. 4 in Table 1	+ Objective change	Spec. 4 in Table 1	+ Objective change
Unemployed	−1.773***	−1.395***	−1.547***	−1.149***	−2.051***	−1.772***
	(0.139)	(0.142)	(0.147)	(0.140)	(0.150)	(0.177)
Change of log(equivalent income)		1.203***		1.650***		0.736***
		(0.245)		(0.272)		(0.240)
Observations	63,801	63,801	30,403	30,403	33,398	33,398
R²	0.467	0.489	0.537	0.565	0.545	0.553

Source. SOEP 1999–2015.

Note. The table presents estimates of the difference-in-difference effect of unemployment due to plant closure on the two-year change in free-time satisfaction and income satisfaction from $t=$-2 to $t = 0$. The full sample consists of 194 treated observations and 63,607 control group observations. The female sample consists of 92 treated observations and 30,311 control group observations. The male sample consists of 102 treated observations and 33,296 control group observations. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

unemployed. While statistical significance varies, which could be due to the small number of cases, the effect size is stable (at roughly +0.4 scale points). We conclude that this finding of higher family-life satisfaction may unveil a hidden benefit of unemployment.[22]

4.4.2. The norm to work for a living

Apart from the hidden benefit of being unemployed in the domain of family life, our evidence also points to a hidden benefit of being employed. In the following, we further investigate the idea that employed people obtain identity utility from complying with the social norm to not live off others (Section 2) and thus lose income satisfaction when becoming unemployed. Given that there are several reasons why people have to leave the labor market and may or may not receive publicly financed transfers, one could think of several ideas for another investigation. For example, many people in Germany leave the labor market temporarily after childbirth and obtain parental leave subsidies ('Elterngeld'). To what extent such a rather short phase outside of the labor market implies changes in identity is however not so clear, especially since many things might change simultaneously in people's lives when a child is born. At this point, we benefit from an existing approach to indirectly measure identity utility effects from compliance with the social norm to work. Based on the finding that workers' life satisfaction soars when switching labor market status from unemployed to retired, Hetschko et al. (2014) argue that transitioning into retirement takes away the pressure of the norm to work, as people switch into a social category where norm compliance is no longer required. This approach relies on the assumption that the retirement of unemployed people mainly changes the social categorization from working age to retirement age, which arguably weakens the need to fulfill the social norm to work.[23] Contrary to the original study's focus on life satisfaction, we employ this idea for the first time to analyze satisfaction with household income.

For this additional analysis, we use the same SOEP waves as before and identify continuous transitions from either unemployment or employment to retirement between two consecutive SOEP interviews (i.e., from one year to the next). We closely follow Hetschko et al. (2014) by excluding any form of non-employment prior to retirement other than cases of registered unemployment. This is because some of the non-employed might already be in a form of early retirement, which renders unclear what their transition into retirement actually means (for a detailed analysis of this aspect, see Ponomarenko et al. 2019). In Table A4, we present descriptive statistics to compare retirees from unemployment and retirees from employment. For individuals who retire from unemployment, we

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[22] In additional analyses, estimating the effects for males and females separately, the increase in family-life satisfaction is slightly more pronounced in women, but not on a statistically significant level. We can also confirm that the change of non-working time explains the effect of unemployment on family-life satisfaction completely, as seen before for free-time satisfaction. Family-life satisfaction also generally decreases with working time.

[23] For more comprehensive discussions on retirement and subjective well-being, see, e.g., Bonsang and Klein (2012); Bender (2012); Abolhassani and Alessie (2013) as well as Nikolova and Graham (2014).

Table 3
DiD effects on satisfaction with family life.

	(1) mean	(2) OLS	(3) EB	(4) EB & OLS
Unemployed	0.432*	0.390*	0.351	0.351***
	(0.222)	(0.206)	(0.225)	(0.131)
Observations	35,636	35,636	35,636	35,636
R^2	0.000	0.253	0.010	0.324

Source. SOEP 2006–2015.
Note. The specifications correspond to Table 1, except that the set of characteristics measured at $t=$-2 also includes family-life satisfaction here. The sample used to estimate changes in satisfaction with family life in Specifications (1)-(4) consists of 82 treated observations and 35,554 control group observations. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

do not observe any substantial changes in their lives. Income levels increase only slightly. In their last SOEP interview before they report retirement, over 90 % of unemployed people have already stopped searching for a job as they approach the end of working life. People also enjoy a large amount of free time before and after the transition. Even uncertainty about future incomes should have been resolved on the eve of retirement as people in Germany are well-informed about the level of their pensions, which means that retirement cannot change expected future incomes. Hence, we can credibly assume that the main implication of retirement is the new social categorization. For the comparison group of formerly employed individuals, we observe that their lives also do not change substantially upon retirement, with the exception of a large increase in free time. Income drops by around 10 %.

Fig. 3 reveals that income satisfaction evolves remarkably different for the employed and for the unemployed around the transition into retirement. People retiring from employment lose satisfaction with household income (–0.2) whereas unemployed people benefit (+0.6, both $p < 0.01$). Interestingly, this result is fairly robust across genders.[24] Given that the retirees differ in general, for example regarding income levels, we consider the role of other potentially relevant factors in this context via DiD regression analyses, where we implicitly control for individual-fixed effects by focusing on changes in income satisfaction. The results in Table A5 confirm that unemployed people benefit much more than employed people from retiring, even when considering changes in free time and income.[25] We conclude that retiring from unemployment is associated with a substantial increase of income satisfaction, which is not due to actual changes in income. Instead, the evidence is consistent with the idea of a gain in identity utility in formerly unemployed retirees who no longer need to meet the social norm to work. In turn, this implies that by entering unemployment people lose identity utility, explaining the non-monetary decline of income satisfaction observed in our analyses above. Following up on this hypothesis, we present and analyze a direct measure of the norm not to live off others in the subsequent Section 5.

5. The survey experiment

5.1. Background and experimental design

To study the possible role the social norm against living off others plays in the labor market, we conducted an experiment on hypothetical labor supply decisions with respondents from an online survey panel in Germany. In this part of the analysis, our primary objective is to provide direct evidence on social norms and the resulting pressure on unemployed men and women to accept paid jobs. Indeed, we note that some researchers question the important role of social norms in explaining the unhappiness of the unemployed (Stam et al. 2016), while others stress the need for measuring the norm directly in this context (Heyne and Voßemer 2023). The main question our survey experiment therefore answers is whether supplying market work instead of staying unemployed serves as a means to restore compliance with the social norm not to live off others.

Based on the seminal contribution by Krupka and Weber (2013), and inspired by a growing body of work using their method to experimentally elicit norms in the labor market (for a recent review, see Görges and Nonsenzo 2021), we present participants with a hypothetical choice scenario of an unemployed person who can turn down a job offer or accept it. To closely align the experimental setting with our SOEP-based analysis, we rely on the same institutional background, and we highlight that unemployment occurs involuntarily due to a plant closure. Each survey participant assesses the appropriateness of the decision made by the person in the scenario, which we randomize across participants to identify the causal impact of complying with the norm. For identification purposes, we make sure that changes in income levels cannot play a part in this assessment. A possible concern is that refusing to take up a well-paid job could be seen as generally inappropriate. Hence, accepting the job offer in our experiment only slightly increases the decision-taker's income in the hypothetical scenario, while it clearly changes the source of income from 'living off others' to 'self--earned'. Thereby, the decision-taker is able to comply with the social norm, which allows obtaining identity utility, as an explanation

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[24] Retiring from unemployment goes along with rising income satisfaction for both males (+0.53) and females (+0.67), which does not conform to the notion of gender-specific strengths of the norm to work. A possible explanation is that older women who are about to retire may be less able than younger women with children to find alternative roles restoring their identity utility when unemployed.

[25] We consider changes in leisure time and in income as differences in levels before and after retirement, in line with Hetschko et al. (2014). The results hold when we use differences in log-linearized leisure time and income. The same is true when we exclude all unemployed individuals who actively searched for a job before retirement.
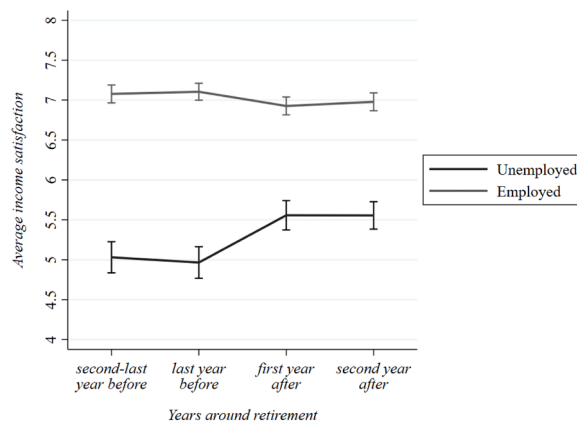
Fig. 3. Retirement entries and satisfaction with household income.
Source. SOEP 1999–2015.
Note. The upper (lower) line displays the evolution of average income satisfaction of 1253 formerly employed retirees (612 formerly unemployed retirees) around the time of entering retirement. Whiskers denote 95 % confidence intervals.

for increased income satisfaction, even in the absence of changes in actual income levels. While this idea is neglected in the standard model of labor supply, it could be very relevant for workers in reality, as our above discussions and results indicate.

The main treatment variation in our experiment is the unemployed person's decision to either accept or reject the job offer. Drawing on our theoretical considerations in Section 2, we hypothesize that rejecting the job offer is considered less socially appropriate than accepting it (i.e., there is a social appropriateness gap). Based on the notion that in particular gender determines whether people are required to comply with the norm to work, one could expect that this matters more to males than to females. As this notion is supported by our results from the panel data analysis, albeit not consistently, the gender of the hypothetical person is our second treatment condition to test whether or not the social appropriateness gap differs between a male and a female decision-taker. Finally, our design allows us to test the hypothesis that market work can be substituted at least to some extent by home production as a means to restore compliance with the norm to work. An example for this might be caring for children which could be seen as a contribution to the greater good in its own right. The comparison activity is leisure in a narrow sense which is seemingly less productive from a societal perspective. Thus, the social appropriateness gap is expected to be smaller if the decision-taker spends the extra time in non-employment on home production, which again might be gender-specific if traditional role models apply in this context.

These considerations translate into an experimental design, whereby participants are randomly assigned to one scenario out of 2 ('job acceptance') × 2 ('gender') × 2 ('time use') treatment conditions. A blueprint for our vignettes of hypothetical choice scenarios may be found in the additional Appendix D, as part of the instructions for participants of the survey experiment. Accordingly, we implement the treatments 'job acceptance' and 'time use' via explicit statements in the text, while we use gender-specific wording (e.g., 'she' vs. 'he') to manipulate the gender of the hypothetical person X. In addition, it is made clear in each scenario that X loses public financial assistance when accepting the job offer so that the overall gain of income is minimal, but the source of income changes.

The participants of the experiment determine the social norm through guessing what the broader population of participants think is the socially appropriate thing to do. This allows us to uncouple the measure of the social norm from the respondents' personal opinions as well as their own hypothetical choices (which are ascertained separately at a later stage, see Appendix D). Participants are asked how socially appropriate they believe the participants of the survey deem the choice of the hypothetical person X on a four-point scale (very socially inappropriate, rather socially inappropriate, rather socially appropriate, very socially appropriate). To incentivize participants to focus on what people in general believe is appropriate, they take part in a lottery with prizes worth 50 euros per winner if their answer is 'correct' in the sense that it matches the answer of the (relative) majority of participants. Within each of the eight treatment conditions, we determine the modal response and randomly draw one prize winner out of all participants with the correct response for their treatment scenario. In combination with another online experiment that was included in the survey and followed after our experiment, 40 prizes of 50 euros were paid out in total.[26]

The experiment was conducted during the first two weeks of December 2021 and was pre-registered in advance (AEA RCT ID: AEARCTR-0,008,638). Potential participants were drawn by the survey provider from a larger pool of previously recruited individuals with the aim of ensuring a quasi-representative distribution in regard of three key demographic characteristics: gender, age, and

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[26] To obtain high-quality responses, the commercial survey provider administering the panel makes use of 'points' that respondents earn for successful survey participation. The points can be redeemed for monetary payments at a later stage. The company demanded that we mention these points using their designation in the instructions in order to avoid confusion. Hence, as can be seen in Appendix D, participants winning a lottery prize in our experiment could earn points, but the monetary value was also indicated (50 euros).

educational level. In total, 1116 participants completed the survey.[27] As part of Appendix E, which covers additional experimental results, we illustrate the data sample in Table E1 by providing an overview of participants' characteristics and by inspecting the randomization. In addition to the main outcome variables, the gathered information from the participants broadly covers the covariates used in the panel data analysis.

5.2. Main experimental findings

To determine the main results from our survey experiment, we follow the standard procedure in the experimental social-norm literature by assigning the following Krupka-Weber scores of social appropriateness: 1 stands for 'very socially appropriate', 1/3 for 'socially appropriate', –1/3 for 'socially inappropriate' and –1 for 'very socially inappropriate' which makes the scores range from –1 to +1 with equal differences between the levels of the scale. To begin with, we examine the average scores for the two main treatment conditions where the decision-taker X either rejects the job offer or accepts it. Accordingly, we aggregate the data from all other treatment conditions (gender and time use) before they are separately analyzed in subsequent steps below. To provide another angle to our main finding, we also use one of the categories from the appropriateness scale as a separate indicator and analyze the likelihood of reporting the lowest possible appropriateness score. In each case, we visualize our main results using graphical illustrations, while we report the results from additional regression analyses in the Appendix E, where we control for covariates, among other things.[28]

Fig. 4 shows the main results for the average score on the left-hand side as well as the results for the probability of reporting the category of 'very socially inappropriate' on the right-hand side. In line with theoretical expectations, there is a strong belief in the population that it is socially inappropriate to turn down a job offer (negative score), and socially appropriate to accept it (positive score), despite the fact that the total income of the hypothetical decision-taker increases only minimally in the latter case. The overall accept-reject difference amounts to 0.6 points, which is a substantial gap on the scale from -1 to +1 and highly significant.[29] With respect to the likelihood of reporting 'very socially inappropriate' behavior, we observe a strongly significant effect of an almost 20 percentage points reduction (from roughly 25 percent) when the worker accepts the job (compared to rejecting the job offer). In the following, we focus on the accept-reject gap in the appropriateness scores, as our measure of the strength of the social norm not to live off others, and inspect possible changes across the treatment dimensions 'gender' and 'time use' in unemployment.

Panel A of Fig. 5 illustrates the average strength of the social norm to work for a living, as measured by the appropriateness effect of the main treatment manipulation ('accept' vs. 'reject') across other conditions. The illustration on the left-hand side suggests a stronger norm for men to supply market work, as it seems less socially appropriate if a male decision-taker X rejects the job offer. While this points to traditional gender roles, the total difference in the accept-reject gap of roughly 0.1 is neither large nor statistically significant.[30] Given that the illustration on the left-hand side is based on data from both 'time use' treatment conditions, one could expect the gender gap to become larger if X engages in home production. This is because the traditional female role of the housewife and mother might affect to what extent people consider it appropriate for women to abstain from market work.

In this context, we first investigate the idea of a gain from home production in terms of social appropriateness for both genders combined. The right-hand side illustration in Panel A of Fig. 5 shows a significant decline of the accept-reject gap by 0.2. This squares with the idea that home production is generally seen as a contribution to society, reducing the social pressure to supply market work.[31] The next step is to test whether this healing role of household chores in terms of norm compliance matters in particular as far as women are concerned.

Panel B of Fig. 5 displays the evidence from comparisons of the accept-reject gaps in appropriateness scores for the male and the female decision-taker X separately for the time use treatment conditions. The illustration on the left-hand side implies a significantly stronger pressure to earn a living on unemployed men who pursue enjoyable leisure activities. In contrast, the illustration on the right-hand side shows that the positive role of home production is not driven at all by a female decision-taker X.[32] Hence, people do not seem to

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[27] The sample includes a 10% excess of participants. This is due to a service of the survey provider which allows for sample restrictions according to data quality indicators such as time stamps. Our main results from the experiment do not change when we restrict the sample, accordingly.

[28] Consistent with our panel data analyses, we determine statistical significance using linear estimation methods. Our main findings do not change if we check our regression results using a non-linear estimation approach, such as ordered probit. We also check the treatment effects using Wilcoxon-Mann-Whitney tests. For a recent review of methods and tests used in experimental research, see, e.g., Weimann and Brosig-Koch (2019).

[29] To put this result into perspective, we screened the available body of applied work based on the Krupka-Weber-method but could not find a treatment effect that comes close to the gap of 0.6 observed in our experiment. The original study by Krupka and Weber (2013) reports various appropriateness ratings for different outcomes in a dictator game, including extreme allocations chosen by a dictator, which reveals a similar gap when comparing the dictator taking all the money vs. the dictator giving 30% of the endowment to the passive responder.

[30] We do find evidence for a significant gender effect when we analyze the likelihood of reporting very inappropriate behavior, whereby the gap is roughly 11 percentage points higher when the decision-taker X is male instead of female. This can be seen in Table E2.

[31] This result raises the question if the negative effect of unemployment on income satisfaction found in our SOEP-based analysis is smaller for those who engage in more home production. To examine this more deeply, we return to our panel data analysis in Table 2 and distinguish individuals who prior to becoming unemployed devote more or less time to home production at the beginning of our time window. Indeed, each daily hour of home production mitigates the negative effect of unemployment on income satisfaction significantly by 0.15 points.

[32] A separate check of the home production premium for unemployed women reveals a weakly significant effect when we compare the time use treatments. As can be seen in Table E3, the interaction effect measuring the home production premium for women is not robust across specifications. This is different for the male decision taker X where the evidence in favor of a home production premium is very robust.
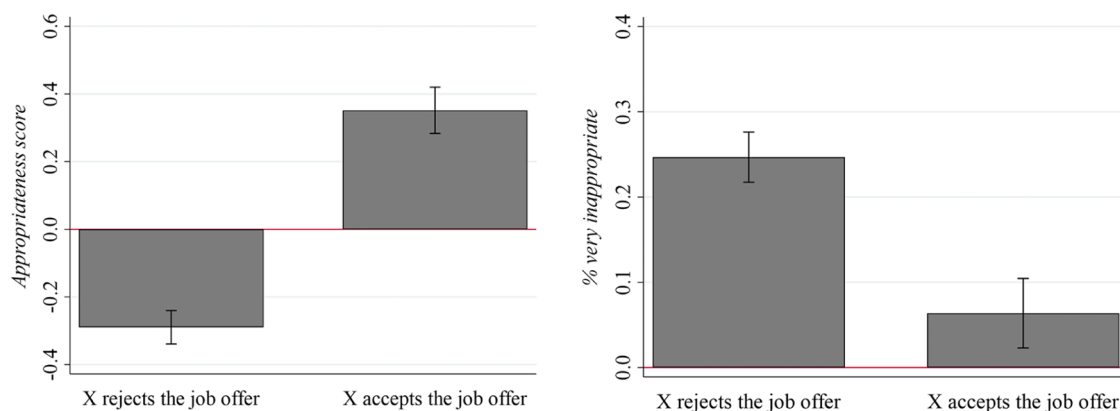
Fig. 4. Social appropriateness of job take-up.
Source. Own survey experiment 2021.
Note. Bars in the left-hand illustration show the average appropriateness score when assessing the behavior of the hypothetical decision-taker X, depending on the treatment ('accept' vs. 'reject' the job). Bars in the right-hand illustration show the average likelihood of reporting 'very inappropriate' when assessing the behavior of the hypothetical decision-taker X, depending on the treatment ('accept' vs. 'reject' the job). 95 % confidence intervals are obtained from linear regressions.

appreciate women being engaged in home production more than they appreciate men taking over the role of the homemaker when unemployed. If anything, we obtain evidence to the contrary, which could explain why, on average, the social norm to earn a living is not much stronger for men.[33]

5.3. Respondents own hypothetical behavior

An important question in terms of the relevance of social norms for real-world outcomes is whether they are reflected in individual decision-making. We pay particular attention to this question for several reasons: First, people often have normative views on what they think is right or wrong, but their own behavior differs from those views. Second, societal developments could be faster than people's knowledge about the speed of these changes and they may therefore underestimate recent developments when asked about it. An example for this could be changing gender norms.[34]

In what follows, we present the results of an approach to experimentally study whether the respondents would behave in line with the norm-prescribed behavior if they themselves were to decide about the job offer. Immediately after assessing the main choice scenario, and conditional on the specific treatment condition, we asked respondents how likely it is that they would make the same decision as the decision-taker X. This means that respondents who read that X accepts the job offer were asked if they would also accept the job offer, while those in the reject condition were asked if they would also reject the job offer. By doing so, we rule out that anchoring or priming effects drive our results: If all the respondents were simply confirming the behavior of the hypothetical person, this would imply no differences at all in the likelihood of agreeing with the decision of X across treatments. Any treatment effect implies that respondents differ in the way they concur with X's decision to either accept or reject the job offer. Due to the randomization, such a treatment effect would then be indicative of a difference in behavior that cannot be driven by individual characteristics.

In keeping with our above predictions and results, we expect a stronger congruence between X's behavior and the respondent's choice if X accepts the job offer, as compared to the conditions where X rejects the job offer. We focus in this analysis on the difference in the probability (measured from 1, very unlikely, to 4, very likely) that respondents would behave like X when comparing the main 'accept' and 'reject' treatment conditions. We call this the 'choice congruence gap' in the following. Moreover, we define 'full agreement' with the choice of X when respondents report that X's decision 'very likely' reflects their own choice. These outcomes can then be compared further across the time use treatment conditions, allowing us to test the idea of reducing social pressure to work via providing home production. Meanwhile, the gender treatment manipulation of the hypothetical person X should not affect the respondents' choices, given that we ask them about their own hypothetical behavior. However, respondents' own gender might matter in this context. The results are again presented visually.

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[33] In addition to our double-interaction analyses to inspect both home production premium and possible gender differences in the social norm to work, we also provide results from triple-interaction analyses based on all three treatment dimensions in Table E4. This again shows that the social appropriateness premium of home production in unemployment is not stronger for women. In fact, respondents rather favor male homemakers. For the assessment of very inappropriate behavior, we even observe a significant triple interaction effect, implying that that the decision to be unemployed for the sake of being productive at home is more often seen as inappropriate for women compared to men who benefit more in terms of social recognition.

[34] For example, a recent study on the perceptions of Saudi Arabian men regarding women's role in the labor market indicates that people's beliefs about traditional gender roles and corresponding social norms may lag behind changes of their own behavior (Bursztyn et al. 2020).
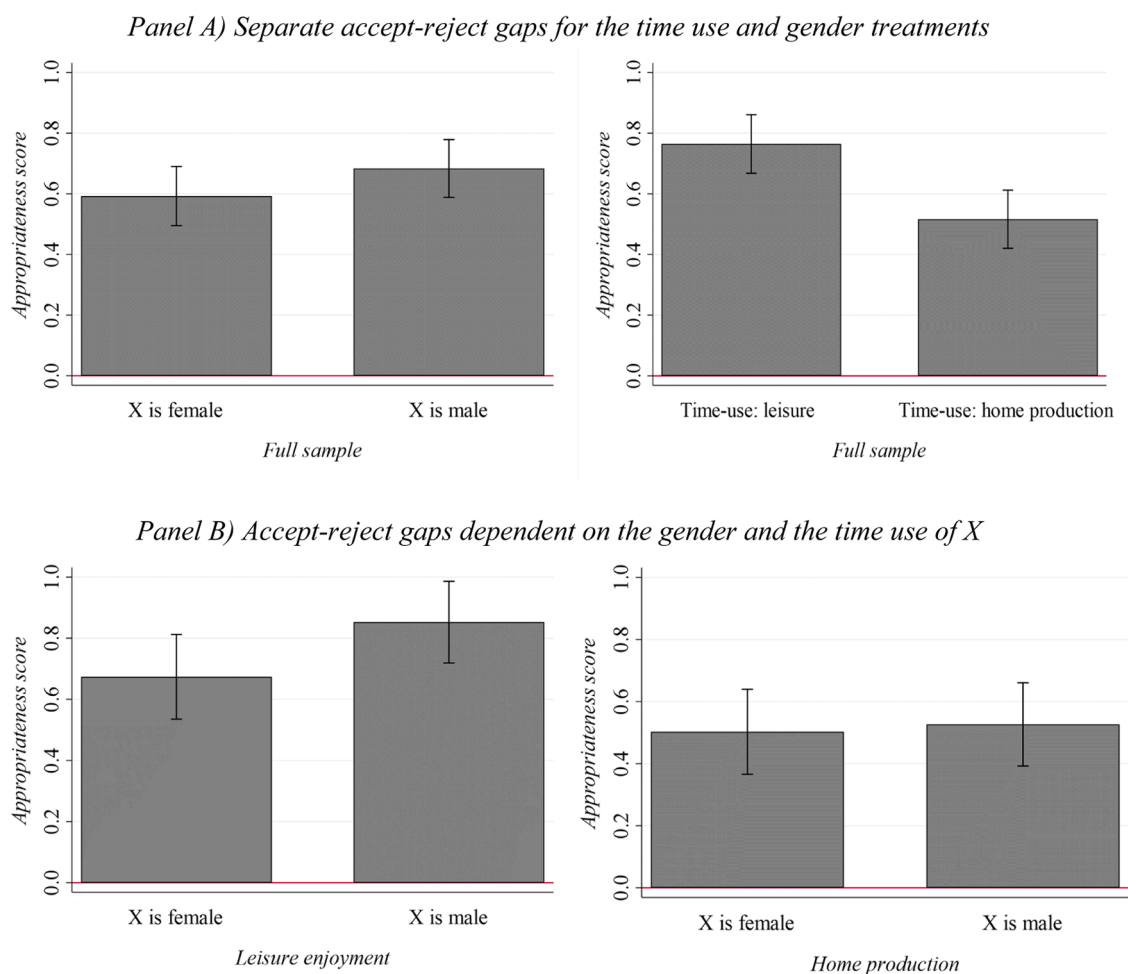
Panel A) Separate accept-reject gaps for the time use and gender treatments

Panel B) Accept-reject gaps dependent on the gender and the time use of X

Fig. 5. Social appropriateness gaps across treatment dimensions.

Source. Own survey experiment 2021.

Note. Bars show the average accept-reject gap in social appropriateness scores when assessing the behavior of the hypothetical decision-taker X for the main treatment variation ('accept' vs. 'reject' the job) across other treatment conditions ('gender' of X and 'time use' in unemployment in Panel A as well as combinations of both treatment dimensions in Panel B). 95 % confidence intervals are from linear regressions.

The left-hand illustration in Panel A of Fig. 6 shows that the behavior prescribed by the social norm to work for a living is mirrored in the respondent's own hypothetical choice. Someone who is in the treatment where decision-taker X accepts the job is much more likely to report behaving in the same way than someone who is assigned the scenario where the job offer is rejected. Respondents in the latter treatment are far less likely to report choice congruence, meaning they clearly disagree with the decision of the hypothetical decision-taker. We confirm this main result from our analysis of own choices in a variety of additional analyses and checks.[35] The right-hand illustration in Panel A presents evidence on the role of time use in unemployment and shows the same picture as above for social appropriateness. If respondents put themselves in X's place performing household chores in unemployment, the choice congruence gap significantly shrinks, i.e., rejecting the job offer becomes more likely relative to accepting the job offer. This implies that the respondents not only consider it less socially inappropriate if unemployed individuals are engaged in home production, they themselves would behave in line with this view by more likely choosing to be unemployed.

In the remainder of Fig. 6, we inspect the role of gender. First, we split the sample according to the gender of X and compare the choice congruence gap. Reassuringly, the left-hand illustration in Panel B shows that the gender of X does not matter for the results, which implies that the respondents are able to abstract from the decision-taker's gender and report on their own hypothetical behavior while taking their own gender into account. This is indeed what the right-hand illustration in Panel B demonstrates when we compare

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[35] In our additional analyses, we find that respondents are roughly 30 percentage points more likely to report that the decision by X 'very likely' reflects their own choice if X accepts the job (42.6% full agreement) as opposed to rejecting the job (11.6% full agreement). Table E5 provides results from triple-interaction analyses based on the relevant treatment dimensions and respondent gender. In another check, we also confirm our findings using a sample that is restricted to individuals of working age.
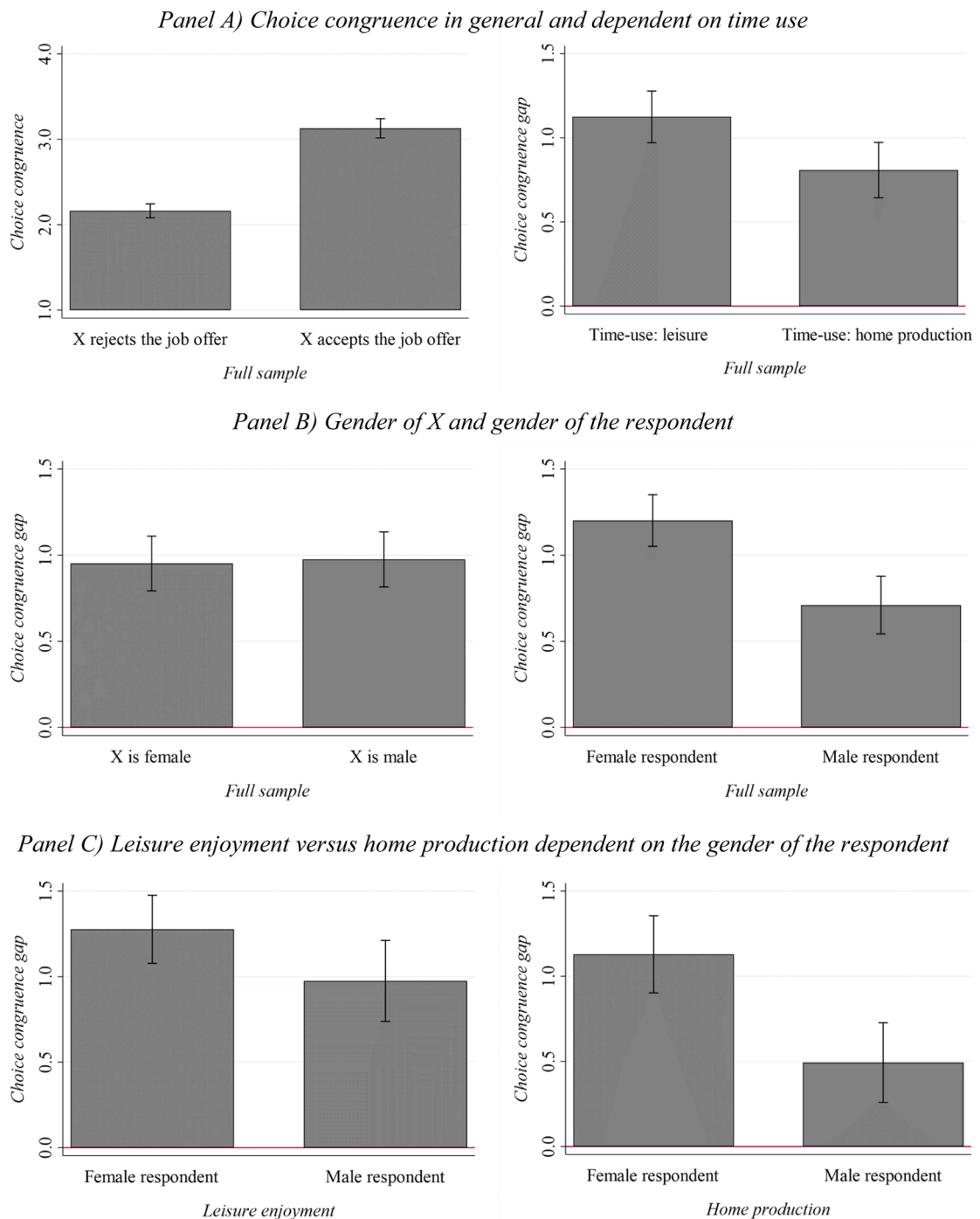
Fig. 6. Choice congruence.

Source. Own survey experiment 2021.

Note. Bars in the left-hand illustration of Panel A show the average likelihood of respondents reporting choice congruence with the hypothetical decision-taker X, depending on the treatment ('accept' vs. 'reject' the job). Bars in all other illustrations show the average choice congruence gap for the main treatment variation ('accept' vs. 'reject' the job) across other treatment conditions ('time use' in unemployment in the right-hand illustration of Panel A, 'gender' of X in the left-hand illustration of Panel B) and across respondent gender (in the right-hand illustration of Panel B) as well as combinations of time use and respondent gender (Panel C). 95 % confidence intervals are from linear regressions.

responses from female and male respondents in a second step, where we observe a significantly stronger choice congruence gap for women. Based on a binary indicator of choice congruence, we find that female respondents more often fully agree with accepting the job offer than male respondents (44.0% vs. 40.8 %), while less often agreeing with rejecting it (9.9% vs. 13.6 %). While these differences are not statistically significant, the picture overall is rather inconsistent with the idea of job take up decisions being driven by traditional gender roles. We suspect that societal trends have changed the social norm and the perception of pressure to earn a living felt as much by women as by men. Finally, Panel C shows that men prefer to reject job offers significantly more often if they engage in useful activities in the household as compared to leisure whereas this difference is small and insignificant among females. This points, again, to modern gender roles, as a result of societal trends related to male engagement in household chores, similar to our above insights on the varying strength of the social norm.

6. Discussion and conclusions

This study provides a comprehensive discussion of one of the most fundamental and policy-relevant concept in economics, which is the idea of a trade-off between income and leisure in people's labor supply decisions. By testing the two crucial assumptions underlying this concept, we obtain strong empirical support for such a trade-off, as individuals benefit in unemployment from having more time, while they benefit in employment from earning income. However, our investigation reveals novel explanations for this trade-off, which, just like the role of gender in this context, have received little to no attention so far in economic theory and discussions of labor market policies.

First, we focus on the social norm not to live off others as an explanation for the severe decline of satisfaction with household income when individuals become unemployed. Violating the norm prevents them from enjoying identity utility, as a hidden benefit of employment, which is neglected when the focus is on consumption and leisure as sole determinants of labor supply. Second, we find that satisfaction with free time significantly increases when entering unemployment. Since more of the time freed up by unemployment is spent on home production rather than on narrowly defined leisure activities, we suspect there is a hidden benefit of unemployment in that gaining time in unemployment resolves work-family conflicts, which also manifests itself in higher satisfaction with family life. Moreover, across studies and empirical approaches, we find mixed evidence for gender differences when it comes to the trade-off between income and leisure. Interestingly, neither the positive free-time nor the family-life satisfaction effects of unemployment are clearly gender-specific, suggesting converging gender roles with regard to domestic activities. This interpretation of the evidence on gender differences based on satisfaction data is in line with the results from our survey experiment, which confirms the idea of a significant benefit from being productive at home in unemployment and an even bigger benefit from employment through which both men and women are able to comply with societal expectations regarding their labor supply.

Our study bears various policy implications, including from a personnel perspective, where our analyses confirm that income considerations matter for labor supply decisions and hence for firms when hiring, retaining and motivating staff. Management must be aware of workers' desire to make a living based on their own earnings. Furthermore, our results point to the importance of home production in worker well-being and labor supply, which supports firms pursuing family-friendly policies and taking steps to help workers balance work demands with household chores, such as childcare.

With respect to labor market policies, our study suggests that it is essential to help the unemployed get back into adequately paid jobs, which informs the debate in various fields of economics on policies to tackle the problem of unemployment (e.g., Boeri 2005; Card et al. 2010; 2017; De Almeida Vilares 2022; Wiese et al. 2023; Dolls 2024). Our study also suggests that those who suffer from non-employment have a motive to enter the labor market for the purpose of ending their reliance on transfers. This is at odds with many policy proposals, typically based on the traditional model of labor supply, which predict that receiving welfare creates large negative incentives to work. Furthermore, the evidence in our study on the social norm not to live off others also helps explain why minimum wages are often a policy intervention that is popular with the public. Regardless of their effects on employment rates, minimum wages promise to reduce a worker's dependency on means-tested social benefits, which is an important aspect of work from a societal perspective.

Finally, our study also speaks to the public-policy discussion of providing everyone with a publicly financed universal basic income. At first glance, our results indicate that people could suffer substantially from such a policy and, if unemployed, still seek to work not to rely on the transfer only. Instead, one may also argue that in a society in which everyone receives the transfer, being entirely self-reliant ceases to be the norm. This could diminish the importance of the social norm not to live off others, so that its impact on labor supply wanes in the longer term, just like norms about the role of gender in the labor market may evolve over time.

Appendix A. Additional output of the panel data analysis

Table A1
Descriptive statistics before and after balancing the main sample.

Conditioning variables as measured at $t = -2$:	Treated		Controls, initially		Controls, reweighted	
	Mean	std. dev.	mean	std. dev.	mean	std. dev.
Pre-treatment levels of outcome variables						
Satisfaction with free time	6.47	2.15	6.67	2.06	6.46	2.15
Satisfaction with income	6.06	2.08	6.60	2.02	6.05	2.08
Time use						
Daily working time (hours)	8.42	2.83	8.65	2.32	8.40	2.83
Daily leisure (hours)	1.53	1.32	1.63	1.32	1.53	1.32
Daily hours of household production (hours)	3.83	3.11	3.75	3.49	3.82	3.11
Socio-demographic characteristics						
Equivalent income	1590.76	824.35	1791.28	859.05	1587.30	823.44
Age	46.29	9.40	42.29	9.32	46.19	9.39
Female (share)	0.45	0.50	0.44	0.50	0.45	0.50
Level of education (ISCED, shares)						
Primary	0.18	0.38	0.08	0.27	0.18	0.38
Secondary	0.65	0.48	0.61	0.49	0.65	0.48
Tertiary	0.17	0.38	0.31	0.46	0.17	0.37
Years of employment	23.06	10.85	19.17	9.82	23.01	10.84
Years of unemployment	0.87	1.78	0.44	1.20	0.87	1.78
Marital status (shares)						
Married	0.67	0.47	0.59	0.49	0.67	0.47
Separated	0.02	0.15	0.02	0.15	0.02	0.15
Divorced	0.14	0.35	0.10	0.30	0.14	0.35
Widowed	0.01	0.10	0.01	0.10	0.01	0.10
Having a partner (share)	0.82	0.38	0.81	0.40	0.82	0.39
Home ownership (share)	0.42	0.49	0.48	0.50	0.42	0.49
Size of apartment (m^2)	97.33	39.27	101.11	41.66	97.11	39.23
Number of persons in household	2.61	1.17	2.63	1.23	2.60	1.17
People in need of care in household (share)	0.01	0.10	0.02	0.12	0.01	0.10
Children living in household (shares)						
None	0.66	0.47	0.64	0.48	0.66	0.47
One	0.22	0.42	0.19	0.39	0.22	0.42
Two	0.08	0.28	0.14	0.35	0.08	0.28
Three or more	0.03	0.17	0.03	0.17	0.03	0.17
Health						
Visits to doctor	2.04	3.08	1.89	2.95	2.03	3.08
Disability (share)	0.11	0.31	0.06	0.24	0.11	0.31
Job characteristics						
Monthly net labor earnings (euros, at 2011 prices)	1415.62	865.32	1775.44	982.91	1412.55	864.37
Autonomy in occupational actions (scale 1–5)	2.15	0.95	2.72	1.05	2.15	0.94
Years of tenure	11.02	10.29	11.75	9.67	11.00	10.28
Being fulltime employed (share)	0.74	0.44	0.78	0.41	0.74	0.44
Sector of industry (shares)						
Manufacturing	0.31	0.46	0.20	0.40	0.31	0.46
Construction	0.15	0.36	0.13	0.33	0.15	0.36
Agriculture	0.02	0.13	0.01	0.09	0.02	0.13
Trade	0.29	0.45	0.14	0.34	0.29	0.45
Transport	0.03	0.17	0.06	0.23	0.03	0.17
Banking and finance	0.01	0.08	0.04	0.21	0.01	0.08
Public administration	0.00	0.05	0.10	0.30	0.00	0.05
Education	0.03	0.17	0.07	0.25	0.03	0.17
Health and social services	0.03	0.18	0.12	0.33	0.03	0.18
Other services	0.13	0.34	0.12	0.33	0.13	0.34
Occupational status (shares)						
White collar	0.55	0.50	0.32	0.47	0.55	0.50
Blue collar	0.45	0.50	0.68	0.47	0.45	0.50
Company size (shares)						
$\leq$ 20 employees	0.29	0.46	0.20	0.40	0.29	0.46
20–199 employees	0.51	0.50	0.30	0.46	0.51	0.50
200–1999 employees	0.12	0.33	0.24	0.43	0.12	0.33
$\geq$ 2000 employees	0.07	0.26	0.26	0.44	0.07	0.26

Source. SOEP 1999–2015.

Note. The table presents population-weighted descriptive statistics for the sample underlying our main estimations. The figures refer to $t=-2$. Note that our estimations are also conditional on the year of the interview and the federal state where the respondent lives, which have been omitted from this table. The treatment group consists of 194 workers whose job termination follows a plant closure between $t = -1$ to $t = 0$. Control group observations (63,607) stay employed three SOEP interviews in a row.
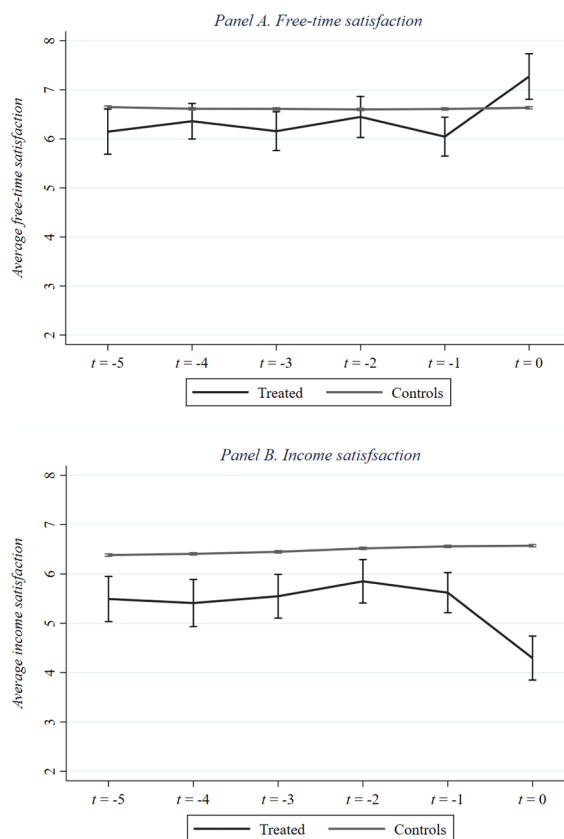
Fig. A1. Evolution of free-time satisfaction and satisfaction with household income.

Source. SOEP 1999–2015.

Note. The sample consists of 228 treated (entering unemployment at $t = 0$ after a plant closure) and 65,000 controls (continuously employed from $t=-2$ to $t = 0$) in Panel A (free-time satisfaction) as well as 224 treated and 65,181 controls in Panel B (income satisfaction). Population weights are used (i.e., samples are not matched). Whiskers denote 95 % confidence intervals. Note that the confidence intervals for the control group are relatively small, presumably because of the relatively large sample size.

Table A2

Pre-treatment outcomes not considered in matching/regressions.

	Free-time satisfaction			Income satisfaction		
	(2) OLS	(3) EB	(4) EB & OLS	(2) OLS	(3) EB	(4) EB & OLS
Unemployed	0.804***	0.811***	0.813***	−1.721***	−1.711***	−1.708***
	(0.250)	(0.251)	(0.171)	(0.220)	(0.221)	(0.168)
Observations	63,801	63,801	63,801	63,801	63,801	63,801
R^2	0.006	0.031	0.197	0.016	0.139	0.265

Source. SOEP 1999–2015.

Note. The specifications correspond to Table 1, Specifications (2)-(4), with the only difference being that pre-treatment outcome variables are no longer considered (neither for weighting via EB, nor as covariates in the OLS regression). Hence, Specification (1) from Table 1 is not affected by this modification of our set of characteristics and not displayed here again. The sample used to estimate changes in satisfaction with free time respectively household income consists of 194 treated observations and 63,607 control group observations. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

Table A3
DiD effects on satisfaction with life.

	(1) mean	(2) OLS	(3) EB	(4) EB & OLS
Unemployed	−1.093***	−0.972***	−1.001***	−0.992***
	(0.160)	(0.158)	(0.161)	(0.114)
Observations	63,801	63,801	63,801	63,801
R²	0.002	0.257	0.083	0.363

Source. SOEP 1999–2015.

Note. The specifications correspond to Table 1, except that the set of characteristics measured at $t=-2$ also includes life satisfaction here. The sample used to estimate changes in satisfaction with life in Specifications (1)-(4) consists of 194 treated observations and 63,607 control group observations. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

Panel (A): Time Use
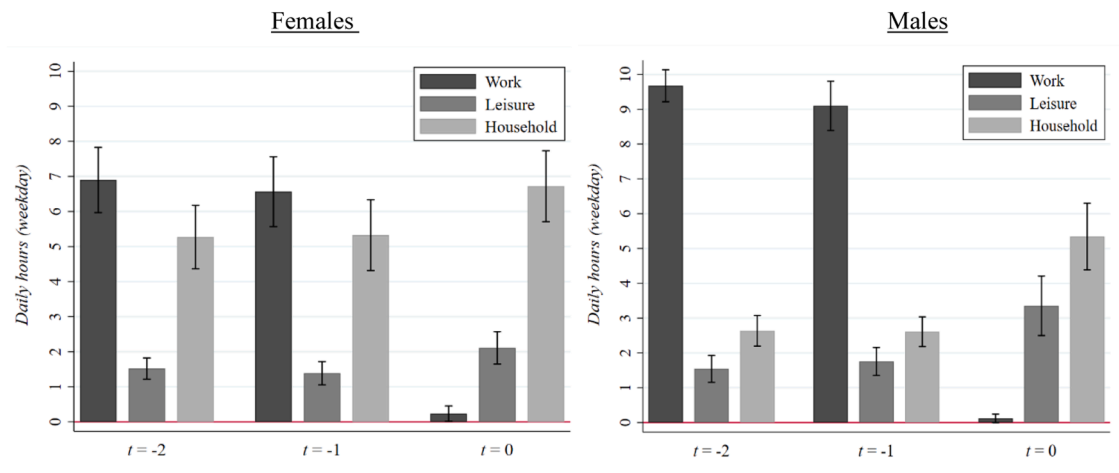
Panel (B): Income
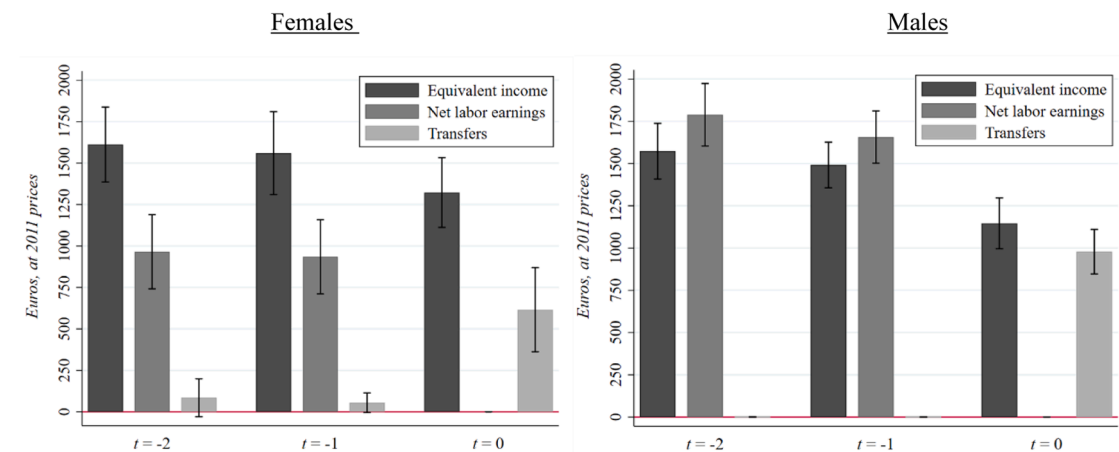
Fig. A2. Time use and income before and after becoming unemployed by gender.
Source. SOEP 1999–2015.
Note. Whiskers denote 95 % confidence intervals. The sample (treatment group only) consists of 92 females and 102 males. Results are population-weighted.

Table A4

Unemployed people and employed people around retirement.

Retiring from …	unemployment		employment	
	year before	year after	year before	year after
Number of persons	612	612	1253	1253
Net equivalence income (in euros, at 2006 prices)	1286.80	1337.28	2389.33	2142.23
Home ownership (share)	51%	52%	69%	70%
Age (in years)	61.10	62.10	61.99	62.99
Non-working time (hours per day)	23.84	23.82	16.21	22.63
Female	45%	45%	40%	40%
Primary education	17%	17%	8%	8%
Secondary education	60%	60%	45%	45%
Tertiary education	23%	23%	47%	47%
Single	17%	17%	16%	16%
Someone in need of care lives in the household	5%	5%	2%	2%
Children younger than 14 years live in the household	2%	1%	1%	1%
Having searched actively for a job, last four weeks	9%	–	–	–

Source. SOEP 1999–2015.

Table A5

Retirement and income satisfaction – regression analysis.

OLS estimates of change in income satisfaction	(1)	(2)	(3)	(4)	No job seekers
Formerly unemployed	0.794***	0.707***	0.559***	0.524***	0.532***
	(0.093)	(0.096)	(0.124)	(0.128)	(0.130)
Relative change in equivalent income		0.496***	0.477***	0.456***	0.479***
		(0.135)	(0.132)	(0.135)	(0.141)
Changes in …					
non-working time (hours per day)			−0.024*	−0.028**	−0.025**
			(0.012)	(0.013)	(0.013)
home ownership (yes / no)				0.087	0.073
				(0.293)	(0.308)
being single (yes / no)				−0.795**	−0.784**
				(0.390)	(0.389)
children in household (yes / no)				0.858*	0.888*
				(0.509)	(0.512)
people in need of care in household (yes / no)				0.539	0.559
				(0.434)	(0.453)
Female				0.136	0.145
				(0.086)	(0.088)
Primary education (ref. secondary)				0.010	0.039
				(0.143)	(0.145)
Tertiary education (ref. secondary)				0.134	0.151*
				(0.089)	(0.089)
Age, difference to 65 years				−0.036**	−0.037**
				(0.017)	(0.017)
Time (survey year)	yes	yes	yes	yes	yes
Constant	−0.417**	−0.370**	−0.215	−0.421**	−0.518***
	(0.167)	(0.164)	(0.181)	(0.194)	(0.195)
Observations	1865	1865	1865	1865	1808
R^2	0.046	0.057	0.059	0.069	0.069

Source. SOEP 1999–2015.

Note. *denotes significance at the 10% level / **5% / ***1%. Robust standard errors in parentheses. The dependent variable is the change of income satisfaction from the last year before retirement to the first year afterwards.

Appendix B. Sensitivity analyses and checks

In this appendix, we provide a deeper discussion of our main results from the panel data analysis based on various sensitivity checks. To ensure that our main findings are robust, we rely on a plethora of analyses. Among other things, we are able to confirm that our results would not change if a different and more recent SOEP version was used. In the following, we highlight some specific issues that are important for various reasons. In particular, it is possible that there is some kind of sample selectivity when it comes to individuals working in plants that close. While this potential issue has been acknowledged in related work focusing on life satisfaction (see, e.g., Nikolova and Ayhan 2019; Chadi and Hetschko 2021), it is worthwhile to consider if selection bias could be of relevance to the satisfaction outcomes used in our paper.

At first, we thus discuss the composition of the sample concerning possible selection into plant-closure induced unemployment taking differences in workers' expectations regarding the loss of work into account. In a second step, we consider possible sampling issues concerning the time after the plant closure with regard to early job take-up. In further analyses, we conduct individual-fixed

effects estimations, as alternative to the matching-based DiD approach which underlies our main analyses. Then we provide a discussion of potential survey measurement errors that could be related to social desirability bias and an analysis of the panel phenomenon of attrition bias in our context. Finally, we discuss the role of long-term income changes in our analysis of income satisfaction among the unemployed.

Sample composition prior to plant closure and anticipation

Our main results rely on the idea that plant closures take place in a way that can be seen as exogenous from the perspective of the individual worker. Yet, the composition of individuals observed in our analysis may be affected by sample selectivity due to anticipation of the event. For instance, some workers may be able to foresee the failure of their employer and hence avoid being affected by the plant closure via changing jobs.

Fortunately, the SOEP data allow us to consider a variety of self-assessments regarding the current as well as the future employment situation. Further improving the ex-ante comparability of treatment and control group, our first check therefore considers the individual ability to anticipate labor market events as well as the perceived level of employment security in the case of a potential job loss. Specifically, we use variables on how concerned people are about their job security and how difficult they think it would be to find an alternative job. Moreover, we account for the self-reported probabilities of job loss / job change / job search / resignation within the next two years. In the process of exploiting this information, the sample size shrinks markedly, as said data are not available across all of the SOEP waves. The additional information augments the set of matching variables as well as the covariates considered in our main specification. Despite the loss of roughly 1 in 5 observations, the effects of unemployment on satisfaction with free time (+0.8) and with income (-1.7) are remarkably similar to our main results in Table 1 and continue to be highly statistically significant. This suggests that anticipation and other phenomena related to the sample composition prior to plant closure do not affect our main findings.

We draw the same conclusion from additional checks that restrict the sample further to enhance the ex-ante comparability of treatment and control group. Specifically, we test whether excluding employer changers in both groups for the whole period from the pre-treatment reference point in time $t = -2$ to the post-treatment reference point in time $t = 0$ affects our results. In addition, we rerun our analysis without civil servants, given that they are very unlikely to face unemployment due to a plant closure. In another check, we make use of the information on firm size and exclude the smallest category (20 or fewer workers) to minimize the likelihood that the individual worker observed in the data could have an influence on the plant closure. What is more, we run regressions that additionally control for ex-ante information about the partner, such as their employment status, as individuals in treatment and control group may differ with respect to the situation of the partner. Note that this also limits the sample to partnered individuals. Finally, we address the fact that each person is included in the panel multiple times and might therefore be considered in the treatment group and the control group in different years. The check keeps in the sample the first observation of a person of unemployment because of a plant closure in the case they are part of the treatment group more than once. The same person is no longer considered as part of the control group. For people in the control group, the respective first observation is kept. Again, our results virtually do not change in any of these checks.

Sample composition after plant closure and job finding

The composition of individuals observed in our analysis could also be affected by selection phenomena that take place after the event of a plant closure. Since we condition on having no job at the time of the interview, those workers who are able to immediately find another job are not part of the analysis. If, for example, more productive workers reenter the labor market sooner, this could lead to sample selectivity towards less productive workers observed in our sample of non-employed individuals. It is rather difficult, however, to assess the importance of such potential selectivity for our findings on the satisfaction with free time and income. One could argue that differences in the marginal utility of leisure may play a role in our results on free-time satisfaction, so that the effects may be underestimated due to a lack of hard-working individuals in the 'treatment group' of the jobless. Regarding income satisfaction, those with particularly strong dissatisfaction in unemployment may be the ones who are highly motivated to find a new job relatively soon, which could also lead to an underestimation of our treatment effects.

To assess the importance of this possible sample selection issue empirically, we repeat our analysis based on the main specification, but exclude individuals who have been unemployed for more than (the median time of) six months since the last SOEP interview. In this way, we limit the time that workers have to find a new job between losing their job due to a plant closure and the next measurement of their well-being. This minimizes the likelihood that a biased selection of workers could drive our results. The treatment effects on free-time satisfaction (+0.5) and income satisfaction (-1.7) closely resemble the main findings above.

In further sensitivity checks, we exclude individuals who may be more or less likely than others to find a new job after a plant closure. First, we consider data on people's contractually agreed work hours and focus on a sample of individuals with at least 15 h per week. Second, we check our broad definition of unemployment by excluding persons who are out of the labor force, but not registered as unemployed. We thereby lose roughly 15 % of the unemployed, most of which are women. None of these additional analyses yield findings that are substantially different from our main results.

Fixed-effects panel analyses

As the most fundamental check of our methodology, we employ an individual-fixed effects approach as an alternative to our matching-based difference-in-differences design. The fixed-effects approach does not require a fixed reference point before job loss but instead determines differences from the individual average satisfaction over time. Just as the DiD design, it provides evidence based on

within-person changes controlling for stable worker characteristics. Note that there are some changes in our set of covariates, which in this analysis have to be available for individuals before and after entering non-employment. Accordingly, we can neither control for pre-treatment satisfaction levels nor for job characteristics.

Table B1

OLS individual-fixed effects approach – full sample.

Panel A) Satisfaction with free time				
	(1)	(2)	(3)	(4)
Unemployed	1.031***	0.695***	0.696***	0.160
	(0.189)	(0.202)	(0.203)	(0.210)
Log. daily non-working time				1.507***
				(0.143)
Socio-demographic characteristics (w/o income)		yes	yes	yes
Health			yes	yes
Individual-fixed effects	yes	yes	yes	yes
Region (federal state)	yes	yes	yes	yes
Time (survey year)	yes	yes	yes	yes
Observations	62,978	62,978	62,978	62,978
Number of persons	11,301	11,301	11,301	11,301
R^2	0.005	0.011	0.011	0.017

Panel B) Satisfaction with household income				
	(1)	(2)	(3)	(4)
Unemployed	−1.452***	−1.278***	−1.266***	−1.006***
	(0.173)	(0.178)	(0.178)	(0.171)
Log. equivalent income, in euros, at 2011 prices				1.219***
				(0.049)
Socio-demographic characteristics (w/o income)		yes	yes	yes
Health			yes	yes
Individual-fixed effects	yes	yes	yes	yes
Region (federal state)	yes	yes	yes	yes
Time (survey year)	yes	yes	yes	yes
Observations	62,978	62,978	62,978	62,978
Number of persons	11,301	11,301	11,301	11,301
R^2	0.019	0.024	0.025	0.054

Source. SOEP 2001–2015.

Note. The table presents individual-fixed effects estimates of becoming unemployed due to plant closure for satisfaction with free-time and satisfaction with household income. Socio-demographic characteristics and health controls as listed in Table A1. *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

The results are presented in Table B1 for both free-time satisfaction (Panel A) and income satisfaction (Panel B). In each case, we start with the main treatment effect based on the most parsimonious specification, controlling for region, time and individual-fixed effects only (Column 1). We then add covariates for socio-demographic characteristics (Column 2), while the full specification also includes covariates capturing differences in health (Column 3). The final estimation in Table B1 provides a separate check of the results on the mechanisms, as presented in Table 2, by including either the objective indicator of non-working time (Panel A) or income (Panel B) as additional covariate. In the same way, we employ the individual-fixed effects approach to check the gender-specific results for both free-time satisfaction and income satisfaction. These additional analyses of gender differences are documented in Table B2. All our results based on the fixed-effects approach resemble the findings based on the matching-based DiD approach, as discussed in the main body of the paper.

Table B2

OLS individual-fixed effects approach – gender split.

Panel A) Satisfaction with free time				
	Females		Males	
Specification according to Table B1	(3)	(4)	(3)	(4)
Unemployed	0.918***	0.451	0.502*	−0.124
	(0.325)	(0.326)	(0.262)	(0.274)
Log. daily non-working time		1.629***		1.457***
		(0.207)		(0.197)

Table B2 (*continued*)

Panel A) Satisfaction with free time

	Females		Males	
Specification according to Table B1	(3)	(4)	(3)	(4)
Socio-demographics (w/o income, working time)	yes	yes	yes	yes
Health	yes	yes	yes	yes
Individual-fixed effects	yes	yes	yes	yes
Region (federal state)	yes	yes	yes	yes
Time (survey year)	yes	yes	yes	yes
Observations	29,401	29,401	33,577	33,577
Number of persons	5344	5344	5957	5957
R^2	0.017	0.023	0.011	0.017

Panel B) Satisfaction with household income

	Females		Males	
Specification according to Table B1	(3)	(4)	(3)	(4)
Unemployed	−0.647**	−0.269	−1.494***	−1.389***
	(0.281)	(0.248)	(0.238)	(0.234)
Log. equivalent income, in euros, at 2011 prices		1.265***		1.193***
		(0.071)		(0.067)
Socio-demographics (w/o income, working time)	yes	yes	yes	yes
Health	yes	yes	yes	yes
Individual-fixed effects	yes	yes	yes	yes
Region (federal state)	yes	yes	yes	yes
Time (survey year)	yes	yes	yes	yes
Observations	29,401	29,401	33,577	33,577
Number of persons	5344	5344	5957	5957
R^2	0.029	0.062	0.028	0.054

Source. SOEP 2001–2015.

Note. The table presents separate individual-fixed effects estimates of becoming unemployed due to plant closure for satisfaction with free time and satisfaction with household income for females and males. Socio-demographic characteristics and health controls as listed in Table A1 (Specifications 3, 4). *denotes significance at the 10 % level **at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

Survey measurement bias

The literature on well-being acknowledges that self-reported measures could be subject to measurement error (see, e.g., Weimann et al. 2015). The consequence of random measurement error is attenuation bias, which means that the effects revealed in our analyses are underestimated. As our main results are large and highly statistically significant, we would not expect qualitatively different findings if this was ruled out. However, there is also the possibility that our results are affected by a systematic measurement bias, which could be due to social desirability. While employed people might feel socially pressured to indicate high levels of satisfaction, this form of social pressure could be reversed for the unemployed. In what follows, we address this potential issue.

Previous research on measurement bias in satisfaction data has highlighted the interview situation as crucial: if a person is physically present to conduct the interview or the respondent lacks experience with the survey, self-reported satisfaction increases, which points to less 'truthful' responses under the influence of social desirability (e.g., Conti and Pudney 2011; Kassenboehmer and Haisken-DeNew 2012; Chadi 2013). Accordingly, if our results were biased by social desirability, this should depend on the interview situation as it influences the degree to which socially desirable answers are prompted. When an interviewer is present, or panel experience is low, the negative income satisfaction effect of unemployment might be particularly pronounced, and the positive free-time satisfaction effect of unemployment might shrink.

We test for these considerations based on the fixed-effects panel approach discussed beforehand in this appendix, which allows us to consider differences in the interview situation by adding covariates reflecting survey characteristics in a straightforward way. Table B3 presents the corresponding results (Panel A: free-time satisfaction, Panel B: income satisfaction) based on the full specification of Column (3) in Table B1, which we complement by adding variables indicating whether an interviewer could have been present during the interview (i.e., all interview modes combined with the exception of 'self-written questionnaire without interviewer') and whether the interviewee has little panel experience (i.e., they belong to the bottom decile of SOEP participation as they have participated less than five times). Indicative of possible social desirability bias, interviewer presence goes along with more positive self-reports of both outcome variables, while for panel experience, we observe a statistically significant effect for free-time satisfaction only (Column 1).

Table B3
OLS individual-fixed effects approach – Interview characteristics.

Panel A) Satisfaction with free time

	(1)	(2)	(3)	(4)	(5)
Unemployed	0.703***	0.818**	0.720***	0.751	0.527**
	(0.204)	(0.375)	(0.217)	(0.524)	(0.237)
Interviewer present	0.149***	0.149***	0.149***	0.150***	0.149***
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Little panel experience (< 5 participations)	0.115***	0.115***	0.115***	0.115***	0.115***
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
Unemployed × interviewer present		−0.141		−0.273	
		(0.435)		(0.577)	
Unemployed × little panel experience			−0.152		0.033
			(0.600)		(0.848)
Unemployed × female				0.179	0.469
				(0.702)	(0.430)
Unemployed × interviewer present × female				0.304	
				(0.839)	
Unemployed × little panel experience × female					−0.456
					(0.983)
Covariates as in Column (3) of Table B1	yes	yes	yes	yes	yes
Observations	62,978	62,978	62,978	62,978	62,978
Number of persons	11,301	11,301	11,301	11,301	11,301
R^2	0.012	0.012	0.012	0.012	0.012

Panel B) Satisfaction with household income

	(1)	(2)	(3)	(4)	(5)
Unemployed	−1.274***	−1.548***	−1.225***	−2.064***	−1.663***
	(0.177)	(0.390)	(0.191)	(0.527)	(0.231)
Interviewer present	0.308***	0.307***	0.308***	0.308***	0.308***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
Little panel experience (< 5 participations)	0.029	0.029	0.030	0.029	0.030
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Unemployed × interviewer present		0.335		0.436	
		(0.433)		(0.571)	
Unemployed × little panel experience			−0.420		−0.391
			(0.478)		(0.664)
Unemployed × female				1.446**	1.061***
				(0.625)	(0.387)
Unemployed × interviewer present × female				−0.442	
				(0.744)	
Unemployed × little panel experience × female					0.168
					(0.856)
Covariates as in Column (3) of Table B1	yes	yes	yes	yes	yes
Observations	62,978	62,978	62,978	62,978	62,978
Number of persons	11,301	11,301	11,301	11,301	11,301
R^2	0.029	0.029	0.029	0.030	0.030

Source. SOEP 2001–2015.
Note. The table presents individual-fixed effects estimates of becoming unemployed due to plant closure for satisfaction with free-time and satisfaction with household income. Socio-demographic characteristics and health controls as listed in Table A1. *denotes significance at the 10 % level**at the 5 % level and ***at the 1 % level. Robust standard errors are in parentheses.

In the next step, we interact these two variables separately with our main indicator of having entered unemployment. According to Columns (2) and (3), panel experience and interviewer presence do not significantly modify the effects of unemployment on income satisfaction or free-time satisfaction. When it comes to income satisfaction, effect sizes of the two interaction variables are non-negligible, but the coefficients point in opposite directions, providing no clear support for a role of social desirability bias in explaining the negative unemployment effect.

In Columns (4) and (5) of in Table B3, we extend the estimation model by adding interaction variables to inspect possible gender differences. One might argue that for men social desirability requires to appear particularly unhappy during unemployment given traditional role models. However, none of the triple interaction effects are statistically significant. They often also fail to show the expected sign that would point to measurement biases in line with social desirability bias as an explanation for our main results.

The robustness of our results is supported by further analyses, where we vary the definitions of the variables capturing interview characteristics, such as using an alternative threshold number of survey participations to measure differences in panel experience. Moreover, we use changes in the interviewer as another proxy for measurement errors due to social desirability bias. In each case, we find statistically insignificant interaction effects.

Overall, we conclude that there is some evidence of social desirability influences, which may be relevant for the satisfaction measures used in our study; however, there is no pattern indicating that this biases our findings, including the results on gender differences. There are two reasons for this in our view: First, it could be that domain satisfactions, such as income satisfaction and free-time satisfaction, are generally less affected by measurement issues, in comparison to a global well-being measure like life satisfaction. Second, both our main outcome variables come from a satisfaction module placed at the beginning of the SOEP questionnaire, when unemployed interviewees are not yet asked questions about their work life. As a result, their focus has not yet been directed to their new (un-) employment status, as the corresponding questions about the employment situation follow later in the questionnaire.

Attrition bias

A potential issue for users of longitudinal survey data could be due to attrition of respondents who may be more or less likely to leave a panel over time. If a) the attrition likelihood differs between treatment and control group, and b) attrition is correlated with satisfaction, then this could lead to attrition bias. In our case, it could be that people with particularly high or low satisfaction are more likely to leave the panel when losing their job due to a plant closure.

There are several approaches to investigate attrition bias in subjective well-being data, such as the idea of analysing future panel exits. To identify attrition due to plant closure, however, we require information about an event that those exiting the panel do not report in their last interview. Hence, we can only analyse if panel quitters generally report satisfaction differently than those who stay in the panel, without being able to compare treatment (unemployment due to a recent plant closure) and control group. When we remove the requirement that respondents report ex-post on a plant closure, we can extend our sample and allow respondents to leave the panel in $t = 0$. Thereby, we identify future panel quitters who report on satisfaction in $t = -1$, before they subsequently leave the panel and are not observed in the panel any longer. In comparison to the respondents who are still in the panel in $t = 0$, we observe that panel quitters report less free-time satisfaction and income satisfaction in their last interview in $t = -1$. This is also true for life satisfaction, for which this phenomenon of the dissatisfied panel quitter has been documented in previous research based on SOEP data. In a comprehensive discussion of this finding, Chadi (2019) shows that the correlation between satisfaction and future panel exits does not necessarily imply that truly dissatisfied respondents are more likely to leave (thereby potentially provoking attrition bias), but it could also be that they misreport their satisfaction due to low motivation. In any event, we cannot clarify to what extent people in our treatment and control groups differ when we pursue an analysis of future panel exits.

A different approach relies on the idea of using refreshment samples, as has been proposed specifically for the case of subjective survey data (Chadi 2021), which allows us to learn more about the attrition due to plant closures. Given that the SOEP from time to time complements the ongoing panel sample with fresh draws from the German population, it is possible to use the refreshment data as a gold standard that ideally reflects how the panel data should look like in the absence of attrition bias. When we leverage the refreshment samples implemented by the SOEP during our investigation period (in 2006, 2011, and 2012), we obtain the following insights: First, the likelihood of a plant closure being reported is significantly higher in the refreshment data. While there is some variation across panel refreshments implemented over the years, this finding is quite robust and suggests that long-term panel respondents are more likely to leave the SOEP data when a plant closure takes place. Second, this type of attrition is not linked in a consistent and significant manner to the satisfaction variables that we focus on in our paper, which speaks against the idea of our results being driven by attrition bias.

Changes of permanent income

In the following, we revisit our conclusion that changes of income triggered by unemployment do not fully explain the loss of income satisfaction. Given that long-term losses of income due to unemployment (e.g., Stevens 1997; Huttunen et al. 2011) could affect one's assessment of the current situation, a few studies examine the role of permanent income changes for the life satisfaction of non-employed individuals (e.g., Knabe and Rätzel 2011; Bayer and Jüssen 2015). We pursue different ways of testing if changes of permanent income explain the decline in income satisfaction observed in unemployed workers. Thereby, we again control for transitory income changes via a variable representing the change of income from the second-last interview ($t = -2$) before the 'treatment group' becomes unemployed to the first interview afterwards ($t = 0$).

First, assuming that unemployment is more detrimental to long-term income for highly educated individuals (e.g., Braga 2018), we analyze possible differences in the income satisfaction effect dependent on educational attainment. We split the sample at 11.5 years, as 12 years are at least needed in most cases to obtain a secondary school-leaving qualification granting access to higher education in Germany ('Abitur'). We find that unemployed people with relatively many years of education hardly differ in their decline of income satisfaction (–1.8) from those with relatively few years of education (–1.7). Second, the SOEP allows us to take into account income prospects based on subjective assessments of the respondents' employment and income situation. We identify workers with low employment security if they indicated at $t = -2$ that they would have a hard time finding a new job in the event of unemployment. Those with bad prospects experience a stronger effect on income satisfaction. Still, those who think they would have relatively good chances to find a new job experience a sharp decline in income satisfaction as well (–1.1). Third, we observe a similar picture based on a SOEP question about how concerned respondents are regarding their financial situation. What is more, analyzing changes in these concerns over time reveals that those who report worsening concerns about their income situation when entering unemployment experience a similar decline in income satisfaction compared to those who report no such change.

In a nutshell, some of the evidence from our additional analyses suggest a possible role of permanent income changes. Still, taken together with transitory changes of income, these permanent changes of income do not fully explain the negative income satisfaction

effect of unemployment. We conclude that jobless people suffer a severe loss in income satisfaction beyond the effects of changes in income due to a non-pecuniary benefit from being employed.

Appendix C. Starting a new job

In our paper, we discuss the subjective benefits of employment and unemployment by focusing on workers leaving the labor market. An important question especially from a policy perspective is whether subjectively valued income and leisure also matter for the individual decision to (re)enter the labor market. If our conclusions on income-related benefits of employment are accurate, and income satisfaction is a suitable measure to capture such benefits (or the lack thereof), we should expect that relatively low levels of income satisfaction may predict job take-up in the future. Similarly, relatively high satisfaction with free time might prevent individuals from entering the labor market.

We address these ideas empirically in this appendix. To do so, we estimate the probability that non-employed individuals start a new job by the time of the next SOEP interview dependent on our subjective indicators of interest. Our analysis relies on 13,625 observations in non-employment, coming from people who are of working age (18–60 years) and have not retired yet. We do not distinguish between types of non-employment, which ensures a large sample of people who are outside of the labor force, including those who may not actively search for a job. Still, in 1561 cases, individuals enter employment by the next SOEP interview, i.e., within approximately 12 months. We estimate whether or not an individual belongs to this group dependent on satisfaction with household income and satisfaction with free time in a regression analysis where we employ binary variables to distinguish between high (above the median of 7) and low satisfaction (at the median and below).

To take other influences on the decision to start a new job into account, we expand the analysis using covariates that could explain why people differ in their individual willingness to search a job or accept job offers. For instance, unmarried people might be more flexible and more mobile. On the contrary, home owners, people with children or other adults in need of care living in the same household may be less flexible to accept job offers. Also, the highly educated might receive more (attractive) job offers and thus enter employment more quickly. Employers may also take previous unemployment experience and employment experience into account. What is more, the demand for labor may vary across federal states and years.

Other important predictors of job take-up could be health and the situation of the partner. To account for the former, we include disability status and doctor visits. Serious (mental) health issues may be accounted for by these variables. Apartment size and home ownership are measured at household level and may reflect the partner's situation to some extent. These variables may also capture differences in wealth. The results are presented in Table C1.

Table C1
Probability of job take-up within next year.

	(1)	(2)	(3)
Satisfaction with income, above median	−0.032***	−0.039***	−0.044***
	(0.008)	(0.009)	(0.009)
Satisfaction with free time, above median	−0.005	0.002	0.004
	(0.008)	(0.008)	(0.008)
Age		−0.004***	−0.006***
		(0.001)	(0.001)
Female		−0.029**	−0.012
		(0.011)	(0.012)
Home ownership		0.014	0.006
		(0.010)	(0.010)
Size of apartment		0.000***	0.000
		(0.000)	(0.000)
Number of persons in household		−0.008*	0.003
		(0.005)	(0.005)
Children (ref.: no)			
One		0.034***	0.010
		(0.013)	(0.013)
Two		0.027*	−0.000
		(0.015)	(0.015)
Three or more		−0.020	−0.046**
		(0.019)	(0.019)
People in need of care in the same household		−0.045***	−0.037***
		(0.015)	(0.014)
Partnership / marital status (ref.: single)			
Married		−0.043**	−0.060***
		(0.019)	(0.019)
Divorced		0.009	−0.006
		(0.018)	(0.018)
Separated		−0.002	−0.018
		(0.032)	(0.032)

Table C1 (*continued*)

	(1)	(2)	(3)
Widowed		−0.016	−0.026
		(0.024)	(0.023)
Partner		0.068***	0.059***
		(0.017)	(0.017)
Level of education (ISCED)			0.021***
			(0.003)
Lifetime employment experience			0.005***
			(0.001)
Lifetime unemployment experience			−0.004***
			(0.001)
Disability			−0.018
			(0.014)
Visits to the doctor			−0.001**
			(0.001)
Constant	0.110***	0.240***	0.256***
	(0.021)	(0.034)	(0.035)
State dummies	yes	yes	yes
Year dummies	yes	yes	yes
Observations	13,625	13,625	13,625
R^2	0.008	0.035	0.056

Source. SOEP 1999–2015.

Note. The table presents OLS estimates of taking up a new job from the current to the next SOEP interview based on a sample of non-employed individuals. *denotes significance at the 10 % level / **5 % / ***1 %. Robust standard errors in parentheses.

In the most parsimonious specification, where we control for region and time effects only (Specification 1), we find that high income satisfaction is negatively related to beginning a new job, while free-time satisfaction plays no clear role. This picture does not change once we consider living conditions that might explain why people hesitate to become employed (Specification 2) or control for demand-sided explanations for the probability of transitioning from non-employment to employment (Specification 3).

We confirm our findings as robust in additional analyses when using linear variants of both satisfaction variables, thereby exploiting the full variation across all levels of the scale, or when using different splits (e.g., based on three satisfaction categories). The qualitative results also hold across genders, although the predictive power of income satisfaction for taking up a job is significantly stronger in the male sample compared to the female sample. Finally, as an alternative to the linear probability model of job take-up until the next interview, we conduct a survival time analysis where we treat job take-up as the 'failure event' to study how long it takes before an individual is observed reemployed. This analysis relies on monthly information and confirms our findings in Table C1. In Fig. C1, we illustrate the key result of this additional exercise that low income satisfaction reduces the time workers take until they reenter employment. Table C2 provides the whole findings, including covariate estimates.
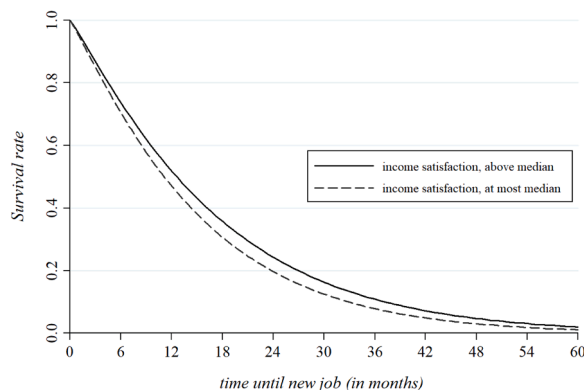
Fig. C1. Income satisfaction and survival in non-employment.
Source. SOEP 1999–2015.
Note. Predicted survival times are based on a survival-time regression with all covariates presented in Table C2 (see the note of the table for further information).

Table C2
Survival-time regression.

'Failure event' new job	(1)	(2)	(3)
Satisfaction with income, above median	−0.198***	−0.144***	−0.142***
	(0.041)	(0.046)	(0.046)
Satisfaction with free time, above median	0.127***	0.048	0.065

(*continued on next page*)

Table C2 (*continued*)

'Failure event' new job	(1)	(2)	(3)
	(0.040)	(0.041)	(0.042)
Age		−0.004	−0.024***
		(0.003)	(0.004)
Female		−0.068	0.022
		(0.060)	(0.064)
Home ownership		0.069	0.002
		(0.047)	(0.047)
Size of apartment		0.001	0.001
		(0.001)	(0.001)
Number of persons in household		−0.051*	0.011
		(0.026)	(0.028)
Children (ref.: no)			
One		−0.060	−0.089
		(0.063)	(0.064)
Two		−0.177**	−0.215***
		(0.073)	(0.075)
Three or more		−0.281***	−0.338***
		(0.098)	(0.100)
People in need of care in the same household		0.178	0.170
		(0.112)	(0.112)
Partnership / marital status (ref.: single)			
Married		−0.159**	−0.196***
		(0.073)	(0.073)
Divorced		−0.076	−0.082
		(0.098)	(0.097)
Separated		0.046	−0.022
		(0.127)	(0.130)
Widowed		−0.361*	−0.265
		(0.194)	(0.198)
Partner		−0.112	−0.175**
		(0.083)	(0.082)
Level of education (ISCED)			0.043**
			(0.017)
Lifetime employment experience			0.031***
			(0.004)
Lifetime unemployment experience			−0.002
			(0.008)
Disability			0.046
			(0.091)
Visits to the doctor			0.001
			(0.006)
State dummies	yes	yes	yes
Year dummies	yes	yes	yes
Observations	2918	2918	2918
Pseudo log-likelihood	−4330	−4281	−4248

Source. SOEP 1999–2015.

Note. Our analysis relies on observations in non-employment, coming from people who are of working age (18–60 years) and have not retired yet. We do not distinguish between reasons for non-employment. Survival time in non-employment is obtained from monthly labor market diaries reported for the previous year. Survival time ranges between 1 and 140 months. Coefficients are based on robust-variance estimator, individual-level clustering and a Weibull distribution. *denotes significance at the 10 % level / **5 % / ***1 %.

Appendix D. Additional material of the survey experiment

Instructions on screen 1 (prior to main choice scenario screen)

Please read the following text thoroughly. In the following, you are requested to assess the social appropriateness of an action by a hypothetical person X in a certain situation. As 'socially appropriate' we consider situational behavior that the majority of people would consider 'right' or 'ethical'. The rating is on a scale from very socially inappropriate to very socially appropriate.

You are asked to estimate how the majority of participants in the survey assess the actions. This is not about your own assessment of X's actions, or about how you would decide if you were in X's place.

Once the survey is completed, we will compute the most frequently indicated rating. If your assessment is accurate, you will be in a draw pot, whereby accurate means that your assessment is identical with the assessment of the majority of participants. Only in this case you have the chance to win [points] worth 50 euros. That is another reason why it is important for you to read the text of the scenario thoroughly before entering your assessment.

Hypothetical choice scenario (vignette) on screen 2

Suppose *Mr (Mrs)* X became unemployed because of a plant closure. He (she) receives financial assistance from public funds in addition to the income of *his (her) partner*[†], with whom *he (she)* has lived in a relationship for several years. The couple have two children.

A short while ago *Mr (Mrs)* X has been offered a job. If *Mr (Mrs)* X decides to accept the job offer the household income of the family will increase slightly. Although *he (she)* would earn labor income again, public assistance could no longer be claimed. If *he (she)* rejects the job offer, *he (she)* will stay unemployed. *He (she)* would then continue to take care of the household and the children. *(He (she) would then continue to pursue hobbies and other leisure activities.)*

Mr (Mrs) X accepts *(rejects)* the job offer.

Note. Treatment variations in italic. The acceptance treatment is only denoted by the last sentence. The gender treatment is denoted by the use of gendered pronouns and salutation. The time use treatment is denoted by the second-last sentence only. [†]*The German language assigns a gender to the word for partner ('Partner' (male) / 'Partnerin' (female)). In each gender treatment, the scenario text gives the gender of the partner in accordance with a heterosexual relationship, meaning that Mr. X has a female partner and Ms. X has a male partner.*

Instructions (below choice scenario) on screen 2

Please assess Mr (Mrs) X's action now. Answer the question, how socially appropriate the participants of the survey consider his (her) behavior. In the process, it is important that you assess as well as possible what the majority of participants think, even if your own opinion would have been different.

Please note that, in the process, you have a chance to win 50 euros in a lottery. Provided that your assessment matches that of the majority of participants presented with the same scenario, you will be in a draw pot and, with a little bit of luck, receive 50 euros.

Own hypothetical choice question on screen 3

Question about your own behavior

Now we would like to know how you would have behaved in the role of X in the context described on the previous page.

I would *(not)* accept the job offer.

very unlikely / somewhat unlikely / somewhat likely / very likely

Please assess your own behavior. Note that this about personal views and your answer cannot increase the chance of winning.

Note. Treatment variation (accept vs. *reject) in italic.*

Appendix E. Additional output of the survey experiment

Table E1
Experimental sample.

Respondent characteristics	Full Sample	Treatment condition		Kruskal-Wallis p-value	N
		Accept	Reject		
Female	0.52	0.52	0.52	0.85	1114
Age	46.94	46.40	47.53	0.24	1106
College degree	0.20	0.21	0.18	0.16	1102
Doctoral visits (3 months)	1.97	1.88	2.06	0.10	1111
Partnership	0.66	0.63	0.68	0.09	1076
Marital status					
Married	0.42	0.40	0.43	0.32	1112
Divorced	0.13	0.13	0.14	0.80	1112
Widowed	0.02	0.02	0.03	0.32	1112
Unmarried	0.40	0.42	0.38	0.16	1112
Other	0.02	0.02	0.02	0.85	1112
Household					
Number of adults	1.84	1.84	1.85	0.52	1103
Number of children	0.38	0.44	0.32	0.03	1111
Home ownership	0.33	0.33	0.34	0.60	1075
Equivalence income	1721.82	1701.83	1743.58	0.26	1090
Employment status					
Marginal	0.13	0.13	0.13	0.84	1114
Part-time	0.05	0.05	0.05	0.96	1114
Full-time	0.40	0.42	0.38	0.17	1114
Self-employed	0.04	0.04	0.03	0.31	1114
Unemployed	0.06	0.07	0.06	0.81	1114
Other	0.32	0.29	0.35	0.07	1114
Daily time use in hours					

Table E1 (*continued*)

Respondent characteristics	Full Sample	Treatment condition		Kruskal-Wallis p-value	N
		Accept	Reject		
Work	5.23	5.41	5.04	0.15	1089
Home production	3.05	2.99	3.11	0.75	1098
Hobbies	3.87	3.86	3.87	0.28	1099

Source. Own survey experiment 2021.

Note. The treatments 'accept' and 'reject' each consist of two times two randomized subgroups regarding the gender of X and time use in unemployment. To check randomization, p-values have been calculated using Kruskal-Wallis tests.

Table E2

Regression analyses of social appropriateness: Interactions with gender.

	Social appropriateness score			Very socially inappropriate (Y/N)		
A) Full sample	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.684***	0.667***	0.665***	−0.235***	−0.228***	−0.227***
	(0.049)	(0.052)	(0.053)	(0.031)	(0.033)	(0.033)
X is female	0.141***	0.110**	0.098*	−0.119***	−0.108***	−0.109***
	(0.047)	(0.050)	(0.050)	(0.037)	(0.039)	(0.039)
X accepts the job #	−0.091	−0.078	−0.079	0.109***	0.097**	0.106**
X is female	(0.069)	(0.074)	(0.074)	(0.042)	(0.045)	(0.044)
Observations	1116	976	976	1116	976	976
R^2	0.239	0.233	0.257	0.078	0.076	0.100
B) Treatment: Leisure	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.852***	0.834***	0.833***	−0.320***	−0.301***	−0.306***
	(0.068)	(0.074)	(0.076)	(0.047)	(0.051)	(0.053)
X is female	0.196***	0.171**	0.173**	−0.185***	−0.177***	−0.194***
	(0.061)	(0.067)	(0.070)	(0.054)	(0.058)	(0.059)
X accepts the job #	−0.179*	−0.176*	−0.185*	0.203***	0.187***	0.209***
X is female	(0.097)	(0.105)	(0.107)	(0.063)	(0.067)	(0.069)
Observations	558	476	476	558	476	476
R^2	0.313	0.302	0.328	0.108	0.100	0.140
C) Treatment: Home production	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.526***	0.520***	0.534***	−0.153***	−0.161***	−0.160***
	(0.068)	(0.072)	(0.074)	(0.041)	(0.043)	(0.043)
X is female	0.100	0.062	0.042	−0.058	−0.046	−0.038
	(0.070)	(0.073)	(0.076)	(0.049)	(0.052)	(0.051)
X accepts the job #	−0.024	−0.004	−0.021	0.017	0.014	0.022
X is female	(0.097)	(0.102)	(0.103)	(0.055)	(0.059)	(0.058)
Observations	558	500	500	558	500	500
R^2	0.172	0.173	0.227	0.056	0.059	0.109
Covariates			yes			yes

Source. Own survey experiment 2021.

Note. Estimates from linear regressions are shown. Covariates include all variables shown in Table E1. Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E3

Regression analyses of social appropriateness: Interactions with time use.

	Social appropriateness score			Very socially inappropriate (Y/N)		
A) Full sample	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.765***	0.745***	0.736***	−0.219***	−0.206***	−0.198***
	(0.049)	(0.053)	(0.053)	(0.032)	(0.034)	(0.034)
Time use:	0.188***	0.151***	0.132***	−0.098***	−0.076*	−0.077*
home production	(0.047)	(0.050)	(0.051)	(0.037)	(0.039)	(0.040)
X accepts the job #	−0.248">***	−0.226>***	−0.215***	0.073*	0.051	0.047
home production	(0.069)	(0.074)	(0.074)	(0.042)	(0.045)	(0.046)
Observations	1116	976	976	1116	976	976
R^2	0.244	0.238	0.261	0.075	0.070	0.096
B) Treatment: Male gender	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.852***	0.834***	0.805***	−0.320***	−0.301***	−0.295***
	(0.068)	(0.074)	(0.076)	(0.047)	(0.051)	(0.053)

(*continued on next page*)

Table E3 (*continued*)

	Social appropriateness score			Very socially inappropriate (Y/N)		
Time use:	0.239***	0.209***	0.191**	−0.163***	−0.144**	−0.150**
home production	(0.064)	(0.070)	(0.076)	(0.054)	(0.058)	(0.061)
X accepts the job #	−0.326***	−0.314***	−0.302***	0.166***	0.140**	0.143>**
home production	(0.096)	(0.103)	(0.107)	(0.062)	(0.066)	(0.070)
Observations	570	497	497	570	497	497
R^2	0.276	0.263	0.296	0.113	0.106	0.139
C) Treatment: Female gender	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.674***	0.658***	0.670***	−0.117***	−0.114***	−0.102**
	(0.069)	(0.075)	(0.075)	(0.041)	(0.044)	(0.044)
Time use:	0.143**	0.100	0.111	−0.036	−0.013	−0.018
home production	(0.068)	(0.070)	(0.072)	(0.049)	(0.052)	(0.051)
X accepts the job #	−0.171*	−0.142	−0.149	−0.020	−0.034	−0.038
home production	(0.098)	(0.104)	(0.105)	(0.055)	(0.059)	(0.060)
Observations	546	479	479	546	479	479
R^2	0.213	0.214	0.260	0.044	0.044	0.112
Covariates			yes			yes

Source. Own survey experiment (2021).
Note. Estimates from linear regressions are shown. Covariates include all variables shown in Table E1. Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E4
Regression analyses of social appropriateness: Triple interactions.

	Social appropriateness score			Very socially inappropriate (Y/N)		
	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.852***	0.834***	0.818***	−0.320***	−0.301***	−0.299***
	(0.068)	(0.074)	(0.075)	(0.047)	(0.051)	(0.052)
X is female	0.196***	0.171**	0.153**	−0.185***	−0.177***	−0.185***
	(0.061)	(0.067)	(0.068)	(0.054)	(0.058)	(0.058)
X accepts the job #	−0.179*	−0.176*	−0.162	0.203***>	0.187***	0.197***
X is female	(0.097)	(0.105)	(0.106)	(0.063)	(0.067)	(0.068)
Time use:	0.239***	0.209***	0.185**	−0.163***	−0.144**	−0.152**
home production	(0.064)	(0.070)	(0.073)	(0.054)	(0.058)	(0.059)
X accepts the job #	−0.326***	−0.314***	−0.289***	0.166***	0.140**	0.137**
home production	(0.096)	(0.103)	(0.105)	(0.062)	(0.066)	(0.068)
X is female #	−0.096	−0.109	−0.095	0.127*	0.131*	0.140*
home production	(0.093)	(0.099)	(0.101)	(0.073)	(0.078)	(0.078)
X accepts # female	0.155	0.172	0.142	−0.186**	−0.174*	−0.173*
# home production	(0.138)	(0.147)	(0.148)	(0.083)	(0.089)	(0.090)
Observations	1116	976	976	1116	976	976
R^2	0.251	0.243	0.265	0.094	0.087	0.113
Covariates			yes			yes

Source. Own survey experiment 2021.
Note. Estimates from linear regressions are shown. Covariates include all variables shown in Table E1. Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E5
Regression analyses of choice congruence: Triple interactions.

	Congruence: Average likelihood			Congruence: Very likely (Y/N)		
	(1)	(2)	(3)	(4)	(5)	(6)
X accepts the job	0.975***	0.920***	0.938***	0.396***	0.367***	0.377***
	(0.120)	(0.130)	(0.132)	(0.054)	(0.059)	(0.060)
Female respondent	−0.390***	−0.436***	−0.436***	−0.064*	−0.070*	−0.052
	(0.119)	(0.128)	(0.134)	(0.037)	(0.041)	(0.046)
X accepts the job #	0.302*	0.369**	0.346**	−0.048	−0.024	−0.025
Female respondent	(0.157)	(0.169)	(0.172)	(0.070)	(0.077)	(0.076)
Time use:	0.070	0.017	0.013	−0.001	−0.004	−0.005
home production	(0.127)	(0.137)	(0.139)	(0.043)	(0.046)	(0.049)
X accepts the job #	−0.482***	−0.435**	−0.442**	−0.222***	−0.199**	−0.197**
home production	(0.169)	(0.182)	(0.185)	(0.073)	(0.078)	(0.080)
Female respondent	0.135	0.153	0.136	0.054	0.053	0.034
# home production	(0.174)	(0.186)	(0.190)	(0.056)	(0.060)	(0.063)

Table E5 (*continued*)

	Congruence: Average likelihood			Congruence: Very likely (Y/N)		
	(1)	(2)	(3)	(4)	(5)	(6)
X accepts # female	0.334	0.332	0.353	0.212**	0.194*	0.201*
# home production	(0.229)	(0.243)	(0.248)	(0.099)	(0.107)	(0.107)
Observations	1111	975	975	1111	975	975
R^2	0.228	0.232	0.241	0.137	0.128	0.161
Covariates			yes			yes

Source. Own survey experiment 2021.

Note. Estimates from linear regressions are shown. Covariates include all variables shown in Table E1. Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Data availability

Data and code will be made available on request.

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