Wealth as Private Insurance. Unequal Protection against the Scarring Effects of Unemployment in the U.S. and Germany

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Abstract

Job loss has detrimental and long-lasting effects on workers, including lower wages after reemployment. The size of these wage scars, however, depends on the safety nets available during unemployment. Whereas prior research has focused on public safety nets, particularly unemployment insurance, we extend this perspective by considering the private safety net provided by household wealth. Because access to wealth is unequally distributed, private insurance may reinforce the stratifying effects of unemployment on subsequent wage inequality. Comparing the United States and Germany allows us to assess how private wealth interacts with public insurance in buffering the effects of job loss. Drawing on two high-quality panel surveys (PSID and SOEP) and difference-in-differences estimators we show that in the United States, wage scars are larger among workers with little wealth than among workers with greater wealth, producing pronounced inequality in post-unemployment outcomes. We find no comparable pattern in Germany, suggesting that wealth becomes an important safety net primarily where public insurance is weak. Additional analyses show that even in the United States, household wealth protects only the wealthiest households and only during shorter unemployment spells. The effect of wealth on wage scars appears to operate mainly through differences in job-search strategies.

Introduction

Job loss carries a host of detrimental effects on the lives and future prospects of individuals, such as the risk of downward social mobility and material deprivation (Bane and Ellwood 1986; Brady, Finnigan, and Hübgen 2017; Brand 2015; DiPrete and McManus 2000a; Jahoda, Lazarsfeld, and Zeisel 1974). The negative effects of job loss have also been shown to be long-lasting, namely in the form of 'scars' in later earnings trajectories: wages earned after unemployment tend to be lower than those before unemployment (Couch and Placzek 2010; Gangl 2004). A large empirical literature has analyzed the individual and institutional factors shaping these 'scarring effects' of unemployment (e.g., Cha and Morgan 2010; Dieckhoff and Giesecke 2024; Gangl 2006; Mooi-Reci and Ganzeboom 2015). In this contribution, we expand this perspective to assess under which institutional conditions *household wealth* serves as an effective insurance system and stratifying force. We argue that the private insurance provided by wealth can be an important contributor to the unequal impacts of unemployment, in particular in a system with insufficient public safety nets. This trade-off between private and public insurance is identified through a comparison between the United States and Germany.

Household wealth is a distinct dimension of social stratification and an important predictor of life chances net of other socio-economic factors (Hällsten and Thaning 2022; Killewald, Pfeffer, and Schachner 2017; Skopek, Buchholz, and Blossfeld 2014). For instance, household wealth is strongly associated with children's educational attainment (Dräger 2023; Pfeffer 2018; Pietrolucci 2023). Less is known so far about the stratifying effects of wealth on further transitions during the life course. The 'trigger events' framework proposed by DiPrete (2002) argues that negative life-course events, such as unemployment or divorce, play a major role in determining the location of individuals in the distribution of socio-economic well-being. A growing body of research rooted in this perspective has examined how the impact of job loss on income varies among different groups, including income groups (Di Nallo and Oesch 2020; DiPrete and McManus 2000b; Ehlert 2013), social classes (Vandecasteele 2011), and family types (Ehlert 2012). In this study, we show how

and under which circumstances household wealth influences the size and heterogeneity of wage scars due to unemployment. By doing so, we also establish a link between wealth inequality and wage inequality beyond the well-known accumulation mechanism (Killewald et al. 2017). According to the latter, wage inequality can lead to wealth inequality. According to our framework, wealth inequality can lead to wage inequality.

The variation in wage scars across groups is a crucial aspect of social stratification as it points us towards the existence of safety nets available to impacted individuals, i.e., resources that limit the consequences of unemployment. Prior research has shown that public safety nets, such as unemployment insurance (UI), not only reduce income losses during unemployment but can also improve post-unemployment wages because impacted individuals can afford to decline low-paying job offers (Burdett 1979; Gangl 2004, 2006). Beyond public insurance schemes, research additionally points towards the importance of private safety nets, showing that family resources such as partner's earnings provide important buffers for unemployment spells (DiPrete and McManus 2000a; Ehlert 2012). Moreover, private safety nets also seem to influence job search behavior and unemployment duration (Jacob and Kleinert 2014; Lammers 2014; Lentz and Tranæs 2005). From a stratification perspective, private safety nets are important to study because, unlike many public safety nets, they are available only to some and thus can be expected to have stratifying effects on future earnings trajectories.

Household wealth may be a particularly effective private safety net as it most closely resembles private unemployment insurance. In fact, economists often conceptualize wealth accumulation as primarily driven by individuals' motivation to "self-insure" against labor market risks (Gruber 2001). Unlike other socio-economic resources, such as earnings, occupational status, or education, it can be converted from a stock to an income flow, creating an income stream during times of unemployment. Recent research provides some evidence for wealth's insurance function (see also Müller, Pforr, and Dräger 2023). For instance, in the U.S., high wealth is associated with a lower probability of experiencing financial hardship after critical life events (Rodems and Pfeffer 2021) as well as higher wage growth after unemployment (Baley et al. 2025). Research on the role of housing wealth shows that the

losses in income after job loss are smaller among homeowners than renters (Bedük 2023; Caliendo, Gielen, and Mahlstedt 2015).

As mentioned, a fundamental difference between a private safety net provided by household resources and a public safety net provided, for instance, by unemployment insurance is that access to the former is more unequally distributed than to the latter. Inequality in household resources is particularly pronounced when it comes to wealth, more so than any other socio-economic resource (Keister and Moller 2000; Killewald et al. 2017). Moreover, those who become unemployed usually own less wealth than those who remain employed. Dickens, Triest, and Sederberg (2017) showed that this disparity even grew over time between the 1980s and the 2010s. Our focus on household net worth is thus also motivated by a concern about the distribution of access to effective insurance against job loss and the resulting further stratification of living conditions based on the potentially heterogeneous impacts of unemployment on future earnings.

We submit that the effectiveness of public and private safety nets against the negative impact of job loss needs to be studied in conjunction, as the two can interact and shape each other's effectiveness (Bedük et al. 2025; DiPrete 2002; Ehlert 2016; Rhodes et al. 2026). Like prior research (e.g., Gangl 2004), we use the United States and Germany as two attractive comparative cases. The two nations are marked by different levels of public insurance against job loss: UI eligibility rates and earnings replacement rates are higher in Germany than in the U.S., thus providing more encompassing public insurance than in the U.S. At the same time, the two countries share a perhaps surprisingly similar and high degree of inequality in the distribution of household wealth, albeit with differences in portfolios (Pfeffer and Waitkus 2021).

We analyze whether wealth reduces wage scars due to unemployment and whether this insurance function varies between the U.S. and Germany, two systems with different levels of welfare state generosity. Drawing on high-quality household panel data from the United States (the Panel Study of Income Dynamics, PSID) and Germany (the German Socio-Economic Panel, SOEP) and difference-in-difference estimators, we estimate the interaction effects of wealth and unemployment on wages within differing country contexts. Our work adds to a broad literature on social stratification by analyzing mechanisms that

connect wealth and income inequality. The two literatures have so far not been well connected, and we join recent efforts to combine them (Brady 2022; Pfeffer and Waitkus 2021). More specifically, this work expands the long-standing and active literature on the scarring effects of unemployment (Gangl 2004, 2006; Nekoei and Weber 2017; Schmieder, von Wachter, and Bender 2016), by considering wealth as a foundational concern when studying the relationship between trigger events and economic insecurity (Western et al. 2012). Finally, we pay particular attention to gender differences as labor market careers and the effect of career disruptions continue to differ between women and men (England, Levine, and Mishel 2020). While we do not directly attend to the causal mechanisms that may produce wage scars (reviewed below), we do provide additional analyses about unemployment duration and wealth dynamics that add suggestive evidence on the factors behind the wealth effect on wage scars.

Theoretical Background and Hypotheses

In this section, we draw on labor market theory and the literature on the role of "trigger events" (DiPrete 2002) to develop theoretical predictions about how and why wage scars due to unemployment vary across the wealth distribution. We furthermore theorize how institutional settings, as well as unemployment length and gender, influence the wealth gradient in wage scars. In particular, we build on the seminal work by Gangl (2003, 2004, 2006) who combined labor market theories with welfare state theories for his analysis of the institutional influences on the scarring effects of unemployment. Gangl's work highlighted the usefulness of job search theory (Mortensen and Pissarides 1999) as a micro-level foundation for such research questions. Our focus on wealth as a fundamental dimension of stratification (Killewald et al. 2017) additionally draws on economic theory on consumption and life-cycle saving (Friedman 1957).

Wage Scarring and Wealth

We begin with the observation that wage scars exist because workers are restricted in their choice and their opportunities to obtain a job that pays the same as the job they lost (Logan 1996). During unemployment, jobseekers make decisions about new job offers they

encounter. Job search theory posits that workers base these decisions on their reservation wage, i.e. the minimum wage they personally perceive as acceptable (Addison, de Freitas Centeno, and Portugal 2004; Mortensen and Pissarides 1999). Reservation wages are influenced by budget constraints that jobseekers face: a lack of economic resources implies a higher urgency of regaining employment and thus lower reservation wages. In line with this argument and drawing on international comparisons, Gangl (2004, 2006) shows that additional economic resources in the form of more generous unemployment benefits are correlated with higher post-unemployment wages.

Of course, budget constraints among the unemployed can also be met by other economic resources; and household wealth may be a particularly relevant one. In fact, this is the basic assumption behind life-cycle models in economics (Friedman 1957; Modigliani and Brumberg 1954). Here, individuals are assumed to save part of their income, i.e., to accumulate wealth, primarily to smooth consumption over their life course, especially during unemployment (Browning and Crossley 2001). In line with this theory, Browning and Crossley (2009) show that consumption during unemployment is higher among households with liquid assets. Life-cycle models thus suggest that wealth carries an "insurance function" for the unemployed much like unemployment insurance (Gruber 2001; Lentz and Tranæs 2005). In both cases, the unemployed can draw on additional resources to be more selective about the job offers and continue their job search if the offers do not fit their expectations.

However, private insurance through wealth differs in several respects from public insurance through UI. Most importantly, wealth is highly unequally distributed and therefore access to a level of wealth that is sufficient in terms of its insurance function is restricted to a small group of the unemployed. In contrast, unemployment insurance, even though it is often subject to eligibility requirements, is much more broadly available. This has important implications for the stratifying effect of the two types of insurance: While we should expect compensatory effects of UI for most of the unemployed, the compensation provided by the insurance function of wealth should be restricted to the upper parts of the wealth distribution.

Another important difference between wealth and UI is how easily it can be used for consumption. While UI is paid out as additional income, wealth comes in different forms of "liquidity". We discuss further below that it is not strictly necessary for wealth to be liquid

to fulfill the insurance function. But here we acknowledge that different degrees of asset liquidity exist: Savings can easily and directly be used for consumption (Basten, Fagereng, and Telle 2016) while other assets, such as stocks, must be sold first and yet others, such as homes, can also be made liquid by serving as collateral for borrowing. These differences are differences in degree and, in our view, do not allow a clear distinction between liquid and non-liquid forms, in particular in the context of financialized economies that provide households with increased access to credit markets (e.g., Fligstein and Goldstein 2015; Lin and Neely 2020). This is but one argument for our decision to follow the established literature (Spilerman 2000) by using net worth as our main wealth measure. Another argument is that net worth considers the role of debt, which may directly restrict the budget of the unemployed, e.g., through the pressure of interest payments or limits to further borrowing.

Finally, very high wealth could also play a different type of insurance function, namely complete decommodification in that it allows individuals to never return to the labor market after unemployment (and instead live off their wealth). If at all, this extreme form of insurance likely emerges only at the very top of the wealth distribution, say, within the top one percent and it may generally be limited by non-monetary benefits of employment (e.g., Eberl, Collischon, and Wolbring 2023) that likely still play a role even at high levels of wealth. While the survey data we use fail to capture this group reliably, a pattern of wealth-selective returns to the labor market would complicate our assessment of wage scarring. We test (and rule out) this option in our analyses.

Based on the aforementioned considerations, we propose our first hypothesis:
Hypothesis 1: Higher household net worth leads to lower unemployment scars in earnings trajectories. We furthermore expect a non-linear relationship between wealth and scars where the reduction in scars mainly occurs for the top of the wealth distribution.

Length of Unemployment

As stated, search theory predicts that sufficient economic resources during unemployment help maintain reservation wages for a longer period. Longer search durations, in turn, increase the likelihood of finding a corresponding offer. Accordingly, economic resources lead to higher wages after re-employment through increased unemployment

durations. Following this line of reasoning, high wealth should become especially impactful for longer spells of unemployment. Clearly, household wealth is finite. We should therefore expect that reservation wages and thus re-employment wages eventually fall.

Contrary to this, a recent contribution by Eeckhout and Sepahsalari (2024) suggests that unemployment duration is not the mechanism behind the effects of wealth on wage scarring. According to the "precautionary search motive" they introduce, unemployed workers without assets tend to opt for "easy-to-get" but low-paid jobs, which provide financial security quickly, whereas their wealthier counterparts apply for jobs in higher tiers of the labor market, which are more difficult to attain and riskier to keep. The unemployed with sufficient assets can afford to pursue this strategy because they have their private safety net. Consequently, high wealth mainly affects risk aversion during unemployment and induces search strategies that lead to better post-unemployment outcomes given the same search duration.

As unemployment duration increases, there are additional pressures for the unemployed: Longer unemployment duration may lead to human capital depreciation (Schmieder et al. 2016) and employers may use a long duration of unemployment as a negative productivity signal (Van Belle et al. 2018). Existing empirical evidence supports the notion that effective insurance against the effects of unemployment is limited to shorter unemployment spells. Nekoei and Weber (2017) show that the extension of unemployment benefits in Austria leads to better post-unemployment outcomes but this result only emerged for short and medium unemployment durations, not for long-term unemployment. In line with this, Schmieder et al. (2016) show that extensions of already long benefit durations in Germany lead to negative effects on post-unemployment wages.

In sum, we expect that the private insurance provided by high wealth reduces the scarring effects of shorter unemployment spells. For long unemployment spells it may be less effective – even high wealth eventually runs out and the negative effects of unemployment duration manifest regardless of net worth. Furthermore, it may even be that different job search strategies – and not increased search duration – is the mechanism behind the wealth gradient in wage scars. With that, the heterogeneity of wage scars across the wealth

distribution should be more pronounced for shorter spells than for longer spells. This leads to our second hypothesis:

Hypothesis 2: The effect of wealth on wage scars is stronger for shorter unemployment spells than for longer unemployment spells.

Welfare State and Institutional Context

We expect the effects hypothesized so far to be highly context dependent as wage scarring and the role of wealth should depend on welfare state institutions, labor market institutions, and institutions governing wealth and credit.

First, as acknowledged before, there should be direct interactions between public and private insurance against unemployment. Gruber (2001) showed that the unemployed use their wealth much less for consumption if they have access to UI. Thus, UI is usually tapped first, presumably to maintain wealth for future consumption. Furthermore, research in the U.S. showed that more generous public insurance programs improve access to credit among low-income households (Rhodes et al. 2026). Thus, if unemployment insurance is generous, the stratifying impact of wealth should be smaller. Conversely, in contexts with less generous unemployment insurance, wealth should become more important.[1]

The United States has much less generous unemployment insurance than Germany. As unemployment insurance is regulated at state level, there is a wide variety of policies across the country (for an overview, see Woodbury 2014). Overall, wage replacement rates amount to around 50% of lost wages, up to a certain earnings threshold. In most states, the maximum duration of regular benefits is 26 weeks. Additionally, federal programs can extend the duration of benefits by up to one year during economic crises. Eligibility criteria also vary widely between states. Some states impose considerable barriers to receiving benefits for low-wage and part-time workers (Wenger 2003). After UI benefits run out, there are only a few scattered and insufficient poverty relief programs such as “food stamps” (Bruch, Meyers, and Gornick 2018).

In Germany, the unemployed receive 67% of their former monthly net wages if they live with children and 60% if they live without children up to an earnings ceiling (for an

overview, see Wörz 2011). The regular duration of benefits is one year after having worked in insured employment for about two years. For older workers with long tenure, the duration of the benefits is longer. As in the United States, short-term employees are excluded from benefits. Low-wage workers, on the other hand, are eligible in Germany because there is no earnings requirement. Beyond unemployment insurance, Germany has offered a means-tested minimum income scheme for the long-term unemployed since 2005. Before, the long-term unemployed received 57% of their former wages (53% without children) indefinitely.

In addition to unemployment insurance, employment protection legislation (EPL) may influence how wealth affects wage scars. EPL governs how difficult it is to hire and fire workers. Strict EPL impedes labor market mobility because there are fewer job displacements and fewer job offers. Research has demonstrated important institutional complementarities between UI and EPL in influencing post-unemployment outcomes: Countries with generous UI and weak EPL experience the least severe wage penalties following unemployment (Biegert 2017; Gangl 2006). This suggests that the scar-reducing effects of UI depend on a labor market offering many job opportunities. The same may be true of the effect of wealth on scars: lower budget constraints only lead to higher post-unemployment wages if the labor market produces a large number of (well-paid) job offers. Typical for a 'liberal' welfare state (Esping-Andersen 1990), the U.S. has a weak EPL, whereas Germany's EPL is much stricter.

The cross-national differences in welfare state and labor market institutions already suggest a more exposed role for wealth in the United States. The same, however, can also be expected based on cross-national differences in wealth portfolios, credit market regulations, and financial cultures. Pfeffer and Waitkus (2021) found that wealth portfolios differ significantly between the U.S. and Germany: While around 30% of household wealth in the U.S. is held in the form of financial assets, this figure is 17% in Germany. Conversely, the majority of German wealth is held in the form of housing equity (64%), whereas in the U.S. it amounts to only 33%. At the same time, the U.S. has a much more deregulated mortgage market and a financial culture that relies heavily on credit (Fligstein and Goldstein 2015). Germany, on the other hand, experienced a decline in household debt during the 2000s (Van Gunten and Navot 2018). The higher accessibility of secondary mortgage markets in the U.S. enables households to more easily use their housing wealth for consumption.

Taken together, these institutional differences lead us to the following hypothesis:
Hypothesis 3: The effect of household wealth on wage scars is larger in the United States than in Germany.

Gender

The effect of wealth on wage scars may also vary by gender due to gender gaps in access to wealth. Research shows that there is a substantial gender wealth gap with women owning less on average both in the population at large and within couples (Grabka, Marcus, and Sierminska 2015; Lee 2022). Growing evidence suggests that access to wealth is unequally distributed even within couples (Bessière and Gollac 2023; Lersch, Struffolino, and Vitali 2022), both due to differences in ownership and in the actual control of wealth (Tisch and Lersch 2021). Such gender gaps in access to wealth would suggest that the insurance function of wealth may be less accessible to women.

The same could also result from situations in which households are less willing to access wealth for the lower-earning partner in the household, more often women, compared to the main earner, more often men. Yet, this mechanism may also lead to the withdrawal of female secondary earners from the labor market after job loss. Research shows that individuals who contributed a small share to household income before unemployment are less likely to exit unemployment (Jacob and Kleinert 2014). We therefore control for the share of household income contributed when assessing the direct effect of gender and also test for gender-specific re-employment patterns.

Taken together, we hypothesize the following:
Hypothesis 4: The effect of household wealth on wage scars is larger for men than for women.

Wealth Scars

What we have outlined so far implies that wealth inequality may impact wage inequality: wealth can be consumed – by some – to avoid lower future wages. This followed from the arguments offered by life-cycle theory and search theory discussed above. In this framework, individuals may face a trade-off between a wage scar and "wealth scar", i.e., a decrease of

net worth during unemployment. They may keep their position in the wage distribution by moving downward in the wealth distribution.

However, wealth could also be hypothesized to lead to better post-unemployment outcomes without a direct reduction in assets. This follows from Eeckhout and Sepahsalari's (2024) "precautionary search motive" introduced earlier. If wealth indeed mainly affects search strategies, we should not find larger wealth scars among unemployed people with high net worth. To shed light on the mechanisms connecting wealth and wage scars, in supplemental analyses, we therefore also explore wealth scars.

Data and Method

Estimands

Based on our hypotheses, we distinguish four estimands. First (a), we assess whether household net worth moderates the effect of unemployment on subsequent wages, i.e., whether there is a wealth gradient in the wage scarring effects of unemployment. Second, we examine whether this moderating effect varies across subgroups: (b) short versus long unemployment episodes, (c) women versus men, and, importantly, (d) the U.S. versus Germany. Empirically, this requires estimating the causal interaction between unemployment and wealth, and whether this interaction varies across unemployment duration, gender, and country.

In the estimation of causal interaction effects, selection and confounding factors for both the main effects and the interaction effect have to be considered. We illustrate the problem using "directed acyclic graphs" (DAGs) (Pearl 1995) and "interaction DAGs" (IDAGs) (Nilsson et al. 2021). IDAGs have been developed with the purpose of depicting interaction effects, i.e., how variables in a DAG influence the size of an effect.

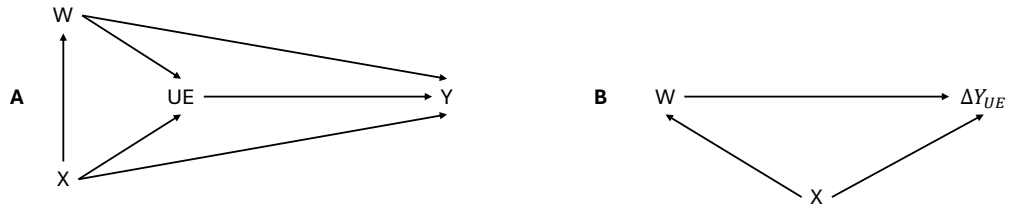

Figure 1. Directed Acyclic Graphs of Our Main Analyses
Note: Panel A shows the DAG for the main effect of unemployment (UE) on future wages (Y). Wealth (W) and other variables (X) confound this effect. Panel B shows the IDAG for the causal interaction between wealth (W) and the effect of unemployment on future wages (ΔY_{UE}). This effect is confounded by other variables (X).

Figure 1 shows a DAG and an IDAG corresponding to our estimands and hypotheses. Panel A shows the effect of unemployment (UE) on future wages (Y). To estimate the causal effect of UE on Y, we need to control for all confounding variables X. Wealth (W) may also have an effect on the occurrence of unemployment and on future wages. Yet, this could be partly due to other factors in X. Panel B shows the main estimand, the interaction between wealth (W) and the effect of unemployment on future wages depicted by ΔY_{UE}. This interaction may be confounded as well, as indicated by the relationship with X. In other words, we need to control for X not only for the effect of unemployment on wealth but also for the interaction between unemployment and wealth.

Below, we first describe the data used to produce our empirical estimates. We then turn to the empirical strategy and how we implement it given the available data.

Data

Our analyses draw on representative longitudinal data from the U.S. Panel Study of Income Dynamics (PSID) and its German counterpart, the Socio-Economic Panel (SOEP). The PSID (PSID 2024) is a nationally representative study that tracks U.S. households over time; we draw on the nationally representative (SRC) and the low-income (Census) oversample. The SOEP (Goebel et al. 2019), launched in the 1980s, initially surveyed West German

households and expanded in 1990 to include East Germany, which accounts for approximately 21% of the German sample by 2022. Both studies track individuals annually (biennially in the PSID since 1997) and collect data on all adult household members, either directly or via proxy. They provide comparable information on earnings histories, unemployment experiences, and household wealth. Due to the switch to biennial interviewing in the PSID, we adjusted the construction of our treatment and control group in the SOEP accordingly as further described in the "Analytical Strategy" section.

To enhance cross-national comparability, we use harmonized labor income measures from the Cross-National Equivalent File (CNEF). The CNEF (2024) harmonizes data from multiple national panel surveys such as the PSID and SOEP by applying country-specific adjustments to account for differences in survey design, question wording, and institutional contexts, ensuring consistency across datasets (Frick et al. 2007). These adjustments are essential to ensuring the comparability of wages—and consequently wage scarring—between Germany and the U.S.

In both the PSID and the SOEP, we restrict our sample to individuals aged 25 to 55, representing the prime working years. We exclude self-employed individuals, as wage scarring is not applicable to them in the same way as it is to wage earners. After removing cases with missing values, our final analytical sample comprises 41,025 person-years from 10,640 individuals in the PSID and 30,643 person-years from 9,720 individuals in the SOEP (see Appendix Table A.1 for sample descriptives). In both countries, most missing values arise from the wealth measure due to the survey design (wealth is collected only in selected waves), followed by the dependent variable and, to a lesser extent, the remaining controls. We describe how we address missingness in the wealth measure in the following section, where we outline the operationalization of our key variables.

Operationalization

Unemployment. The treatment variable, unemployment, is measured using retrospective activity calendars. In both the SOEP and the PSID, respondents were asked to provide monthly information about their employment and unemployment in the previous years using a calendar. We generated unemployment spells from the calendar data and created variables

for every person-year containing information about the occurrence of a spell and its length. We use this data to differentiate between short-term unemployment, defined as an unemployment spell lasting up to five months, and long-term unemployment, defined as six months or more. Note that we cannot differentiate between involuntary and voluntary unemployment due to data issues. Although both datasets capture reasons for job loss in some way, the information in the PSID is inconsistent over waves. As a result, our estimates of the wage scarring effect of unemployment likely represent a lower bound, as our measure also includes periods of voluntary unemployment between jobs.

Wages. To measure our dependent variable wages, we rely on comparable information from the CNEF. Since labor income in the CNEF is available only as annual earnings, we derive hourly wages by dividing annual earnings by total yearly work hours, and we set negative wages to zero. To interpret wage scarring as approximate percentage changes from previous wages and to achieve a more normal and homoscedastic distribution, we apply a logarithmic transformation to hourly wages. Furthermore, we adjust earnings measures for inflation, converting them to 2022 prices using the respective consumer price indices.

Wealth. Both surveys include detailed modules on household assets. In the PSID, these modules were administered every five years between 1984 and 1999 and every other year thereafter. We obtain harmonized and imputed wealth data from PSID-SHELF, a user-generated dataset that harmonizes PSID variables to improve comparability across waves by accounting for changes in coding frames, top-coding, and question modifications (Pfeffer, Daumler, and Friedman 2025). In the SOEP, we use wealth measures from 1988, 2002, 2007, 2012, and 2017, drawing on imputed variables provided by the SOEP Team (Grabka and Westermeier 2015). Although a more recent wealth module was administered in 2022, we cannot use this wave as the subsequent employment history data are not yet available.

For this study, we focus on net worth, defined as the total value of financial assets (e.g., savings, stocks) and real assets (e.g., home equity, real estate wealth, business wealth) minus any debts. After listwise deletion on all other variables, we lose a substantial number of cases with missing wealth (U.S.: 52.8%; Germany: 86.5%), chiefly because wealth is not collected in every survey wave. For this reason, we logically impute wealth data by carrying

forward observed values for two subsequent years (e.g., assuming a person's wealth in 2012 remains the same in 2013 and 2014). We do not carry wealth observations backward because wealth could be influenced by unemployment. Additionally, we inflation-adjust all wealth measures to 2022 prices using the respective consumer price indices for the U.S. and Germany. To account for differences in household size, we further adjust net worth by dividing total wealth by the square root of the number of individuals in the household (SOEP) or family (PSID) unit.

Given the highly skewed distribution of wealth and potential non-linearities in its relationship with the outcome, we categorize net worth into four groups. Given the centrality of budget constraints for our argument, we first group all individuals with negative or zero net worth into one group. Then we compute tertiles of positive net worth (based on the weighted sample distribution) to create three equally sized groups with low, medium, and high wealth, respectively (see Table 1). As robustness checks, we re-estimate our analyses (i) using an alternative wealth specification that groups the relatively low number of individuals with exactly zero net worth with the low-wealth rather than the negative-wealth category, (ii) based on quartiles of net worth, and (iii) employing alternative wealth measures, namely housing wealth and financial wealth. Results for these robustness checks are presented in Appendix B.

Table 1. Mean and Median Net Worth and Wealth-to-Income Ratios by Wealth Level

	United States		Germany	
Wealth Levels	Net Worth (Mean/Median)	Wealth-to-Income Ratio (Mean/Median)	Net Worth (Mean/Median)	Wealth-to-Income Ratio (Mean/Median)
Negative or zero	-16,026 / -4,760	-0.49 / -0.08	-17,423 / -4,335	-0.40 / -0.09
Low+	15,996 / 13,170	0.72 / 0.17	16,793 / 13,665	0.38 / 0.27
Mid+	94,867 / 83,794	5.15 / 0.76	112,560 / 104,953	2.00 / 1.49
High+	492,481 / 314,882	5.15 / 2.19	486,413 / 321,312	7.05 / 4.09
Overall	88,976 / 28,684	2.37 / 0.33	137,171 / 56,289	2.13 / 0.84

Note: Wealth-to-income ratio calculated as pre-treatment household net worth divided by pre-treatment yearly household labor income.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Controls. We select our control variables guided by prior research, which shows demographic characteristics (e.g., education, age, race, household composition) and employment histories (e.g., employment sector and tenure) to be linked with unemployment risk, post-unemployment wage trajectories, and wealth.[2] Accordingly, we control for hourly wages, share of an individual's earnings in the total household income, working hours, job tenure, occupational sector, employment and unemployment history, age, gender, marital status, number of children in the household, race in the U.S. and migration background in Germany, education, and, in the German sample, residence in East versus West Germany (see Appendix A, Table A.1). All control variables are measured prior to unemployment.

Analytical Strategy

Estimator. To obtain our empirical estimate, we apply a difference-in-difference (DiD) strategy. Thus, we aim to compare wage changes among those who experience unemployment, the treatment group, with wage changes among those who remain employed, the control group. Our treatment occurs in a staggered way, i.e., not everyone becomes unemployed at the same time. Recent studies have identified threats to the identification of

causal effects in such a set-up (Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020). For this approach to yield valid results, the definition of the control group is crucial. However, many prior applications that use two-way fixed effects to estimate DiD models include cases in the control group that do not serve as good counterfactuals to the treatment group. Callaway and Sant'Anna (2021) propose to only use "never treated" or "not-yet-treated" units in the control group. For the analysis of the effects of unemployment, a control group consisting of "never treated" or "not-yet-treated" may lead to misleading estimates because individuals who never become unemployed during their careers are strongly positively selected. Krolikowski (2018) argues that such a strategy greatly overstates the effect of unemployment because "earnings declines from all future job instability will be attributed to the initial displacement event" (p. 1233). Instead, the author proposes to use all periods without unemployment as control cases. This provides a more realistic counterfactual that also takes the population unemployment risk into account.

Based on these considerations, we define our treatment group as individuals who became unemployed and returned to a job afterwards. The control group consists of all consecutive periods of employment also including individuals who were unemployed before or who become unemployed at some later point. We then compare wage trends between these two groups. Thus, wage scarring is defined both with respect to the prior job and to wage trends among those who remain employed. To adjust for differences in wage trends prior to treatment, we further adjust for the control variables mentioned above using both entropy balancing (Hainmueller 2012) and parametric regression, rendering our estimator "doubly robust" (Sant'Anna and Zhao 2020). This implies that the estimate is unbiased if either the weights or the regression model are correctly specified. We thus estimate the following average treatment effect on the treated (ATT),

$$ATT = (Y_t^T - Y_{t-1}^T) - W(Y_t^C - Y_{t-1}^C) \quad (1)$$

where Y is the dependent variable, logged hourly wages, at different points in time t. T signifies those experiencing unemployment (treatment group) while C is the control group. The first differencing removes time-constant unobserved heterogeneity between individuals. The weight W furthermore balances the control group so it becomes similar to the treatment

group on the observables included in our control variable list. To identify our main estimand, the effect of wealth on wage scars, we extend our model further. As discussed above, the simple interaction between wealth and unemployment does not identify the causal interaction effect if there are confounding interactions. Therefore, we additionally add interactions between our control variables and unemployment in the final regression model.

Implementation of the estimation strategy. Since the PSID switched to two-year survey intervals after 1997, we are forced to rely on two-year differences in wages for a large part of the available data. To render estimates comparable, we use these same intervals for the whole U.S. sample as well as for the German sample. Individuals in both datasets must meet the following criteria to contribute a treatment episode for our analysis: they must have been employed for at least ten months two years prior to the observed unemployment spell (t–2), must not have experienced unemployment in the year prior (t–1), must be unemployed for at least one month in t, and must regain employment two years after (t+2).[3] For further analyses, we distinguish between short-term unemployment, defined as an unemployment spell lasting up to five months, and long-term unemployment, defined as six months or more. The control episodes accordingly come from individuals who were employed at t-2, t and t+2. Our resulting analytic sample comprises 1,461 treatment episodes and 39,564 control episodes in the U.S. In Germany, we have a smaller sample with 461 treatment episodes and 30,182 control episodes. Table 2 shows that there are fewer treatment episodes in the upper part of the wealth distribution reflecting the unequal risk of unemployment.

Table 2: Sample Sizes of Treatment and Control Episodes by Country and Analytical Subsample

Wealth Levels	All Cases (Main Analysis)	Long Unemployment	Short Unemployment	Men	Women
Germany					
Negative or zero	T = 114 C = 3,978	T = 58 C = 3,978	T = 56 C = 3,978	T = 55 C = 2,120	T = 59 C = 1,858
Low+	T = 168 C = 8,965	T = 74 C = 8,965	T = 94 C = 8,965	T = 75 C = 4,990	T = 93 C = 3,975
Mid+	T = 120 C = 10,785	T = 51 C = 10,785	T = 69 C = 10,785	T = 60 C = 6,229	T = 60 C = 4,556
High+	T = 59 C = 6,454	T = 22 C = 6,454	T = 37 C = 6,454	T = 28 C = 3,534	T = 31 C = 2,920
Overall	T = 461 C = 30,182	T = 205 C = 30,182	T = 256 C = 30,182	T = 218 C = 16,873	T = 243 C = 13,309
United States					
Negative or zero	T = 295 C = 5,294	T = 90 C = 5,294	T = 206 C = 5,294	T = 130 C = 2,532	T = 165 C = 2,762
Low+	T = 707 C = 16,295	T = 174 C = 16,295	T = 535 C = 16,295	T = 365 C = 8,754	T = 342 C = 7,541
Mid+	T =337 C = 13,151	T = 109 C = 13,151	T = 228 C = 13,151	T = 188 C = 7,383	T = 149 C = 5,768
High+	T =122 C = 4,824	T = 32 C = 4,824	T = 90 C = 4,824	T = 69 C = 2,601	T = 53 C = 2,223
Overall	T = 1,461 C = 39,564	T = 405 C = 39,564	T = 1,059 C = 39,564	T = 752 C = 21,270	T = 709 C = 18,294

Note: T = treatment group; C = control group.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Wage scarring is accordingly measured as the four-year change in logged hourly wages, comparing the wages from two years before to two years after unemployment. We implement the estimation by applying OLS regressions with entropy balancing weights of the following form:

$$y_{it} = \alpha + \beta_1 UE_{it} + \beta_2 W_{it} + \beta_3 (UE_{it} \times W_{it}) + \sum_{k=1}^{K} \gamma_k X_{itk} + \varepsilon_{it} \quad (2)$$

Where y_{it} is the four-year change in earnings, UE_{it} indicates an unemployment episode, $W_{it}$is the wealth level indicator and X_{itk} contains our K control variables. Our central

estimate is β_3, the interaction between unemployment and wealth. Standard errors are clustered at the individual level. We furthermore estimate equation (2) separately by country, unemployment duration, and gender to obtain estimates for the respective contrasts.

Within each subgroup, we test whether the unemployment effect differs between wealth group 1 (WG1, negative/zero wealth) and wealth group 4 (WG4, high wealth), as well as between wealth group 2 (WG2, low wealth) and WG4. For estimands (b)–(d), we assess whether these wealth gradients (WG1 − WG4 and WG2 − WG4) vary across unemployment durations (b), gender (c), and countries (d). Substantively, this amounts to testing difference-in-difference-in-differences contrasts, for example whether (WG1_men − WG4_men) − (WG1_women − WG4_women) differs significantly from zero.[4] The results of all hypothesis tests are reported in Appendix A, Table A.2.

One potential threat to identification is a violation of the common trends assumption, i.e., the assumption that the counterfactual wage trends are similar in both groups. Since this assumption is not directly testable, we compare 2-year pre-trends between treatment and control episodes both across and within wealth groups. Appendix Table A.3 reports the corresponding tests, and Figure A.1 provides a graphical illustration of pre-trends within wealth groups. Unfortunately, we do not have pre-trends for all episodes due to missing data and temporary panel dropout. The analysis shows that pre-trends are mostly similar for our main analysis. Yet, entropy balancing and the conditioning on the control variables did not fully remove differences in pre-trends between treatment and control groups for the analysis of short unemployment. The estimates point to larger wage growth prior to short unemployment episodes among U.S. workers with no or negative wealth and lower wage growth in short unemployment episodes in the first wealth tertile. In Germany, high-wealth individuals who become unemployed have a larger wage growth than the control episodes. For Germany, we also observe potential deviations from parallel pre-trends in the gender-specific analysis for men, with somewhat stronger pre-treatment wage growth than men in the control group. We will interpret the main results below considering these potential violations.

Findings

Wealth and Re-employment

Before we begin with the estimation of the effect of wealth on wage scars, we show that selection into re-employment by wealth group is negligible. Figure 2 shows that the majority of unemployed individuals have returned to employment after two years. The share is quite similar in the two countries, at about 85%. Furthermore, conditional on our control variables, the return to the labor market after unemployment does not vary much across the wealth distribution. In the U.S., unemployed individuals with some wealth even return at higher rates than those with debt or zero wealth, indicating potential negative selection on unobservables in the group with debt. Thus, it seems unlikely that our results are driven by selection into re-employment based on household net worth.

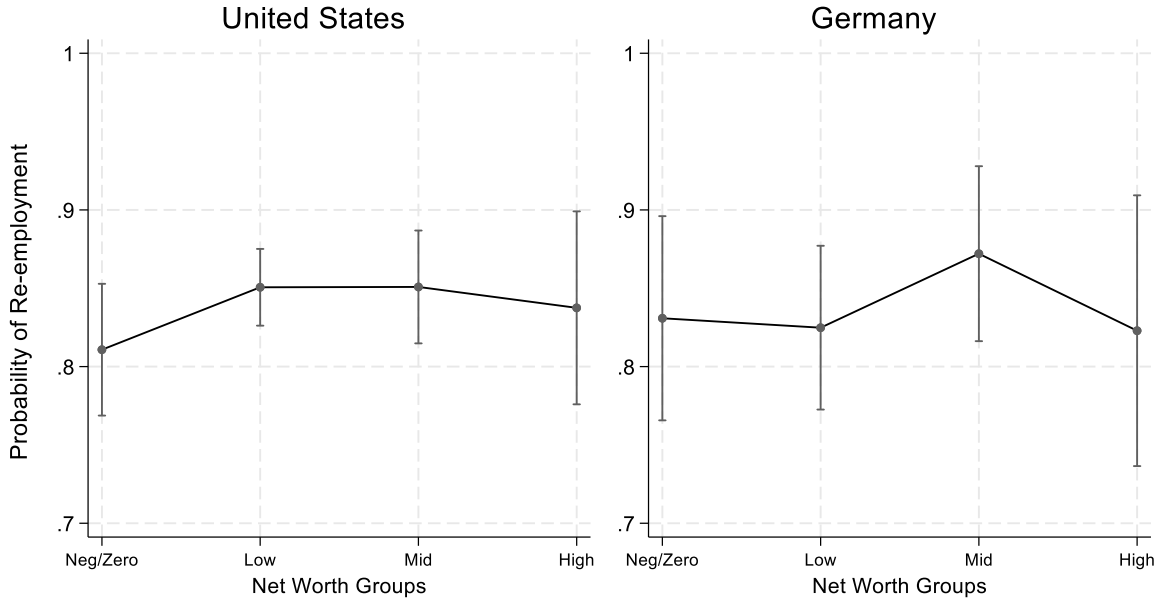

Figure 2. Probability of Reemployment Two Years After Unemployment by Net Worth Group
Note: Predicted probabilities from linear probability models (LPM) estimating reemployment at *t+2* among individuals who experienced unemployment at *t*. Models control for pre-unemployment wages, working hours, job tenure, the individual's contribution to total household income, employment history, age, gender, marital status, number of children, education, migration background/race, sector, and survey wave dummies, as well as region (East/West) for the German analysis. Robust standard errors are clustered at the individual level. Vertical lines represent 95 percent confidence intervals.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

The Effects of Wealth on Wage Scar

Figure 3 displays our central estimates, the wage scar, i.e., the change in hourly wages that can be attributed to an unemployment spell, for each wealth group in the U.S. and Germany. In the U.S., individuals with negative or zero wealth experience the largest scars with about 18% of former hourly earnings.[5] In other words, the experience of unemployment cuts the earnings of U.S. workers without wealth by a full fifth following unemployment, which we consider a very large wage scar. U.S. workers with low or medium wealth (i.e., those in the lower two tertiles of the U.S. wealth distribution) still experience substantial and statistically significant wage scars due to unemployment of about 15%. In contrast, individuals in the top third of the U.S. wealth distribution do not appear to experience substantial wage scars: the point estimate of about 5% is not significantly different from zero. The resulting difference in wage scars of about 13 percentage points between those without wealth and those in the top third of the wealth distribution is statistically significant ($p<0.05$, see Table A.2). Also, the difference between households with some wealth and the high wealth group of about 10 percentage points is marginally significant ($p<0.1$, see Table A.2). This pattern amounts to clear support for Hypothesis 1, in which we expected such non-linear patterns with the safety net function of wealth limited to the top of the wealth distribution. Furthermore, the results demonstrate that the lack of assets – and, potentially, the ensuing further budget constraints due to interest payments among those with debt – lead to much worse post-unemployment outcomes. In the U.S., wealth amounts to a substantial stratifying force in the context of interrupted working histories.

The findings for Germany in Figure 3 reveal a different picture. We observe no clear relationship between wealth levels and scarring effects. Wage scars vary somewhat between about 11% and 15% with the largest scars in the high wealth group. Note, however, that the estimate in the high wealth group may be biased upward because of differences in wage trends as discussed above. Overall, there is no trend visible and none of the differences between the groups are statistically significant. Thus, we find no evidence for Hypothesis 1 in Germany.

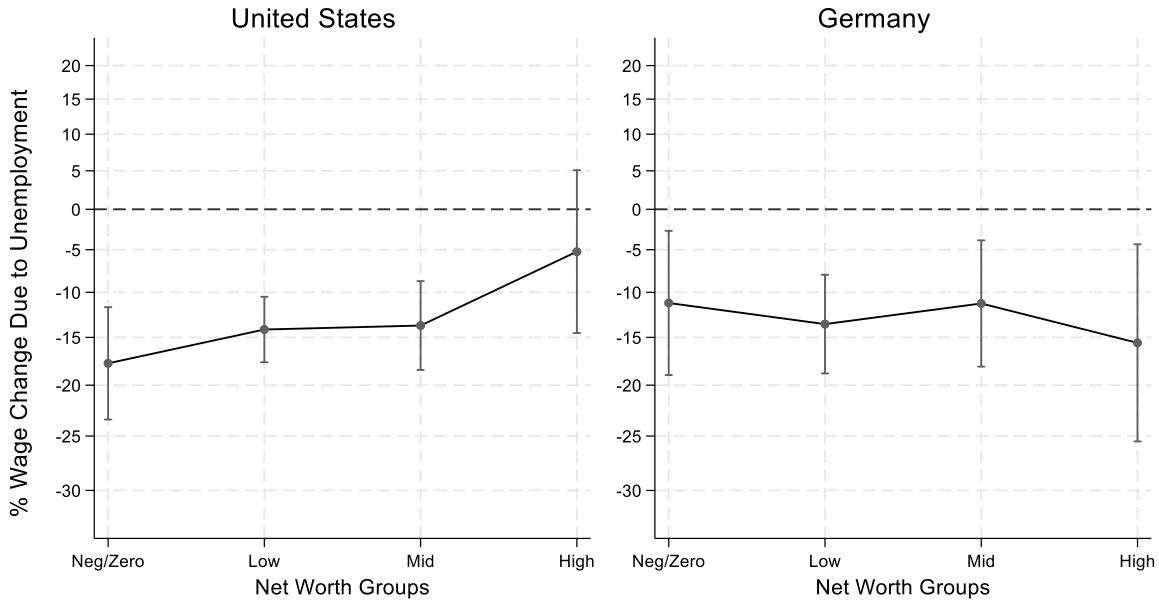

Figure 3. Wage Scarring Effects by Net Worth Group
Note: Estimated effects of unemployment on log hourly wages by wealth groups with 95 percent confidence intervals. Results from conditional difference-in-difference models controlling for prior hourly wages, contribution to household income, working hours, job tenure, sector, employment and unemployment history, age, gender, marital status, number of children in the household, race (or migration background for the German sample), education, and region (East/West) for the German analysis. The values on the y-axis have been derived from the regression coefficients x applying $(e^x - 1) \times 100$. For significance tests between the coefficients see Appendix A, Table A.2.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

The direct cross-national comparison thus reveals a substantial difference: While we find the proposed correlation between wealth and wage scars in the U.S., it does not exist in Germany. The size of the wage scar in the middle of the wealth distribution in the U.S. is similar to the wage scars in Germany. But at the extremes of the two wealth distributions, we find marked cross-national differences: Wage scars in the U.S. are much larger than in Germany among those with negative net worth and much lower among those with high net worth. We interpret this difference among the unemployed with debt – in line with Gangl (2004) – as an indication of the effectiveness of public insurance in Germany and lack or insufficiency of public insurance in the United States. In this sense, being in debt has fewer consequences for later wage trajectories in Germany, while indebted households in the U.S.

are presumably under much stronger pressure to find new employment quickly with disastrous outcomes for their post-unemployment wages.

In contrast, high net worth individuals seem to profit more from their wealth in the U.S. than in Germany in terms of their later wage trajectories, which also aligns with our considerations of the institutional differences: First, the high-turnover labor market in the U.S. creates more jobs, including those in the high-wage segment. Second, the larger share of "fluid" wealth as well as the great ease of borrowing against real estate wealth may offer more readily accessible possibilities for the wealthy in the U.S. to use their wealth for consumption.

Overall, the cross-country differences provide evidence in favor of Hypothesis 4, which predicted a stronger wealth gradient in the U.S. compared to Germany. A formal test of statistical significance between the results for the two countries reveals suggestive evidence in favor of Hypothesis 4 ($p < 0.10$, see Table A.2).

Differences by Unemployment Length

The analyses in Figure 4 show that the effect of wealth on wage scars in the U.S. is mainly present for those with short unemployment spells. Here, we find rather large scars in households with negative net worth of about 15% while the estimate in the high wealth group is zero. This difference is statistically significant ($p<0.05$, see Table A.2). By contrast, we find no wealth gradient in the effect of unemployment on earnings among those with longer unemployment spells of five months or more. Workers with long unemployment in the U.S. show quite large wage scars across the distribution, from about 24% for those without wealth to about 21% for those in the high wealth group (the decrease is not statistically significant). In Germany, it appears that public insurance is sufficient in largely preventing wage scarring for short unemployment spells. Across the wealth distribution, workers with short unemployment spells avoid substantive (and statistically significant) wage scars. The wage scars for longer unemployment are larger, although still not as large as in the U.S. They also do not show strong variation across the wealth distribution, if anything wage scars for long unemployment are higher for the wealthiest group (although this pattern of heterogeneity is not statistically significant).

Taken together, we find clear evidence in favor of Hypothesis 2 for the U.S., which predicted a stronger wealth gradient among those with short-term unemployment. However, in Germany, we find no significant wealth gradient regardless of unemployment length.

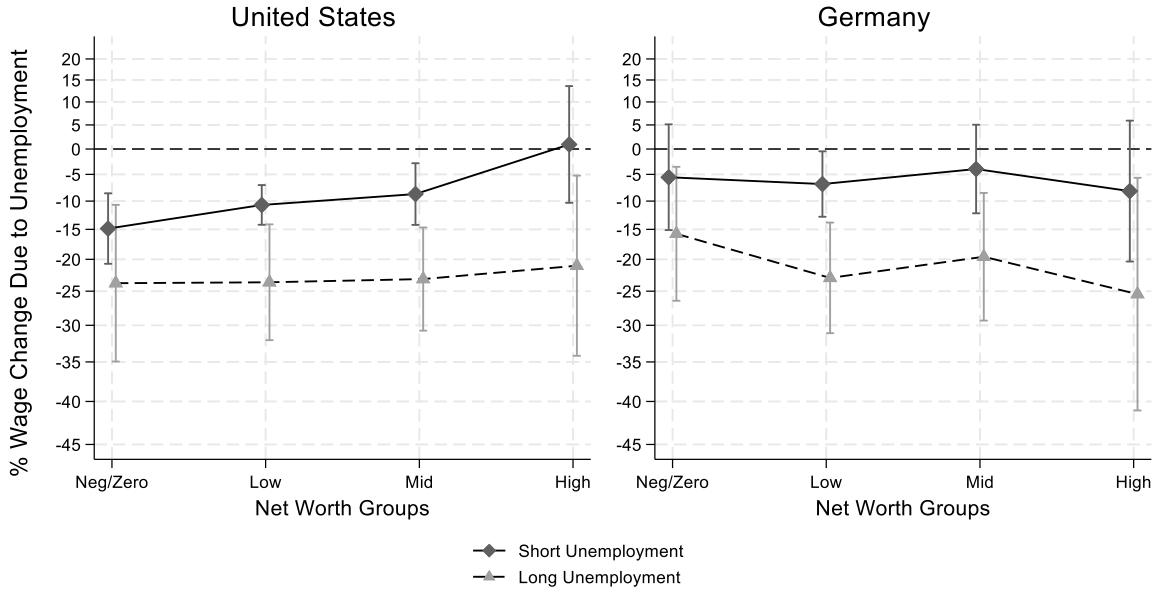

Figure 4. Wage Scarring Effects by Net Worth Group and Unemployment Duration
Note: Estimated effects of unemployment on log hourly wages by wealth group and unemployment duration with 95 percent confidence intervals. Results from a conditional difference-in-difference model controlling for prior hourly wages, contribution to household income, working hours, job tenure, sector, employment and unemployment history, age, gender, marital status, number of children in the household, race (or migration background for the German sample), education, and region (East/West) for the German analysis. The values on the y-axis have been derived from the regression coefficients x applying $(e^x - 1) \times 100$. For significance tests between the coefficients see Appendix A, Table A.2.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

To investigate the underlying mechanisms, Figure 5 provides a closer look at unemployment durations. It shows that the length of time spent searching for a job does not vary much with wealth. In the U.S., short spells of unemployment last about two weeks longer among the wealthy unemployed. This difference is marginally statistically significant at the 10% level. At the same time, however, unemployment duration among the long-term unemployed decreases with higher net worth. In Germany, we find that unemployment durations increase with wealth among the long-term unemployed, albeit with large statistical uncertainty. The variation among the short-term unemployed in Germany appears to be unsystematic.

Thus, the evidence in Figure 5 is in line with the idea that the wealth effect on wage scars is caused by differences in search strategies and not unemployment duration. Wealth apparently does not encourage the unemployed to search much longer. Instead, the results align with the idea that wealth influences job search strategies and that there is a "precautionary job search motive" among unemployed without assets (Eeckhout and Sepahsalari 2024). Below, we will produce additional support for this idea.

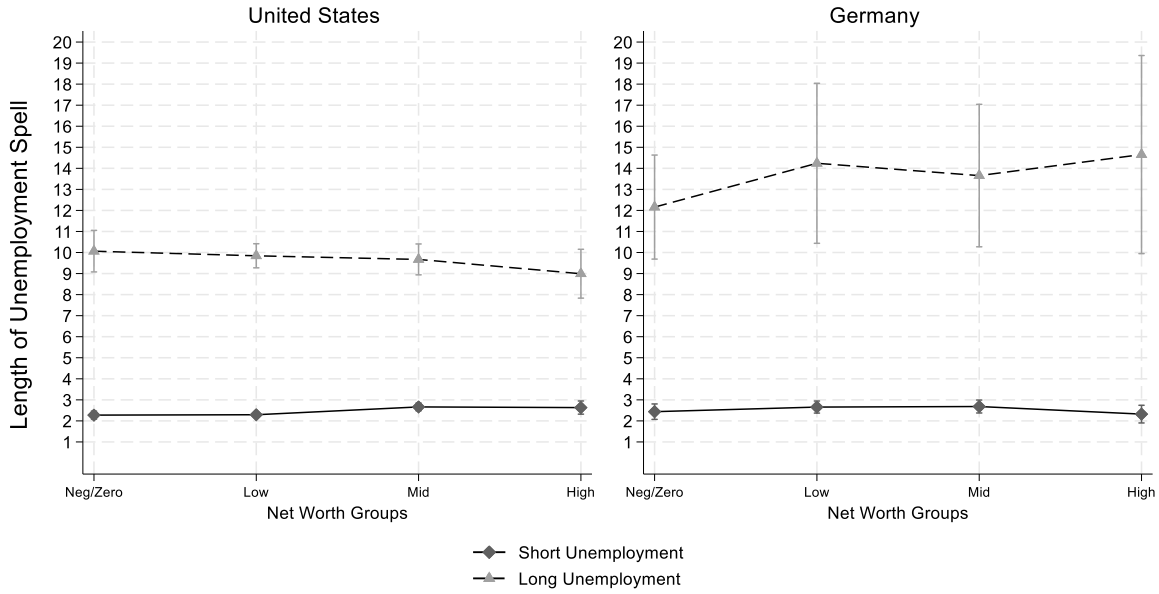

Figure 5. Unemployment Duration among Short- and Long-Term Unemployed by Net Worth Group
Note: Average unemployment duration (in months) by pre-treatment net worth group, distinguishing between short-term (≤ 5 months) and long-term (≥ 6 months) unemployment spells. OLS regression models control for pre-unemployment wages, working hours, job tenure, the individual's contribution to total household income, employment history, age, gender, marital status, number of children, education, migration background/race, sector, and survey wave dummies, as well as region (East/West) for the German analysis. Robust standard errors are clustered at the individual level. Vertical lines represent 95 percent confidence intervals.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Gender Differences

Next, we assess how the main patterns observed so far differ by gender. Comparing U.S. men with U.S. women in Figure 6, we first observe that scars are quite similar, except for the wealthiest group: Here, women have much smaller point estimates of wage scars than men, albeit with very large statistical uncertainty. Overall, the data show a trend towards somewhat

lower scars among high-net-worth individuals for both genders. Our most robust conclusion is that the role of wealth in preventing wage scars follows a relatively similar pattern for men and women in the U.S. This is not in line with Hypothesis 4, which predicted a lower wealth effect for women.

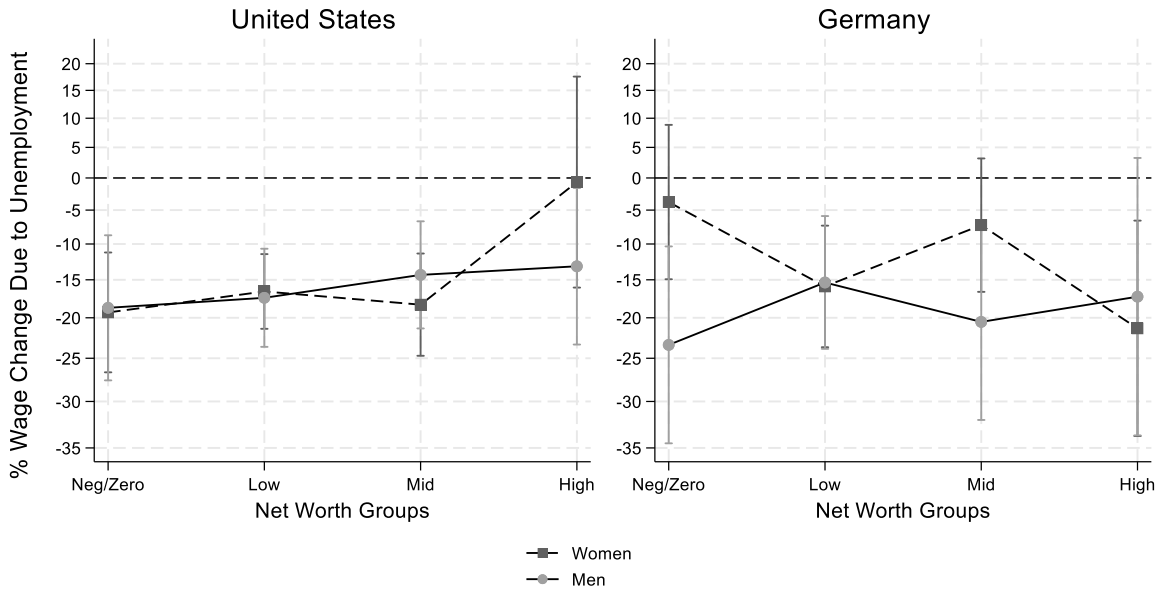

Figure 6. Wage Scarring Effects by Net Worth Group and Gender
Note: Estimated effects of unemployment on log hourly wages by wealth group and Gender with 95 percent confidence intervals. Results from a conditional difference-in-difference model controlling for prior hourly wages, contribution to household income, working hours, job tenure, sector, employment and unemployment history, age, marital status, number of children in the household, race (or migration background for the German sample), education, and region (East/West) for the German analysis. The values on the y-axis have been derived from the regression coefficients x applying. For significance tests between the coefficients see Appendix A, Table A.2.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Turning to Germany, Figure 6 shows a somewhat more complex pattern, partly due to the lower sample size in Germany and resulting wider confidence intervals. An accordingly cautious interpretation of these results suggests that for German men, wage scars are most pronounced among those with negative net worth (about 24%) and that there is a slight trend towards lower scars with increasing wealth. Among the wealthiest men in Germany, the scars are also not significantly different from zero, although the point estimate

is still somewhat substantial at 17%. The difference between the richest and the poorest men in Germany is not statistically significant. For women, the pattern is strikingly different. Unlike men in Germany and unlike men and women in the U.S., German women at the bottom of the wealth distribution experience increasing wage scars with higher levels of wealth. In particular, the wealth safety net does not appear to be effective for German women at the top of the wealth distribution, as they experience the largest wage scars, namely 21%. Thus, for Germany, we find some indication of the gender pattern theorized in Hypothesis 4: High wealth appears to be less readily activated as insurance when women become unemployed. However, due to large statistical uncertainty in the estimates, this finding has to be interpreted with caution. Overall, our analyses suggest that the insurance function of wealth during unemployment is more or less equally distributed between women and men, at least in the U.S. where wealth matters more for post-unemployment wages.

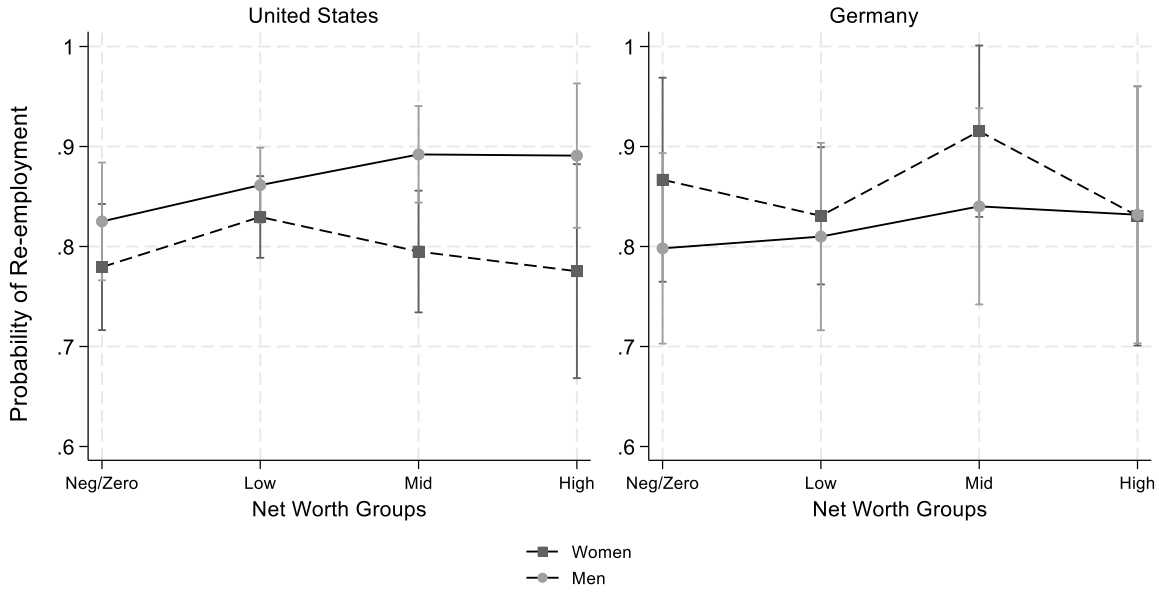

Figure 7. Reemployment Probabilities by Net Worth Group and Gender
Note: Predicted probabilities from linear probability models (LPM) estimating reemployment at *t+2* among individuals who experienced unemployment at *t*, shown separately for men and women. Models control for pre-unemployment wages, working hours, job tenure, the individual's contribution to total household income, employment history, age, marital status, number of children, education, migration background/race, sector, and survey wave dummies, as well as region (East/West) for the German analysis. Robust standard errors are clustered at the individual level. Vertical lines represent 95 percent confidence intervals.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

The analyses reported above, in principle, could be biased by unequal re-employment patterns between men and women at different points in the wealth distribution. Figure 7 shows that U.S. women have overall lower re-employment rates than men. In Germany, re-employment rates are generally more similar for women and men. More importantly, among U.S. women there is no clear heterogeneity of re-employment by wealth: about 80% of women return to a job regardless of net worth. For U.S. men, there is a slight upward trend in re-employment with higher net worth. This leads to a larger gender gap in re-employment in the richest group. Thus, men's re-employment is even less selective in the high-net-worth group. This may explain why wage scars are not as strongly linked to wealth among men compared to women in the U.S. Importantly, we again find no evidence that wealthy women or men retreat from the labor market at higher rates.

Exploration of Wealth Scars

Finally, we provide evidence that speaks to the potential mechanisms linking wealth to wage scars by analyzing changes in wealth, rather than wages, after unemployment. If the unemployed use their wealth for consumption during unemployment, we should observe decreases in wealth after unemployment. Since we observe the lowest wage scars among the wealthiest individuals in the U.S., according to this mechanism we would also expect them to have the largest reductions in wealth. If, on the other hand, the effect of wealth originates from differences in search behavior as predicted by the "precautionary search motive", we should not observe wealth scars.

To implement this analysis, we compare wealth before and after unemployment episodes in all wealth groups. We can only implement this analysis for the U.S. based on a reduced sample. For many unemployment episodes, we do not observe post-unemployment wealth due to the timing of the wealth measurements (see Data section). In Germany, this reduces the sample in the high-wealth group to $N<20$ unemployment episodes and thus renders statistical analyses infeasible. Fortunately, the U.S. is the more interesting case for this analysis given our earlier findings that wealth only matters for wage scars in the U.S.

Table 3 shows that the average change in wealth (an initial descriptive approach to "wealth scarring") is much lower among the wealthiest unemployed than in the other wealth groups. At the same time, the higher median and large standard deviation point to a much larger spread of changes in wealth in this group. While some seem to lose much compared to their pre-unemployment levels, others experience larger gains in wealth than poorer households.

Table 3: Net Worth Changes Among the Unemployed

	U.S. (PSID) Four-year changes in household net worth							
Wealth Groups	Mean	Standard deviation	p25	Median	p75	Min	Max	N
Negative or zero	15,044	74,958	-4,766	2,158	22,773	-187,049	719,991	219
Low+	19,885	88,028	-8,186	3,316	27,329	-555,598	983,618	438
Mid+	38,005	339,796	-52,128	5,707	67,814	-1,536,487	4,204,695	215
High+	1,146	522,253	-119,488	10,419	230,531	-2,400,073	1,308,374	79

Note: Values are rounded to whole numbers. The table reports within-person changes in net worth (first differences) following unemployment and therefore includes only treated units. Four-year wealth changes are available from 2000 onward; earlier periods rely on five-year changes, and for the 1994–2000 interval we use the six-year change in wealth, reflecting the PSID wealth measurement schedule.
Source: Panel Study of Income Dynamics (PSID); authors' calculations.

Figure 8 additionally shows that there is no significant "wage scar", i.e., effect of unemployment on wealth, across the positive wealth distribution. The figure displays results from a difference-in-differences model resembling our main analyses with log household net worth as the dependent variable. The small, negative point estimate of a wage scar in the two upper tertiles is not statistically significant. Reflecting the descriptive results of Table 3, we find large uncertainty in these estimates.

Taken together, we find no evidence that the wealthy unemployed consume their wealth to avoid wage scars. Adding to our findings on unemployment duration, this suggests that the wealth effect on wage scars is due to job search behavior and not search duration.

However, we are cautious about drawing conclusions from this analysis because of the small number of cases and the ensuing large statistical uncertainty.

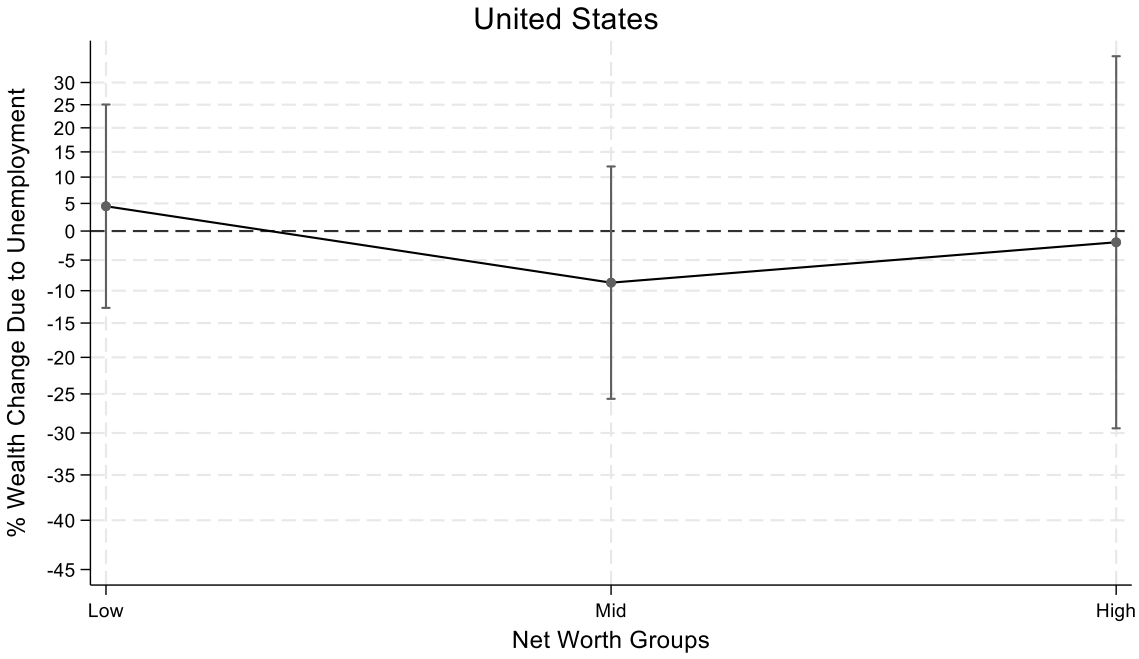

Figure 8. Wealth Scarring by Net Worth Group
Note: Estimated effects of unemployment on log household net worth by wealth group. Results are based on the same difference-in-differences specification as in the main analyses but using changes in log net worth as the dependent variable instead of log hourly wages. Wealth scarring is defined as the change in net worth between two years prior to unemployment (t–2) and two years after re-employment (t+2). Treatment episodes are defined as in the main analysis: individuals are required to be employed at t–2, not unemployed at t–1, experience at least one month of unemployment at t, and return to employment at t+2. Due to the timing of wealth measurements in the PSID, the pre-treatment observation cannot always be measured at t–2 (four-year wealth change); instead, it refers to t–3 (5-year wealth change) for waves prior to 2000 and to t–4 (6-year wealth change) for the 1994–2000 interval.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Additional analyses and robustness checks

To probe the robustness of our conclusions, we perform several additional analyses. First, we conduct further subgroup analyses of wealth-specific re-employment probabilities and show that our prior conclusions hold. The results in Figure A.2, in particular, reveal that the likelihood of re-employment based on unemployment duration is not strongly influenced by wealth. In fact, the long-term unemployed with debt are slightly less likely to find re-employment. Therefore, wage scars may be underestimated for this group. Analyses of unemployment length in Figure A.3 also show that our conclusion that wealth has less influence on the length of unemployment spells than we initially hypothesized also holds across the full sample and by gender.

Furthermore, the different operationalizations of net worth do not influence our main conclusions. First, we modify the categories of household total net worth to assess whether our findings depend on the categorization we chose for the main analysis. Both a version where the first category only contains individuals with debt (and not zero wealth) and a version with net worth quartiles generate qualitatively similar results as depicted in Figures B.1 and B.2 in Appendix B. However, the finding that zero or negative wealth has additional negative effects on scars is no longer visible in the quartiles, as this specification mixes households with debt and low wealth in the first quartile. We therefore conclude that our main categorization is more informative by showing that net debt has an additional effect.

Second, we re-estimate our analysis with the two main components of household net worth: housing wealth and financial wealth (see Appendix B, Figures B.3 and B.4). Overall, we find the same pattern of decreasing wage scars in the U.S. for both wealth components. Interestingly, the wealth gradient for financial wealth is more linear than for housing wealth in the U.S. This may indicate that financial wealth helps the unemployed across the distribution while the insurance effect of housing wealth mainly materializes at high levels. Yet, this analysis has to be interpreted with caution because there may be interactions within household portfolios that we do not model here. In Germany, we again find hardly any systematic patterns and a high level of uncertainty in estimates based on select wealth components.

Conclusion

A spell of unemployment is one of the major risks that individuals face as they pursue their labor market careers. Many of the negative impacts of unemployment are felt immediately (Jahoda et al. 1974), but some also persist after individuals have regained employment. This contribution focuses on wage scars, i.e., the reduction in wages after regaining employment, as one such long-term labor market disadvantage associated with unemployment. As in any situation of risk, insurance can help buffer the negative effects of unemployment. By estimating wage scars across the wealth distribution in two countries with distinct welfare state arrangements, we are able to provide a joint assessment of the effectiveness of private and public insurance against the risks that unemployment entails for later labor market outcomes.

In line with prior research, we find substantial wage scars of unemployment in both the U.S. and Germany (Couch and Placzek 2010; Gangl 2004). However, these wage scars vary systematically by wealth in the U.S. but not in Germany. In fact, those with high wealth in the U.S., namely, those among the top third of the national positive wealth distribution, are shielded from the later wage impacts of short spells of unemployment, suggesting that high wealth provides an effective private insurance function in the U.S. This is in line with previous research showing lower negative impacts of trigger events among the wealthy (Baley et al. 2025; Bedük 2023; Rodems and Pfeffer 2021). Furthermore, we find that the insurance function of even high wealth reaches its limits when unemployment spells are longer. That is, the wealthy in the U.S. may successfully self-insure only against shorter but not longer spells of unemployment. Presumably, the negative signals of long-term unemployment or human capital depreciation are more important for wage outcomes at that point (Schmieder et al. 2016; Van Belle et al. 2018).

In Germany, in contrast, we find no heterogeneity of wage scarring by wealth, indicating that the private insurance mechanism is context dependent: Systems with stronger public insurance can be more effective and, importantly, less unequal in their protection against the negative effects of unemployment spells. The finding that indebted individuals experience significantly worse outcomes following unemployment in the U.S. than in Germany can be attributed to Germany's more generous unemployment insurance, which

provides more adequate support even for individuals facing financial strain from interest payments. Conversely, the lower impact on high-net-worth individuals in the U.S. compared to wealthy Germans may be due to greater job opportunities and more accessible wealth in the U.S. These findings stress the importance of wealth as a means to influence post-unemployment outcomes in conjunction with the welfare state and labor market institutions (Gangl 2006).

Our results regarding gender differences point to equal access to household wealth in times of need, at least in the U.S. The effect of wealth on wage scars is even stronger among U.S. women, indicating that household wealth affects their job search at least as much as men's. This is somewhat surprising given that, on average, men own a larger share of households' net worth, and it indicates that household decisions in the U.S. about wealth are made on the basis of equality rather than equity (Tisch and Lersch 2021). The evidence regarding gender differences in Germany is inconclusive. While there is some tendency towards larger wage scars among high-net-worth women, we lack sufficient statistical power for this part of the analysis.

One way in which insurance can effectively reduce wage scarring is by affording unemployed individuals longer searches for better jobs (e.g., Bloemen and Stancanelli 2001; Lammers 2014; Mortensen and Pissarides 1999). However, our findings suggest that "search theory" does not account for the insurance effects of high wealth in the U.S.: Among the short-term unemployed, search duration is only slightly longer among high-net-worth individuals and, furthermore, we find smaller wealth effects for longer periods of unemployment. These two findings, instead, point to a different micro-level mechanism by which wealth impacts job search strategies (rather than length): Unemployed individuals without assets may apply for easy-to-get jobs that provide quick financial security while wealthy unemployed individuals aim for higher paid jobs (see also Eeckhout and Sepahsalari 2024).

Overall, our results demonstrate that private insurance during periods of unemployment plays a significant role in driving stratified employment outcomes. In the U.S., where public insurance is insufficient, individuals with high net worth manage to avoid wage losses. Consequently, they experience reduced economic insecurity and a lower risk of

downward mobility throughout their lifetime, even if they encounter a "trigger event", such as job loss. Conversely, unemployed people in significant debt face worse long-term outcomes. The reduction in scars of about thirteen percentage points due to wealth in the U.S. is even larger than the eight-percentage-point difference that Gangl (2006) found between liberal and social-democratic welfare states. Therefore, alongside the welfare state, wealth proves to be an important buffer against life course risks. Since wealth is more unequally distributed than unemployment benefits or even additional household income, its impact on inequality in life chances is greater. This is an important addition to the literature on the consequences of wealth inequality, which has previously mainly focused on educational trajectories. Furthermore, the private insurance mechanism connects wealth inequality and income inequality. Our analyses show that wealth inequality translates into unequal post-unemployment wages. This may partly explain the combination of high wealth inequality and high wage inequality in the U.S., which is somewhat unique among western societies (Pfeffer and Waitkus 2021). Other countries with very high wealth inequality such as Norway or Sweden, but also Germany, feature much more generous welfare states, apparently limiting the scope of private insurance mechanisms for driving up wage inequality.

Our findings come with several limitations. First, our estimates of the effect of wealth rely partly on conditioning on observables. Thus, we cannot rule out that there are confounding influences that we could not include in the model. Second, the low number of wealth measurements combined with the rather rare event of unemployment leads to imprecise estimates, especially in the upper part of the wealth distribution where unemployment is less common. Third, we are not able to test all of the proposed mechanisms directly due to lack of data, for example on reservation wages. Finally, with our two-country comparison we are not able to disentangle the effects of individual institutions and possible complementarities.

Our findings show that wealth inequality can have important consequences for life chances. Initiatives to reduce wealth inequality may therefore also affect social inequality in general. At the same time, the findings also stress the importance of public safety nets that are available for everyone. In the case of unemployment, this is especially relevant since only a minority of the unemployed have sufficient assets to avoid downward mobility. Future

research should investigate if the cross-national patterns shown here also hold for a larger set of countries. This would also allow to disentangle institutional complementarities in influencing private insurance through wealth. Furthermore, it would be interesting to investigate the effect of wealth on the impact of other important trigger events such as divorce or health shocks.

Notes

1. In principle, a direct interaction between the two systems can exist in the form of asset tests for unemployment benefits. However, neither the U.S. nor Germany apply such asset tests to unemployment insurance. Minimum income schemes and poverty relief programs, however, include asset tests in both countries. Yet, we cannot assess this interaction empirically because we have very few long-term unemployed in our samples (see Data and Method).
2. Older workers in well-paid full-time jobs with long average employee tenure have lower unemployment risk and higher wages (Damaske, Frech, and Wething 2024; Keys and Danziger 2008) and higher wealth (Killewald et al. 2017). However, such privileged workers usually suffer larger wage scars after unemployment (Burda and Mertens 2001; Gregory and Jukes 2001). High education leads to lower wage scars (Schmelzer 2011) and is positively associated with employment stability, wages, and wealth (Keister 2004; Keys and Danziger 2008; Wilke 2005). Minoritized groups (Blacks in the U.S., migrants in Germany) have disadvantages with respect to unemployment risk, wages, and wealth (Conley 2001; Erlinghagen 2006; Keys and Danziger 2008). Also, previous unemployment affects labor market opportunities and wealth accumulation (Damaske et al. 2024; Killewald et al. 2017).
3. We also assert that they do not start another unemployment spell one year after (t+1) to focus on one spell of unemployment. Data for t+1 can be obtained from the activity calendars in the PSID even after the switch to 2-year interviewing.
4. To implement the comparisons between men and women, we estimate a fully interacted version of equation (2), allowing the unemployment effect and all other

terms to vary by gender. We then conduct linear hypothesis tests based on the three-way interaction between unemployment, wealth group, and gender. For comparisons across countries and unemployment durations, we estimate subgroup-specific models (e.g., only considering short-term unemployment) using the respective treatment and control groups described in Table 2 and sample-specific entropy balancing weights. We then apply seemingly unrelated estimation (SUEST) in Stata 18 to obtain a joint covariance matrix across models and conduct cross-model linear hypothesis tests of differences in the wealth gradient across countries and unemployment durations.

5. Coefficients from the log-wage regressions are reported as percentage changes, computed as $(e^x - 1) \times 100$; this transformation converts a log-point change in wages into the corresponding percent change in wages.

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Appendix A. Further Results

Table A.1 Descriptive Statistics for the Main Analysis Sample and Treated Subsample, by Country

	U.S. (PSID)		Germany (SOEP)	
Variable	Full Sample N = 41,025	Treated N = 1,461	Full Sample N = 30,643	Treated N = 461
Unemployment Episode	3.56%	100.00%	1.50%	100.00%
Wealth Groups (1 = Lowest; 4 = Highest)	2.43 (0.87)	2.20 (0.85)	2.65 (0.96)	2.27 (0.97)
Log Hourly Wage	2.71 (1.05)	2.30 (1.07)	2.79 (1.04)	2.08 (1.02)
Share of Household Income	68.57%	70.98%	66.43%	65.30%
Prior Unemployment Experience[a]	13.22 (28.44)	22.90 (36.35)	31.21%	57.05%
Married	77.49%	68.45%	78.41%	65.73%
Weekly Working Hours	41.80 (10.08)	40.70 (10.47)	39.07 (11.78)	37.21 (13.75)
Employment Tenure (Years)	8.59 (7.13)	5.39 (6.11)	11.01 (8.36)	5.61 (5.93)
Age (Years)	37.25 (7.37)	35.46 (7.30)	40.83 (6.80)	39.18 (7.43)
Female	53.68%	51.47%	55.77%	47.29%
Black (U.S.) / Migration Background (Germany)	29.68%	35.32%	12.89%	15.62%
Education				
Less than High School	6.40%	7.73%	6.29%	7.81%
High School	34.27%	40.73%	62.52%	72.23%
More than High School	59.32%	51.54%	31.19%	19.96%

Has Children	65.66%	64.20%	45.77%	42.30%
Employment Sector				
Private industry	28.31%	34.84%	33.81%	40.13%
Private services	48.46%	54.76%	35.25%	47.29%
Public sector	23.23%	10.40%	30.93%	12.58%
Change in Household Size (t–2 to t+2)	-0.01 (1.01)	0.00 (1.16)	-0.09 (0.75)	-0.06 (0.79)
Region of Residence (Germany only)				
West Germany	-	-	78.78%	72.02%
East Germany	-	-	21.22%	27.98%

Note: Descriptive statistics are based on the analytical sample used in the regression models after all sample restrictions. Continuous variables are reported as means (standard deviations). Binary and categorical variables are reported as percentages. All values are rounded to two decimal places.

[a] Prior unemployment experience is measured as cumulative duration in months for the U.S. and as a binary indicator for Germany.

Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Table A.2: Average Marginal Effects and Significance Tests by Wealth Group

Wealth Groups (WG)	Main	Long Unemployment	Short Unemployment	Men	Women
United States					
WG1: Neg./zero	-0.195***	-0.271**	-0.161***	-0.202**	-0.156**
WG2: Low+	-0.153***	-0.270***	-0.113***	-0.186***	-0.123***
WG3: Mid+	-0.147***	-0.263***	-0.091**	-0.149***	-0.144**
WG4: High+	-0.054	-0.236*	0.009	-0.135*	0.052
Tests of Differences in Treatment Effects Across Wealth Groups:					
WG1 vs. WG4	-0.142*	-0.035	-0.170*	-0.066	-0.208*
WG2 vs. WG4	-0.099†	-0.033	-0.122†	-0.050	-0.175†
Gender difference in (WG1 − WG4)				0.142	
Gender difference in (WG2 − WG4)				0.124	
Unemp. Length Diff. in (WG1 − WG4)		-0.136			
Unemp. Length Diff. in (WG2 − WG4)		-0.088			
Germany					
WG1: Neg./Zero	-0.119*	-0.171*	-0.057	-0.215**	-0.047
WG2: Low+	-0.146***	-0.261***	-0.071*	-0.115**	-0.181***
WG3: Mid+	-0.119**	-0.218**	-0.040	-0.178**	-0.084†
WG4: High+	-0.169**	-0.293*	-0.085	-0.138	-0.248**
Tests of Differences in Treatment Effects Across Wealth Groups:					
WG1 vs. WG4	0.051	0.122	0.028	-0.077	0.202*
WG2 vs. WG4	0.024	0.033	0.014	0.023	0.067
Gender difference in (WG1 − WG4)				-0.278†	

Gender difference in (WG2 − WG4)				-0.044	
Unemp. Length Diff. in (WG1 − WG4)	-0.094				
Unemp. Length Diff. in (WG2 − WG4)	-0.019				
Cross-Country Differences (U.S. vs. Germany)					
WG1 vs. WG4	0.188†	0.156	0.198†	-0.011	0.406**
WG2 vs. WG4	0.122	0.067	0.137	0.070	0.242†

Note: Entries report average marginal effects of unemployment on log hourly wages by wealth group. WG1–WG4 denote wealth groups (WG1 = negative/zero wealth; WG4 = high wealth). "WG1 vs. WG4" and "WG2 vs. WG4" report linear hypothesis tests comparing treatment effects between the respective wealth groups within each model specification. "Gender difference in (WG1 − WG4)" and "Gender difference in (WG2 − WG4)" report difference-in-differences-in-differences tests of whether wealth gradients in treatment effects differ between men and women. "Unemp. Length Diff. in (WG1 − WG4)" and "Unemp. Length Diff. in (WG2 − WG4)" report difference-in-differences-in-differences tests of whether wealth gradients in treatment effects differ between long and short unemployment spells. "Cross-Country Differences" report tests of whether the difference in treatment effects between Wealth Groups 1 and 4 (or 2 and 4) varies between Germany and the United States.

† $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Table A.3: Tests for Differential Pre-Treatment Trends between Treated and Control Observations

Wealth Groups	Main Analysis	Long Unemployment	Short Unemployment	Gender (M = men; W = women)
Germany				
Negative or zero	p = 0.62	p = 0.61	p = 0.83	M: p = 0.02; W: p = 0.09
Low+	p = 0.21	p = 0.10	p = 0.92	M: p = 0.08; W: p = 0.25
Mid+	p = 0.91	p = 0.67	p = 0.71	M: p = 0.95; W: p = 0.33
High+	p = 0.09	p = 0.87	p = 0.01	M: p = 0.06; W: p = 0.68
Heterogeneity across wealth groups	p = 0.37	p = 0.66	p = 0.04	p = 0.04
Overall	p = 0.35	p = 0.43	p = 0.35	M: p = 0.03; W: p = 0.89
United States				
Negative or zero	p = 0.60	p = 0.01	p = 0.04	M: p = 0.74; W: p = 0.70
Low+	p = 0.03	p = 0.51	p = 0.03	M: p = 0.05; W: p = 0.46
Mid+	p = 0.97	p = 0.47	p = 0.50	M: p = 0.74; W: p = 0.77
High+	p = 0.62	p = 0.95	p = 0.69	M: p = 0.39; W: p = 0.10
Heterogeneity across wealth groups	p = 0.41	p = 0.24	p = 0.02	p = 0.23
Overall	p = 0.17	p = 0.04	p = 0.69	M: p = 0.21; W: p = 0.53

Note: P-values are from tests of the null hypothesis that pre-treatment two-year outcome trends do not differ between treated and not-simultaneously treated observations. Wealth-specific rows (e.g., Low+) report tests conducted within each wealth group. The "Heterogeneity across wealth groups" row reports joint tests of treatment–wealth interaction terms, i.e., whether pre-treatment trend differences vary across wealth groups. In the "Gender" column, this row reports a joint test of the triple interaction terms (treatment × wealth level × gender), testing whether heterogeneity in pre-treatment trend differences across wealth groups differs by gender. The "Overall" row reports the test across all treated and not-simultaneously treated observations, irrespective of wealth group. Values are rounded to two decimal places.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

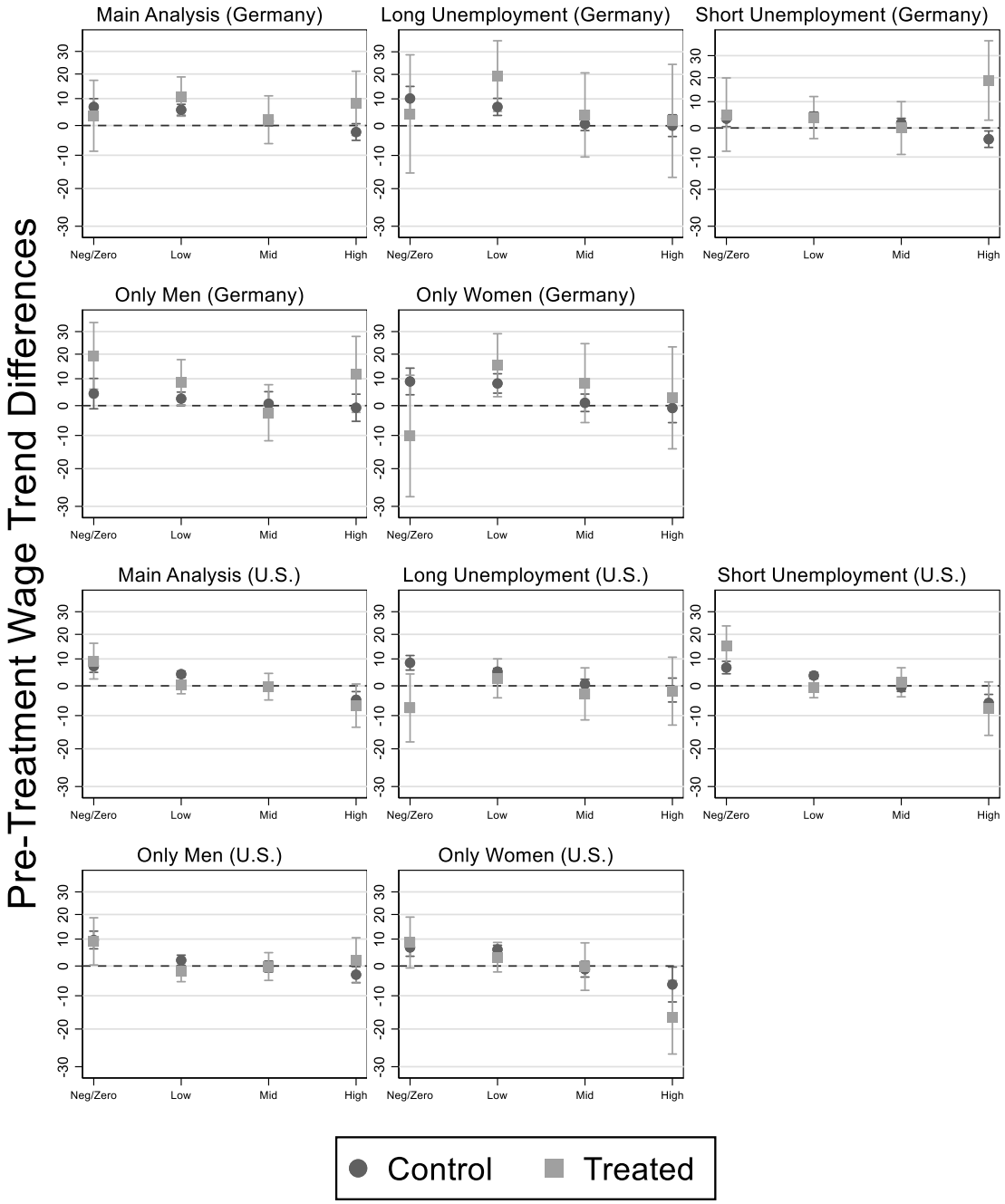

Figure A.1. Pre-Treatment Wage Trend Differences by Wealth Group and Gender
Note: P-values are from tests of the null hypothesis that pre-treatment two-year outcome trends do not differ between treated and not-simultaneously treated ("control") observations. The values on the y-axis have been derived from the regression coefficients x applying $(e^x - 1) \times 100$.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

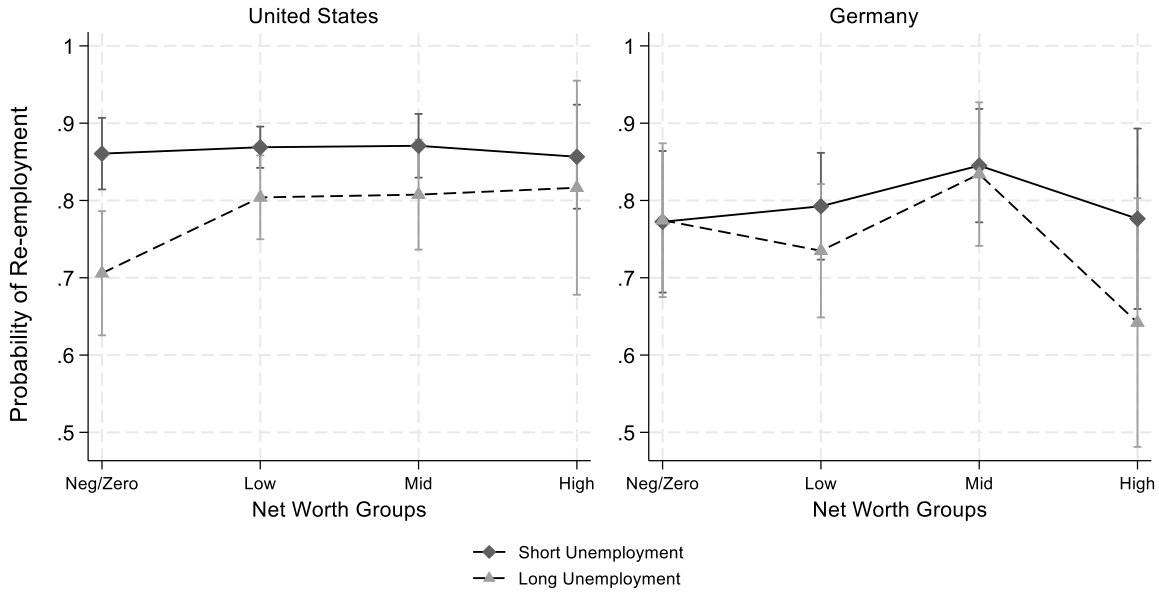

Figure A.2. Reemployment Probabilities by Net Worth Group and Unemployment Duration
Note: Predicted probabilities from linear probability models (LPM) estimating reemployment at *t+2* among individuals who experienced unemployment at *t*, shown separately by unemployment duration. Models control for pre-unemployment wages, working hours, job tenure, the individual's contribution to total household income, employment history, age, gender, marital status, number of children, education, migration background/race, sector, and survey wave dummies, as well as region (East/West) for the German analysis. Robust standard errors are clustered at the individual level. Vertical lines represent 95 percent confidence intervals.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

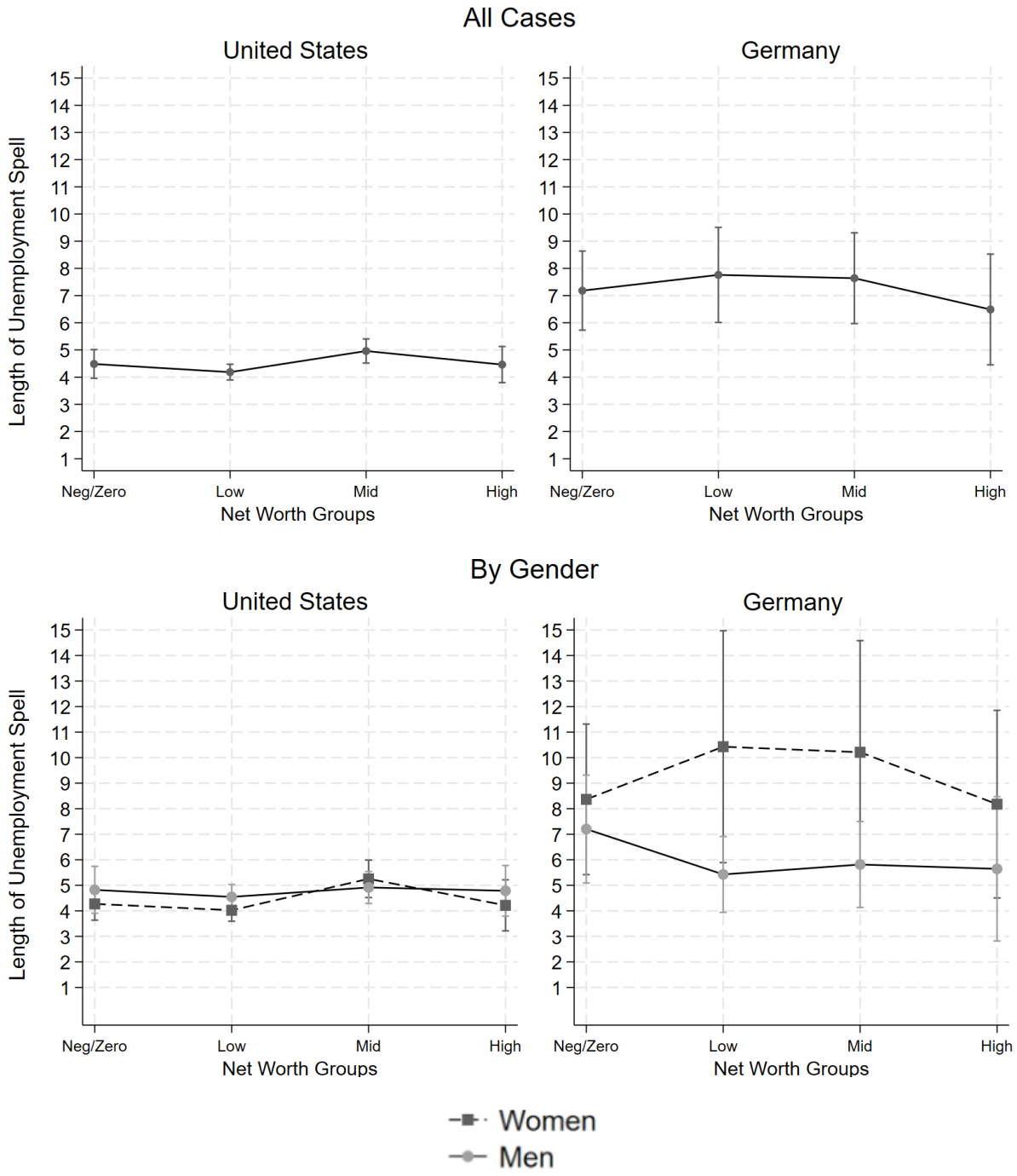

Figure A.3. Unemployment Duration by Net Worth Group, Overall and by Gender

Note: Average unemployment duration (in months) by pre-treatment net worth group, shown for the full sample (top panel) and separately by gender (bottom panel). OLS regression models control for pre-unemployment wages, working hours, job tenure, the individual's contribution to total household income, employment history, age, gender, marital status, number of children, education, migration background/race, sector, and survey wave

dummies, as well as region (East/West) for the German analysis. Robust standard errors are clustered at the individual level. Vertical lines represent 95 percent confidence intervals.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Appendix B. Robustness of Main Results to Alternative Wealth Specifications

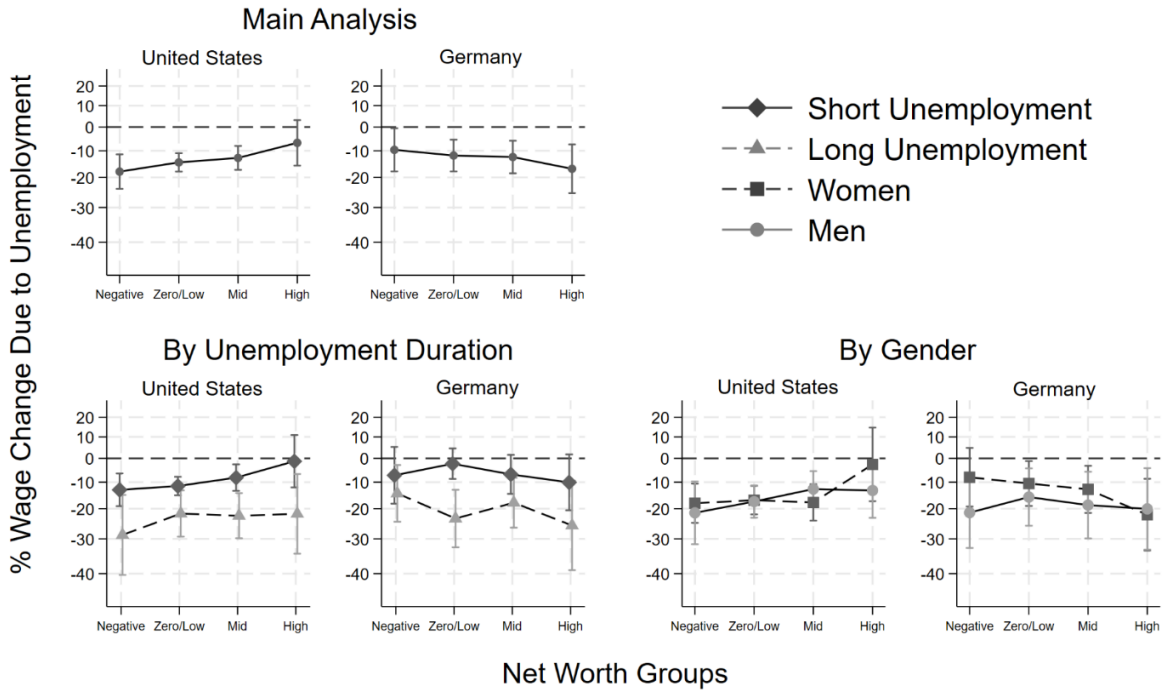

Figure B.1. Wage Scarring Effects by Net Worth Group (Putting Zero Wealth into the Low-Wealth Category)
Note: Estimated effects of unemployment on log hourly wages with 95 percent confidence intervals. The y-axes have been relabeled to reflect percentage changes in wages rather than log-point changes. These robustness checks use an alternative specification of the wealth variable, in which individuals with zero net worth are grouped with the low-wealth (rather than negative net worth) group.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

Figure B.2. Wage Scarring Effects by Net Worth Group (Using Quartiles)
Note: Estimated effects of unemployment on log hourly wages with 95 percent confidence intervals. These robustness checks use an alternative specification of the wealth variable based on net worth quartiles (0–25%, 25–50%, 50–75%, 75–100%). The y-axis has been relabeled to reflect percentage changes in wages rather than log-point changes.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

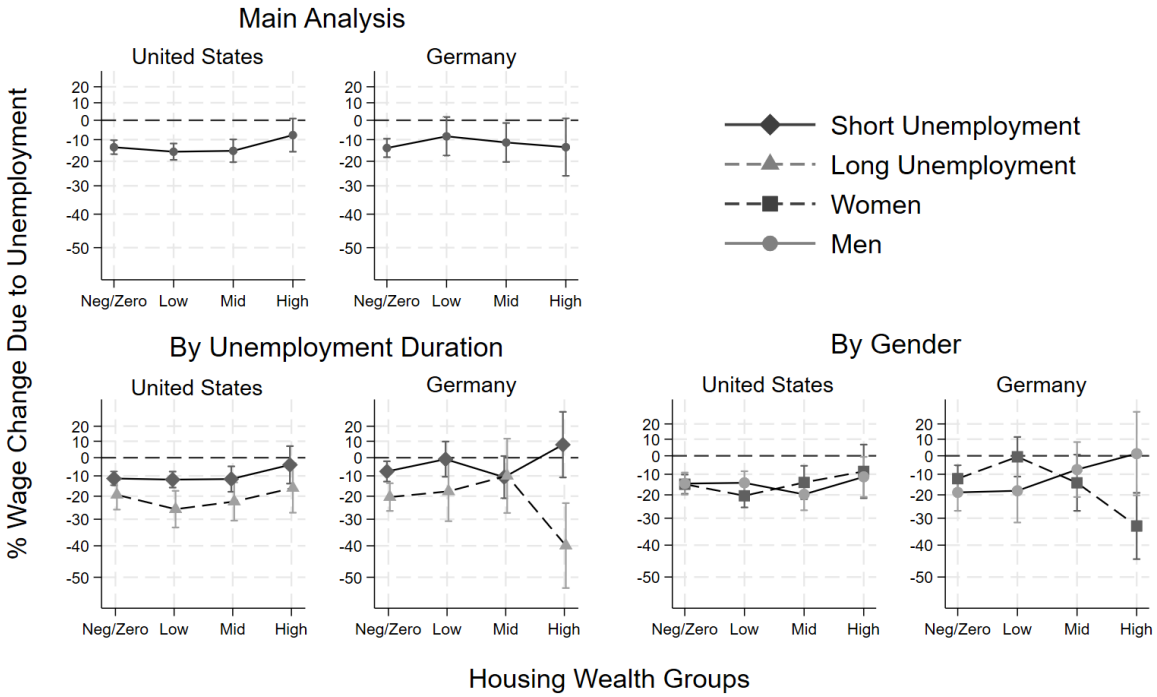

Figure B.3. Wage Scarring Effects by Wealth Group (Housing Wealth Only)
Note: Estimated effects of unemployment on log hourly wages with 95 percent confidence intervals. Wealth groups are defined using the value of the primary residence only, excluding all other components of net worth. The y-axis has been relabeled to reflect percentage changes in wages rather than log-point changes.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.

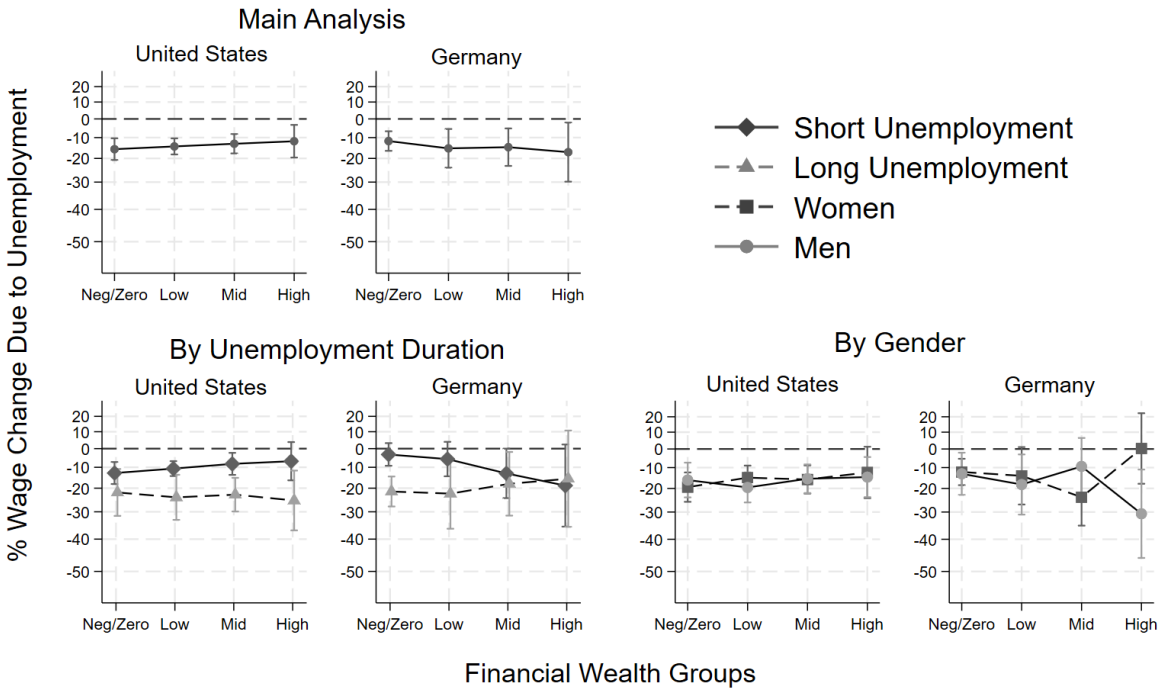

Figure B.4. Wage Scarring Effects by Net Worth Group (Financial Wealth Only)
Note: Estimated effects of unemployment on log hourly wages with 95 percent confidence intervals. Wealth groups are defined using financial wealth only. In the U.S., financial wealth is measured as net worth excluding owner-occupied housing and other real estate assets; business assets remain included. In Germany, financial wealth is approximated as the value of financial assets minus consumer debt; SOEP 1988 does not contain the information required for this measure and is therefore excluded. Owing to differences in data availability and asset definitions, the wealth measures are not directly comparable across countries. The y-axis has been relabeled to reflect percentage changes in wages rather than log-point changes.
Source: Panel Study of Income Dynamics (PSID) and German Socio-Economic Panel (SOEP); authors' calculations.