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On the Extent, Correlates, and Consequences of Reporting Bias in Survey Wages*

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Abstract

We study the extent, correlates, and consequences of reporting bias in survey wages using German linked survey-administrative data (SOEP-CMI-ADIAB). Survey wages differ systematically from administrative records: mean survey wages are 7% lower, and discrepancies follow a mean-reverting pattern. Individual characteristics explain little of the reporting bias, whereas firm context explains most of the variation. Since neither data source alone is sufficient, we construct a hybrid wage that combines their respective strengths. Measurement choice matters, but in ways that depend on how administrative top-coding is handled across the full wage distribution: when wages are outcomes, censoring administrative wages at the assessment limit understates returns to education by 4–11% and the gender wage gap by up to 23%, while imputation reverses the bias for returns to education. When wages are regressors, wage-satisfaction gradients are 9–28% steeper with survey than with administrative wages below the assessment limit, a pattern inconsistent with classical attenuation bias and pointing to non-classical, context-dependent misreporting. We provide guidance for choosing between administrative, survey, and hybrid wages depending on the application, with lessons that extend to any setting where self-reported wages are collected alongside top-coded administrative records.

Keywords: reporting bias, measurement error, wage, income, administrative data, survey data, data linkage

JEL Classification: J30, C81, D31

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1 Introduction

Applied researchers working on wage-related issues can choose between two main types of data, each with distinct strengths and limitations. Register-based administrative data are considered highly reliable, often offering superior measurement quality, larger population coverage, and greater consistency and precision (Künn, 2015). Survey-based self-reported data, on the other hand, provide access to variables that are essential for many modern applications in labor economics. Examples include personality traits, risk preferences, and cognitive skills, which are typically not available in administrative data.[1] Survey data can therefore complement, or even replace, administrative records depending on the research question, but the reliability and quality of self-reported wage information have often been questioned (Bound *et al.*, 2001).

Against this background, we study the extent, correlates, and consequences of reporting bias using the German Socio-Economic Panel (SOEP), one of the longest-running and most widely used household surveys worldwide (Goebel *et al.*, 2019), linked to administrative earnings records and establishment information. Throughout, we use the term *reporting bias* to denote the individual-level difference between a worker's survey-reported wage and the corresponding administrative wage for the same job and month.[2] The linked dataset (SOEP-CMI-ADIAB) merges individual SOEP responses with social security wage records from the Integrated Employment Biographies (IEB) maintained by the Institute for Employment Research (IAB, *Institut für Arbeitsmarkt- und Berufsforschung*) and establishment characteristics from the Establishment History Panel (*Betriebs-Historik-Panel, BHP*). This exceptional data environment links survey and administrative wages at the individual level for up to four decades, covering 108,711 observations on 17,987 individuals. Moreover, it connects workers to their establishments, allowing us to analyze reporting behavior across heterogeneous workplace contexts.

Our contribution is to move beyond documenting the extent of reporting bias to its *sources* and *consequences*: the linked data we use – pairing long within-person wage histories with rich covariates at the individual, household, job, and firm levels – let us pinpoint which of these characteristics drive the bias and trace how the choice of wage measure – survey, administrative, or a combination of the two – changes the answers to canonical empirical questions. In doing so, we build on a long-standing literature on the accuracy of self-reported wages[3] ranging from classic U.S. studies (Duncan and Hill,

[1] Borghans *et al.* (2008), Arni *et al.* (2014), Künn (2015). In addition, survey instruments can be flexibly tailored to the specific needs of researchers (Stantcheva, 2023).

[2] We use "bias" in this descriptive sense, to denote a systematic discrepancy between two wage measures, rather than in the econometric sense of the bias of a particular estimator. Whether this reporting bias translates into biased coefficient estimates depends on the application, as we discuss in Section 3.

[3] See Moore *et al.* (2000), Bound *et al.* (2001) for comprehensive reviews.

1985, Bound and Krueger, 1991) to more recent international – and especially German[4] – evidence. Prior work shows systematic gaps between survey and administrative wages that vary across workers and contexts, with mechanisms such as recall error, social desirability bias, and survey design effects proposed to explain these deviations.[5] We add three elements: a comparatively large linked sample, a broadened set of potential correlates with quantified relative importance, and a systematic analysis of how the choice of wage measure affects standard empirical applications, with explicit attention to the role of top-coding in administrative data. Because the SOEP is a fundamental dataset in applied economics,[6] these contributions speak directly to widely used wage measures and provide practical guidance for applied users of SOEP-based research and similar data.

First, we quantify the extent of reporting bias in levels and in year-to-year changes. Our main outcome variable throughout is the gross monthly wage. About 67% of SOEP respondents underreport their wages relative to administrative records, and mean SOEP wages lie about 7% (€170)[7] below mean IEB wages. The distributional pattern is strongly mean-reverting: respondents at the bottom of the IEB wage distribution substantially overreport, while those toward the top increasingly underreport. In the top decile, the average underreporting is about 12% (€605). We find an analogous pattern for year-to-year changes in monthly wages: low-wage earners overreport changes, whereas higher earners underreport them. Changes are noisier than levels. We also examine how including bonus payments, which SOEP-based studies typically omit, affects the reporting bias. Mean underreporting in levels is somewhat lower when bonuses are included (about 3% rather than 7%). Because bonuses are measured differently in the two sources, this difference reflects the change in wage concept rather than a change in how accurately regular monthly wages are reported.

Second, we identify which factors at the individual, household, job, and firm levels correlate with reporting bias. Individual sociodemographics and personality traits explain only a small share of the variation. Household factors, too, have only modest predictive power. Job attributes matter somewhat more: white-collar status and full-time employment are associated with larger absolute reporting bias. The dominant predictors are firm-level attributes. Firm size, establishment median-wage tercile, sector, and workforce composition jointly account for the bulk of the explained variation. Notably, larger and higher-paying establishments exhibit a more negative mean bias, consistent with more

[4]See Antoni *et al.* (2019), Schmillen *et al.* (2024), Valet *et al.* (2019).

[5]Angel *et al.* (2019), Meyer *et al.* (2026), Roth and Slotwinski (2021), Celhay *et al.* (2024).

[6]Within the past 15 years, SOEPlit, the official SOEP literature database, lists 1,234 peer-reviewed SOEP-based publications, of which 105 appeared in economics journals ranked among the RePEc Top 50.

[7]This corresponds to US-$202 at the exchange rate of June 30, 2021.

complex and standardized compensation structures that make it harder for workers to accurately recall their exact remuneration.

Third, we construct a hybrid wage measure that combines the strengths of administrative and survey information. The hybrid measure uses administrative wages when they are observed below the assessment limit, where IEB wages are reliable and free of model-dependent adjustments, and switches to survey wages when administrative values are top-coded or unavailable, thereby avoiding the need to extrapolate the upper tail through imputation. The resulting hybrid wage is a practical, ready-to-use measure that preserves administrative precision where it is warranted and survey coverage where it is needed.

Fourth, we assess the consequences of measurement choice for standard empirical applications. A key institutional feature of the IEB is that wages are top-coded at the social security assessment limit, that ranged from €5,500 to €7,100 per month in West Germany over our main observation period. Wages above this limit are either censored or recovered through Tobit-style imputation. To separate the role of this top-coding from genuine reporting bias, we organize the results into three estimation samples: one with observations below the assessment limit, one with observations above it, and one spanning the full distribution. The full sample is the basis for all our substantive conclusions, including the returns and gaps we interpret as population parameters. The below- and above-limit samples are diagnostic: they localize whether differences across wage measures originate below the assessment limit, above it, or from combining both parts of the distribution, by comparing the measures on a fixed set of observations.

When wages enter as outcomes, the key finding is that in the full sample, how top-coded IEB wages are handled matters more than the choice between survey and administrative data as such. Simply censoring IEB wages at the assessment limit substantially understates returns to education with censored IEB estimates lying 0.4–0.8 percentage points (4–11%) below SOEP-based returns. It also understates the gender wage gap, as censored IEB estimates are up to 3.5 percentage points (23%) lower than survey based estimates. These discrepancies arise because gender and education premia are concentrated precisely where administrative data are censored. Imputation recovers the censored variation but can overshoot: for returns to education, imputed IEB estimates lie 11–13% above SOEP estimates, reversing the direction of the bias. For the gender wage gap, by contrast, imputation and SOEP estimates are virtually indistinguishable once top-coding is addressed. On the common set of observations below the assessment limit, differences across data sources remain systematic: small and largely consistent with mean reversion for returns to education, but more pronounced for the gender wage gap, where they reflect a non-classical reporting pattern that persists and grows as controls are added, reaching 13% of the SOEP estimate in the most controlled specification.

When wages enter as regressors, the picture is more concerning. Below the assessment limit, where both wage measures are observed without censoring or imputation, estimated associations between wages and life satisfaction are 17–28% steeper when using survey than when using administrative wages; for satisfaction with personal income, the gap is 9–17%. These differences persist with individual-fixed effects and are inconsistent with classical attenuation bias, pointing instead to correlated reporting tendencies – such as hedonic recall or aspirational bias (Prati, 2017) – that inflate survey-based gradients and are not eliminated by standard controls.

Across all applications, the lesson is that the consequences of reporting bias are application-specific, depending on how the bias relates to the other variables in the model. Our findings therefore carry practical guidance for three groups of researchers. Users of SOEP-CMI-ADIAB should be aware that relying on either data source alone can distort standard estimates; the hybrid wage we construct offers a ready-to-use alternative. Users of stand-alone SOEP or IEB data face specific limitations and should cross-check key results accordingly. Survey wages are subject to systematic non-classical reporting bias, whereas administrative wages are top-coded in ways that matter more for some applications than for others. Whether to include bonus payments depends on the question at hand, but researchers should be aware that the choice matters: including them brings survey and administrative wages closer in levels. Finally, the error structures we document are not uniquely German: mean reversion, upper-tail divergence under assessment limits, and strong firm-context effects arise naturally wherever self-reported wages are collected alongside administrative records subject to top-coding. The hybrid strategy and the cautions regarding censoring we develop here are therefore applicable to other linked-data environments, and the lessons extend to researchers working with analogous data in other countries, where administrative earnings measures based on social-security contribution records are often censored at statutory limits. In Austria, for example, earnings recorded in the Austrian Social Security Database are top-coded at a yearly cap, and Kleven *et al.* (2024) address this by replacing right-censored earnings with predictions based on uncensored income-tax records, which are not linked at the individual level. This case is particularly relevant given the recent launch of the Austrian Socio-Economic Panel (ASEP) and its planned register-data linkage. In Spain, the Continuous Sample of Working Lives is bottom- and top-coded at occupation- and year-specific limits, and Bonhomme and Hospido (2017) use linked income-tax records to assess alternative extrapolation methods.

2 Data

2.1 Data Sources

Our analysis is based on the linkage of individual survey data from the *German Socio-Economic Panel* (SOEP) with individual social security records (Integrated Employment Biographies, *Integrierte Erwerbsbiographien*, IEB). This data linkage is publicly available as SOEP-CMI-ADIAB (Antoni *et al.*, 2025)[8] and is accessible via the German Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung*, IAB). We use version 7521, which covers the period 1984-2021.[9]

The SOEP has been conducted annually since its inception in 1984 and currently includes around 30,000 respondents from approximately 15,000 households. The survey provides self-reported gross monthly wages together with the covariates at the individual and household levels. These covariates includes basic sociodemographic characteristics (age, gender, education, citizenship), personality traits, partnership status, and partner's income. Additionally, the SOEP provides detailed information on the employment situation, including working hours, tenure, as well as information on union membership and contract type.

The administrative IEB provide data on individuals' employment and unemployment histories in Germany, such as exact gross wages for each job held. The dataset covers employees subject to social security contributions, workers in marginal employment,[10] recipients of social security benefits, registered unemployed individuals, and participants in active labor market programs. It does not cover civil servants or the self-employed, who are not subject to social security payments. The individual-level IEB data contain additional variables from the Establishment History Panel (*Betriebs-Historik-Panel, BHP*), which provides basic information on establishments as of June 30 each year, such as location, industry, and total number of employees, as well as additional information on employee composition and the wage structure of full-time employees.

2.2 Consent and Linkage

Beginning in 2019, SOEP participants were informed about the record-linkage project and explicitly asked to consent to the linkage of their survey responses to administrative records for research purposes.[11] Respondents were asked repeatedly across survey years, and the most recent response gov-

[8] DOI: 10.5164/IAB.SOEP-CMI-ADIAB7521.de.en.v1

[9] SOEP-CMI-ADIAB combines data from several SOEP subsamples: SOEP-Core (including refreshment samples), the SOEP Migration Samples, the IAB-BAMF-SOEP Survey of Refugees, and the SOEP Innovation Sample (SOEP-IS).

[10] See Section 2.4 for details.

[11] For a number of pilot cases, consent was already collected in earlier waves (from 2013 onwards). Our analyses rely on the most recent consent status.

erns inclusion, so only individuals who consented and did not subsequently withdraw are included. By 2021, 45,345 respondents had been asked for consent and 33,733 (74%) had provided it (see Appendix Table A.1).[12] Conditional on consent and successful linkage, individuals are also observable for years prior to giving consent. Linking SOEP and IEB wage information requires overcoming a mismatch in data structure. The SOEP uses a person-year panel structure and includes wages reported for the month before the interview. The IEB data are initially stored as employment spells with start and end dates, as well as gross daily wages. To align the two datasets, we link each SOEP interview to the IEB spell that overlaps with the interview month or ends in the month before. In cases of parallel IEB employment spells, we use the information from the main spell (i.e., the spell with the highest wage and longest tenure).[13] These steps ensure that wages in both data sources refer to the same job and month. To prepare IEB spells for linkage, we follow established data preparation procedures for administrative labor market data, as documented by Stüber *et al.* (2023a).[14]

2.3 Sample Construction

To construct our analysis sample, we start from the 29,778 consenters who could be successfully matched to an IEB employment biography (see Appendix Table A.1) and impose a series of restrictions to ensure the comparability of wage information across sources. Specifically, we retain only individuals who report positive wages in the SOEP (22,498 individuals), thereby focusing on misreporting along the intensive margin.[15] We exclude cases in which respondents report to work as self-employed, as well as civil servants, who are not covered by the IEB system. This leaves us with 21,661 respondents.[16] To align the sample with the working-age population, we restrict it to respondents aged 20 to 65, reducing it further to 20,320 individuals. Dropping spells that do not overlap between the IEB and SOEP (as described in the linkage procedure) leaves us with a final sample of 17,987 individuals. On

[12] All appendix tables and figures are available in the Online Appendix. Mean comparisons by consent status show only modest differences (see Appendix Table C.1), and we test in Section 4.1 whether results are robust to reweighting by the inverse probability of consent.

[13] This approach aligns with the structure of the SOEP questionnaire, which instructs respondents to report job and wage information for their main job ("If you currently have more than one job, please answer the following questions for your main job only."). Aggregating administrative wages across all concurrent employment spells yields virtually identical results.

[14] In brief, this involves harmonizing overlapping and multi-year spells by splitting episodes into consistent time units, constructing biographical variables that summarize individuals' employment histories, and cleaning key variables such as education, occupation, and industry to address inconsistencies and institutional features of the data. In addition, we merge establishment-level information based on the establishment identifiers. These steps ensure a consistent longitudinal dataset suitable for empirical analysis.

[15] We acknowledge, however, that income nonresponse is an additional source of misreporting (Bollinger *et al.*, 2019). Furthermore, IEB wages of zero occur in cases of "employment interruptions", e.g., illness after continued pay ends, maternity leave, and sabbaticals. During these periods, the employment relationship legally continues, but no remuneration is paid (Frodermann *et al.*, 2021).

[16] We also exclude individuals in informal employment, i.e., those who are listed with a zero wage in the administrative data but have a positive wage from non-self-employment in the SOEP. This amounts to only about 0.35% of the spells in the merged sample before any other restrictions are imposed.

average, we observe respondents for about six waves. The longest individual linked histories go back to the very first SOEP wave in 1984 and extend through 2021. This leaves us with a sample of 108,711 individual-year observations.[17]

2.4 Wages in Survey and Administrative Data

Wage Information in the SOEP In the SOEP, respondents are asked to report their gross *monthly* wage earned the month before the interview excluding special payments, such as vacation pay or back pay, but including overtime compensation.[18] These wages appear in the data with no changes. Additionally, respondents report bonus payments received during the previous calendar year, separately from the monthly wage. Because the SOEP wage question explicitly instructs respondents to exclude special payments, our main measure follows this definition and excludes bonus payments. This is also the predominant approach in the literature: as Table E.1 documents, of 66 SOEP-based wage papers published between 2022 and 2025, only five (8%) add bonus payments to reported monthly wages. We report robustness checks throughout the paper that add reported bonuses to the monthly wage.

Wage Information in the IEB The IEB data contain information on gross *daily* wages. Employers are legally required to submit an annual report for each employee to the social security system, ensuring that wage information is updated, even within long-term employment relationships. The average daily wage in the IEB is constructed as the total wage paid within an employment episode divided by the exact number of days in that episode. This approach ensures that we accurately account for variation in employment duration within a year. For example, if an individual works only six months in a given year, this is fully reflected in the calculation.[19] We construct a *monthly* wage measure by multiplying the daily wage by $\frac{365.25}{12} = 30.4375$, the average number of days per month, taking leap years into account. Starting in 2013, bonus payments are typically recorded in separate spells (Antoni *et al.*, 2023, Drechsler *et al.*, 2023). Prior to 2013, these payments were generally included directly in employers' wage reports, although in some cases they were also reported separately in distinct spells. Since most observations in our sample stem from the post-2013 period (approximately 60%),

[17]The number of observations vary for different analyses throughout the paper due to item nonresponse.

[18]The exact wording in the 2019 English version of the SOEP is as follows: "*What did you earn from your work last month? Please state both: gross income, which means income before deduction of taxes and social security, and net income, which means income after deduction of taxes, social security, unemployment insurance, and health insurance. If you received extra income, such as vacation pay or back pay, please do not include this. Please do include overtime pay. If you are self-employed: Please estimate your monthly income before and after taxes.*" Respondents may refer to their pay slip to answer this question, but they are not required to do so.

[19]As a consequence, IEB spells do not capture month-to-month fluctuations in wages, for example those arising from seasonal variation in hours or within-year wage adjustments. However, we do not observe any systematic month-of-interview patterns in the difference between SOEP and IEB wages that such fluctuations would imply. The results of regressions of the difference between SOEP and IEB wages on month-of-interview fixed effects are available on request.

separately reported bonus spells are the dominant case in our data. We therefore exclude separately reported bonus spells from our main IEB wage measure. This mirrors our treatment of the SOEP wage, which likewise excludes special payments, so that both sources measure regular monthly earnings on a comparable basis. As a robustness check, we additionally construct a version in which bonus payments from the separate spells are added to the recorded wages and compare the resulting measure with SOEP wages that include bonus payments, and we verify that this specification is robust to restricting the sample to the post-2013 period, when separate reporting is most complete.

Several institutional features ensure that the quality of the IEB data are very high. IEB wage information is derived directly from employers' reports to the social security system, which are used to determine social security contributions and individual benefit entitlements. This institutional setup creates strong incentives for accurate reporting (Schmillen *et al.*, 2024, Stüber *et al.*, 2023b). Employers are legally required to submit standardized wage information through an integrated notification procedure, and these records form the basis for calculating unemployment benefits and statutory pensions. Employees receive annual statements summarizing the reported information, providing an additional layer of verification. Given these features, we treat IEB wages throughout the paper as our reference measure of true wages.

Nonetheless, a number of well-known limitations must be considered when using administrative IEB wage data. First, wages are top-coded at the maximum earnings level on which social security contributions are levied ("assessment limit"), which varies across years and between East and West Germany. The monthly limit in West Germany increased from €2,660 (DM 5,200) in 1984 to €7,100 in 2021. East Germany, covered from 1990 onward, had a separate lower limit throughout, rising from €1,380 (DM 2,700) in 1990 to €6,700 in 2021. Most observations stem from the period 2011–2021, when the limit ranged between €5,500 and €7,100 in West Germany (€4,800–€6,700 in East Germany). A common approach to imputing wages above the assessment limit uses a standard Tobit-style imputation procedure (Stüber *et al.*, 2023a), building on Card *et al.* (2013) and Dustmann *et al.* (2009). The procedure estimates the upper tail of the wage distribution separately by year and relevant subgroups (education, East/West Germany) and draws predicted values for wages above the assessment limit. It therefore recovers information that is mechanically lost through top-coding, but also introduces model-based variation.[20] Following the approach suggested by Stüber *et al.* (2023a), all observations with monthly wages within €121 below the assessment limit (corresponding to €4

[20]This type of imputation has been used in seminal studies of the German wage structure, including Dustmann *et al.* (2009) and Card *et al.* (2013), and remains common in applications using German administrative wage records, such as Cornelissen *et al.* (2017), Goldschmidt and Schmieder (2017), Jäger *et al.* (2021), Dustmann *et al.* (2022).

daily) are treated as censored and included in the imputation procedure. When imputation is not applied, those monthly wages are top-coded at €121 below the actual assessment limit.

Second, the IEB does not cover marginal employment before 1999. Marginal employment refers to jobs with monthly earnings below a legally defined threshold that are exempt from social security contributions. Over our observation period, this threshold increased from approximately €196 (DM 384) per month in 1984 to €450 per month in 2021. Prior to 1999, earnings from these jobs (*"mini-jobs"*) were not reported to the social security system. However, they account for only 1–3% of the pre-1999 observations, corresponding to fewer than 500 individual-year observations.

Third, the administrative records contain no information on hours worked, which limits their usefulness for applications requiring hourly wages, such as analyses of minimum wages.

2.5 Wage Measures Across the Distribution

We define the reporting bias as the difference between equivalent measures of gross monthly wages in the SOEP and the IEB at the individual level, considering both the absolute deviation and the deviation relative to the administrative wage. The reliability of this comparison, however, varies fundamentally over the wage distribution: below the social security assessment limit, IEB wages reflect true earnings, and deviations from SOEP wages constitute genuine reporting bias; above the limit, both sources are imperfect in different ways. This asymmetry motivates our use of three distinct estimation samples, summarized in Table 1, which we describe in turn.

Below-Limit Sample Below the social security assessment limit, IEB wages are recorded at their true value and serve as a reliable benchmark for true earnings. Any divergence between SOEP and IEB wages in this range therefore reflects genuine reporting bias on the part of survey respondents. We restrict both sources to observations below the official assessment limit and refer to this as the *below-limit sample*. As shown in Table 1, this sample comprises 17,355 individuals and 99,618 observations, representing the large majority of the data. It forms the basis for our analysis of the *extent* and *correlates* of reporting bias in Sections 4 and 5.

Above-Limit Sample Above the assessment limit, IEB wages are censored and no longer reflect true earnings. To recover the censored information, they can be imputed as described above. Imputed values are model-based predictions and therefore subject to imputation bias. SOEP wages above the assessment limit remain susceptible to reporting bias, which can no longer be quantified since the IEB no longer provides a reliable benchmark. We refer to observations in which either IEB or SOEP wages

Table 1: Overview – Samples and Wage Measures

Sample[a] (1)	Region (2)	Individuals (3)	Obs. (4)	IEB Wage[b] (5)	SOEP Wage[c] (6)	Used for (7)
Below-Limit	Below AL	17,355	99,618	Recorded	Reported	Extent, Correlates
Above-Limit	Above AL	1,361	8,429	Imputed	Reported	Imputation Bias
Full	All	17,987	108,711	Imputed *or* Censored *or* Hybrid	Reported	Consequences

Note: This table reports the definitions of our samples, the number of individuals (3) and individual × year observations (4) and defines the wages we use in each sample for both IEB (5) and SOEP (6). Column (7) summarizes the analysis for which those samples are relevant. AL denotes the social security assessment limit.
[a] The below-limit sample includes observations for which both IEB and SOEP wages are below the AL. The above-limit sample includes observations for which the IEB wage is above the AL. The full sample includes all observations with non-missing wage information and contains the below-limit and above-limit samples, as well as observations for which the IEB wage is below the AL while the SOEP wage is above it.
[b] *Recorded* IEB wages refer to wages as recorded in the social security data. *Imputed* refers to wages that are imputed using a Tobit-style procedure above the AL. *Censored* indicates that wages above the AL are top-coded at the AL. *Hybrid* describes a combination of recorded IEB wages below the AL and reported SOEP wages above the AL.
[c] *Reported* SOEP wages refer to wages as reported in the SOEP without any adjustments.

exceed the limit as the *above-limit sample*, comprising 1,361 individuals and 8,429 observations. We use this sample to document the extent of imputation bias and to motivate the need for the hybrid wage measure introduced below.

Full Sample For the analysis of *consequences*, we require a wage measure that spans the full distribution. The *full sample* comprises 17,987 individuals and 108,711 observations, and is used exclusively for the consequences analysis in Section 6.[21] Since the appropriate treatment of top-coded wages above the assessment limit is not obvious, we compare three alternative wage measures – *imputed*, *censored*, and *hybrid* – that differ in how this is addressed. The *imputed* measure uses IEB wages below the assessment limit as recorded and imputes values above it. The *censored* measure uses IEB wages below the assessment limit as recorded and treats all wages above it as censored at the limit. For SOEP wages, we use the reported values throughout.

In addition, given the limitations of both sources above the assessment limit – imputation bias in the IEB and reporting bias in the SOEP – we construct a *hybrid* wage measure that exploits the relative strengths of each source across the distribution. The hybrid wage measure is constructed in three steps. First, for all observations below the social security assessment limit, we use IEB wages, as administrative records reflect true earnings without model-dependent adjustments. Second, for wages above the assessment limit, we replace IEB wages with SOEP wages. Although SOEP wages are subject to reporting bias, we nonetheless treat SOEP wages as the preferred source in this range, given that im-

[21] The full sample includes the below-limit sample, for which both IEB and SOEP wages are below the assessment limit, and the above-limit sample, for which IEB wages are above the assessment limit. It also includes observations for which the IEB wage is below the assessment limit while the SOEP wage is above it.

puted IEB wages introduce model-dependent error by construction. Third, for individuals who report a SOEP wage below the marginal-employment threshold before 1999, we use SOEP wages, as these employment relationships were recorded in the IEB only from 1999 onward. The resulting measure closely tracks the IEB wage distribution at the lower end, where it relies entirely on administrative information. Deviations begin to appear around the assessment limit, above which the hybrid wage measure fully coincides with the SOEP wage. Table 1 provides a complete overview of all samples and wage measures.

3 Theoretical Framework and Empirical Implications

We use a measurement-error framework in the spirit of Bound and Krueger (1991) to guide the empirical analysis. Let y^*_{it} denote an individual i's true wages at time t. For simplicity and clarity, our main empirical analysis follows the common practice of treating the administrative IEB wage as a reference measure of true wages, i.e., $y^A_{it} = y^*_{it}$, with the implied assumption that the administrative measurement error $v^A_{it} = y^A_{it} - y^*_{it}$ is zero. The observed SOEP wage equals true wages plus an error:

$$y^S_{it} = y^*_{it} + v^S_{it}. \tag{1}$$

The *reporting bias* can be expressed as the difference between survey and administrative wage signals:

$$r_{it} = y^S_{it} - y^A_{it} = v^S_{it}.\text{[22]} \tag{2}$$

As a standard signal-to-noise measure, we consider the *reliability ratio*, $\kappa = \frac{\mathrm{Var}(y^*)}{\mathrm{Var}(y^S)}$. Since true wages y^* are unobserved, we follow Bound and Krueger (1991) and estimate the slope coefficient κ from a regression of administrative wages on survey wages:

$$\kappa = \frac{\mathrm{Cov}(y^S, y^A)}{\mathrm{Var}(y^S)}. \tag{3}$$

Under the assumption that administrative wages reflect true wages and that survey wages are measured with purely classical measurement error, κ captures the share of variation in the observed signal y^S that reflects true variation in y^*, rather than noise. However, if survey measurement error

[22] Equally, we can express the reporting bias in first-differences, which is relevant for applications that rely on changes in wages rather than levels: $\Delta r_{it} = \Delta y^S_{it} - \Delta y^A_{it} = \Delta v^S_{it}$.

is non-classical, such that $\mathrm{Cov}(y^*, v^S) \neq 0$, the slope coefficient from projecting administrative wages on survey wages becomes

$$\frac{\mathrm{Var}(y^*) + \mathrm{Cov}(y^*,\, v^S)}{\mathrm{Var}(y^*) + \mathrm{Var}(v^S) + 2\,\mathrm{Cov}(y^*,\, v^S)}.$$

In this case, the estimated coefficient is no longer a reliability ratio and need not lie between zero and one. It should instead be interpreted as the slope coefficient from a linear projection of administrative wages on survey wages, combining signal strength with systematic components of reporting error.[23]

The empirical implications of the reporting bias differ depending on whether wages enter the estimation equation as the dependent variable or as an explanatory variable. For the case in which wages are the explained variable, suppose that the outcome equation of interest is

$$y^*_{it} = X_{it}\beta + u_{it} \tag{4}$$

where β is the coefficient of interest, which measures the effect of X_{it} on wages. Researchers observe $y^S_{it} = y^*_{it} + v^S_{it}$, i.e. the sum of latent true wage and reporting bias. The empirical implications depend on the nature of v^S_{it}. If measurement error is purely classical, standard measurement-error theory predicts that slope coefficients β remain unbiased, with v^S_{it} adding to u_{it} as additional noise, reducing precision (see, e.g., Bound and Krueger, 1991).

If measurement error is non-classical, e.g., correlated with the regressors, OLS coefficients become biased in the direction implied by these correlations – formally analogous to omitted-variable bias. Mean-reverting error is a canonical case: because v^S_{it} covaries with the true wage y^*_{it}, regressors that are correlated with true wages may also become correlated with the composite regression error. The resulting slope bias can be attenuating or inflating, depending on the sign and magnitude of these induced correlations (Bound *et al.*, 2001).

For the case in which wages are the explanatory variable, suppose that the outcome equation of interest is:

$$h_{it} = \gamma y^*_{it} + X_{it}\delta + u_{it}, \tag{5}$$

where γ is the coefficient of interest, which measures the effect of wages on h_{it}, for example subjective satisfaction, and researchers observe survey wages $y^S_{it} = y^*_{it} + v^S_{it}$ rather than true wages.

[23]This interpretation still relies on the assumption that administrative wages measure true wages. If administrative wages are also measured with error, as discussed by Abowd and Stinson (2013) and Kapteyn and Ypma (2007), the covariance in Equation (3) incorporates error components from both sources, and the reliability ratio is not identified without further assumptions.

Again, empirical implications depend on the nature of v_{it}^S. Under classical measurement error, the probability limit of the OLS estimator is

$$\text{plim}\,\hat{\gamma} = \gamma \frac{\text{Var}(y_{it}^*)}{\text{Var}(y_{it}^S)} = \gamma \left(1 - \frac{\text{Var}(v_{it}^S)}{\text{Var}(y_{it}^S)} \right), \tag{6}$$

and the coefficient on wages is attenuated toward zero.

Under mean-reverting error, $\text{Cov}(y_{it}^*, v_{it}^S) < 0$, the classical attenuation result no longer holds in this form. Since the measurement error is systematically related to true wages, mean reversion affects both the numerator and denominator of the projection coefficient. Abstracting from other sources of endogeneity, the probability limit becomes

$$\text{plim}\,\hat{\gamma} = \gamma \frac{\text{Var}(y_{it}^*) + \text{Cov}(y_{it}^*, v_{it}^S)}{\text{Var}(y_{it}^S)}. \tag{7}$$

When $\text{Var}(y_{it}^*) > \text{Var}(v_{it}^S)$ – plausible in many empirical settings – moderate mean reversion tends to weaken attenuation relative to the classical benchmark, whereas sufficiently strong mean reversion may generate bias away from zero.

If the error is instead correlated with other covariates, coefficients may be biased in either direction depending on the sign and magnitude of those correlations. Finally, reporting bias may be correlated with the outcome error term itself, leading to endogeneity through reverse causality or omitted factors such as reporting style. This may be of particular interest when the outcome is subjectively reported (Bertrand and Mullainathan, 2001). For the case of satisfaction, Prati (2017) argues that individuals who are more satisfied may tend to overreport their wages, leading to an upward bias when estimating the "effect" of wages on satisfaction.

Taken together, the framework implies that classical reporting error primarily adds noise when wages are the outcome and attenuates coefficients when wages enter as an explanatory variable, whereas non-classical reporting error can generate bias in either setting, with the direction and magnitude depending on how reporting errors covary with true wages, covariates, and outcomes.

4 The Extent of the Reporting Bias

4.1 Below-Limit Sample

Table 2 summarizes average and relative deviations in the reporting bias by decile of the IEB wage distribution in the below-limit sample. Across the entire distribution, the mean reporting bias amounts to -€170 (-7%), and the mean absolute bias to €307 (13%), confirming that deviations between survey

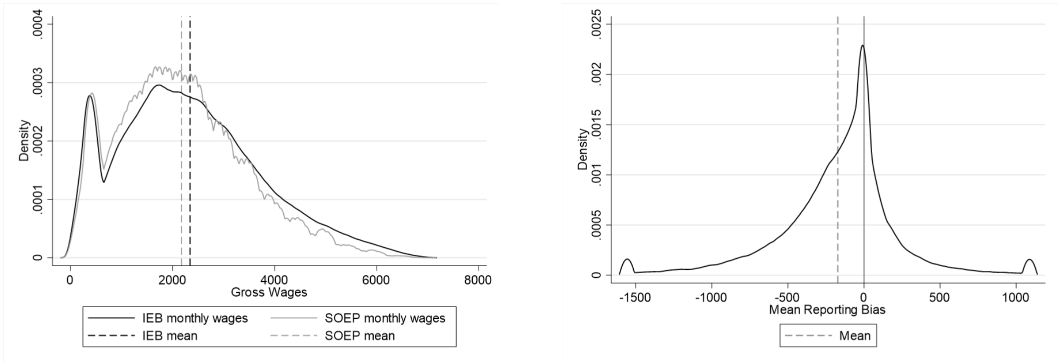
Table 2: Reporting Bias Along the Wage Distribution (Below-Limit Sample)

Decile	Mean Wage[a]	Mean Bias		Mean Abs. Bias		N
	(1) in €	(2) in €	(3) in %	(4) in €	(5) in %	(6)
1	346	124	36	156	45	10,026
2	831	27	3	142	17	9,915
3	1,325	-26	-2	169	13	9,968
4	1,704	-52	-3	193	11	9,954
5	2,054	-109	-5	249	12	9,967
6	2,417	-152	-6	286	12	9,963
7	2,803	-212	-8	326	12	9,957
8	3,238	-298	-9	399	12	9,960
9	3,831	-398	-10	491	13	9,963
10	4,879	-605	-12	657	13	9,945
All	2,342	-170	-7	307	13	99,618

Note: This table reports summary statistics for reporting bias for the below-limit sample, by decile of the IEB wage distribution. Column (1) reports the mean IEB wage in each decile (€). Columns (2) and (3) report the mean reporting bias (SOEP minus IEB wage), in € and percent. Columns (4) and (5) report the mean absolute reporting bias, in € and percent. Column (6) reports the number of observations in each decile.
[a]The mean IEB wage in the below-limit sample is €2,342 with a standard deviation of €1,380, and in the SOEP €2,171 with a standard deviation of €1,240.

and administrative wages are both economically significant and pervasive. Three patterns stand out. First, the direction of the bias is strongly mean-reverting: respondents in the bottom decile overreport their wages by an average of €124 (36%), while those in the top decile underreport by €605 (12%). Overreporting turns into underreporting around the third decile. This pattern is consistent with systematic mean reversion in reporting behavior: low-income individuals have more scope to overreport, whereas high-income individuals have more scope to underreport, as described by Bound and Krueger (1991), Kapteyn and Ypma (2007), and Bollinger *et al.* (2018). Second, the absolute magnitude of the bias increases monotonically along the distribution, from €156 in the bottom decile to €657 in the top decile, reflecting the mechanical scope for deviation at each income level. Third, the relative bias is remarkably stable in the upper half of the distribution, hovering between 6% and 12% across deciles six through ten.[24] Figure 1 complements this picture. Panel (a) shows that the two wage distributions align closely overall. Both distributions exhibit bunching at the time-varying earnings thresholds for marginal employment (see Section 2.4). The standard deviation of SOEP wages (€1,240) is notably smaller than that of IEB wages (€1,380), reflecting a narrower distribution. Panel (b) displays the distribution of the reporting bias, revealing that 67% of respondents underreport their wages.

[24]This pattern is not driven by age. Appendix Table A.2 shows no systematic age-related trend in wage reporting bias. The absolute bias increases with age, but this reflects rising income levels, leaving the relative bias stable across ages.



(a) Distribution of Gross Monthly Wages (b) Distribution of the Reporting Bias

Figure 1: Distribution of SOEP and IEB Wages and the Reporting Bias (Below-Limit Sample)

Note: This figure plots the distributions of SOEP and IEB wages and the reporting bias. Panel (a) shows the distribution of wages in SOEP and IEB. The mean IEB wage is €2,342 with a standard deviation of €1,380, and in the SOEP €2,171 with a standard deviation of €1,240. Panel (b) shows the distribution of the individual-level reporting bias, defined as the difference between a respondent's reported SOEP wage and the recorded IEB wage. Because IEB wages are essentially continuous, the heaping visible in the SOEP wage distribution in Panel (a) does not carry over to the distribution of reporting bias. The mean (median) reporting bias is -€170 (-€114). 67% of respondents underreport their wages. Reporting biases that lie below the 1st percentile or above the 99th percentile of the distribution of differences are winsorized at these bounds.

Table 3 shows the distribution of the reporting bias in year-to-year wage changes. The mean-reverting pattern persists, although it is much smaller than in levels. Individuals in the bottom decile overreport wage changes by approximately €56 (13%). The crossover to underreporting occurs in the third decile, and individuals in the top decile underreport wage changes by roughly €61 (-1%). The mean bias across the full distribution is close to zero at -€9 (0%), indicating that signed errors in wage changes largely cancel out in the aggregate.

This does not imply, however, that wage changes are measured with little error at the individual level. The mean absolute bias remains substantial at €247 (10%). Moreover, the regression-based reliability ratio in Appendix Figure A.1 falls to 0.618, compared with 1.00 for wage levels. Thus, first-differencing removes much of the systematic level bias, but it also amplifies the noise-to-signal ratio because true year-to-year wage changes are small relative to reporting error (Angrist and Krueger, 1999). Taken together, the results suggest that wage changes are nearly unbiased on average but measured with substantial individual-level noise.[25]

Robustness Appendix Table B.1 shows that the results are robust to a range of alternative specifications, as documented in detail in Appendix B. Mean underreporting ranges from €150 to €172 across alternative sample definitions, wage measures, and rounding restrictions, and the interquartile range

[25]Consistent evidence is reported for U.S. and German data by Bound and Krueger (1991), Bound *et al.* (1994), Pischke (1995), Schmillen *et al.* (2024), and Antoni *et al.* (2019). Appendix Table A.3 reports the full variance-covariance decomposition. The negative covariance between true wages and measurement error (-0.067 in levels, -0.038 in changes) confirms systematic mean reversion in both specifications.

Table 3: Reporting Bias in First-Differences (Below-Limit Sample)

Decile	Mean Wage[(a)]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	427	56	13	148	35	7,404
2	1,048	2	0	157	15	7,386
3	1,513	-5	0	183	12	7,389
4	1,878	-8	0	207	11	7,382
5	2,222	-13	-1	231	10	7,381
6	2,572	-10	0	253	10	7,396
7	2,941	-8	0	277	9	7,387
8	3,349	-22	-1	309	9	7,387
9	3,907	-26	-1	334	9	7,385
10	4,875	-61	-1	376	8	7,368
All	2,472	-9	0	247	10	73,865

Note: This table reports summary statistics for reporting bias in first differences for the below-limit sample, by decile of the IEB wage-change distribution. Column (1) reports the mean IEB wage change in each decile (€). Columns (2) and (3) report the mean reporting bias in first differences (SOEP minus IEB wage changes), in € and percent. Columns (4) and (5) report the mean absolute reporting bias in first differences, in € and percent. Column (6) reports the number of observations in each decile.
[(a)] In the below-limit sample, the mean IEB wage change is €123 with a standard deviation of €392, and in the SOEP €113 with a standard deviation of €451.

remains stable throughout. The one notable exception is the bonus-inclusive specification, which adds separately reported bonus payments to both SOEP and IEB wages and reduces mean underreporting to €75 (3%). This specification captures a different wage concept: it includes an annualized component of irregular remuneration, whereas our main measure compares regular monthly wages. Appendix Tables D.1 and D.2 replicate our main descriptive results for this specification. In levels, both the mean bias and mean absolute bias are smaller across most of the distribution below the assessment limit. In first differences, the bonus-inclusive and bonus-exclusive specifications are virtually identical. As discussed in Section 2.4, we use wages excluding bonus payments as our main measure and revisit the bonus-inclusive specification as a robustness check in the correlates and consequences analyses below.

4.2 Above-Limit Sample

Table 4 extends the analysis to the above-limit sample, where IEB wages are imputed. The picture changes markedly. Across all observations in this range, the mean bias is -€2,416 (-27%) and the mean absolute bias is €3,421 (38%), far larger than in the below-limit sample in both absolute and relative terms. Even in the bottom quintile of the above-limit distribution – with a mean IEB wage of €5,693 – the mean absolute bias reaches €1,310 (23%), more than twice the largest value observed in the below-limit sample.

Table 4: Imputation Bias Along the Wage Distribution (Above-Limit Sample)

Quintile	Mean Wage[(a)]	Mean Bias		Mean Abs. Bias		N
	(1) in €	(2) in €	(3) in %	(4) in €	(5) in %	(6)
1	5,693	409	7	1,310	23	1,700
2	6,588	-122	-2	1,528	23	1,686
3	7,679	-1,089	-14	2,065	27	1,683
4	9,485	-2,744	-28	3,294	34	1,687
5	15,761	-8,572	-50	8,968	53	1,673
All	9,026	-2,416	-27	3,421	38	8,429

Note: This table reports summary statistics for reporting bias among observations above the assessment limit, by IEB wage quintile. Column (1) reports the mean IEB wage in each quintile (€). Columns (2) and (3) report the mean reporting bias (SOEP minus imputed IEB wage), in € and percent. Columns (4) and (5) report the mean absolute reporting bias, in € and percent. Column (6) reports the number of observations in each quintile.
[(a)] The mean IEB wage in the above-limit sample is €9,026 with a standard deviation of €4,919, and in the SOEP €6,615 with a standard deviation of €3,454.

The bias grows steeply along the distribution: individuals in the top quintile, with a mean IEB wage of €15,761, show a mean absolute bias of €8,968 (53%). The mean bias is predominantly negative, indicating that SOEP wages are substantially lower than imputed IEB wages in the top quintile. Table 5 shows an analogous pattern for wage changes: signed differences largely cancel out on average, but absolute deviations remain large, indicating substantial individual-level divergence between SOEP wages and imputed IEB wages even in first differences. Including bonus payments reduces the mean imputation bias in levels from -27% to -17% (Table D.3), while the mean imputation bias in first differences remains largely unaffected (Table D.4).

However, as discussed in Section 2.5, neither source constitutes a reliable benchmark above the assessment limit. These patterns motivate the use of the hybrid wage measure in the consequences analysis.

5 Correlates of Reporting Bias

5.1 Empirical Strategy

In this section, we analyze factors that predict the reporting bias. We estimate a simple OLS regression restricted to the below-limit sample, i.e., individuals for whom the reporting bias r_{it} is well defined:

$$r_{it} = \alpha + X_{it}'\beta + \xi_t + \varepsilon_{it}, \tag{8}$$

Our primary interest is to identify which groups of factors account for the most variation in the reporting bias, using a Shorrocks–Shapley decomposition of Equation (8). This approach allocates

Table 5: Imputation Bias in First-Differences (Above-Limit Sample)

Quintile	Mean Wage[(a)]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	5,716	1,716	30	2,135	37	1,460
2	6,604	1,516	23	2,320	35	1,448
3	7,700	927	12	2,675	35	1,442
4	9,508	415	4	3,174	33	1,449
5	15,847	-5,407	-34	7,160	45	1,430
All	9,054	-318	-4	3,482	38	7,229

Note: This table reports summary statistics for imputation bias in first differences for the above-limit sample, by quintile of the IEB wage-change distribution. Column (1) reports the mean IEB wage change in each quintile (€). Columns (2) and (3) report the mean imputation bias in first differences (SOEP minus imputed IEB wage), in € and percent. Columns (4) and (5) report the mean absolute imputation bias in first-differences, in € and percent. Column (6) reports the number of observations in each quintile.
[(a)] The mean IEB wage change in the above-limit sample is €569 with a standard deviation of €5,620, and in the SOEP €251 with a standard deviation of €1,276.

the model's R^2 to predefined predictor blocks by averaging each block's marginal contribution over all possible inclusion orders. Unlike incremental R^2 decomposition, the resulting allocation is order-invariant and well defined under collinearity.[26]

We report estimates for both the simple reporting bias r_{it} and its absolute counterpart $|r_{it}|$, and present explained variance for five blocks of explanatory factors in X_{it}:

- *Individual characteristics* include age, gender, German citizenship, years of education, standardized Big Five personality traits, and region of residence.[27]
- *Household characteristics* include indicators for being married, household size, number of children, having a partner, and partner income.[28]
- *Job characteristics* include indicators for white-collar status (ISCO-88 and ISCO-08 one-digit groups 1-5), full-time employment, union membership, and firm tenure.
- *Firm characteristics* include firm size (less than 20, 20-199, 200-1,999, 2,000 or more employees), median wage of full-time employees (in terciles), workforce composition (average age;

[26]We use the *shapley2* post-estimation command following Juarez (2012).
[27]North: Schleswig-Holstein, Hamburg, Lower Saxony, and Bremen; East: Berlin, Brandenburg, Saxony, Saxony-Anhalt, Thuringia, and Mecklenburg-Western Pomerania; South: Bavaria and Baden-Württemberg; West: Hesse, North Rhine-Westphalia, Rhineland-Palatinate, and Saarland.
[28]Partner income is defined as zero if the individual has no partner indicated in the SOEP, and as the mean income of the income distribution if the individual has a partner but their income is missing.

share of highly qualified, female, full-time, and German employees), and one-digit industry controls.[29]

– *Survey controls and time effects* include survey experience, survey mode, and time-fixed effects for trends in the reporting behavior.

We control for level shifts in reporting behavior over time, such as changes in the survey instrument or institutional setting, by including year-fixed effects ξ_t. In our main specification, individual, household, and job characteristics are drawn from self-reported SOEP information, whereas firm characteristics (size, median wage, and employee composition) are drawn from the IEB. In robustness checks, we assess the sensitivity of our results to alternative covariate definitions, drawing all variables to the greatest extent possible from either the IEB or the SOEP alone.

5.2 Results

Table 6 summarizes the shares of explained variance by covariate block based on the Shorrocks-Shapley decomposition of Equation (8). Starting with the mean absolute bias (Column 2), firm-level factors account for the largest share of explained variance: firm size (9%), median-pay tercile (32%), sector (9%), and workforce composition (9%) together explain 59% of the variation in the absolute deviation. Job characteristics explain a further 22%, and sociodemographic characteristics add 12%. Personality traits, household characteristics, and survey characteristics (interview mode and panel tenure) contribute only marginally to the explanation of the reporting bias. Taken together, these results imply that firm context is the primary driver of reporting bias variation. Firm size and sector – which are commonly observed in both IEB and SOEP data – account for a combined 18% of explained variance, suggesting that applied researchers working with either source can address a meaningful share of the reporting-bias variation through standard firm-level controls. The median-pay tercile (32%) and workforce composition (9%), however, are drawn from administrative IEB data and thus available only in linked datasets. We provide direct evidence on the role of controls for empirical implications in Section 6.

The decomposition for the mean reporting bias (Column 1) tells a broadly similar story, but with one notable difference: firm-level dominance is even more pronounced, with the median-pay tercile alone accounting for 40% of explained variance and firm-level factors jointly explaining 71%. By contrast, sociodemographic characteristics contribute only 6%, compared with 12% for the absolute bias.

[29]The median-wage terciles are computed by ranking establishments by the median wage of full-time employees each year and splitting the distribution into three equal parts; establishments are then assigned to the low, middle, or high median-wage tercile.

Table 6: Shorrocks-Shapley Decomposition of Reporting Bias

	Share of explained variation	
	Mean Bias (1)	Mean Abs. Bias (2)
Sociodemographic characteristics	0.06	0.12
Personality traits	0.01	0.02
Household characteristics	0.01	0.00
Job characteristics	0.19	0.22
Firm size	0.11	0.09
Median-wage tercile	0.40	0.32
Firm sector	0.12	0.09
Workforce composition	0.08	0.09
Survey characteristics	0.01	0.01
Year FE	0.02	0.04
N	64,599	64,599

Note: This table reports Shorrocks-Shapley decomposition values for mean reporting bias (in €) and mean absolute reporting bias (in €). Values show the share of explained variation attributed to each group. The number of observations is smaller than in the main sample because of missing values in the covariates.

This divergence reflects the fact that individual characteristics such as age, education, and gender predict higher wage levels, which mechanically inflate absolute deviations in euros, but they matter less for the direction of reporting bias. Firm context, by contrast, shapes both magnitude and direction, consistent with informational mechanisms in the workplace driving systematic over- or underreporting.[30]

We present regression coefficients and their statistical significance in Appendix Table A.4. Consistent with the share of explained variation, the strongest associations show up for job and firm characteristics. For job characteristics, associations are statistically significant and large for factors that generally predict higher wages: absolute deviations are larger for white-collar, full-time, and unionized workers and rise with firm tenure. These same job contexts are also associated with more negative mean bias: white-collar, full-time, and unionized workers, as well as workers with longer firm tenure, not only exhibit larger absolute gaps but also systematically underreport relative to administrative records. Firm-level effects dominate in magnitude. Absolute deviations rise monotonically with firm size and firm pay level, and workers in larger and higher-paying firms similarly exhibit more negative mean bias, consistent with the stronger firm-level dominance in Column (1) of Table 6. Workers in firms with a higher share of German nationals show smaller absolute gaps. Taken together, the

[30]The Shorrocks-Shapley decomposition is robust to including bonus payments. The broad pattern is preserved: firm-level factors, and the median-wage tercile in particular, account for the largest share of explained variation in both the mean and absolute bias. The main difference is that including bonus payments reduces the share attributed to the median-wage tercile somewhat, from 40% to 36% for the mean bias and from 32% to 25% for the absolute bias. At the same time, the shares of individual sociodemographic characteristics and job characteristics increase modestly. This suggests that part of the firm-level gradient in reporting bias reflects differences in bonus pay structures across firms, but the dominance of firm context remains intact. Full results are available in Appendix Table D.5.

firm-level patterns are consistent with informational mechanisms within the workplace shaping the reporting bias. Larger and higher-paying firms tend to feature more complex and standardized compensation structures and are less likely to rely on individually negotiated wages (Caldwell *et al.*, 2025).

For individual characteristics, absolute deviations are larger for male, more educated, and native German workers, and smaller for workers in East Germany, and all differences are statistically significant. This pattern is consistent with the idea that characteristics associated with higher wage levels also tend to mechanically increase absolute deviations. Notably, men show systematically larger absolute deviations than women, a pattern to which we return in Section 6.2.[31] Among personality traits, conscientiousness is associated with larger deviations, whereas agreeableness and neuroticism are associated with smaller deviations.[32] For household characteristics, partner income exhibits a small negative association with absolute reporting bias, while marital status shows no statistically significant association. With respect to survey characteristics, interviews not conducted in person are associated with smaller deviations; survey experience has no detectable effect on the absolute gap.

We further assess the sensitivity of our results to the choice of data source by re-estimating Equation (8) with covariates drawn primarily from either the SOEP or the IEB. Specifically, we run one specification using the maximum number of SOEP variables ("Max SOEP", with only firms' median-wage tercile and workforce composition from the IEB), and another specification in which we replace SOEP variables with IEB information wherever possible ("Max IEB", with only education, personality traits, household variables, white-collar status, and union membership from the SOEP). The findings, summarized in Appendix Table A.5, indicate that our main results are highly robust.

6 Consequences for Economic Analysis

We now examine the implications of reporting bias, imputation, and top-coding for common empirical applications: returns to education (e.g., Becker, 1962, Mincer, 1974), the gender wage gap (e.g., Bertrand *et al.*, 2010, Goldin, 2014), and wage–satisfaction gradients (e.g., Boyce *et al.*, 2010, Cheung and Lucas, 2015). The first two applications cover the case in which wages enter as the dependent variable (henceforth: as outcomes), the third covers wages as an explanatory variable (as regressors). As discussed in Section 3, the consequences of reporting error differ fundamentally across these two settings. In each case, we compare estimates obtained from SOEP wages, IEB wages, and the hybrid

[31] Previous results on the relationship between reporting bias and individual characteristics are mixed. Some studies find gender differences (Bound and Krueger, 1991, Bollinger, 1998, Angel *et al.*, 2019, Antoni *et al.*, 2019, Roth and Slotwinski, 2021), whereas others do not (Bricker and Engelhardt, 2008, Kim and Tamborini, 2014, Angel *et al.*, 2018). Evidence on education is similarly inconclusive, with positive (Bricker and Engelhardt, 2008, Angel *et al.*, 2018, 2019), null (Bound and Krueger, 1991, Gottschalk and Huynh, 2005), and negative correlations (Kim and Tamborini, 2014, Antoni *et al.*, 2019).

[32] Similar patterns are documented for the German National Educational Panel Study (Antoni *et al.*, 2019).

measure, estimated separately for the different estimation samples introduced in Section 2, representing different ranges of the wage distribution: the full sample, the below-limit sample, and the above-limit sample. Below the assessment limit, differences in estimates across wage measures reflect reporting bias alone. Above the limit, they additionally reflect how top-coded administrative wages are treated. The full sample combines both margins and is most relevant for applied research. Comparing across samples thus helps separate the contributions of reporting bias and top-coding to differences across wage measures. Because the below- and above-limit samples are defined by the wage level, we use them only to compare wage measures on a common set of observations, holding the sample fixed across measures. We do not interpret the resulting within-sample coefficients as population returns or gaps. The substantive conclusions rest on the full sample.

For each application, we estimate three specifications that differ in the set of included control variables X. The *raw* specification includes only year-fixed effects. The *basic* specification adds age, age squared, gender, years of education,[33] firm tenure, part-time employment status, and marginal employment status, with full-time employment as the reference category. The *extended* specification further adds German citizenship, accumulated full-time and unemployment experience (in years), firm size, one-digit industry, presence of children under age 16, marital status, and state-fixed effects. This sequential structure allows us to assess whether the sensitivity of estimates to the wage measures diminishes as more controls are included – which would indicate that differences across data sources partly reflect observable compositional differences – or persists even in the most saturated specification.[34]

6.1 Returns to Education

We estimate:

$$ln(Y_{it}) = \alpha + \beta_{YED} YED_{it} + X'_{it}\beta_I + \xi_t + \gamma_c + \varepsilon_{it}, \tag{9}$$

where YED denotes years of education for individual i at time t and X'_{it} follows the control structure described above. β_{YED} denotes the wage gain associated with an additional year of education, commonly referred to as the "return to education"; however, as we do not address the endogeneity of schooling decisions, our estimates capture conditional wage differentials rather than causal effects.

[33]Years of education is excluded from the control set in the returns-to-education analysis, where it serves as the main explanatory variable. Analogously, gender is excluded from the control set in the gender wage gap analysis.

[34]Throughout this section, we verify robustness to including bonus payments on both sides of the comparison. The qualitative patterns are preserved throughout. Minor reversals in the ordering occur only between estimates that are economically negligible or statistically indistinguishable. Results are reported in Appendix Tables D.6–D.9.

With the exception of firm tenure, firm size, and industry, which are drawn from the IEB, all control variables come from the SOEP. Standard errors are clustered at the individual level; results are insensitive to clustering at the household level.

Table 7 reports the estimated returns across samples, wage measures, and covariate specifications. Five findings emerge.

Table 7: Consequences – Returns to Education

Sample[a]		Wage measure[b, c]	Specification (1) Raw	(2) Basic	(3) Extended
Full sample	(I)	SOEP reported	0.101*** (0.003)	0.082*** (0.002)	0.079*** (0.002)
	(II)	IEB censored	0.096*** (0.002)	0.077*** (0.002)	0.071*** (0.002)
		Difference (II) - (I)	-0.004*** (0.001)	-0.005*** (0.001)	-0.008*** (0.001)
		Difference, % of SOEP	-4.0	-6.2	-10.6
	(III)	IEB imputed	0.112*** (0.003)	0.093*** (0.002)	0.088*** (0.002)
		Difference (III) - (I)	0.012*** (0.001)	0.011*** (0.001)	0.009*** (0.001)
		Difference, % of SOEP	11.9	13.1	10.8
	(IV)	Hybrid	0.103*** (0.003)	0.084*** (0.002)	0.078*** (0.002)
		Difference (IV) - (I)	0.002*** (0.001)	0.001* (0.001)	-0.001* (0.001)
		Difference, % of SOEP	2.5	1.7	-1.8
Below-Limit	(I)	SOEP reported	0.068*** (0.002)	0.063*** (0.001)	0.060*** (0.001)
	(II)	IEB recorded	0.074*** (0.003)	0.068*** (0.002)	0.063*** (0.002)
		Difference (II) - (I)	0.006*** (0.001)	0.006*** (0.001)	0.003*** (0.001)
		Difference, % of SOEP	8.9	8.8	5.1
Above-Limit	(I)	SOEP reported	0.020*** (0.004)	0.021*** (0.004)	0.030*** (0.004)
	(II)	IEB imputed	0.020*** (0.003)	0.024*** (0.003)	0.029*** (0.003)
		Difference (II) - (I)	-0.000 (0.004)	0.003 (0.004)	-0.001 (0.004)
		Difference, % of SOEP	-2.1	15.0	-2.0

Note: This table reports coefficient estimates of β_{YED} from regression Equation (9). All estimates are obtained from separate regressions; specifications are described in Section 6.
[a] Samples as defined as in Section 2.5. The full sample contains 91,541 observations, the below-limit sample 83,306 observations and the above-limit sample 7,690 observations.
[b] Wage measures as defined in Section 2.5 and Table 1.
[c] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $YED \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

First, how top-coded IEB wages are handled matters more than the choice between survey and administrative data as such. Within the full sample, censored IEB wages yield the lowest estimated returns (7.7% in the basic specification), whereas imputed IEB wages yield the highest (9.3%), with the SOEP (8.2%) and the hybrid measure (8.4%) falling in between and close to each other. The gap between the censored and imputed IEB estimates thus exceeds the gap between SOEP and imputed IEB wages. The mechanism is straightforward: top-coding wages at the assessment limit compresses wage variation in the upper tail, attenuating the education gradient precisely where it is steepest (among highly educated high-wage earners) and introducing a downward bias.

Second, the direction of the residual gap depends systematically on how top-coding is handled: censored IEB wages yield returns 4–11% *below* the SOEP estimate, with the relative gap widening as controls are added, whereas imputed IEB wages yield returns 11–13% *above*. Once top-coding is addressed, the imputed IEB, SOEP, and hybrid estimates lie close together. In the extended specification they differ by at most one percentage point, roughly 10% of the estimated effect, yet these differences remain statistically significant, as confirmed by the paired test reported in Table 7.[35] Both patterns mirror the findings from Section 4: censoring compresses the upper tail and biases the gradient downward, whereas imputation recovers the highest earners but adds model-based variation that amplifies the education gradient in aggregate. This overshooting echoes the upward imputation bias documented in the above-limit sample.

Third, below the assessment limit, where top-coding does not play a role, the choice of data source matters little. Comparing the two wage measures here isolates the contribution of reporting bias: IEB wages yield slightly higher returns than SOEP wages (0.6 percentage points in the basic specification, narrowing to 0.3 in the extended, corresponding to roughly 9% and 5% of the SOEP estimate). Although statistically significant, these differences are economically small. This residual gap is consistent with the mean-reverting reporting bias documented in Section 4, whereby underreporting is more pronounced among higher-wage, better-educated workers, mildly compressing the education gradient in self-reported wages.

Fourth, above the assessment limit, one might expect the imputation overshooting to generate a visible gap between SOEP and imputed IEB wages. Yet the above-limit sample shows differences that are small and statistically not significant throughout. Interpretation is difficult in this range, as both sources are imperfect: SOEP wages carry unquantified reporting bias (no IEB benchmark is available

[35]For this test, we duplicate the dataset – once with SOEP wages and once with IEB wages as the dependent variable – and interact all covariates with a binary data-source indicator (D_{IEB}). The coefficient on $YED \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$-$\hat{\beta}_{SOEP}$. This paired design is more informative than a naive comparison of the separate estimates: because the education regressor is drawn from the SOEP and is therefore identical across both equations, the two estimators correlate at $\rho \approx 0.97$. As a result, the standard error of their difference ($SE_{diff} = 0.001$) is approximately five times smaller than an independent-samples calculation based on the individual standard errors ($SE_{SOEP} = 0.002$, $SE_{IEB} = 0.003$) would suggest – the analogue of running a paired-sample comparison as an unpaired test. The interaction approach yields $t = 7.3$ ($p < 0.001$). Results are corroborated by a GMM two-sample test, which derives the standard error of the difference from the joint variance-covariance matrix of both equations and reaches an identical conclusion.

above the limit), and IEB wages are entirely model-based. Additionally, the above-limit sample is substantially smaller ($N = 7,690$), reducing statistical power. The absence of a statistically significant difference may therefore reflect increased noise on both sides rather than genuine equivalence.

Finally, the hybrid estimate closely tracks SOEP-based estimates throughout – virtually identical in the extended specification (7.8% vs. 7.9%) – confirming that substituting SOEP for IEB wages only above the assessment limit is sufficient to reproduce the survey-based pattern. Note that comparing estimates across SOEP, IEB, and hybrid wages requires identical samples for all three. Consequently, individuals in marginal employment prior to 1999 – for whom no IEB wage is observed – are excluded from the consequences analysis.

The practical recommendation is clear: the critical concern is not the choice between survey and administrative wages per se, but whether top-coded IEB observations are handled appropriately. Simply censoring wages at the assessment limit understates returns to education; imputation, the hybrid measure, and SOEP wages all yield estimates that are closer to each other – differing by at most one percentage point in the extended specification – although imputed IEB wages remain somewhat above SOEP and hybrid estimates. An alternative approach to dealing with top-coded wages is median regression, which accommodates top-coded wages directly without imputation. In this application, it yields estimates close to the censored IEB specification (7.8% vs. 7.7% in the basic specification, and 7.4% vs. 7.1% in the extended specification). This close correspondence suggests that the treatment of top-coded wages, rather than the choice of estimation method, is the primary driver of the results (Appendix Table A.6).

6.2 Gender Wage Gap

We estimate:

$$ln(Y)_{it} = \alpha + \beta_{FEM} Female_i + X'_{it}\beta_I + \xi_t + \gamma_c + \varepsilon_{it}, \tag{10}$$

where $Female_i$ is an indicator for women and β_{FEM} quantifies the gender wage gap. The control structure follows the one described in the introduction to this section. Standard errors are clustered at the individual level.

Table 8 reports the estimated gender wage gaps across samples, wage measures, and specifications. Three findings emerge.

First, as in the returns-to-education analysis, the treatment of top-coded IEB wages matters more than the choice between survey and administrative data per se. In the full sample, censored IEB wages

Table 8: Consequences – Gender Wage Gap

Sample[(a)]		Wage measure[(b, c)]	Specification		
			(1) Raw	(2) Basic	(3) Extended
Full sample	(I)	SOEP reported	-0.608*** (0.013)	-0.202*** (0.009)	-0.148*** (0.008)
	(II)	IEB censored	-0.585*** (0.013)	-0.165*** (0.009)	-0.114*** (0.008)
		Difference (II) - (I)	0.023*** (0.004)	0.038*** (0.004)	0.035*** (0.004)
		Difference, % of SOEP	3.8	18.6	23.4
	(III)	IEB imputed	-0.638*** (0.015)	-0.205*** (0.010)	-0.142*** (0.010)
		Difference (III) - (I)	-0.029*** (0.004)	-0.002 (0.004)	0.007 (0.004)
		Difference, % of SOEP	-4.8	-1.1	4.5
	(IV)	Hybrid	-0.605*** (0.014)	-0.181*** (0.009)	-0.127*** (0.009)
		Difference (IV) - (I)	0.003 (0.003)	0.021*** (0.003)	0.022*** (0.003)
		Difference, % of SOEP	0.6	10.4	14.6
Below-Limit	(I)	SOEP reported	-0.495*** (0.012)	-0.143*** (0.008)	-0.111*** (0.007)
	(II)	IEB recorded	-0.501*** (0.013)	-0.131*** (0.009)	-0.097*** (0.008)
		Difference (II) - (I)	-0.006* (0.003)	0.011*** (0.003)	0.014*** (0.003)
		Difference, % of SOEP	-1.2	8.0	12.8
Above AL	(I)	SOEP reported	-0.103*** (0.027)	-0.096*** (0.026)	-0.045 (0.028)
	(II)	IEB imputed	-0.076*** (0.019)	-0.058** (0.018)	-0.033 (0.017)
		Difference (II) - (I)	0.027 (0.030)	0.038 (0.029)	0.012 (0.030)
		Difference, % of SOEP	26.4	39.3	27.0

Note: This table reports coefficient estimates of β_{FEM} from regression Equation (10). All estimates are obtained from separate regressions; specifications are described in Section 6.
[(a)] Samples as defined in Section 2.5. The full sample contains 91,541 observations, the below-limit sample 83,306 observations and the above-limit sample 7,690 observations.
[(b)] Wage measures as defined in Section 2.5 and Table 1.
[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Female \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

yield substantially smaller gaps in absolute terms than either SOEP wages or imputed IEB wages. In the basic specification, for instance, the censored IEB estimate is -16.5%, compared with -20.2% for SOEP and -20.5% for imputed IEB – a spread of 4.0 percentage points between the two IEB treatments, far larger than the difference between SOEP and imputed IEB wages. The same mechanism is at work as in Section 6.1: top-coding wages at the assessment limit compresses wage variation precisely where gender differences are most pronounced, mechanically attenuating the estimated gap. Notably, however, the relative distortion from censoring is larger here than for returns to education. In the extended specification, the censored IEB estimate lies 3.5 percentage points (23.4%) below the SOEP estimate, reflecting the fact that the gender earnings gap is especially concentrated at the top of the distribution. This confirms that the large SOEP–IEB divergence visible with censored wages reflects top-coding bias rather than genuine misreporting of the gender gap.

Second, the direction of the SOEP–IEB comparison reverses relative to the returns-to-education analysis. Below the assessment limit, where both sources are observed without censoring or imputation, IEB wages yield a somewhat *smaller* gender gap than SOEP wages (e.g., -13.1% vs. -14.3% in the basic specification). This pattern is statistically significant and consistent with differential reporting bias by gender. In the full sample with imputed IEB wages, the difference becomes negligible and is statistically not significant: SOEP and imputed IEB estimates are virtually indistinguishable in the basic and extended specifications (-20.2% vs. -20.5% and -14.8% vs. -14.2%). Above the assessment limit, differences between SOEP and imputed IEB wages are small and statistically not significant throughout, again for the same reasons as in the returns-to-education analysis: the small sample size and the inherent imprecision of both sources in this range preclude reliable inference.

Third, the below-limit gap between SOEP and IEB reflects a genuine, non-classical reporting pattern rather than an artefact of censoring. Because the gender gap is estimated in logs, the relevant quantity is the *relative* reporting bias by gender. Although men exhibit larger *absolute* deviations than women (Section 5.2) – a consequence of their higher wage levels under mean reversion – women's wages are underreported *proportionally* more (equivalently, men report relatively closer to their administrative wage), so that the female wage penalty appears 1.1–1.4 percentage points larger in the SOEP than in the administrative records. This gap *widens* as controls are added (from 8% to 13%), the opposite of the returns-to-education case (Section 6.1). A difference driven by observable composition – for example, the sorting of men and women into different firms or sectors – would be expected to shrink as richer controls are introduced. Its growth instead points to a gender-differential reporting pattern of the kind discussed in Section 3, which standard controls do not capture. The hybrid measure tracks the SOEP estimate closely, lying between the censored and imputed IEB values and confirming that coverage above the assessment limit is the primary driver of cross-source differences.

The practical recommendation mirrors that of Section 6.1, with one important addition: simply censoring wages at the assessment limit understates the gender wage gap substantially – more so than returns to education – because gender differences are concentrated precisely where administrative data are censored. Imputation or the hybrid wage measure restores comparability with survey-based estimates; the residual SOEP–IEB gap that remains below the assessment limit reflects genuine non-classical reporting differences by gender and should be interpreted as such. Under median regression, the censored IEB estimate (-17.5% in the basic specification) in Appendix Table A.6 lies somewhat closer to the SOEP estimate (-20.2%) than the censored IEB estimate based on OLS (-16.5%), reflecting that median regression is less sensitive to the compressed upper tail. The gap relative to survey-based estimates nonetheless remains substantial, confirming that median regression does not fully solve the problem of top-coded IEB observations.

6.3 Satisfaction and Wages

We estimate:

$$SI_{it} = \alpha + \beta_W Wage_{it} + X_{it}'\beta_I + \xi_t + \gamma_c + \mu_i + \varepsilon_{it}, \tag{11}$$

where SI_{it} is a standardized satisfaction indicator (mean zero, standard deviation one) for individual i in year t measured on an 11-point Likert scale. We consider two subjective well-being outcomes that have been shown to respond to individual income (e.g., Boyce *et al.*, 2010, Cheung and Lucas, 2015): life satisfaction and satisfaction with personal income.[36] $Wage_{it}$ is the monthly wage, scaled by €1,000, based on SOEP, IEB, or hybrid wage measures. The control structure follows the one introduced at the beginning of this section. In addition to the raw, basic, and extended specifications, we estimate a fourth specification that adds individual-fixed effects μ_i, thereby isolating the association between wage changes and changes in satisfaction within individuals over time. Standard errors are clustered at the individual level.[37]

Tables 9 and 10 report the estimated wage–satisfaction gradients for life satisfaction and satisfaction with personal income. Several patterns emerge.

First, the consequences of measurement choice are substantially larger here than in the applications with wages as the dependent variable in Sections 6.1 and 6.2. For satisfaction with personal income, the difference between censored IEB and SOEP wages in the basic specification amounts to 0.097 standard deviations per €1,000 – more than 80% of the SOEP estimate – and remains large and statistically significant across all specifications, including with individual-fixed effects. For life satisfaction, the differences are smaller in absolute terms but similarly large in relative terms (28–84% of the SOEP estimate across specifications). This sensitivity persists even after adding rich controls

[36]The exact wording of the 2019 English version of the SOEP is as follows: "a) For satisfaction with personal income: *How satisfied are you today with the following areas of your life? ... with your personal income?*; b) for life satisfaction: *In conclusion, we would like to ask you about your satisfaction with your life in general. How satisfied are you with your life, all things considered?* Possible answers in both cases: 0 'completely dissatisfied' to 10 'completely satisfied'."

[37]Results are qualitatively and quantitatively similar when clustering at the household level.

Table 9: Consequences – Life Satisfaction

Sample[a]		Wage measure[b, c]	Specification (1) Raw	(2) Basic	(3) Extended	(4) Individual FE
Full sample	(I)	SOEP reported	0.040*** (0.004)	0.046*** (0.006)	0.031*** (0.005)	0.015** (0.005)
	(II)	IEB censored	0.052*** (0.004)	0.071*** (0.005)	0.048*** (0.006)	0.027*** (0.007)
		Difference (II) - (I)	0.011*** (0.003)	0.025*** (0.007)	0.016** (0.006)	0.012* (0.006)
		Difference, % of SOEP	27.9	54.9	52.5	84.1
	(III)	IEB imputed	0.027*** (0.002)	0.027*** (0.003)	0.016*** (0.002)	0.003* (0.001)
		Difference (III) - (I)	-0.013*** (0.003)	-0.019*** (0.006)	-0.016*** (0.004)	-0.012** (0.004)
		Difference, % of SOEP	-33.3	-41.9	-50.5	-78.8
	(IV)	Hybrid	0.039*** (0.004)	0.044*** (0.006)	0.029*** (0.005)	0.010* (0.005)
		Difference (IV) - (I)	-0.001* (0.001)	-0.002 (0.001)	-0.002* (0.001)	-0.004 (0.002)
		Difference, % of SOEP	-3.4	-3.4	-6.9	-28.7
Below-Limit	(I)	SOEP reported	0.053*** (0.005)	0.084*** (0.007)	0.059*** (0.007)	0.036*** (0.007)
	(II)	IEB recorded	0.044*** (0.004)	0.064*** (0.006)	0.042*** (0.006)	0.029*** (0.007)
		Difference (II) - (I)	-0.009*** (0.001)	-0.019*** (0.002)	-0.016*** (0.003)	-0.007 (0.005)
		Difference, % of SOEP	-16.7	-23.1	-27.9	-19.5
Above-Limit	(I)	SOEP reported	0.015** (0.005)	0.017** (0.005)	0.013** (0.005)	-0.008 (0.006)
	(II)	IEB imputed	0.002 (0.002)	0.002 (0.002)	-0.000 (0.002)	-0.001 (0.001)
		Difference (II) - (I)	-0.012* (0.005)	-0.015** (0.005)	-0.013** (0.005)	0.008 (0.006)
		Difference, % of SOEP	-84.1	-87.1	-102.5	92.2

Note: This table reports coefficient estimates of β_W from regression Equation (11). All estimates are obtained from separate regressions; specifications are described in Section 6, wages are scaled by €1000.
[a] Samples as defined in Section 2.5. The full sample contains 91,008 (88,594 in (4)) observations, the below-limit sample 82,819 (80,352 in (4)) observations and the above-limit sample 7,646 (7,429 in (4)) observations.
[b] Wage measures as defined in Section 2.5 and Table 1.
[c] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Wage \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

and individual-fixed effects, indicating that the differences are not driven by observable compositional differences or time-invariant individual heterogeneity.

Second, the direction of the IEB–SOEP comparison depends critically on how top-coding is handled and differs from the case with wages as outcomes. Censored IEB wages yield *higher* wage–satisfaction gradients than SOEP wages in the full sample (e.g., 0.071 vs. 0.046 standard deviations per €1,000 for life satisfaction in the basic specification), whereas imputed IEB wages yield *lower* gradients (0.027 vs. 0.046). The spread between the two IEB treatments thus substantially exceeds the distance of either from the SOEP estimate, and the two bracket the SOEP in opposite directions. This pattern is consistent with the imputation mechanism documented in Section 4: imputed wages introduce

Table 10: Consequences – Satisfaction with Personal Income

Sample[(a)]		Wage measure[(b, c)]	Specification			
			(1) Raw	(2) Basic	(3) Extended	(4) Individual FE
Full sample	(I)	SOEP reported	0.131*** (0.011)	0.119*** (0.017)	0.106*** (0.017)	0.114*** (0.012)
	(II)	IEB censored	0.186*** (0.003)	0.216*** (0.005)	0.206*** (0.005)	0.207*** (0.008)
		Difference (II) - (I)	0.055*** (0.011)	0.097*** (0.017)	0.100*** (0.017)	0.094*** (0.013)
		Difference, % of SOEP	42.2	81.8	93.8	82.2
	(III)	IEB imputed	0.091*** (0.003)	0.073*** (0.004)	0.063*** (0.003)	0.020*** (0.002)
		Difference (III) - (I)	-0.040*** (0.011)	-0.045** (0.017)	-0.043** (0.017)	-0.094*** (0.012)
		Difference, % of SOEP	-30.8	-38.1	-40.9	-82.3
	(IV)	Hybrid	0.131*** (0.011)	0.120*** (0.017)	0.107*** (0.017)	0.117*** (0.012)
		Difference (IV) - (I)	-0.000 (0.001)	0.002 (0.001)	0.000 (0.001)	0.003 (0.006)
		Difference, % of SOEP	-0.1	1.3	0.4	2.7
Below-Limit	(I)	SOEP reported	0.209*** (0.004)	0.262*** (0.006)	0.247*** (0.006)	0.240*** (0.008)
	(II)	IEB recorded	0.183*** (0.004)	0.217*** (0.005)	0.205*** (0.006)	0.219*** (0.008)
		Difference (II) - (I)	-0.026*** (0.001)	-0.045*** (0.002)	-0.042*** (0.003)	-0.021*** (0.006)
		Difference, % of SOEP	-12.6	-17.1	-16.9	-8.7
Above-Limit	(I)	SOEP reported	0.025*** (0.007)	0.026*** (0.008)	0.023** (0.007)	0.023** (0.008)
	(II)	IEB imputed	0.003 (0.002)	0.002 (0.002)	0.002 (0.002)	-0.001 (0.001)
		Difference (II) - (I)	-0.022** (0.007)	-0.023** (0.008)	-0.022** (0.007)	-0.023** (0.008)
		Difference, % of SOEP	-88.5	-90.4	-93.3	-103.5

Note: This table reports coefficient estimates of β_W from regression Equation (11). All estimates are obtained from separate regressions; specifications are described in Section 6, wages are scaled by €1000.
[(a)] Samples as defined as in Section 2.5. The full sample contains 76,431 (74,035 in (4)) observations, the below-limit sample 69,742 (67,290 in (4)) observations and the above-limit sample 6,250 (6,057 in (4)) observations.
[(b)] Wage measures as defined in Section 2.5 and Table 1.
[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Wage \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$-$\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

smooth model-based variation in the upper tail that attenuates the estimated gradient, whereas top-coding high earners' wages at the assessment limit compresses wage variation precisely where wages and income satisfaction are most strongly associated, artificially inflating the estimated gradient per unit of observed wage variation.

Third, the below-limit sample – where both sources are observed without censoring or imputation – consistently shows that SOEP wages yield *higher* gradients than IEB wages. For life satisfaction, the SOEP estimate exceeds the IEB estimate by 17–28% across specifications; for satisfaction with personal income, by 9–17%. These differences are statistically significant and persist with individual-fixed effects for satisfaction with personal income. This pattern is the opposite of what classical measurement error would predict: attenuation bias from random noise in survey wages would compress SOEP-based gradients toward zero, not inflate them. Instead, the result points to systematic, non-classical reporting behavior, consistent with the interpretation that respondents who report higher wages than they actually earn also tend to report higher income satisfaction, thereby inflating the observed gradient in survey data (Prati, 2017).

Fourth, the hybrid wage measure tracks the SOEP estimate remarkably closely throughout – differences between hybrid and SOEP coefficients are small and typically statistically not significant (e.g., essentially zero for personal income satisfaction across all specifications). This stands in contrast to the applications with wages as outcomes, where the hybrid measure fell between SOEP and IEB. Here, even though the hybrid measure substitutes SOEP wages for IEB wages only above the assessment limit, this limited substitution is sufficient to reproduce the full SOEP pattern. The result indicates that the upper tail of the wage distribution – where survey and administrative wages diverge most – exerts disproportionate influence on the estimated wage–satisfaction gradient, consistent with the non-classical, context-dependent nature of the reporting bias documented in Section 4.

Taken together, these findings imply that the estimated relationship between wages and subjective well-being is somewhat sensitive to the choice of data source but highly sensitive to the wage measures, more so than when wages enter as outcomes. The gradient is steepest when survey wages are used and flattest when imputed IEB wages are used, with the hybrid measure closely reproducing the survey pattern. These differences persist with individual-fixed effects, ruling out time-invariant individual heterogeneity – including a constant individual-specific reporting bias – as the primary driver. Their inconsistency with classical attenuation bias points instead to correlated reporting tendencies – such as hedonic recall or aspirational bias (Prati, 2017, Bertrand and Mullainathan, 2001) – that inflate survey-based gradients, rather than to random measurement error.

7 Conclusion

This paper documents that reporting errors in self-reported wages are large and systematic, but that their empirical consequences depend critically on the application and, above all, on how administrative top-coding is handled.

On the extent of misreporting, we find pervasive mean reversion: about 67% of respondents underreport their wages, with mean survey wages lying 7% (€170) below administrative records. The bias is larger in absolute terms at the top of the distribution, but remarkably stable in relative terms. It is also noisier in year-to-year changes, so simple differencing does not solve the problem. Adding bonus payments to both sources reduces the average discrepancy, to about 3%, while leaving the mean-reverting pattern and our substantive conclusions unchanged. Whether to include bonus payments therefore depends on the research question, rather than being universally advisable.

Regarding the correlates, firm context dominates: size, pay structure, sector, and workforce composition account for most of the explained variation, consistent with structured workplace environments shaping reporting behavior rather than idiosyncratic survey mistakes. Individual characteristics, by contrast, explain comparatively little.

The consequences analysis confirms the central implication of the framework in Section 3 and has direct implications for applied work: the empirical relevance of reporting error depends critically on whether wages enter as the outcome or as an explanatory variable. When wages enter as outcomes, the primary concern is not the choice between survey and administrative data per se, but how top-coded administrative values are handled across the full distribution. Simply censoring IEB wages at the assessment limit substantially understates returns to education and the gender wage gap. Imputation recovers the censored variation but can overshoot: for returns to education, imputed IEB estimates lie above SOEP estimates, reversing the direction of the bias, whereas for the gender wage gap, imputation and SOEP estimates are virtually indistinguishable once top-coding is addressed. Below the assessment limit, differences across data sources are systematic rather than incidental. For returns to education they are small and largely traceable to mean reversion, whereas for the gender wage gap they reflect a non-classical, gender-differential reporting pattern. Richer controls shrink the former but widen the latter. When wages enter as regressors, the picture is more concerning: below the assessment limit, survey-based wage–satisfaction gradients are 9–28% steeper than administrative ones. This pattern persists with individual-fixed effects and is inconsistent with classical attenuation bias, pointing instead to bias away from zero. It reflects correlated reporting tendencies, such as hedonic recall or aspirational bias, that inflate observed gradients in survey data and are not eliminated by standard controls.

To navigate these trade-offs, we propose a hybrid wage measure that uses administrative records below the assessment limit and survey wages above it, in the spirit of approaches that combine information across data sources to improve wage measurement (Meijer *et al.*, 2012, Abowd and Stinson, 2013). This measure sidesteps both the downward bias from censoring and the model-dependent variation introduced through imputation, though it inherits the reporting bias of survey wages above the limit. In the consequences analysis, the hybrid measure closely tracks survey-based estimates, confirming that the upper tail of the wage distribution exerts disproportionate influence on estimated relationships. Moreover, the hybrid wage measure can be converted into hourly wages using survey-reported hours, making it directly applicable to analyses of minimum wages, wage floors, and overtime premia. We recommend cross-checking key results against administrative or hybrid wages and reporting sensitivity to top-coded treatments.

The broader lesson is that the consequences of reporting bias are application-specific. Before choosing a wage measure, researchers should ask how the bias relates to the other variables in their model: with the regressors when wages are an outcome, and with the wage itself and the outcome when wages are a regressor. Because the SOEP and IEB are among the most widely used data sources in applied labor economics, the practical guidance we provide speaks directly to a large body of existing and ongoing research. Yet our findings extend beyond the German setting to researchers working with analogous survey and administrative data in other countries. Mean-reverting survey reports are a recurring pattern in linked earnings data, including recent U.S. evidence (Bee *et al.*, 2023). Similar top-coding of administrative records occurs elsewhere: in Austria, for example, a researcher linking survey data from the Austrian Socio-Economic Panel to social-security earnings records would face the same choice we analyze here, since administrative earnings there are top-coded at statutory limits. The strong role of job and firm characteristics in our setting further suggests that reporting error may depend on the institutional and organizational context in which wages are set and communicated, although the exact mechanisms may differ across countries. Finally, our results suggest that researchers should not treat administrative wages as uniformly superior, especially under institutional censoring. Increasingly available linked-data environments instead create scope for hybrid wage measures that mitigate measurement problems when the relative quality of survey and administrative sources varies across the wage distribution.

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Online Appendix:

On the Extent, Correlates, and Consequences of Reporting Bias in Survey Wages

A Supplementary Tables and Figures

Table A.1: Number of Observed SOEP Individuals at Different Sampling Stages

SOEP Individuals by Sampling Step			
Sampling stage	Restriction	Individuals	Individuals × Year
1.	Contacted	45,345	349,100
2.	Consented	33,733	259,224
3.	Ever IEB spell ("linked")	29,778	232,276
4.	Employed SOEP (wage > 0)	22,498	150,169
5.	No pure self-employment spell (SOEP)	21,661	140,660
6.	No civil servant spell (SOEP)	21,158	134,371
7.	Age 20-65 (SOEP)	20,320	128,128
8.	Overlapping working spell IEB & SOEP	17,987	108,711

Note: This table summarizes the number of observations in the linked SOEP-CMI-ADIAB7521 dataset at different sampling stages. Each sampling step is conditional on the ones before. *1. Contacted* displays SOEP individuals who were asked for their consent to the linkage. *2. Consented* counts all SOEP individuals who consented to the linkage. *3. Linked* refers to SOEP individuals who – conditional on consent – ever had a spell in the IEB data. This is not necessarily an overlapping spell and can also be an unemployment spell. *4. Employed SOEP* restricts the sample to observations with positive wages in the SOEP. Restrictions 5 to 7 further restrict the sample to individuals who are employed subject to social security contributions and aged 20-65 years. Finally, the sample is restricted to individuals who have working spells with positive wage in the IEB data overlapping or having ended no more than 30 days before the SOEP interview.

Table A.2: Reporting Bias over Age

Age	Mean Wage	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
20	975	-61	-6	179	18	1,088
21	1,160	-70	-6	182	16	1,290
22	1,373	-82	-6	223	16	1,388
23	1,530	-80	-5	236	15	1,461
24	1,683	-105	-6	237	14	1,546
25	1,819	-104	-6	261	14	1,599
26	1,968	-127	-6	268	14	1,672
27	2,112	-136	-6	293	14	1,812
28	2,226	-135	-6	289	13	1,908
29	2,327	-168	-7	311	13	2,007
30	2,357	-160	-7	296	13	2,111
31	2,386	-155	-6	294	12	2,190
32	2,375	-168	-7	308	13	2,210
33	2,402	-160	-7	313	13	2,245
34	2,377	-159	-7	300	13	2,321
35	2,352	-155	-7	297	13	2,478
36	2,334	-151	-6	301	13	2,534
37	2,321	-147	-6	297	13	2,636
38	2,363	-170	-7	301	13	2,722
39	2,328	-163	-7	294	13	2,731
40	2,326	-157	-7	297	13	2,837
41	2,327	-164	-7	289	12	2,857
42	2,366	-166	-7	302	13	2,913
43	2,382	-166	-7	308	13	2,971
44	2,411	-174	-7	310	13	2,962
45	2,456	-179	-7	320	13	2,929
46	2,489	-192	-8	323	13	2,977
47	2,475	-179	-7	307	12	2,933
48	2,491	-193	-8	323	13	2,891
49	2,530	-197	-8	319	13	2,868
50	2,538	-188	-7	322	13	2,867
51	2,579	-196	-8	329	13	2,801
52	2,577	-202	-8	331	13	2,711
53	2,584	-203	-8	332	13	2,594
54	2,595	-196	-8	336	13	2,492
55	2,589	-190	-7	326	13	2,380
56	2,568	-195	-8	317	12	2,228
57	2,537	-200	-8	334	13	2,050
58	2,501	-182	-7	325	13	1,943
59	2,521	-223	-9	344	14	1,809
60	2,520	-210	-8	361	14	1,670
61	2,522	-235	-9	366	15	1,467
62	2,520	-254	-10	386	15	1,295
63	2,518	-251	-10	367	15	1,067
64	2,180	-194	-9	344	16	687
65	1,765	-111	-6	265	15	478
All	2,342	-170	-7	307	13	99,618

Note: This table reports summary statistics by age. Column (1) reports the mean IEB wage of each year of age. Columns (2) and (3) report the mean reporting bias (SOEP minus IEB wage), in € and percent. Columns (4) and (5) report the mean absolute reporting bias, in € and percent. Column (6) reports the number of observations in each age group.

Table A.3: Variance–Covariance Matrix

	$\ln y_{IEB,t}$	$\Delta \ln y_{IEB,t}$	$\ln y_{SOEP,t}$ - $\ln y_{IEB,t}$	$\Delta[\ln y_{SOEP,t}$ - $\ln y_{IEB,t}]$
$\ln y_{IEB,t}$	0.550			
$\Delta \ln y_{IEB,t}$	0.026	0.093		
$\ln y_{SOEP,t}$ - $\ln y_{IEB,t}$	-0.067	-0.016	0.067	
$\Delta[\ln y_{SOEP,t}$ - $\ln y_{IEB,t}]$	-0.015	-0.036	0.035	0.071

Note: This table reports the variance–covariance matrix of log IEB wages ($\ln y_{IEB,t}$) and the reporting bias ($\ln y_{SOEP,t}$ - $\ln y_{IEB,t}$) in levels and first differences. The sample is restricted to individuals observed in consecutive years, yielding 74,422 observations, therefore differs both in sample and structure from reliability ratios documented in Figure A.1.

Table A.4: Factors of Reporting Bias

	Mean Bias (1)	Mean Abs. Bias (2)
Individual Characteristics		
Sociodemographic characteristics		
Age	.288	.095
	(.316)	(.249)
Female	-6.302	-31.659***
	(7.483)	(6.061)
Years of education	-7.104***	9.410***
	(1.641)	(1.356)
German citizenship	-40.291***	16.734**
	(9.439)	(7.568)
Region (ref. cat.: North)		
East	12.542	-36.586***
	(8.531)	(6.768)
South	-4.089	9.497
	(9.222)	(7.217)
West	6.450	-2.719
	(8.426)	(6.662)
Personality Traits		
Openness (std.)	1.184	3.541
	(3.657)	(2.819)
Conscientiousness (std.)	1.244	9.978***
	(4.125)	(3.308)
Extraversion (std.)	3.814	3.978
	(3.575)	(2.743)
Agreeableness (std.)	-4.853	-5.586*
	(3.733)	(2.952)
Neuroticism (std.)	9.042**	-8.643***
	(3.591)	(2.911)
Household Characteristics		
Married	-12.665	3.925
	(7.871)	(6.063)
Household size	-6.232*	8.808***
	(3.701)	(2.961)
Number of kids in HH	6.419	-7.002**
	(4.246)	(3.310)
Partner	-6.611	-7.064
	(9.156)	(7.318)
Partner × Partner inc in €1000	7.854***	-2.529**
	(1.479)	(1.258)
Job and Firm Characteristics		
Job Characteristics		
White collar status	-39.236***	28.541***
	(7.226)	(5.786)
Full-time employment	-78.802***	102.484***
	(6.167)	(4.971)
Union membership	-32.457***	26.417***
	(7.975)	(6.475)
Firm Tenure	-3.894***	3.023***
	(.379)	(.316)
Firm Size (ref. cat.: 1-19 employees)		
20-199 employees	-34.926***	9.079*
	(6.645)	(5.131)
200-1999 employees	-55.192***	33.498***
	(8.217)	(6.322)
2000+ employees	-114.869***	89.309***
	(15.901)	(13.397)
Median Wage Tercile (ref. cat.: 1st Tercile)		
2nd Tercile	-116.285***	68.661***
	(6.170)	(4.730)
3rd Tercile	-262.449***	187.994***
	(9.007)	(7.282)

Continued on next page

	Mean Bias (1)	Mean Abs. Bias (2)
Firm Sector (ref. cat.: Agriculture, hunting and forestry)		
Fishing	-48.017	-97.268***
	(32.878)	(19.143)
Mining and quarrying	-27.434	-7.440
	(55.846)	(34.275)
Manufacturing	-75.296***	46.676***
	(23.875)	(15.740)
Electricity, gas, and water supply	-155.745***	119.730***
	(52.982)	(39.548)
Construction	-31.014	19.464
	(25.535)	(17.194)
Wholesale and retail trade	-48.944**	12.259
	(23.600)	(15.548)
Hotels and restaurants	37.119	36.658**
	(26.187)	(18.252)
Transport, storage and communication	23.881	9.394
	(26.074)	(17.147)
Financial intermediation	-100.990***	102.336***
	(30.780)	(20.846)
Real estate, renting and business activities	-13.808	10.205
	(23.736)	(15.772)
Public administration and defence; compulsory social security	-89.501***	51.417***
	(24.994)	(17.001)
Education	-37.132	17.518
	(26.164)	(18.572)
Health and social work	-15.252	9.537
	(24.009)	(16.019)
Other community, social and personal service activities	-31.572	14.988
	(25.706)	(17.819)
Private households with employed persons	36.451	-61.765
	(60.604)	(44.158)
Workforce Composition		
Share of highly qualified employees	7.890	-17.017
	(19.650)	(16.208)
Share of female employees	-50.086***	-16.765
	(13.927)	(10.948)
Share of German employees	79.827***	-20.028**
	(12.236)	(9.229)
Share of fulltime employees	-80.960***	27.424***
	(11.703)	(8.933)
Mean age of employees in firm	-.941*	.239
	(.518)	(.411)
Survey Characteristics		
Survey: Not In Person	-13.079**	-24.791***
	(6.446)	(5.156)
Survey: Unclear	-25.192**	-4.096
	(12.002)	(10.326)
1-2 previous interviews	-15.710**	4.650
	(6.491)	(5.219)
3-5 previous interviews	-17.418**	2.946
	(7.168)	(5.773)
6-8 previous interviews	-29.603***	1.318
	(7.944)	(6.333)
> 8 previous interviews	-30.074***	3.455
	(8.682)	(7.010)

Continued on next page

Factors of Reporting Bias – Continued

	Mean Bias (1)	Mean Abs. Bias (2)
Year FE		
1985	-13.189	7.723
	(18.852)	(14.570)
1986	-14.114	13.522
	(19.009)	(15.504)
1987	-24.988	13.296
	(19.407)	(15.955)
1988	-18.454	27.281
	(21.150)	(17.434)
1989	-16.212	22.700
	(20.793)	(16.307)
1990	-35.675*	50.708***
	(20.496)	(16.633)
1991	-60.100***	81.433***
	(20.974)	(17.412)
1992	-39.442**	80.348***
	(19.720)	(15.912)
1993	-55.406***	83.706***
	(19.703)	(15.958)
1994	-21.890	60.499***
	(20.217)	(16.564)
1995	-74.081***	104.216***
	(20.938)	(17.023)
⋮	⋮	⋮
2010	-38.949**	133.208***
	(19.022)	(15.150)
2011	-38.116**	123.205***
	(19.093)	(15.144)
2012	-50.098***	130.354***
	(19.154)	(15.222)
2013	-46.354**	131.716***
	(19.125)	(15.192)
2014	-50.399**	143.320***
	(19.567)	(15.497)
2015	-63.296***	145.536***
	(19.271)	(15.335)
2016	-54.452***	141.970***
	(19.514)	(15.543)
2017	-66.670***	159.660***
	(19.193)	(15.286)
2018	-75.437***	175.270***
	(19.661)	(15.727)
2019	-75.768***	185.524***
	(19.761)	(15.744)
2020	-37.566*	172.413***
	(19.944)	(15.869)
2021	-61.550**	193.284***
	(24.454)	(19.423)
Constant	365.637***	-195.451***
	(41.671)	(31.924)
N	64,599	64,599
R2	.168	.180
Mean dep. var.	-196	312

Note: This table reports coefficient estimates of OLS regressions based on Equation (8). Dependent variables are mean reporting bias and mean absolute reporting bias, both measured in €. Rows are grouped according to the covariate categories used in Table 6. Year fixed effects for 1996–2009 are omitted for brevity. Standard errors are in parentheses and clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Shorrocks-Shapley Decomposition of Reporting Bias – Robustness to Covariate Sources

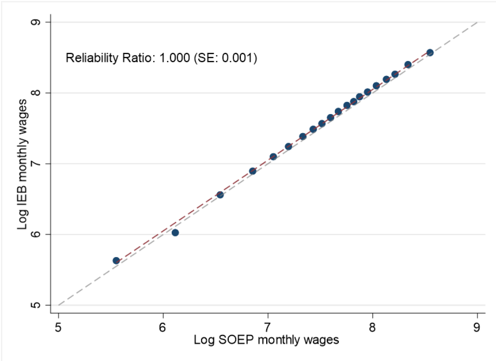
	Share of explained variation			
	Max IEB[(a)]		Max SOEP[(b)]	
	Mean Bias (1)	Mean Abs. Bias (2)	Mean Bias (3)	Mean Abs. Bias (4)
Sociodemographic characteristics	0.06	0.13	0.07	0.12
Personality traits	0.01	0.02	0.01	0.02
Household characteristics	0.01	0.00	0.01	0.00
Job characteristics	0.19	0.14	0.20	0.22
Firm size	0.11	0.10	0.12	0.10
Median-wage tercile	0.40	0.35	0.41	0.33
Firm sector	0.12	0.10	0.07	0.07
Workforce composition	0.08	0.10	0.08	0.09
Survey characteristics	0.01	0.01	0.01	0.01
Year FE	0.02	0.05	0.01	0.04
N	64,599	64,599	64,599	64,599

Note: This table reports Shorrocks-Shapley decomposition values for mean reporting bias (in €) and mean absolute reporting bias (in €), using the max IEB and max SOEP specifications. The rows indicate groups of explanatory variables. Values show the share of explained variation attributed to each group. N reports the number of observations used in each decomposition.
[(a)] All variables except education, personality, household variables, white-collar worker, and union membership are drawn from the IEB.
[(b)] All variables expect firms' median-wage tercile and workforce composition are drawn from the SOEP.

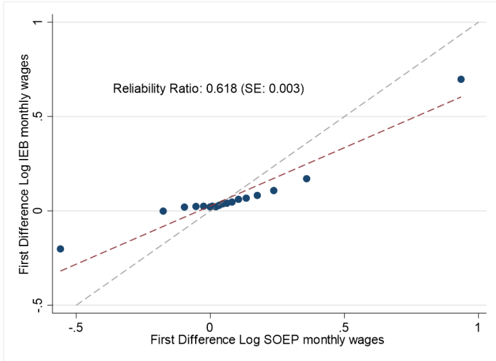
Table A.6: Consequences – Median Regression

	(1) Raw	(2) Basic	(3) Extended[(a)]
Panel A: Returns to Education			
IEB censored (median reg)	0.094***	0.078***	0.074***
	(0.003)	(0.001)	(0.001)
Panel B: Gender Wage Gap			
IEB censored (median reg)	-0.507***	-0.175***	-0.135***
	(0.015)	(0.009)	(0.009)

Note: This table reports coefficient estimates of β_{YED} from regression Equation (9) and of β_{FEM} from regression Equation (10), estimated using median regression.
[(a)] As the median regression does not converge when cells get too small, we re-grouped the industries into broader categories and the years into periods in column (3).
Standard errors are clustered at the individual level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$



(a) Reliability Ratio Levels



(b) Reliability Ratio Changes

Figure A.1: Reliability Ratios

Note: Panel (a) shows a binscatter of log SOEP wages against log IEB wages; Panel (b) uses wage changes instead of levels. The documented reliability ratio is obtained from the regression coefficients of log IEB wages on log SOEP wages, and corresponds to the slope of the fitted line in the binscatter (red line). The grey dashed line indicates the 45-degree line.

B Robustness I: Extent of Reporting Bias

Table B.1 summarizes robustness checks for our baseline findings on the extent of reporting bias. Panel (a) repeats the baseline specification (mean underreporting of €170, SD = €444) as a reference. Panel (b) demonstrates the stability of the results across alternative sample definitions. Restricting to observations with complete information on all control variables ($N = 93,739$) leaves the mean virtually unchanged at €168. Limiting the analysis to the year of consent alone reduces the sample substantially ($N = 11,190$) but yields a comparable mean underreporting of €150. Dropping the top and bottom 10% of reporters produces a tighter distribution by construction (SD = €199) but leaves the mean largely intact at €152. Finally, restricting the analysis to the SOEP Core sample (excluding Migration and Innovation subsamples) confirms the baseline with a mean of €172. Panel (c) shows that explicitly accounting for leap years in the daily-to-monthly wage conversion changes the mean underreporting by less than €1, confirming that this calculation detail is inconsequential.

Panel (d) uses retrospectively reported annual wages instead of the monthly wage for the last month worked. The resulting mean annual underreporting of €1,340 (corresponding to €112 per month) is notably smaller than in the baseline, suggesting that respondents may recall their annual earnings more accurately than their current monthly wage. A natural candidate mechanism for reporting discrepancies is rounding. Panel (e) re-estimates the reporting bias after sequentially excluding respondents who report rounded values. Excluding multiples of €50 – the most prevalent form, affecting 65% of respondents – leaves the mean nearly unchanged, at €165. Excluding multiples of €100 reduces it modestly, to €149. We conclude that rounding contributes to the reporting gap but explains only a small share of the overall bias. A remaining concern is that individuals who consent to data linkage may differ systematically from non-consenters, potentially limiting the representativeness of our results. Appendix C describes a selection model that predicts consent based on individual, state, sector, and industry characteristics and constructs inverse probability weights accordingly. The mean reporting bias using selection weights (€174) is close to the unweighted baseline (€170), confirming that selective consent does not materially distort our findings.

Finally, Panel (g) examines the sensitivity of the baseline specification to the treatment of supplementary payments. Including one-twelfth of bonus payments (such as vacation pay or Christmas bonuses) on both sides, added to the SOEP wage and to the IEB wage via separately reported bonus spells, reduces mean underreporting to €75 over the full period. Because separate bonus spells are rare in the IEB before 2013, this adjustment mainly affects observations from 2013 onward. Restricting the analysis to the post-2013 period, when separate reporting is most complete, yields a slightly larger but comparable estimate of €79. Our baseline specification compares like with like: both sources measure regular monthly wages and exclude supplementary payments, since the SOEP questionnaire explic-

Table B.1: Robustness of Reporting Bias for the Below-Limit Sample

	Mean	SD	P25	P75	N
Panel (a): Baseline specification					
Baseline: Cut at assessment limit IEB	-170	444	-343	14	99.618
Panel (b): Sample restrictions					
Only keeping observations with all controls	-168	438	-347	16	93.776
Only year of consent is kept	-150	505	-341	40	11.790
Without the 10% most extreme over- or underreporting	-153	200	-285	-1	79.725
Only SOEP Core	-173	441	-344	12	92.628
Panel (c): Technical specifications					
Exact calculation for leap years	-170	446	-343	15	98.339
Panel (d): Alternative reporting period					
Reported wage last year (instead of last month)	-1,341	5,901	-3,537	720	93.750
Panel (e): Rounding restrictions					
Excluding rounding at 50	-165	349	-291	0	34.242
Excluding rounding at 100	-150	353	-275	6	44.385
Excluding rounding at 500	-167	418	-326	12	85.149
Excluding rounding at 1000	-168	431	-334	14	92.592
Panel (f): Weighting					
Cut at assessment limit IEB, weighted with consent weights	-174	437	-347	11	87.849
Panel (g): Alternative income definitions					
Including add. payments from SOEP and IEB	-75	403	-205	67	79.925
Only 2013 onwards	-79	434	-218	76	46.812

Note: This table shows the robustness of the reporting bias to other income definitions for the below-limit sample.

itly instructs respondents to exclude one-time payments and our main IEB wage measure excludes separately reported bonus spells. The reduction under the bonus-inclusive specification therefore reflects this broader wage concept rather than a difference in how accurately regular monthly wages are reported. The standard deviation and interquartile range remain close to the baseline specification, confirming that the distributional shape of the reporting bias is not sensitive to this choice.

C Selection Model for Consent

A potential concern for our analysis is that individuals who consent to data linkage may differ systematically from those who do not, potentially limiting the representativeness of our results. Table C.1 documents differences between consenters and non-consenters across a broad set of individual, household, job, and firm characteristics; mean differences are modest throughout. We nonetheless implement a formal selection model to assess whether non-consent introduces systematic bias into our estimates of reporting behavior:

$$\Pr(\text{Consent}_i = 1) = \Lambda(\alpha + \beta_1 \text{Age}_i + \beta_2 \text{Female}_i + \beta_3 \text{German}_i + \beta_4 \text{HighEd}_i + \gamma_s + \delta_j + \varepsilon_i) \tag{12}$$

where $\Lambda(\cdot)$ is the logistic cumulative distribution function. The model includes – based on information provided in the SOEP – individual characteristics (age, gender, German citizenship, high education), state-fixed effects (γ_s), sector-fixed effects, and industry-fixed effects (δ_j). The model is estimated using data from the consent year for all individuals who were asked to provide consent.

Based on the estimated model, we predict each individuals' probability of consenting and construct inverse probability weights following the standard approach:

$$w_i = \frac{1}{\hat{p}_i} \tag{13}$$

where $\hat{p}_i$ is the predicted probability of consent for individual i. These weights adjust for the differential probability of selection into our linked sample based on observable characteristics.

Figure C.1 provides a visual comparison of the SOEP and IEB wage distributions using the original and the selection-corrected weights. The weighted and unweighted distributions overlap almost perfectly, confirming that selection into consent does not substantially alter the fundamental characteristics of our wage data. Also, the mean reporting bias remains virtually unchanged when applying selection weights, suggesting that observable differences between consenters and non-consenters do not materially distort our wage distributions. The patterns of reporting bias we document are therefore unlikely to be artefacts of selective consent.

Table C.1: Differences Between Consenters and Non-Consenters, Cross-Section Year of Consent

	Consent	No Consent	Difference Consent / No Consent
	Mean (SD) [N]	Mean (SD) [N]	t-Test (SE)
	(1)	(2)	(3)
Individual Characteristics			
Age	43.66	45.84	2.18***
	(17.55)	(18.47)	(0.18)
	[33,729]	[13,028]	
Female	0.49	0.51	0.02***
	(0.50)	(0.50)	(0.01)
	[33,730]	[13,028]	
German citizenship	0.62	0.67	0.05***
	(0.48)	(0.47)	(0.00)
	[33,723]	[13,028]	
Married	0.55	0.55	0.00
	(0.50)	(0.50)	(0.01)
	[33,730]	[13,028]	
Ever Partnered	0.66	0.64	-0.02***
	(0.48)	(0.48)	(0.00)
	[33,730]	[13,028]	
Years of education	11.48	11.67	0.19***
	(2.97)	(2.90)	(0.03)
	[31,182]	[11,868]	
Personality Traits			
Openness (std)	0.15	0.02	-0.13***
	(0.92)	(0.90)	(0.01)
	[21,586]	[7,865]	
Conscientiousness (std)	0.02	-0.02	-0.04**
	(0.91)	(0.90)	(0.01)
	[21,716]	[7,904]	
Extraversion (std)	0.10	-0.01	-0.11***
	(0.93)	(0.92)	(0.01)
	[21,686]	[7,905]	
Agreeableness (std)	0.11	0.02	-0.09***
	(0.92)	(0.88)	(0.01)
	[21,748]	[7,916]	
Neuroticism (std)	-0.08	-0.00	0.08***
	(0.92)	(0.90)	(0.01)
	[21,755]	[7,921]	
Job and Firm Characteristics			
Monthly Wage SOEP	2,864.62	2,795.67	-68.95
	(2,822.90)	(3,131.67)	(43.60)
	[16,278]	[6,136]	
White Collar Status (0/1)	0.71	0.69	-0.02*
	(0.45)	(0.46)	(0.01)
	[14,450]	[5,785]	
Union Membership (0/1)	0.11	0.08	-0.02***
	(0.31)	(0.28)	(0.00)
	[20,351]	[8,226]	
Firm Size <=10 (0/1)	0.16	0.18	0.02**
	(0.37)	(0.38)	(0.01)
	[13,919]	[5,141]	
Firm Size < 100 (0/1)	0.26	0.26	-0.00
	(0.44)	(0.44)	(0.01)
	[13,919]	[5,141]	
Firm Size < 2000 (0/1)	0.29	0.29	0.00
	(0.45)	(0.45)	(0.01)
	[13,919]	[5,141]	
Firm Size >=2000 (0/1)	0.29	0.27	-0.02*
	(0.45)	(0.45)	(0.01)
	[13,919]	[5,141]	

Note: This table shows differences between consenters and non-consenters in the SOEP across various individual, household, job, and firm characteristics in the year of consent. Column (3) shows the difference in means between Columns (1) and (2). Standard errors are reported in parentheses, the number of observations in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

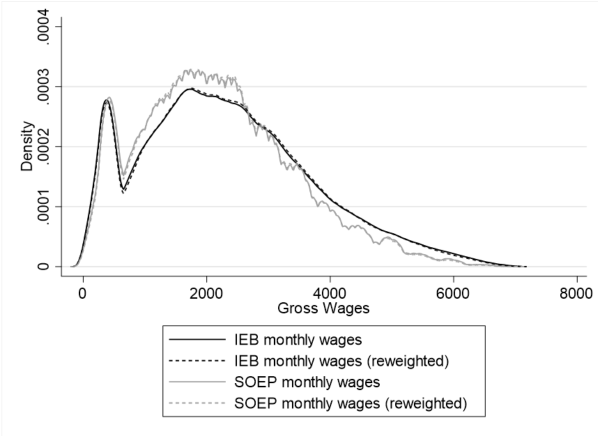


Figure C.1: Distribution of SOEP and IEB Wages Weighted by the Consenting Decision

Note: This figure plots the distributions of SOEP and IEB wages, both unweighted and weighted using a selection model of the consent decision. To obtain the weights, we estimate a logistic regression predicting the probability of consent based on age, gender, German citizenship, education, state, sector, and industry in the year of consent. The weights are then calculated as 1 divided by the predicted probability of consent. The average reporting bias with selection weights is -€174 compared with -€170 without weights.

D Robustness II: Correlates and Consequences including Bonus Payments

Table D.1: Reporting Bias Along the Wage Distribution (Below-Limit Sample, incl. Bonus Payments)

Decile	Mean Wage[(a)]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	347	119	34	147	42	7,875
2	836	43	5	134	16	7,811
3	1,316	4	0	150	11	7,839
4	1,690	-15	-1	174	10	7,833
5	2,037	-50	-2	226	11	7,822
6	2,389	-68	-3	239	10	7,850
7	2,765	-97	-4	266	10	7,829
8	3,177	-143	-5	308	10	7,837
9	3,735	-200	-5	365	10	7,830
10	4,714	-341	-7	459	10	7,819
All	2,299	-75	-3	247	11	78,345

Note: This table reports summary statistics for reporting bias for the below-limit sample, by decile of the IEB wage distribution. Both SOEP and IEB wages contain additional payments (e.g. bonus, vacation pay, and Christmas pay). Column (1) reports the mean IEB wage in each decile (€). Columns (2) and (3) report the mean reporting bias (SOEP minus IEB wage), in € and percent. Columns (4) and (5) report the mean absolute reporting bias, in € and percent. Column (6) reports the number of observations in each decile.

Table D.2: Reporting Bias in First-Differences (Below-Limit Sample, incl. Bonus Payments)

Decile	Mean Wage[(a)]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	425	51	12	140	33	5,901
2	1,037	3	0	151	15	5,882
3	1,485	-6	0	178	12	5,886
4	1,846	-4	0	200	11	5,889
5	2,182	-10	0	227	10	5,877
6	2,522	-17	-1	247	10	5,887
7	2,878	-6	0	274	10	5,885
8	3,265	-19	-1	303	9	5,886
9	3,787	-30	-1	332	9	5,881
10	4,696	-60	-1	375	8	5,867
All	2,411	-10	0	243	10	58,841

Note: This table reports summary statistics for reporting bias in first differences for the below-limit sample, by decile of the IEB wage-change distribution. Both SOEP and IEB wage changes contain additional payments (e.g. bonus, vacation pay, and Christmas pay). Column (1) reports the mean IEB wage change in each decile (€). Columns (2) and (3) report the mean reporting bias in first differences (SOEP minus IEB wage changes), in € and percent. Columns (4) and (5) report the mean absolute reporting bias in first differences, in € and percent. Column (6) reports the number of observations in each decile.

Table D.3: Imputation Bias Along the Wage Distribution (Above-Limit Sample, incl. Bonus Payments)

Quintile	Mean Wage[a]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	5,527	992	17	1,509	27	1,474
2	6,397	705	11	1,719	27	1,456
3	7,477	-353	-4	1,920	25	1,455
4	9,244	-1,824	-19	2,966	32	1,457
5	15,092	-7,150	-43	7,866	48	1,441
All	8,727	-1,508	-17	3,182	36	7,283

Note: This table reports summary statistics for reporting bias among observations above the assessment limit, by IEB wage quintile. Both SOEP and IEB wages contain additional payments (e.g. bonus, vacation pay, and Christmas pay). Column (1) reports the mean IEB wage in each quintile (€). Columns (2) and (3) report the mean reporting bias (SOEP minus imputed IEB wage), in € and percent. Columns (4) and (5) report the mean absolute reporting bias, in € and percent. Column (6) reports the number of observations in each quintile.

Table D.4: Imputation Bias in First-Differences (Above-Limit Sample, incl. Bonus Payments)

Quintile	Mean Wage[a]	Mean Bias		Mean Abs. Bias		N
	(1)	(2)	(3)	(4)	(5)	(6)
	in €	in €	in %	in €	in %	
1	5,558	1,812	33	2,325	42	1,264
2	6,427	1,328	21	2,228	35	1,251
3	7,519	807	11	2,625	35	1,248
4	9,267	-380	-4	3,168	34	1,252
5	15,063	-4,848	-32	6,758	45	1,237
All	8,747	-242	-3	3,411	39	6,252

Note: This table reports summary statistics for imputation bias in first differences for the above-limit sample, by quintile of the IEB wage-change distribution. Both SOEP and IEB wage changes contain additional payments (e.g. bonus, vacation pay, and Christmas pay). Column (1) reports the mean IEB wage change in each quintile (€). Columns (2) and (3) report the mean imputation bias in first differences (SOEP minus imputed IEB wage), in € and percent. Columns (4) and (5) report the mean absolute imputation bias in first differences, in € and percent. Column (6) reports the number of observations in each quintile.

Table D.5: Shorrocks-Shapley Decomposition of the Reporting Bias (incl. Bonus Payments)

	Mean Bias (1)	Mean Abs. Bias (2)
Sociodemographic characteristics	0.06	0.13
Personality traits	0.01	0.04
Household characteristics	0.03	0.01
Job characteristics	0.14	0.20
Firm size	0.11	0.07
Median-Wage Tercile	0.36	0.25
Firm sector	0.14	0.06
Workforce composition	0.10	0.10
Survey characteristics	0.01	0.03
Year FE	0.04	0.10
N	60,340	60,340

Note: This table reports Shorrocks-Shapley decomposition values for mean reporting bias and mean absolute reporting bias, both measured in €. Values show the share of explained variation attributed to each group. The sample only contains observations for which both IEB wages including the bonus and SOEP wages including the bonus are below the assessment limit. Therefore, the number of observations is smaller than in Table 6.

Table D.6: Consequences – Returns to Education (incl. Bonus Payments)

Sample[(a)]		Wage measure[(b, c)]	Specification (1) Raw	(2) Basic	(3) Extended
Full sample	(I)	SOEP reported	0.105*** (0.003)	0.085*** (0.002)	0.082*** (0.002)
	(II)	IEB censored	0.096*** (0.003)	0.076*** (0.002)	0.071*** (0.002)
		Difference (II) - (I)	-0.008*** (0.001)	-0.009*** (0.001)	-0.012*** (0.001)
		Difference, % of SOEP	-7.9	-10.5	-14.4
	(III)	IEB imputed	0.113*** (0.003)	0.093*** (0.002)	0.088*** (0.002)
		Difference (III) - (I)	0.009*** (0.001)	0.008*** (0.001)	0.006*** (0.001)
		Difference, % of SOEP	8.1	9.1	7.2
	(IV)	Hybrid	0.107*** (0.003)	0.087*** (0.002)	0.082*** (0.002)
		Difference (IV) - (I)	0.002*** (0.001)	0.001* (0.001)	-0.001 (0.001)
		Difference, % of SOEP	2.1	1.7	-0.9
Below-Limit	(I)	SOEP reported	0.067*** (0.003)	0.062*** (0.002)	0.061*** (0.002)
	(II)	IEB recorded	0.071*** (0.003)	0.066*** (0.002)	0.062*** (0.002)
		Difference (II) - (I)	0.004*** (0.001)	0.004*** (0.001)	0.002* (0.001)
		Difference, % of SOEP	6.2	6.2	3.0
Above-Limit	(I)	SOEP reported	0.019*** (0.004)	0.021*** (0.004)	0.033*** (0.004)
	(II)	IEB imputed	0.021*** (0.003)	0.026*** (0.003)	0.030*** (0.003)
		Difference (II) - (I)	0.001 (0.004)	0.005 (0.004)	-0.003 (0.004)
		Difference, % of SOEP	6.0	21.8	-8.0

Note: This table reports coefficient estimates of β_{YED} from regression Equation (9). All estimates are obtained from separate regressions; specifications are described in Section 6. Both SOEP and IEB wages contain additional payments (e.g. bonus, vacation pay, and Christmas pay).

[(a)] Samples as defined as in Section 2.5. The full sample contains 80,557 observations, the below-limit sample 72,601 observations and the above-limit sample 7,123 observations.

[(b)] Wage measures as defined in Section 2.5 and Table 1.

[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $YED \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).

Standard errors clustered at the individual level are reported in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Consequences – Gender Wage Gap (incl. Bonus Payments)

Sample[(a)]		Wage measure[(b, c)]	Specification (1) Raw	(2) Basic	(3) Extended
Full sample	(I)	SOEP reported	-0.636*** (0.014)	-0.215*** (0.010)	-0.154*** (0.009)
	(II)	IEB censored	-0.601*** (0.014)	-0.171*** (0.010)	-0.118*** (0.009)
		Difference (II) - (I)	0.035*** (0.004)	0.044*** (0.004)	0.037*** (0.005)
		Difference, % of SOEP	5.6	20.3	23.7
	(III)	IEB imputed	-0.657*** (0.015)	-0.214*** (0.011)	-0.148*** (0.010)
		Difference (III) - (I)	-0.021*** (0.004)	0.001 (0.004)	0.006 (0.004)
		Difference, % of SOEP	-3.2	0.5	4.2
	(IV)	Hybrid	-0.636*** (0.015)	-0.199*** (0.010)	-0.138*** (0.010)
		Difference (IV) - (I)	0.001 (0.003)	0.016*** (0.003)	0.017*** (0.003)
		Difference, % of SOEP	0.1	7.4	10.7
Below-Limit	(I)	SOEP reported	-0.506*** (0.013)	-0.145*** (0.009)	-0.113*** (0.008)
	(II)	IEB recorded	-0.511*** (0.014)	-0.134*** (0.010)	-0.100*** (0.009)
		Difference (II) - (I)	-0.004 (0.003)	0.011** (0.003)	0.013*** (0.004)
		Difference, % of SOEP	-0.9	7.3	11.5
Above-Limit	(I)	SOEP reported	-0.116*** (0.029)	-0.110*** (0.028)	-0.040 (0.029)
	(II)	IEB imputed	-0.095*** (0.019)	-0.080*** (0.018)	-0.053*** (0.016)
		Difference (II) - (I)	0.022 (0.030)	0.031 (0.030)	-0.013 (0.031)
		Difference, % of SOEP	18.7	28.0	-32.6

Note: This table reports coefficient estimates of β_{FEM} from regression Equation (10). All estimates are obtained from separate regressions; specifications are described in Section 6. Both SOEP and IEB wages contain additional payments (e.g. bonus, vacation pay, and Christmas pay).
[(a)] Samples as defined as in Section 2.5. The full sample contains 80,557 observations, the below-limit sample 72,601 observations and the above-limit sample 7,123 observations.
[(b)] Wage measures as defined in Section 2.5 and Table 1.
[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Female \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Consequences – Life Satisfaction (incl. Bonus Payments)

Sample[(a)]		Wage measure[(b, c)]	Specification			
			(1) Raw	(2) Basic	(3) Extended	(4) Individual FE
Full sample	(I)	SOEP reported	0.037***	0.042***	0.029***	0.012**
			(0.003)	(0.005)	(0.004)	(0.004)
	(II)	IEB censored	0.053***	0.075***	0.050***	0.028***
			(0.004)	(0.006)	(0.006)	(0.008)
		Difference (II) - (I)	0.016***	0.033***	0.021***	0.016**
			(0.003)	(0.006)	(0.006)	(0.006)
		Difference, % of SOEP	42.8	77.5	71.6	134.3
	(III)	IEB imputed	0.028***	0.029***	0.017***	0.002
			(0.002)	(0.003)	(0.002)	(0.002)
		Difference (III) - (I)	-0.009**	-0.013**	-0.012**	-0.010*
			(0.003)	(0.005)	(0.004)	(0.004)
		Difference, % of SOEP	-24.0	-31.8	-40.9	-80.7
	(IV)	Hybrid	0.036***	0.040***	0.026***	0.009*
			(0.003)	(0.005)	(0.004)	(0.004)
		Difference (IV) - (I)	-0.001**	-0.002**	-0.003**	-0.003
			(0.000)	(0.001)	(0.001)	(0.002)
		Difference, % of SOEP	-3.5	-4.8	-9.3	-25.5
Below-Limit	(I)	SOEP reported	0.055***	0.087***	0.063***	0.042***
			(0.005)	(0.007)	(0.007)	(0.008)
	(II)	IEB recorded	0.046***	0.068***	0.044***	0.030***
			(0.005)	(0.007)	(0.007)	(0.008)
		Difference (II) - (I)	-0.009***	-0.019***	-0.019***	-0.011*
			(0.001)	(0.002)	(0.003)	(0.005)
		Difference, % of SOEP	-16.0	-21.9	-30.2	-27.2
Above-Limit	(I)	SOEP reported	0.014***	0.017***	0.014***	-0.004
			(0.004)	(0.004)	(0.004)	(0.005)
	(II)	IEB imputed	0.004	0.003	0.001	-0.002
			(0.003)	(0.002)	(0.002)	(0.002)
		Difference (II) - (I)	-0.011*	-0.013**	-0.013**	0.002
			(0.004)	(0.005)	(0.005)	(0.005)
		Difference, % of SOEP	-75.1	-79.6	-91.4	50.1

Note: This table reports coefficient estimates of β_W from regression Equation (11). All estimates are obtained from separate regressions; specifications are described in Section 6, wages are scaled by €1000 and contain additional payments (e.g. bonus, vacation pay, and Christmas pay).
[(a)] Samples as defined in Section 2.5. The full sample contains 80,051 (78,026 in (4)) observations, the below-limit sample 72,141 (70,108 in (4)) observations and the above-limit sample 7,079 (6,843 in (4)) observations.
[(b)] Wage measures as defined in Section 2.5 and Table 1.
[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Wage \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$ - $\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).
Standard errors clustered at the individual level are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: Consequences – Income Satisfaction (incl. Bonus Payments)

Sample[(a)]		Wage measure[(b, c)]	(1) Raw	(2) Basic	(3) Extended	(4) Individual FE
			Specification			
Full sample	(I)	SOEP reported	0.117*** (0.010)	0.105*** (0.014)	0.093*** (0.013)	0.092*** (0.011)
	(II)	IEB censored	0.190*** (0.003)	0.222*** (0.005)	0.212*** (0.006)	0.209*** (0.008)
		Difference (II) - (I)	0.073*** (0.010)	0.117*** (0.014)	0.118*** (0.014)	0.117*** (0.012)
		Difference, % of SOEP	62.1	111.7	126.5	127.0
	(III)	IEB imputed	0.094*** (0.003)	0.078*** (0.004)	0.066*** (0.003)	0.021*** (0.002)
		Difference (III) - (I)	-0.023* (0.010)	-0.027* (0.014)	-0.027* (0.014)	-0.071*** (0.011)
		Difference, % of SOEP	-19.9	-26.0	-29.0	-77.0
	(IV)	Hybrid	0.117*** (0.010)	0.104*** (0.014)	0.091*** (0.013)	0.090*** (0.011)
		Difference (IV) - (I)	-0.001 (0.001)	-0.001 (0.001)	-0.003** (0.001)	-0.002 (0.004)
		Difference, % of SOEP	-0.5	-1.2	-2.7	-2.1
Below-Limit	(I)	SOEP reported	0.204*** (0.004)	0.257*** (0.006)	0.246*** (0.007)	0.237*** (0.008)
	(II)	IEB recorded	0.186*** (0.004)	0.223*** (0.006)	0.212*** (0.007)	0.224*** (0.009)
		Difference (II) - (I)	-0.018*** (0.001)	-0.034*** (0.002)	-0.034*** (0.003)	-0.014* (0.006)
		Difference, % of SOEP	-8.6	-13.1	-13.9	-5.7
Above-Limit	(I)	SOEP reported	0.023*** (0.006)	0.025*** (0.006)	0.023*** (0.006)	0.023*** (0.007)
	(II)	IEB imputed	0.004* (0.002)	0.005** (0.002)	0.004* (0.002)	0.001 (0.001)
		Difference (II) - (I)	-0.019** (0.006)	-0.020** (0.006)	-0.019** (0.006)	-0.022*** (0.007)
		Difference, % of SOEP	-80.8	-81.7	-83.2	-96.6

Note: This table reports coefficient estimates of β_W from regression Equation (11). All estimates are obtained from separate regressions; specifications are described in Section 6, wages are scaled by €1000 and contain additional payments (e.g. bonus, vacation pay, and Christmas pay).

[(a)] Samples as defined as in Section 2.5. The full sample contains 65,808 (63,857 in (4)) observations, the below-limit sample 59,455 (57,487 in (4)) observations and the above-limit sample 5,703 (5,493 in (4)) observations.

[(b)] Wage measures as defined in Section 2.5 and Table 1.

[(c)] The reported differences are based on paired tests. For these tests, the dataset is duplicated – once using SOEP wages and once using IEB wages as the dependent variable – and all covariates are interacted with a binary data-source indicator (D_{IEB}). The coefficient on $Wage \times D_{IEB}$ directly identifies $\hat{\beta}_{IEB}$-$\hat{\beta}_{SOEP}$. In addition, each difference is reported as a percentage of the SOEP estimate in row (I).

Standard errors clustered at the individual level are reported in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E Literature Review Bonus Payments in SOEPpapers

Table E.1: Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1157	2022	Christos Koulovatianos, Carsten Schröder	Income-Dependent Equivalence Scales and Choice Theory: Implications for Poverty Measurement	Dep. Var.	No
1160	2022	Christopher Prömel	Belonging or Estrangement – The European Refugee Crisis and Its Effects on Immigrant Identity	Indep. Var.	No
1162	2022	Niklas Gohl, Peter Haan, Claus Michelsen, Felix Weinhardt	House Price Expectations	Indep. Var.	No
1163	2022	Joachim Merz	Are Retirees More Satisfied? Anticipation and Adaptation Effects: A Causal Panel Analysis of German Statutory Insured and Civil Service Pensioners	Indep. Var.	No
1164	2022	Sebastian Will, Timon Renz	In Debt but Still Happy? – Examining the Relationship Between Homeownership and Life Satisfaction	Indep. Var.	No
1165	2022	Antonio Ciccone, Jan Nimczik	The Long-Run Effects of Immigration: Evidence across a Barrier to Refugee Settlement	Dep. Var.	No
1166	2022	Pia Schilling, Steven Stillman	The Impact of Natives' Attitudes towards Immigrants on Their Integration in the Host Country	Indep. Var.	No
1168	2022	Andrew E. Clark, Luis Diaz-Serrano	Do Individuals Adapt to All Types of Housing Transitions?	Indep. Var.	No
1169	2022	Susanne Elsas, Annika Rinklake	Wohnkosten und materielles Wohlergehen von Familien – Analyse der Wohnkostensituation und damit zusammenhängender Wohlfahrtsvorteile	Indep. Var.	No
1170	2022	Adam Ayaita	Does Money Change Who You Are? Quasi-Experimental Evidence on the Effects of Wage Increases on Personality	Indep. Var.	No
1173	2022	Thilo N. H. Albers, Charlotte Bartels, Moritz Schularick	Wealth and Its Distribution in Germany, 1895-2018	Indep. Var.	No

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Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025 – Continued

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1174	2022	Rania Gihleb, Osea Giuntella, Luca Stella	Exposure to Past Immigration Waves and Attitudes toward Newcomers	Indep. Var.	No
1175	2022	Kim Leonie Kellermann	Trust We Lost: The Impact of the Treuhand Experience on Political Alienation in East Germany	Indep. Var.	No
1177	2022	Thilo N. H. Albers, Felix Kersting, Fabian Kosse	Income Misperception and Populism	Indep. Var.	No
1178	2022	Anthony Lepinteur, Andrew E. Clark, Ada Ferrer-i-Carbonell, Alan Piper, Carsten Schröder, Conchita D'Ambrosio	Gender, Loneliness and Happiness during COVID-19	Indep. Var.	No
1179	2022	Kai Ingwersen, Stephan L. Thomsen	Minimum Wage in Germany: Countering the Wage and Employment Gap between Migrants and Natives?	Dep. Var.	No
1180	2023	Marina Bonaccolto-Töpfer, Claus Schnabel	Is There a Union Wage Premium in Germany and Which Workers Benefit Most?	Dep. Var.	No
1181	2023	Hend Sallam	Holding the Door Slightly Open: Germany's Migrants' Return Intentions and Realizations	Indep. Var.	No
1182	2023	Matthias Diermeier, Madeleine L. Fischer, Judith Niehues	Punching up or Punching down? How Stereotyping the Rich and the Poor Impacts Redistributive Preferences in Germany	Indep. Var.	No
1183	2023	Emanuele Albarosa, Benjamin Elsner	Forced Migration and Social Cohesion: Evidence from the 2015/16 Mass Inflow in Germany	Indep. Var.	No
1184	2023	Ansgar Hudde, Marita Jacob	There's More in the Data! Using Month-Specific Information to Estimate Changes Before and After Major Life Events	Indep. Var.	No
1185	2023	Christian Merkl, Timo Sauerbier	Public Employment Agency Reform, Matching Efficiency, and German Unemployment	Indep. Var.	No

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Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025 – Continued

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1186	2023	Steffen Künn, Juan Palacios	Health Implications of Building Retrofits: Evidence from a Population-Wide Weatherization Program	Indep. Var.	No
1187	2023	Hamed Markazi Moghadam, Patrick A. Puhani, Joanna Tyrowicz	Pension Reforms and Couples' Labour Supply Decisions	Indep. Var.	No
1188	2023	Michele Ubaldi, Matteo Picchio	Intergenerational Scars: The Impact of Parental Unemployment on Individual Health Later in Life	Indep. Var.	No
1189	2023	Nabanita Datta Gupta, Jonas Jessen, C. Katharina Spiess	Maternal Life Satisfaction and Child Development from Toddlerhood to Adolescence	Indep. Var.	No
1190	2023	Christian Bayer, Moritz Kuhn	Job Levels and Wages	Dep. Var.	Yes
1191	2023	Ingo S. Seifert, Julia M. Rohrer, Stefan C. Schmukle	Using Within-Person Change in Three Large Panel Studies to Estimate Personality Age Trajectories	Indep. Var.	No
1192	2023	Alexander Bertermann, Daniel A. Kamhöfer, Hannah Schildberg-Hörisch	More Education Does Make You Happier – Unless You Are Unemployed	Indep. Var.	No
1193	2023	Daniel Graeber, Viola Hilbert, Johannes König	Inequality of Opportunity in Wealth: Levels, Trends, and Drivers	Indep. Var.	No
1194	2023	Osea Giuntella, Johannes König, Luca Stella	Artificial Intelligence and Workers' Well-being	Indep. Var.	No
1195	2023	Daniel Graeber	Intergenerational Health Mobility in Germany	Indep. Var.	No
1196	2023	Michael Fritsch, Alina Sorgner, Michael Wyrwich	Are Senior Entrepreneurs Happier than Who?: The Role of Income and Health	Indep. Var.	No

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Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025 – Continued

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1197	2023	Christian Krekel, Johannes Rode, Alexander Roth	Do Wind Turbines Have Adverse Health Impacts?	Indep. Var.	No
1198	2023	Ann-Marie Sommerfeld	The Effect of Schooling on Parental Integration: Evidence from Germany	Indep. Var.	No
1199	2023	Laszlo Goerke, Markus Pannenberg	Minimum Wage Non-compliance: The Role of Co-determination	Dep. Var.	Yes
1200	2023	Mattis Beckmannshagen, Rick Glaubitz	Is There a Desired Added Worker Effect?: Evidence from Involuntary Job Losses	Indep. Var.	No
1201	2023	Jennifer Feichtmayer, Regina T. Riphahn	Intergenerational Transmission of Welfare Benefit Receipt: Evidence from Germany	Indep. Var.	No
1202	2023	Milena Nikolova, Olga Popova	Echoes of the Past: The Enduring Impact of Communism on Contemporary Freedom of Speech Values	Indep. Var.	No
1203	2023	Adriana Cardozo Silva, Yuliya Kosyakova, Aslıhan Yurdakul	Gendered Implications of Restricted Residence Obligation Policies on Refugees' Employment in Germany	Indep. Var.	No
1205	2024	Verena Löffler	The Cost of Fair Pay: How Child Care Work Wages Affect Formal Child Care Hours, Informal Child Care Hours, and Employment Hours	Indep. Var.	No
1206	2024	Deborah A. Cobb-Clark, Sarah C. Dahmann, Daniel A. Kamhöfer, Hannah Schildberg-Hörisch	Schooling and Self-Control	Indep. Var.	No
1207	2024	Ronald Bachmann, Myrielle Gonschor	Technological Progress, Occupational Structure, and Gender Gaps in the German Labour Market	Dep. Var.	No
1208	2024	Marina Krauß, Niklas Rot	Early Childcare Expansion and Maternal Health	Indep. Var.	No

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Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025 – Continued

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1210	2024	Jascha Dräger, Nora Müller, Klaus Pforr	The Keys to the House - How Wealth Transfers Stratify Homeownership Opportunities	Indep. Var.	No
1211	2024	Martina Metzger, Hans Walter Steinhauer, Jennifer Pédussel Wu	Mitigating Adverse Social and Health Impacts of COVID-19 with Applied Arts	Indep. Var.	No
1212	2024	Charlotte Bartels, Eva Sierminska, Carsten Schröder	Wealth Creators or Inheritors? Unpacking the Gender Wealth Gap from Bottom to Top and Young to Old	Indep. Var.	No
1214	2024	Michael D. Krämer, Wiebke Bleidorn	The Well-Being Costs of Informal Caregiving	Indep. Var.	No
1215	2024	Marc Kaufmann, Joël Machado, Bertrand Verheyden	Why Do Migrants Stay Unexpectedly? Misperceptions and Implications for Integration	Indep. Var.	No
1216	2024	Henning Hermes, Marina Krauß, Philipp Lergetporer, Frauke Peter, Simon Wiederhold	The Causal Impact of Gender Norms on Mothers' Employment Attitudes and Expectations	Indep. Var.	No
1217	2025	Eliana Coschignano, Robin Jessen	The Diverging Trends of Male and Female Bottom Earnings in Germany	Dep. Var.	Yes
1219	2025	Ayhan Adams, Katrin Golsch	Child Sick Care-Related Absence from Work and the Consequences on Parents' Income	Dep. Var.	No
1220	2025	Christina Siegert	The Interplay of Poverty and Employment Trajectories in Couples Around the Transition to Parenthood in Germany	Dep. Var.	No
1221	2025	Katharina Adler, Fabian Kosse, Markus Nagler, Johannes Rincke	Earnings Expectations of "First-in Family" University Students and Their Role for Major Choice	Dep. Var.	No
1222	2025	Fabian Kosse, Tim Leffler, Arna Woemmel	Digital Skills: Social Disparities and the Impact of Early Mentoring	Indep. Var.	No

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Wage Variables and Bonus Payments in SOEP-Based Studies, 2022–2025 – Continued

SOEP DP no.	Year	Authors	Title	Role of Wage var	Bonus payment mentioned?
1223	2025	Adriana Rocío Cardozo Silva, Christopher Prömel	Feeling Equal before the Law? The Impact of Access to Citizenship and Legal Status on Perceived Discrimination	Indep. Var.	No
1224	2025	Sachintha Fernando, Katharina Kolb, Christoph Wunder	Rising Waters, Falling Well-Being: The Effects of the 2013 East German Flood on Subjective Well-Being	Indep. Var.	No
1226	2025	Alexander Eriksson Byström, María Sól Antonsdóttir	Thanks, but No Thanks: A Microsimulation of BAföG Eligibility and Non-Take-Up	Indep. Var.	No
1227	2025	Stefan Bach, Charlotte Bartels, Theresa Neef	The Distribution of National Income in Germany, 1992-2019	Dep. Var.	Yes
1228	2025	Helena Manger	Benefits and Employees' Work Effort: An Empirical Analysis of Non-monetary Incentives	Indep. Var.	Yes
1229	2025	Alan Piper	Narcissism and Wellbeing in Midlife and Beyond	Indep. Var.	No
1230	2025	Daniel Graeber, Lorenz Meister, Carsten Schröder, Sabine Zinn	Random Forests for Labor Market Analysis: Balancing Precision and Interpretability	Dep. Var.	No
1231	2025	Alyna Paul	Refugees (Un)Welcome – Regional Demographic Changes and Individual Attitudes Towards Refugees	Indep. Var.	No
1233	2025	Stefan Schneck	The Origins of Entrepreneurship: How Parental Role Models and Socialization Shape Later Entrepreneurial Intentions	Indep. Var.	No
1234	2025	Melanie Borah, Susanne Elsas	Endogeneity of Household Size and Income in the Estimation of Equivalence Scales from Satisfaction Data	Indep. Var.	No
1235	2025	Kausik Chaudhuri, Alan Piper	Order Out of Chaos: A Specification Curve Analysis of Age and Wellbeing	Indep. Var.	No

Note: This table summarizes the SOEPpapers discussion paper series for 2022–2025. "No" in the bonus-payment column includes cases in which bonus payments, special payments, Christmas or vacation bonuses, 13th/14th salaries, or profit-sharing payments were not mentioned or their treatment was not stated. Overall, wage/income/earnings appears as the dependent variable in 13 papers (19.7%) and as an independent variable or control in 53 papers (80.3%). Five papers (7.6%) mention that bonus or special payments are included in the wage/income/earnings measure; 61 papers (92.4%) do not mention such inclusion or do not state the treatment of these payments.