

Gendered Occupational Titles and Perceived Pay

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Abstract

We study whether gender enters beliefs about the economic returns associated with occupations. Adults and adolescents in Germany were randomly assigned to estimate the earnings of five occupations presented with either masculine or feminine titles. Masculine titles raise perceived pay by 11.3 percent among adults and 7.9 percent among adolescents. Among adults the effect is similar for male and female respondents; among adolescents it is approximately zero for boys and 16.1 percent for girls. Title effects also vary sharply across occupations. We benchmark this heterogeneity against occupation-specific male–female pay differences from administrative earnings data and a DAX CEO compensation study. Actual gaps are positively aligned with adult title effects but cannot account for their occupational pattern: occupations with nearly identical observed gender gaps generate radically different experimental responses. The results imply that gender is part of the prior people attach to the economic value of an occupation rather than merely a reflection of known wage differences. They also have a measurement implication: the linguistic form used to elicit labor-market beliefs can materially change the beliefs researchers observe.

JEL codes: D83, J16, J31, Z13.

Keywords: gender, labor-market beliefs, perceived returns, wages, occupational titles, language.

1 Introduction

Beliefs about the economic returns to occupations are central to labour-market decisions. Expected earnings help shape educational and occupational choices, wage expectations and negotiations, and beliefs about inequality and policy [Kiessling et al., 2024, Settele, 2022]. These beliefs need not be neutral representations of an occupation. Social categories can enter the prior itself: the same job may be associated with a different level of economic remuneration depending on whom people have in mind when they evaluate it. We ask whether gender is one such component of occupational earnings beliefs.

We identify this link using a minimal linguistic intervention. In a preregistered module embedded in two large German survey samples, respondents were randomly assigned to see five occupations in either their masculine or feminine grammatical form and were asked to estimate typical monthly gross earnings. The occupation, the quantity being elicited, and the remainder of the question were held fixed. Masculine titles increase perceived pay by 11.3 per cent among adults and 7.9 per cent among adolescents. The result is robust against including within-item 1st/99th-percentile winsorisation and appears independently in both samples. The intervention therefore reveals a gendered component of perceived occupational returns: changing only the gender cue changes the economic value respondents associate with the same occupation.

The average effect masks substantial heterogeneity. First, treatment effects vary strongly across occupations. Among adults, presenting the head of a large company with a masculine rather than feminine title raises perceived pay by 33 per cent, and the corresponding effect for physicians is 16 per cent. The effects are only 3 per cent for supermarket salespersons, 6 per cent for unskilled factory workers, and 1 per cent for members of the Bundestag. Second, the adolescent response differs sharply by respondent gender. The pooled effect is essentially zero among boys but 16.1 per cent among girls, while adult men and women respond similarly. This pattern indicates that gendered pay priors are not uniform across either occupations or people.

A natural explanation is factual knowledge. Respondents may infer a male or female worker from the grammatical title and then reproduce known within-occupation gender pay gaps. We therefore combine the randomised experiment with external benchmarks for male and female earnings.¹ The benchmarking exercise suggests that factual wage differences account for part of the observed pattern, but cannot explain it fully. Among adults, title effects tend to be larger in occupations with larger observed gender pay gaps, and a simple calibration regression confirms a positive relationship. At the same time, the hypothesis that occupation-specific title effects merely reproduce observed pay gaps is strongly rejected. Occupations with very similar realised gender gaps generate markedly different experimental title effects. Respondents therefore do not appear to translate factual earnings differences mechanically into their estimates. Instead, gender inequality seems to be represented selectively across occupations. Among adolescents, even the positive alignment with

¹For physicians, salespersons, and factory workers we use detailed occupational earnings data from the German Federal Statistical Office. For the head of a large company, where broad occupational earnings data poorly match the survey item, we use compensation for DAX chief executives. Parliamentary compensation provides an institutional zero-gap benchmark because statutory compensation does not depend on the officeholder’s gender.

external earnings gaps is not robust.

This distinction is directly relevant for how we understand labour-market beliefs. The experiment establishes that gender changes the earnings distribution people associate with an otherwise identical occupational category, and that this mapping has its own occupation-specific structure. Such priors are of direct relevance wherever expected occupational returns enter decisions, for example career choice, expected wages, reservation wages, or bargaining. The adolescent heterogeneity further suggests that the formation of these priors may differ across groups before extensive labour-market experience accumulates.

Our paper relates to three strands of literature. First, a large psychology and psycholinguistics literature shows that gendered and gender-fair language changes the mental representation of social roles and occupations [Stahlberg et al., 2001, Sczesny et al., 2016, Horvath et al., 2016, Brohmer et al., 2024]. Second, Williams et al. [2010] document a “salary estimation effect” in which men are assumed to earn more than women and connect it to a male-wealth stereotype. We embed the gender cue in the occupational category itself and show that its economic effect varies sharply across occupations and respondent groups. Third, economic research documents persistent gender wage differences [Blau and Kahn, 2017], gender differences in subjective wage expectations [Kiessling et al., 2024], and policy consequences of beliefs about the gender wage gap [Settele, 2022]. Our external-benchmark exercise asks whether gendered earnings beliefs are simply a direct statistical reflection of realised within-occupation gaps and finds that they are not.

The design also yields a distinct measurement contribution. Because grammatical gender is randomly varied while the economic object is held fixed, the experiment shows that the wording used to elicit a belief can become part of the belief that is measured. Economic surveys, information treatments, and vignettes that refer to occupations or workers using gendered language may therefore recover a mixture of beliefs about the underlying economic object and priors activated by its linguistic representation.

2 Experimental Design and Data

The broader survey contains 1,575 adult respondents and 1,629 adolescent respondents. The adult study was fielded as a standardised online survey (CAWI) by horizoom GmbH from May 28 to June 10, 2026. Its target population was the German residential population aged 18–75, and quota sampling was used for gender, age, and education. The reported average interview duration was 15 minutes. The realised adult sample was 49 per cent male and 51 per cent female, with a mean age of 48; 24 per cent had low, 31 per cent medium, and 43 per cent had high education according to the fieldwork report’s grouped categories [SINUS-Institut, 2026]. The preregistration specified a separate adolescent target population of residents of Germany aged 14–17, recruited through a quality-controlled access panel and surveyed online via CAWI, with the sample structured by age and gender. The realised adolescent sample contains 1,629 respondents and was fielded in the same time span, again as a standardised online survey (CAWI) by horizoom GmbH.

The gender-title module used a simple two-group randomisation: respondents were assigned to a masculine-title or feminine-title condition, and the preregistration specified that respondents would be unaware of their assigned treatment. In the full adult sample, 788 respondents were assigned to masculine titles and 787 to feminine titles. In the adolescent sample, the corresponding counts are 813 and 816. The treatment was applied to five occupations: physician, head of a large company, supermarket salesperson, unskilled factory worker, and Bundestag politician.²

The preregistered hypothesis was directional: “Respondents report higher income values if shown male occupation titles than if shown female ones.”³ Our main outcome is the cleaned, non-winsorised positive monetary estimate. Because responses are highly right-skewed and treatment effects are naturally interpreted proportionally, we use the logarithm of the reported amount. The main stacked specification is

$$\log \hat{y}_{ij} = \alpha_j + \tau T_i + X_i' \gamma + \varepsilon_{ij}, \quad (1)$$

where $\hat{y}_{ij}$ is respondent i ’s earnings estimate for occupation j , α_j are occupation fixed effects, and T_i equals one for assignment to masculine occupational titles. X_i contains the controls from the corrected preregistered analysis. Adult models control for age, respondent gender, education, federal state, municipality size, and logged household income. Youth models use age, respondent gender, education, federal state, municipality size, and school attendance. Standard errors are clustered by respondent.

The corrected complete-case stacked sample contains 7,226 occupation-level observations from 1,447 adults and 7,940 observations from 1,591 adolescents. We report two-sided p -values. As a robustness check, we also use a within-item 1st/99th-percentile winsorised version of the outcome. To estimate occupation-specific effects, we interact T_i with occupation indicators. Respondent-gender heterogeneity is estimated by interacting the randomised assignment with respondent gender; all subgroup comparisons are evaluated using formal interaction tests rather than comparisons of subgroup significance.

3 Results

Table 1 reports the pooled five-occupation treatment effect. Masculine occupational titles raise adult income estimates by 0.107 log points, corresponding to 11.3 percent ($p = 0.00056$). In the adolescent sample, the effect is 0.076 log points, or 7.9 percent ($p = 0.012$). Within-item 1st/99th-percentile winsorisation produces very similar effects of 12.5 per cent in adults and 7.8 per cent in adolescents.

²The adult questionnaire introduced the block with the exact wording: “Was schätzen Sie: Wie hoch ist das typische monatliche Bruttoeinkommen in den folgenden Berufen? Was denken Sie, wie viel verdient man ungefähr pro Monat (brutto) als ... ?” The randomized labels were *Arzt/Ärztin*, *Chef/Chefin eines großen Unternehmens*, *Verkäufer/Verkäuferin in einem Supermarkt*, *Fabrikarbeiter/Fabrikarbeiterin ohne Ausbildung*, and *Politiker/Politikerin im Bundestag* [SINUS-Institut, 2026].

³The broader survey contained additional modules on income and wealth perceptions, information sources, and distributive beliefs. These modules are studied separately and are not used in the present paper.

Table 1: Randomised effect of masculine occupational titles on perceived pay

Sample	Outcome	Log effect	SE	Effect (%)	<i>p</i> -value
Adult	Main cleaned	0.107	0.031	11.34	0.00056
Adult	1st/99th percentile winsorised	0.118	0.025	12.48	0.000003
Youth	Main cleaned	0.076	0.030	7.85	0.012
Youth	1st/99th percentile winsorised	0.075	0.026	7.78	0.0041

Notes: The outcome is the log estimated monthly gross income. All models include occupation fixed effects and the sample-specific controls described in the text. Standard errors are clustered by respondent. The percentage effect is $100(\exp(\hat{\tau}) - 1)$. Two-sided *p*-values are reported.

The average effect conceals large differences across occupations. In the adult sample, the masculine-title effect is 0.285 log points for the head of a large company, equivalent to 33.0 per cent, and 0.148 log points for physicians, equivalent to 15.9 percent. The corresponding effects are 5.9 per cent for unskilled factory workers, 3.3 per cent for supermarket salespersons, and 1.4 per cent for Bundestag politicians. A joint test rejects equality across occupations ($\chi^2(4) = 25.29$, $p = 4.4 \times 10^{-5}$). The adolescent pattern is qualitatively similar in that the largest effect again occurs for the head of a large company (23.1 per cent), while the joint heterogeneity test also rejects equality ($\chi^2(4) = 11.11$, $p = 0.025$).

Figure 1 shows the focal respondent-gender heterogeneity. Among adults, the pooled masculine-title effects are 9.6 per cent for male respondents and 13.1 per cent for female respondents. The female-minus-male interaction is 0.032 log points (SE 0.062, $p = 0.607$), providing little evidence that adult men and women respond differently to the wording manipulation. Among adolescents, the contrast is pronounced: the pooled effect is 0.2 per cent for boys and 16.1 per cent for girls. The female-minus-male interaction is 0.147 log points (SE 0.060, $p = 0.0147$). The contrast persists under within-item 1st/99th-percentile winsorisation: the youth interaction is 0.125 ($p = 0.017$).

The adolescent effect is not a company-head artefact. In the fully interacted occupation-by-treatment-by-respondent-gender model, the title effect among girls is positive for all five occupations. Among boys it is near zero or negative for physician, salesperson, factory worker, and Bundestag politician, while remaining positive for the company-head item. The occupation-specific respondent-gender pattern differs strongly among adolescents ($\chi^2(4) = 17.60$, $p = 0.0015$), whereas the corresponding adult test shows essentially no differential pattern ($\chi^2(4) = 1.40$, $p = 0.845$). Appendix Figure A1 displays the full set of estimates. The formal three-way comparison — youth versus adult in the female-minus-male title response — is positive but imprecise (0.115, SE 0.086, $p = 0.181$).

Other observable characteristics provide little systematic heterogeneity. Interactions with age, education, and adult household income do not survive Holm correction within sample (Appendix Table A1). This makes respondent gender the clearest additional dimension of treatment heterogeneity in the present study.

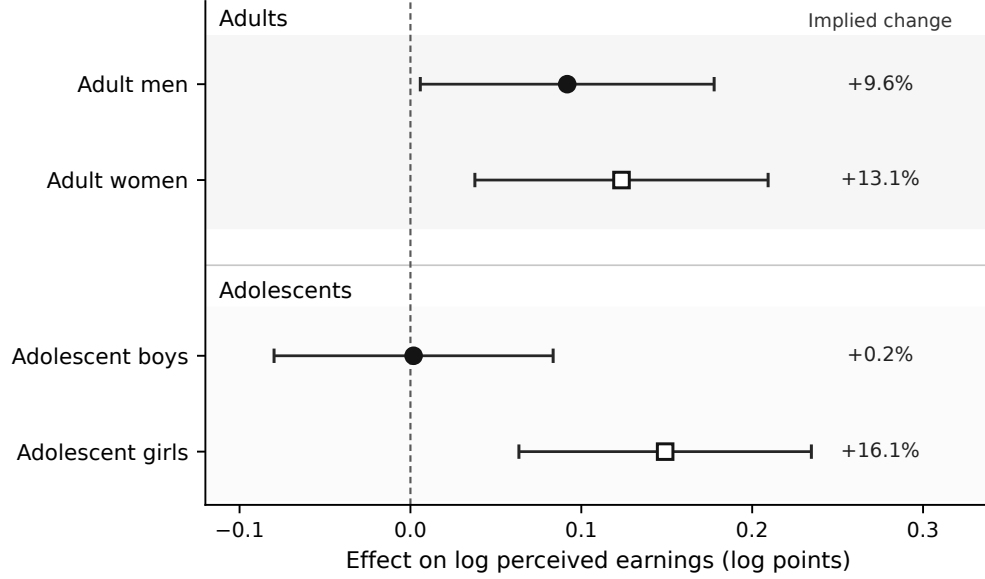


Figure 1: Randomised masculine-title effects by respondent gender and sample

Notes: The figure reports the estimated effect of assignment to masculine rather than feminine occupational titles on log perceived earnings, pooling the five randomised occupations. Models include occupation fixed effects and the standard sample-specific controls; standard errors are clustered by respondent. Bars indicate 95 per cent confidence intervals. The respondent-gender interaction has $p = 0.607$ among adults and $p = 0.0147$ among adolescents. The direct cross-sample difference in respondent-gender heterogeneity is 0.115 log points (SE 0.086, $p = 0.181$).

4 External Benchmarks and Mechanism

The occupation-specific treatment effects can reflect several kinds of gendered priors. One possibility is that respondents simply reproduce factual knowledge about male–female pay differences within occupations. This benchmark is useful because it gives the knowledge account a sharp quantitative prediction: occupations with larger observed gender pay gaps should exhibit larger randomised title effects, and occupations with similar observed gaps should generate similar effects. We therefore construct the external benchmark

$$g_j = \log \left(\frac{w_j^M}{w_j^F} \right), \quad (2)$$

where w_j^M and w_j^F are observed male and female earnings for occupation j .

For physicians, supermarket salespersons, and unskilled factory workers, we use April 2025 data from the German Federal Statistical Office’s earnings survey at the five-digit KldB occupation level [Federal Statistical Office (Destatis), 2025]. The respective log male–female gaps are 0.134, 0.161, and 0.153. For the company-head item, the broad KldB category “managing directors and board members” is economically far below the pay concept respondents were asked to estimate.

Table 2: Observed occupational gender gaps and randomised title effects

Occupation	External benchmark		Adult experiment		Youth experiment
	g_j	Gap (%)	Log effect	Effect (%)	Effect (%)
Physician	0.134	14.36	0.148	15.91	6.54
Head of large company	0.159	17.23	0.285	33.02	23.06
Supermarket salesperson	0.161	17.47	0.033	3.35	0.93
Unskilled factory worker	0.153	16.49	0.058	5.94	2.72
Bundestag politician	0.000	0.00	0.013	1.36	7.38

Notes: For physician, salesperson, and factory worker, external gaps use median monthly gross earnings by gender from Destatis. The company-head gap uses mean total FY2024 compensation of DAX management-board chairs from DSW/TUM, the statistic closest to the survey’s CEO compensation benchmark. Bundestag is an institutional zero-gap benchmark. External percentage gaps are $100(\exp(g_j) - 1)$. Experimental percentages are $100(\exp(\hat{\tau}_j) - 1)$.

We therefore use the DSW/TUM study of total compensation of DAX management-board chairs in fiscal year 2024 [DSW and TUM, 2025]. The mean reported total compensation is EUR 5.83 million for 36 male-coded company rows and EUR 4.98 million for four female-coded rows, implying $g_{CEO} = 0.159$ or a 17.2 per cent male/female ratio difference. Two of the four female CEO rows reflect October 2024 chair transitions, so alternative CEO statistics are reported as sensitivity checks. For Bundestag politicians, statutory parliamentary compensation does not differ by gender, and we consequently set the external benchmark to zero.

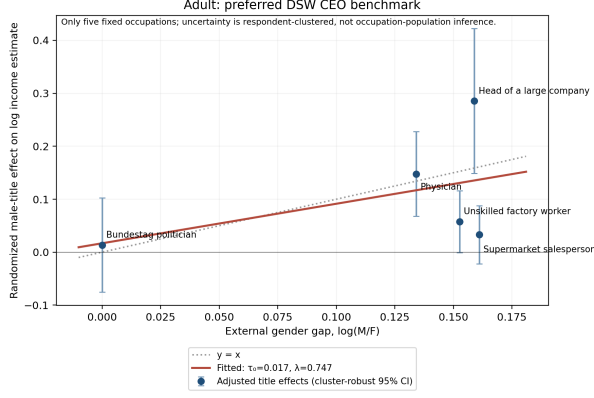
Table 2 juxtaposes the external gaps and the randomised title effects. The central observation is that the external gaps among the four labour-market occupations are tightly clustered while the experimental effects are not. CEO, salesperson, and factory-worker external gaps are all close to 16–17 per cent; their adult title effects are 33.0, 3.3, and 5.9 per cent.

We incorporate the external gap directly into the respondent-by-occupation experiment:

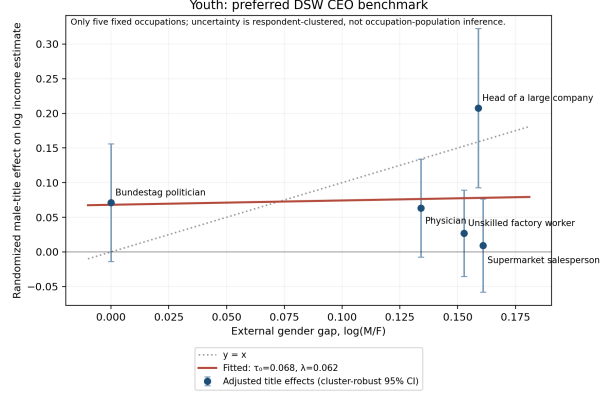
$$\log \hat{y}_{ij} = \alpha_j + \tau_0 T_i + \lambda(T_i \times g_j) + X_i' \gamma + \varepsilon_{ij}. \quad (3)$$

Occupation fixed effects absorb the main effect of g_j . The parameter τ_0 is the residual masculine-title effect for an occupation with an external gender gap of zero. The slope λ measures whether the causal title effect is larger in occupations with larger observed male–female earnings differences. A benchmark of $\tau_0 = 0$ and $\lambda = 1$ corresponds to exact one-for-one calibration. Even this benchmark would not identify conscious knowledge because stereotypes can themselves correlate with realised gender gaps.

Figure 2 plots the five occupation-specific causal effects against the preferred external gaps. For adults, $\hat{\lambda} = 0.747$ (SE 0.252, $p = 0.003$) and $\hat{\tau}_0 = 0.017$ (SE 0.045, $p = 0.712$). The joint hypothesis $(\tau_0, \lambda) = (0, 1)$ is not rejected ($p = 0.522$). This does not imply that the occupation-specific effects lie close to the perfect-calibration line: the test concerns the intercept and slope of the restricted calibration model, not how well that two-parameter relationship describes all five occupation-specific effects. Indeed, a Wald test strongly rejects the restrictions that map the five unrestricted occupation-



(a) Adults



(b) Adolescents

Figure 2: Randomised title effects and external occupational gender gaps

Notes: The horizontal axis is the external log male/female earnings ratio. The vertical axis is the occupation-specific randomised masculine-title effect in log points. Vertical bars are respondent-clustered 95 per cent confidence intervals. The dashed 45-degree line represents exact calibration; the fitted line uses the same five fixed occupations as the individual-level calibration model.

specific treatment effects onto the two-parameter calibration model ($\chi^2(3) = 20.34$, $p = 0.00014$). The reason is visible in the individual occupations. CEOs, supermarket salespersons, and factory workers have very similar observed gender gaps, but their randomised title effects differ sharply. Actual gender gaps therefore capture one dimension of the adult treatment pattern, but they do not provide an adequate account of its occupation-specific heterogeneity.

For adolescents, the preferred slope is $\hat{\lambda} = 0.062$ (SE 0.244, $p = 0.798$), while $\hat{\tau}_0 = 0.068$ (SE 0.043, $p = 0.115$). Exact one-to-one calibration is rejected ($p = 0.00015$), and the unrestricted model again fits better ($p = 0.011$).

The calibration exercise also helps interpret the respondent-gender result. Among adults, gender-specific calibration slopes are similar: 0.837 for male respondents and 0.649 for female respondents, with no evidence of a difference ($p = 0.706$). Among adolescents, the five-occupation fit produces a positive slope for boys and a negative slope for girls, with a formal difference ($p = 0.003$). Importantly, the large pooled title effect among adolescent girls is not evidence that they reproduce actual gender gaps more accurately; their occupation-specific effects are positive broadly and especially large at the zero-gap Bundestag item.

Several robustness checks lead to the same substantive conclusion. Replacing mean DAX CEO total compensation with median total compensation yields an adult calibration slope of 0.915 ($p < 0.001$); using mean fixed CEO compensation yields 0.889 ($p < 0.001$); combining gender-specific Federal Employment Agency values for the three ordinary occupations with the DAX CEO benchmark yields 0.681 ($p = 0.008$). The unrestricted occupation-specific model remains strongly preferred.

Taken together, the benchmark evidence supports neither a constant male-pay prior nor a simple exact-knowledge account. Actual gender gaps are related to the adult response pattern,

but occupations with nearly identical observed gaps generate very different causal title effects. The perceived return attached to gender is therefore occupation-specific in a way that cannot be reduced to the realised wage gap. Our results thus indicate that the relationship reflects selective or incomplete calibration.

5 Discussion and Conclusion

Our main result is substantive: gender enters beliefs about the economic return associated with an occupation. Across five occupations, randomly presenting a masculine rather than feminine occupational title raises estimated pay by 11.3 per cent among adults and 7.9 per cent among adolescents. Because the treatment changes only the grammatical gender of the label, the experiment isolates a component of perceived occupational value that is activated by gender itself. The same occupation is assigned a different expected level of pay depending on whether respondents are induced to represent it in male or female terms.

These gendered returns are not a mere reproduction of observed labour-market inequality. CEOs, salespeople, and factory workers have very similar male-female pay gaps in external benchmarks, yet their experimental responses differ dramatically. Adults attach a much larger gender differential to the pay of a large-company head than to selling or factory work. The external gaps are positively related to the adult treatment pattern, but a one-dimensional calibration model is rejected. This means that some gender inequalities appear to be much more strongly represented in occupational earnings beliefs than others. Thus, in light of the experiment, it appears unlikely that respondents are merely reporting known wage statistics.

The adult-adolescent respondent-gender contrast adds a second substantive dimension. Adult men and women react similarly to the title manipulation, whereas adolescent boys show essentially no average effect and adolescent girls show a 16 per cent effect. The female-adolescent response is positive across all five occupations and is not driven by the company-head item. The direct difference in gender heterogeneity between the adult and adolescent samples is not precisely estimated, so these data do not establish a developmental trajectory. They do, however, show that gendered earnings priors can differ sharply across respondent groups before adulthood. Whether this difference reflects identification with female occupational roles, attention to gender inequality, social learning, or subsequent labour-market experience is an important question for future work.

The broader economic implication concerns belief formation. Expected occupational pay enters many decisions, including educational and career choices, wage expectations, reservation wages, and bargaining. Our experiment does not measure those downstream choices, so it cannot establish that the title manipulation changes behaviour. But it shows that the perceived return to an occupational category is not fixed independently of social representation. If similar priors are present when people evaluate actual options, gendered beliefs about returns could contribute to differences in occupational aspirations and expectations even before any employer decision is made. Testing that channel directly is a natural next step.

The paper also retains an important measurement implication. Economic surveys frequently ask respondents to reason about workers, occupations, salaries, or labour-market prospects using linguistically gendered examples. In languages with grammatical gender, those choices are not innocuous formatting. They can change the cardinal quantity respondents report. Researchers should therefore treat occupational wording as part of the elicitation environment, especially when survey responses are interpreted as factual beliefs or expectations about the general population rather than a gender-specific subset. At the same time, the finding cautions that different linguistic implementations of the same nominal question can elicit systematically different belief distributions.

In sum, we find that gender is embedded in perceived occupational returns, this representation is selective across occupations and respondents, and it is only incompletely anchored in actual wage differences.

A Additional Results and Robustness

Occupation-specific respondent-gender heterogeneity. Figure A1 reports the fully interacted occupation-specific effects. Among adolescents, female respondents have positive point estimates for all five occupations. Male adolescents are near zero or negative outside the company-head item. The omnibus test of differential occupation patterns is $\chi^2(4) = 17.60$ ($p = 0.0015$) for adolescents and $\chi^2(4) = 1.40$ ($p = 0.845$) for adults. The corresponding cross-sample four-way omnibus is $\chi^2(4) = 8.14$ ($p = 0.087$).

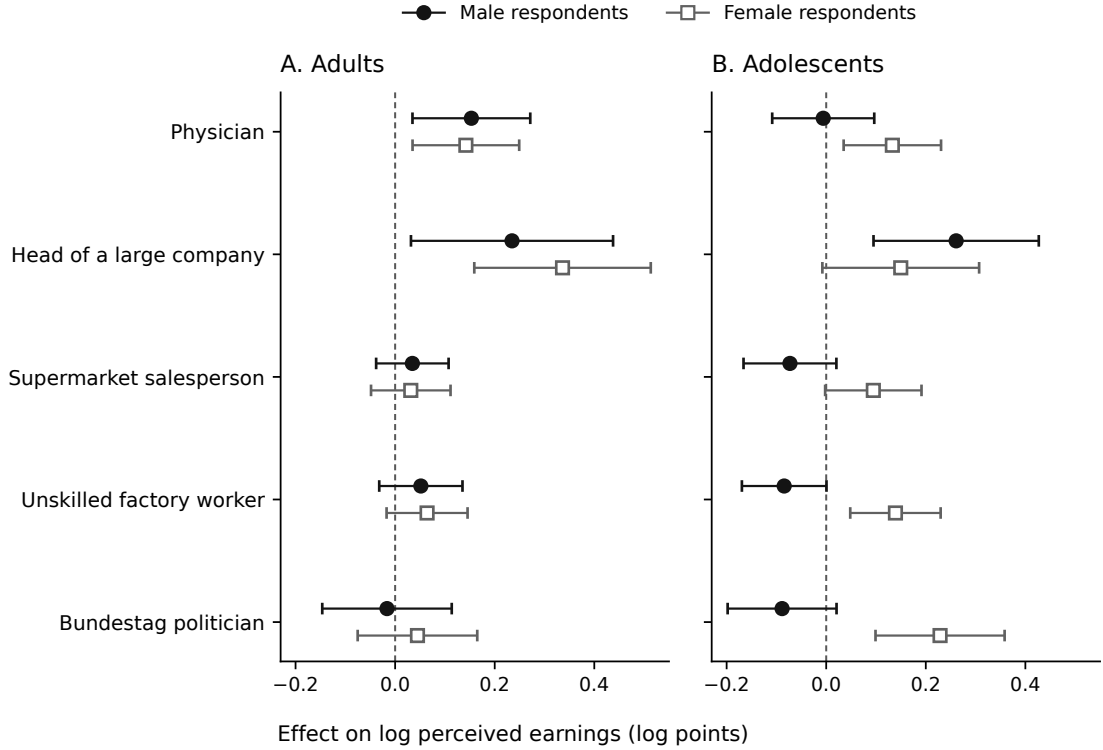


Figure A1: Occupation-specific randomised masculine-title effects by respondent gender

Notes: Estimates come from fully interacted occupation $\times$ treatment $\times$ respondent-gender models, estimated separately for adults and adolescents. Points are effects on log perceived earnings; bars are respondent-clustered 95 per cent confidence intervals. The omnibus test that the respondent-gender difference varies across occupations has $p = 0.845$ among adults and $p = 0.0015$ among adolescents.

Other pre-treatment heterogeneity. Table A1 reports interactions with age, education, and adult household income. None survives Holm correction within sample. The adult categorical-education sensitivity produces an omnibus $p = 0.021$, but this result is driven by a cell of only 15 model-eligible respondents coded as currently in school.

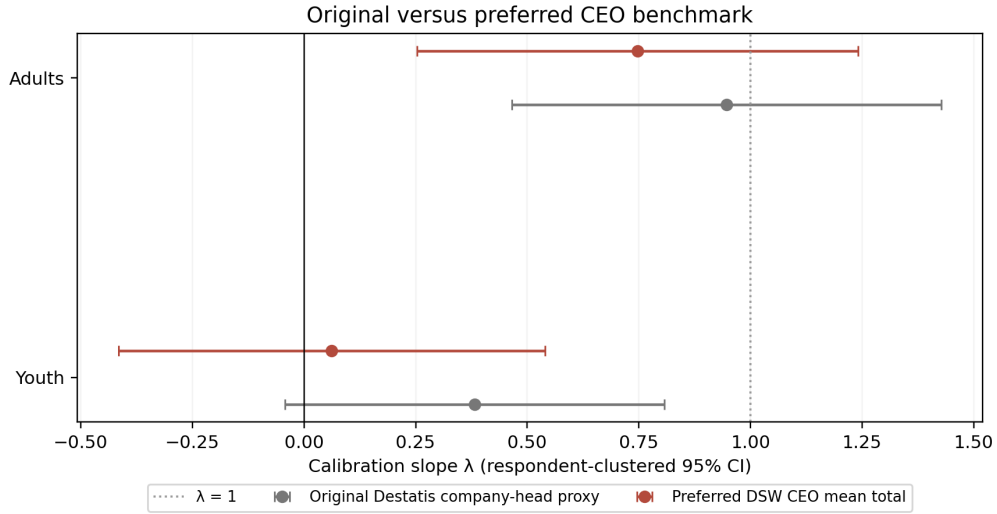
Sensitivity to the CEO benchmark. The broad Destatis category “managing directors and board members” originally implied a company-head gap of 0.234 log points. Replacing it with the

Table A1: Additional treatment heterogeneity

Sample	Moderator	Interaction	SE	Raw p	Holm p
Adult	Age	0.041	0.033	0.215	0.431
Adult	Education	-0.062	0.036	0.080	0.239
Adult	Household income	-0.012	0.030	0.693	0.693
Youth	Age	0.046	0.031	0.140	0.281
Youth	Education	0.001	0.032	0.965	0.965

Notes: Entries are treatment-by-moderator interactions from the same stacked five-occupation specification. Age, education, and household income are standardised in the underlying heterogeneity analysis. Holm adjustment is applied within sample to the Tier-2 exploratory moderator family. Youth household income is unavailable.

conceptually better-matched DAX CEO mean-total-compensation gap reduces the adult calibration slope from 0.946 to 0.747 but leaves it positive and statistically distinguishable from zero. Figure A2 summarises the change. Alternative DSW/TUM summaries likewise yield positive adult calibration slopes, but all such calculations condition on a very small female CEO cell.

**Figure A2:** Calibration slope under the original and preferred CEO benchmarks

Preregistration and analysis status. The randomised masculine-versus-feminine occupational-title contrast was preregistered [Schulz et al., 2026]. The preregistration states H1e as: “Respondents report higher income values if shown male occupation titles than if shown female ones.” Occupation-specific heterogeneity, respondent-gender heterogeneity, external-gap calibration, and pooled adult–youth comparisons are secondary analyses.

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