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Klára Kantová
Michaela Hasíková

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Institute of Economic Studies,
Faculty of Social Sciences,
Charles University in Prague

[UK FSV – IES]

Opletalova 26
CZ-110 00, Prague
E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

Institut ekonomických studií
Fakulta sociálních věd
Univerzita Karlova v Praze

Opletalova 26
110 00 Praha 1

E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

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Do Pay Transparency Laws Reduce the Gender Wage Gap? Insights from a Meta-Analysis

Klára Kantová[1]
Michaela Hasíková[2]

[1]Institute of Economic Studies, Charles University, Prague, Czech Republic,
Email: klara.kantova@fsv.cuni.cz.
[2] Institute of Economic Studies, Charles University, Prague, Czech Republic

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Abstract:
Pay transparency laws are a key policy response to persistent gender wage disparities, yet evidence on their effectiveness is mixed. This meta-analysis synthesizes 268 estimates from 12 studies. Across a broad suite of publication bias diagnostics, we find at most weak evidence of selective reporting, while most approaches indicate a small but significant positive effect beyond bias. The pooled mean effect is 0.012 log points, corresponding to an average 1.2% increase in women's wages relative to men, consistent with a modest narrowing of the gap. Heterogeneity analysis using Bayesian and frequentist model averaging shows that policy design is pivotal. Public disclosure regimes produce larger reductions than internal access or job-ad disclosure, while evidence for pay-secrecy bans is imprecise. Specification choices also matter, with regional and employee controls attenuating effects and sector controls amplifying them. Overall, effective transparency depends on both robust policy design and careful empirical specification.

JEL: J16, J31, J38
Keywords: pay transparency, gender gap, wages, policy-design, model averaging, meta-analysis

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1 Introduction

The gender wage gap represents a persistent barrier to economic equality and social justice, with significant consequences for women's lifetime earnings, financial independence, and career advancement (Kabeer, 2021; Kossek & Buzzanell, 2018). Although women's participation in higher education and the labor force has increased substantially in recent decades, they continue to earn less than men on average (Fluchtmann *et al.*, 2024; OECD, 2020). Part of this disparity can be attributed to observable characteristics such as occupation, work experience, or hours worked, yet a substantial share remains unexplained (Blau & Kahn, 2017; Boll *et al.*, 2016), often attributed to structural and behavioral factors. Structural discrimination, gender differences in negotiations, and systematic misperceptions of pay contribute to this gap (Babcock & Laschever, 2021; Flory *et al.*, 2015; Cullen & Pakzad-Hurson, 2023; Card *et al.*, 2012; Cullen & Perez-Truglia, 2022).

Pay transparency policies have gained considerable attention as a promising tool to address these disparities. However, evidence from individual countries varies widely. Denmark's law modestly narrowed the gap, primarily by slowing male wage growth (Bennedsen *et al.*, 2022); Germany's request-based system had limited uptake (Brütt & Yuan, 2023); Austria's job-ad disclosure produced weaker effects (Bamieh & Ziegler, 2025); while the United Kingdom's and Canada's mandatory public reporting generated more substantial impacts (Blundell *et al.*, 2025). These contrasting outcomes underscore how institutional context and policy design shape effectiveness.

Transparency can influence pay-setting through several mechanisms. First, by reducing information asymmetries, it enhances underpaid workers' ability to negotiate and compare pay levels (Roussille, 2024; Mas, 2017). Second, public and comparable reporting fosters accountability, prompting firms to review pay structures and correct unjustified gaps (Blundell *et al.*, 2025; Duchini *et al.*, 2024). Third, greater visibility can encourage sorting, as workers move toward more transparent or higher-paying employers, gradually shifting labor-market equilibria (Duchini *et al.*, 2024). However, several studies find that transparency laws often narrow the gap not only by raising women's wages but also by slowing wage growth among higher-paid men (Bennedsen *et al.*, 2022; Morin, 2025). This "compression channel" improves relative equality but may limit absolute gains for women and could also weaken performance-based incentives or the retention of high performers, particularly in competitive sectors.

Recent meta-analytic evidence further suggests that men and women respond similarly to monetary incentives under performance-based pay, implying that behavioral differences in effort or responsiveness are unlikely to explain most of the observed wage gap (Bandiera *et al.*, 2021). This finding supports the view that structural and informational frictions, rather than intrinsic productivity or motivational differences, play a central role in sustaining gender pay disparities. While performance-pay studies reveal how workers react to incentives, transparency interventions reshape the information environment that determines those incentives. In this context, pay transparency policies directly target these institutional asymmetries by making compensation structures more visible and comparable. Moreover, existing reviews suggest that transparency is generally associated with reductions in the gender wage gap (Bennedsen *et al.*, 2023; Duchini *et al.*, 2024), but they remain largely qualitative and emphasize that results depend heavily on design and enforcement. To date, no quantitative synthesis has systematically aggregated the empirical evidence despite a rapidly growing number of causal studies employing diverse identification strategies.

This study fills that gap. We conduct a meta-analysis of 12 studies reporting 268 estimates of transparency's effects on the gender wage gap. The mean effect implies a 1.2% increase in women's wages relative to men, a small but consistent shift. For a woman earning EUR 2,000 per month, this corresponds to about EUR 24 more per month, or EUR 288 annually (in real terms). We also assess potential publication bias using a comprehensive set of diagnostic tools, finding at most weak indications of selective reporting. Finally, we analyze heterogeneity using Bayesian and frequentist model averaging, showing that policy design is a dominant source of variation: public disclosure laws are considerably more effective than internal disclosure, minimum-salary job ads, or pay-secrecy bans.

The remainder of the paper is structured as follows. Section 2 describes the dataset and preliminary analysis. Section 3 addresses publication bias. Section 4 examines heterogeneity in reported effects. Section 5 concludes with policy implications and directions for future research.

2 Data

To gather relevant studies, Google Scholar was used as the primary search engine due to its comprehensive coverage of academic literature and access to full-text papers. The search strategy was designed to maximize the likelihood of retrieving relevant studies among the top-ranked results. Key terms included "pay transparency," "wage transparency," "transparency policies," "gender pay gap," "gender wage gap," and "gender gap." Various combinations of these terms were tested, and the final query (("pay transparency") AND ("gender pay gap" OR "gender gap" OR "gender wage gap")) was selected after verifying that it consistently returned the most relevant results.

This search generated approximately 2,520 hits, of which the first 200 were screened manually. The review cutoff was based on the observation that only two additional relevant studies appeared among the final 130 screened results, suggesting diminishing returns from extending the search further. To supplement this, backward citation searches ("snowballing") were conducted, yielding 38 potentially relevant studies.

The inclusion criteria required that (i) the study estimate a causal effect of a pay transparency policy on wages, (ii) the outcome measure reflect the post-reform change in women's relative to men's wages, and (iii) the standard error be reported or derivable from test statistics. After applying these criteria, 12 studies with 268 effect estimates remained. Study identification and screening followed PRISMA guidelines, and the full list is provided in Appendix A.

To ensure comparability, estimates based on a male interaction term were inverted so that all effects measure the change in female wages relative to male wages, with positive values indicating a reduced gender wage gap. All studies reported log-wage outcomes, ensuring consistent effect units. A small number of mild outliers were retained, as their influence was negligible and winsorization offered no analytical gain.

Most primary studies estimate variants of a difference-in-differences (DiD) model to identify the effect of pay transparency laws on wages. The typical specification can be written as:

$$\ln w_{ivt} = \alpha + \beta_1(T_v \times \text{Post}_t) + \beta_2(\text{Female}_i \times T_v \times \text{Post}_t) + \mathbf{X}'_{ivt}\gamma + \mu_v + \lambda_t + \varepsilon_{ivt}, \quad (1)$$

where $\ln w_{ivt}$ denotes the logarithm of the wage of individual i in firm or establishment v at time t; T_v is an indicator for treatment exposure (typically firms or sectors covered by a pay transparency requirement); and Post_t equals one for periods after the policy introduction. The interaction term $\text{Female}_i \times T_v \times \text{Post}_t$ captures the differential post-reform wage change for women relative to men. The coefficient of interest, β_2, thus measures the impact of pay transparency on the gender wage gap. The vector $\mathbf{X}_{ivt}$ includes worker and firm-level controls such as occupation, industry, region, and tenure, while μ_v and λ_t denote firm and time fixed effects, respectively.

Some studies extend this baseline framework by applying difference-in-discontinuities designs (e.g., exploiting wage thresholds for mandatory reporting) or alternative treatment definitions (e.g., variation in firm size or timing of enforcement). Regardless of the exact setup, these models yield causal estimates of transparency-induced wage adjustments, net of common macroeconomic trends or unobserved heterogeneity.

All studies were coded following a predefined codebook that specified inclusion criteria, effect size extraction rules, and variable definitions. Data extraction was conducted by M. Hasíková and independently reviewed by K. Kantová to ensure reliability and consistency. Discrepancies were resolved through joint discussion and re-checking of the original study tables and appendices.

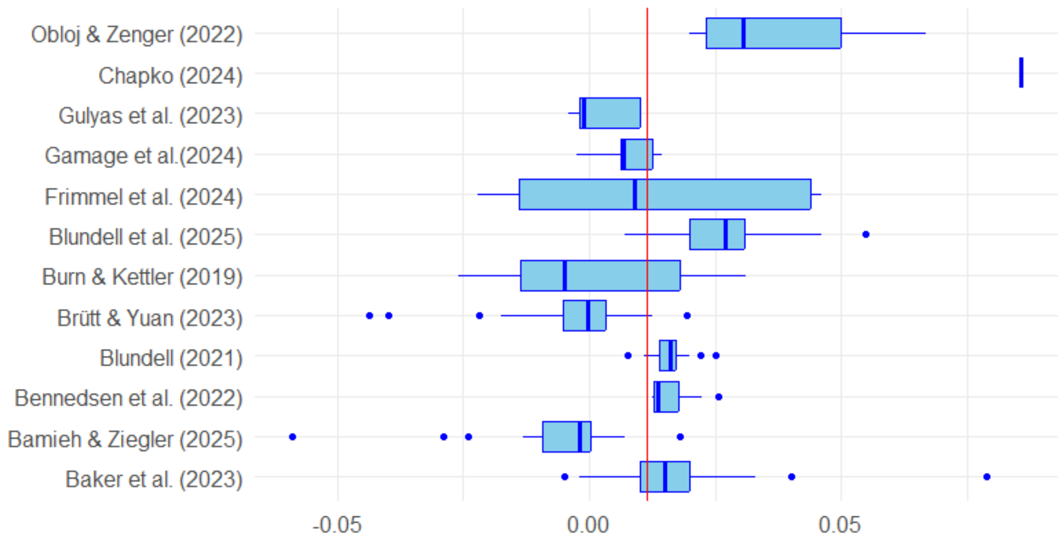
2.1 Preliminary Analysis

Figure 1 presents box plots of effect sizes from each study, allowing a visual comparison of the distribution and variability of the estimates collected. The number and magnitude of the reported effects vary substantially between studies. For example, the study by Chapko (2024) provides only a single estimate with a value of 0.086, which is also the maximum among all estimates. This outlier could disproportionately influence analyses that apply weighting based on the inverse number of estimates per study. Such weighting schemes should therefore be interpreted with caution, particularly when summarizing subsets where the Chapko's estimate is included. In contrast, other studies usually contribute a substantially larger number of estimates, most notably Baker *et al.* (2023) with 64 estimates.

While the majority of estimates are positive, suggesting a narrowing of the gender wage gap following pay transparency interventions, some studies, such as Gulyas *et al.* (2023), Burn & Kettler (2019), Brütt & Yuan (2023), and Bamieh & Ziegler (2025), exhibit effect sizes that are centered around zero or even predominantly negative. These discrepancies underscore the variability in study designs, sample characteristics, and methodological approaches, which will be explored in greater depth in subsequent analyses.

In addition, we examine the distribution of effect estimates across different types of

Figure 1: Comparison of effect sizes across studies

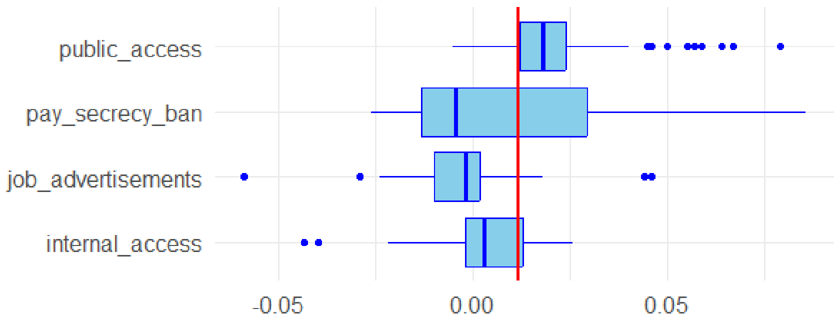


Notes: The figure shows box plots of effect sizes of individual studies. The red line depicts the sample mean of all estimates, which equals to 0.012.

pay transparency, as illustrated in Figure 2. Among them, public access to institutional gender equality data is associated with the largest effect sizes. This finding aligns with theoretical expectations, since it represents the most extensive form of pay transparency, due to exerting additional pressure through public scrutiny. In contrast, the estimated effects of other transparency types tend to cluster closer to zero, which may indicate that public accountability, rather than enhanced individual bargaining power, might be the primary mechanism driving reductions in the gender wage gap as suggested by Blundell *et al.* (2025).

The pay secrecy ban estimates exhibit greater variability. However, this is likely attributable to the limited sample, consisting of only 12 observations drawn from two U.S.-based studies (Burn & Kettler, 2019; Chapko, 2024). Consequently, the wider dispersion might reflect reduced precision rather than substantive heterogeneity in the underlying effects. The dataset covers six countries with distinct transparency regimes.

Figure 2: Comparison of effect sizes across pay transparency types



Notes: The figure shows box plots of effect sizes of all different pay transparency types from our data set. Public access mandates public disclosure of gender gap reports. Internal access mandates internal disclosure of gender gap reports to employees within the organization. Job advertisements represent the obligation of providing minimum wage in job advertisements. Pay secrecy ban bans employers from prohibiting wage discussions between employees. The red line depicts the sample mean of all estimates, which equals to 0.012. Table A2 summarizes the distribution of estimates by country and transparency type.

Table A2 summarizes the distribution of estimates by country and transparency type.

Table 1 reports unweighted and inverse–study size-weighted means of effect sizes across data, policy, and methodological subsets. The overall mean is 0.012, implying that pay-transparency laws raise women's wages by about 1.2% relative to men. Its 95% confidence interval excludes zero, providing preliminary evidence of a positive effect. Weighted means are slightly higher, suggesting that studies with fewer estimates tend to find somewhat larger effects, which could reflect context, study quality, or small-sample publication bias.

Consistent with Figure 2, policies mandating public disclosure yield the largest mean effects, whereas internal disclosure and job-advertisement requirements cluster near zero. The pay-secrecy ban mean is inflated by a single large estimate from Chapko (2024). Among data characteristics, university settings and North-American studies show higher means than firm-level or European samples, but these dimensions are highly correlated (r = 0.827), making their separate influence unclear. Methodologically, difference-in-discontinuity designs and male-treated interaction terms produce slightly lower means, though confidence intervals for difference-in-discontinuity include zero.

Table 1: Summary statistics of selected subsets

	Unweighted		**Weighted**		
	Mean	**CI**	**Mean**	**CI**	**N**
All Data	0.012	(0.010, 0.014)	0.018	(0.006, 0.030)	268
Data characteristics					
Subject: Firms	0.008	(0.005, 0.010)	0.017	(0.001, 0.033)	177
Subject: Universities	0.019	(0.016, 0.023)	0.021	(0.016, 0.026)	91
Work: Full-time Only	0.013	(0.011, 0.016)	0.015	(0.012, 0.018)	176
Work: Part-time Included	0.008	(0.004, 0.012)	0.021	(-0.002, 0.044)	92
Wage: Hourly	0.018	(0.014, 0.021)	0.014	(0.009, 0.020)	63
Wage: Other	0.010	(0.007, 0.012)	0.019	(0.003, 0.035)	205
Continent: Europe	0.008	(0.005, 0.010)	0.009	(0.005, 0.013)	174
Continent: North America	0.019	(0.015, 0.023)	0.035	(0.009, 0.061)	94
Methodology					
Method: Diff-in-diff	0.012	(0.010, 0.014)	0.018	(0.006, 0.030)	258
Method: Diff-in-disc	0.002	(-0.006, 0.009)	0.002	(-0.005, 0.009)	10
Treated Gender: Female	0.012	(0.010, 0.015)	0.020	(0.006, 0.033)	227
Treated Gender: Male	0.009	(0.006, 0.012)	0.008	(0.005, 0.010)	41
Interaction: Triple	0.011	(0.008, 0.014)	0.020	(0.002, 0.037)	141
Interaction: Double	0.012	(0.009, 0.016)	0.014	(0.010, 0.018)	127
PT characteristics					
Public Access	0.020	(0.018, 0.022)	0.021	(0.018, 0.024)	142
Internal Access	0.004	(0.001, 0.007)	0.004	(0.001, 0.006)	73
Job Advertisements	-0.003	(-0.008, 0.002)	0.004	(-0.010, 0.018)	41
Pay Secrecy Ban	0.008	(-0.012, 0.028)	0.043	(-0.006, 0.093)	12
Study characteristics					
Status: Published	0.013	(0.011, 0.015)	0.013	(0.010, 0.016)	205
Status: Unpublished	0.007	(0.003, 0.012)	0.028	(-0.002, 0.058)	63

Notes: The table reports mean statistics of the effect sizes from various subsets. The weighted mean is weighted by the inverse of the study size. For a detailed description of the variables, refer to Table 4. CI = 95% confidence interval, N = number of observations, DV = Dependent Variable, PT = Pay Transparency.

These descriptive patterns, study-level variation, policy-type differences, and potential geographical or methodological contrasts, motivate the formal heterogeneity analysis in Section 4, where model-averaging techniques assess the joint influence of these factors while accounting for publication bias.

3 Publication Bias

Publication bias is the systematic tendency for studies with statistically significant or expected results to be more likely published than studies with null or contrary findings, which can distort evidence syntheses (Thornton & Lee, 2000; Bartoš *et al.*, 2024; Stanley, 2005). It arises when journals prefer striking results, authors refrain from submitting null findings (file drawer problem, Rosenthal, 1979), and significant subgroups within studies receive disproportionate attention (Stanley, 2005). In meta-analysis, failing to account for such bias risks overstating average effects and misleading policy decisions (Sutton, 2009). We therefore combine graphical and econometric diagnostics, including linear and non-linear regressions and methods that relax exogeneity (Irsova *et al.*, 2024a).

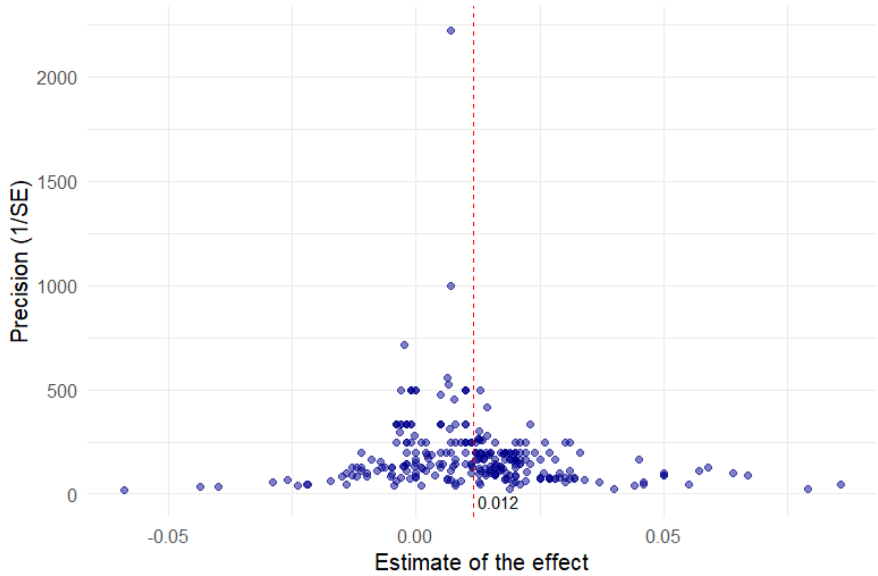
A funnel plot (Egger *et al.*, 1997) of all 268 estimates (Figure 3) shows approximately symmetrical scatter around the mean effect of 0.012, with more precise estimates clustered near the mean and less precise ones spread widely. Such symmetry provides no visual indication of strong publication bias, though heterogeneity or methodological differences can also generate symmetry (Sterne *et al.*, 2011), so formal tests are required.

Following Stanley (2005), we estimate

$$\text{estimate}_{ij} = \beta_0 + \beta_1 \cdot \text{SE}_{ij} + u_{ij}, \tag{2}$$

where $estimate_{ij}$ represents the i-th estimate of the effect of pay transparency law on the gender wage gap from the j-th study, SE_{ij} is its standard error, and u_{ij} represents the error term. In the following tables, the parameters β_0 and β_1 are referred to as the "effect beyond bias" and the "publication bias," respectively. To guide interpretation, it is useful to recall the meaning of the two key parameters in regression-based bias tests. The

Figure 3: Funnel plot



Notes: The figure shows the funnel plot of all collected estimates of the effect of pay transparency on the gender gap. The precision of the effect is the inverse of its standard error. The red dashed line depicts the sample mean of the estimates, which is approximately 0.012.

slope coefficient (β_1) captures whether estimated effects vary systematically with their precision, a significant β_1 is evidence of selective reporting or small-sample bias. The intercept (β_0) represents the "effect beyond bias," i.e., the estimated underlying impact of pay transparency once potential bias is taken into account. In practice, four cases arise: (i) insignificant β_1 with significant β_0 indicates little bias and a genuine effect; (ii) both coefficients insignificant suggest no robust evidence either of bias or of an effect; (iii) both significant imply some bias and a persisting true effect; and (iv) significant β_1 with insignificant β_0 indicates that once bias is corrected, no reliable underlying effect remains.

In Table 2, we report the results of publication bias diagnostics. Among the linear regressions (OLS, inverse-SE weighting, and fixed- and random-effects models), β_1 is insignificant, while β_0 is positive and significant, ranging from 0.007 to 0.016 and closely matching the simple mean of 0.012 log points. These specifications therefore suggest a small but genuine effect of pay transparency with no strong evidence of bias. By contrast, the inverse-study-size weighted regression yields an insignificant effect beyond bias, reflecting the disproportionate weight it places on small-sample studies. The between-effects model indicates significant publication bias and no effect beyond bias, but this result is driven by the single large estimate from Chapko (2024), illustrating the sensitivity of BE to outliers when only 12 studies are available.

To relax the linearity assumption, we turn to complementary approaches. The Top10 estimator (Stanley *et al.*, 2010), which averages the most precise 10% of estimates, gives a smaller effect beyond bias (0.005*), suggesting some bias but with low power because it uses only 26 observations. The WAAP (Ioannidis *et al.*, 2017), which weights 72 adequately powered estimates, gives 0.011 (insignificant) effect beyond bias and is close to the overall mean. The stem-based method (Furukawa, 2019), using 255 estimates, reports 0.007** and supports broad symmetry. A selection model (Andrews & Kasy, 2019) detects substantial selection (0.744***) but still finds a significant true effect of 0.009***. This suggests that although studies with statistically significant results are more likely to be reported, the estimated effect remains positive and significant, which means that even after accounting for the tendency of significant results to be overrepresented, a genuine effect of pay transparency on narrowing the gender wage gap persists. In other words, publication bias exists but does not overturn the substantive conclusion that pay transparency reduces the gender wage gap.

Finally, we address possible endogeneity between effect sizes and standard errors using an instrumental variable regression with the inverse square root of sample size as instrument. Both the bias and effect estimates are positive but insignificant, again consistent with weak bias and a modest underlying effect. Taken together, these diagnostics show at most mild selective reporting. More importantly, across nearly all approaches the estimated effect beyond bias remains positive and statistically significant, reinforcing the conclusion that pay transparency policies modestly but genuinely reduce the gender wage gap.

Table 2: Comparison of publication bias tests across methods

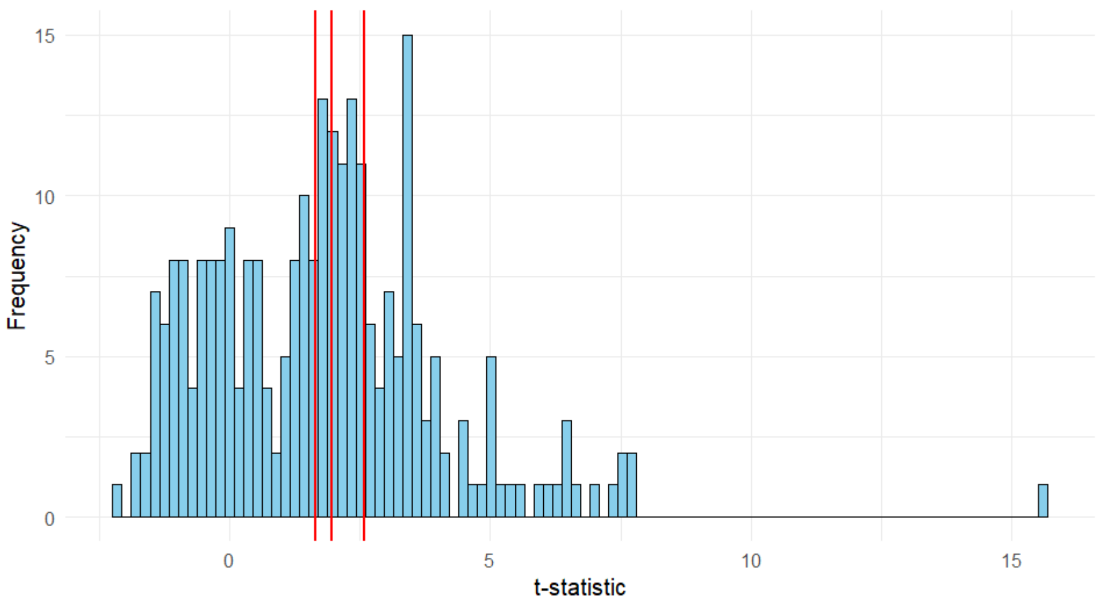
	OLS	**Study**	**Precision**	**FE**	**BE**	**RE**
Publication bias	0.081	1.261	0.558	-0.005	2.015*	0.017
SE	(0.472)	(1.030)	(0.468)	(0.552)	(1.058)	(0.630)
Effect beyond bias	0.011***	0.004	0.007***	0.016***	-0.003	0.016**
Constant	(0.003)	(0.007)	(0.003)	(0.003)	(0.013)	(0.007)
Studies	12	12	12	12	12	12
Observations	268	268	268	268	268	268

	Top10	**WAAP**	**Stem**	**SM**		**IV**
Publication bias	-	-	-	0.744***		0.295
	-	-	-	(0.142)		(0.874)
Effect beyond bias	0.005*	0.011	0.007**	0.009***		0.009
	(0.002)	(0.005)	(0.002)	(0.001)		(0.008)
Studies	12	12	12	12		12
Observations	268	268	268	268		268

Notes: The table reports results of Equation 4.1 using different linear regression specifications along with non-linear methods. OLS = Ordinary Least Squares, Study = inverse of study size weights, Precision = inverse standard error weights, FE = Fixed Effects, BE = Between Effects, RE = Random Effects. WAAP = Weighted Average of the Adequately Powered, Stem = Stem-based method, SM = Selection model, IV = instrumental variable regression with the inverse of the square root of sample size as the instrumental variable. Cluster-robust (CR2) standard errors, clustered at the study level, are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Lastly, we employ the caliper test by Gerber & Malhotra (2008), which analyzes the distribution of t-statistics by applying regression discontinuity designs. Specific thresholds corresponding to the conventional significance levels are selected, and then the distribution around them is assessed. Specifically, the caliper test compares the frequency of observations in narrow bins (or "calipers") just above and below the threshold. Under no publication bias, the frequency around the cut-off should be symmetrical. Figure 4 shows the distribution of t-statistics. The values are predominantly positive, therefore we will concentrate on the positive t-values 1.645, 1.96, and 2.58 corresponding to p-values of 0.1, 0.05 and 0.01 respectively. Since the largest t-statistic threshold of 2.58 clearly has less values just above than just below, it contradicts the theoretical foundation of this publication bias test. Consequently, we decided to estimate only the two lower cut-offs (see Table 3).

Figure 4: T-statistic distribution



Notes: The figure shows the distribution of t-statistics of all estimates. The red vertical lines depict the significance thresholds 1.645, 1.96, and 2.58.

Caliper tests around common significance thresholds show no excess of t-statistics just above the 1.96 (5%) cutoff and only weak evidence at the 1.645 (10%) level again pointing to limited bias. The weak excess just above the 10% significance threshold may indicate marginal preference for results reaching conventional significance, but the absence of bunching at the 5% cutoff suggests this tendency is not strong. Combined with the other diagnostics, the caliper tests reinforce the view that selective reporting is modest.

Table 3: Caliper tests for publication bias

	Threshold	
	1.645	**1.96**
Caliper size 0.4	0.109	0.040
	(0.082)	(0.094)
	N = 46	N = 50
Caliper size 0.6	0.125^{*}	0.039
	(0.066)	(0.069)
	N = 64	N = 76
Caliper size 0.8	0.146^{***}	0.033
	(0.057)	(0.063)
	N = 82	N = 92

Notes: The table reports results of caliper tests for two t-statistic thresholds and different caliper sizes. Standard errors in parentheses. N = number of observations around the threshold within the caliper width. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Overall, the combined evidence from funnel visualization, linear and non-linear regressions, selection and stem methods, instrumental-variable estimation, and caliper tests indicates at most a mild tendency toward selective reporting. Crucially, most models yield a positive and statistically significant effect beyond bias (approx. 0.01), reinforcing the conclusion that pay transparency policies modestly but genuinely reduce the gender wage gap.

These diagnostics are standard in the meta-analysis literature, see Egger *et al.* (1997), Stanley (2005); Stanley *et al.* (2010), Ioannidis *et al.* (2017), Furukawa (2019), Andrews & Kasy (2019), Bom & Rachinger (2019), van Aert & van Assen (2018), Havranek *et al.* (2024) and the guidance in Sterne *et al.* (2011), Irsova *et al.* (2024b), Irsova *et al.* (2024a), and Sutton (2009).

4 Heterogeneity

Heterogeneity matters for interpretation. Reported effects vary by policy design, context, and methods (see Table 1). We therefore code covariates capturing data, methodology, pay transparency (PT) design, controls, and study traits, and use model averaging to identify robust predictors of effect variation. Our goal is interpretive rather than purely predictive. We use model averaging to rank robust moderators and then explain why particular covariates are likely to raise or lower estimated impacts.

We compile 30 covariates (see Table 4) grouped as: (i) *Data characteristics* (sample size, year range, firms vs. universities, full-time vs. part-time, hourly vs. other wages, Europe vs. North America, baseline gender gap), (ii) *Methodology* (difference-in-differences vs. difference-in-discontinuities, treated gender, double vs. triple interactions), (iii) *PT characteristics* (public access, internal access, job-ad disclosure, pay-secrecy ban, implementation year), (iv) *Controls* (region, sector, employer, employee), and (v) *Study characteristics* (study size, publication status, citations). These dimensions reflect the design and context differences highlighted earlier. Table 4 describes each variable, including definition and summary statistics.

Table 4: Description and overview of variables

Variable	Description	Mean	SD
Effect Size	The effect of the pay transparency on the increase in wages of women relative to men	0.012	0.017
Standard Error	The standard error of the effect	0.009	0.007
Data characteristics			
Sample Size	The number of observations used for the estimation of the effect	642,877	1,273,045
Year Range	The range of years of the data sample used for the estimation	14.642	11.814
Subject: Firms*	=1 if the subjects to which the pay transparency applies are firms	0.660	0.474
Subject: Universities*	=1 if the subjects to which the pay transparency applies are universities	0.340	0.474
Work: Full-time Only	=1 if only full-time workers are included in the sample	0.657	0.476
Work: Part-time Included	=1 if part-time workers are included in the sample	0.343	0.476
Wage: Hourly	=1 if the dependent variable is the log of hourly earnings	0.235	0.425
Wage: Other	=1 if the dependent variable is the log of annual, weekly or daily earnings	0.765	0.425
Continent: Europe*	=1 if the data sample is from Europe	0.649	0.478
Continent: North America*	=1 if the data sample is from North America	0.351	0.478
Original Gender Gap*	The gender wage gap in the country at the year of the pay transparency implementation	18.186	3.437
Methodology			
Method: Diff-in-diff	=1 if diff-in-diff method is used for the estimation of the effect	0.963	0.190
Method: Diff-in-disc	=1 if diff-in-disc method is used for the estimation of the effect	0.037	0.190
Treated Gender: Female*	=1 if the treatment group of gender is female	0.847	0.361

Continued on next page

Table 4 – continued from previous page

Variable	Description	Mean	SD
Treated Gender: Male*	=1 if the treatment group of gender is male	0.153	0.361
Interaction: Triple*	=1 if Treated × Post × Gender is the interaction term used for estimating the effect	0.526	0.500
Interaction: Double*	=1 if Post × Gender is the interaction term used for estimating the effect	0.474	0.500
Control variables			
Control: Region	=1 if the regression uses region controls	0.481	0.501
Control: Sector	=1 if the regression uses sector controls	0.157	0.364
Control: Employer	=1 if the regression uses firm/university controls	0.608	0.489
Control: Employee	=1 if the regression uses individual controls	0.690	0.463
PT characteristics			
PT: Public Access	=1 if the pay transparency mandates public disclosure of gender gap reports	0.530	0.500
PT: Internal Access	=1 if the pay transparency mandates internal disclosure of gender gap reports to employees within the organization	0.272	0.446
PT: Job Advertisements	=1 if the pay transparency mandates provision of minimum wage in job advertisements	0.153	0.361
PT: Pay Secrecy Ban	=1 if the pay transparency bans employers from prohibiting wage discussions between employees	0.045	0.207
Implementation Year	The implementation year of the pay transparency	2011	5.968
Study characteristics			
Study Size*	The number of estimates collected from the study	3.388	0.609
Status: Published	=1 if the study was published in a journal	0.765	0.425
Status: Unpublished	=1 if the study was not published in a journal	0.235	0.425
Citations	The number of citations of the study	99.821	104.930

Notes: This table presents the descriptions and summary statistics for all variables included in our dataset. Variables marked with * are not included in the model averaging models due to high correlation. SD = Standard Deviation, PT = Pay Transparency.

To address model uncertainty, we employ Bayesian Model Averaging (BMA) with posterior model probabilities and Posterior Inclusion Probabilities (PIPs), implemented in R via the `bms` package (Zeugner & Feldkircher, 2015). We use a unit-information g-prior with uniform model priors, and conduct robustness checks with alternative g-priors and a random model prior (see Appendix B). As a complementary benchmark, we also report Frequentist Model Averaging (FMA). This approach reduces reliance on any single specification and quantifies the robustness of predictors through their inclusion probabilities. Throughout, higher values of the dependent variable indicate larger reductions in the gender wage gap (women's wages rising relative to mens).

We avoid dummy traps by setting reference categories and reduce the initial 30 variables to 15 by excluding highly collinear predictors (VIF $>$ 10). The meta-regression is

$$y_{ij} = \beta_0 + \beta_1\, SE_{ij} + \sum_{k=1}^{15} \beta_k X_{ij,k} + \varepsilon_{ij}, \tag{3}$$

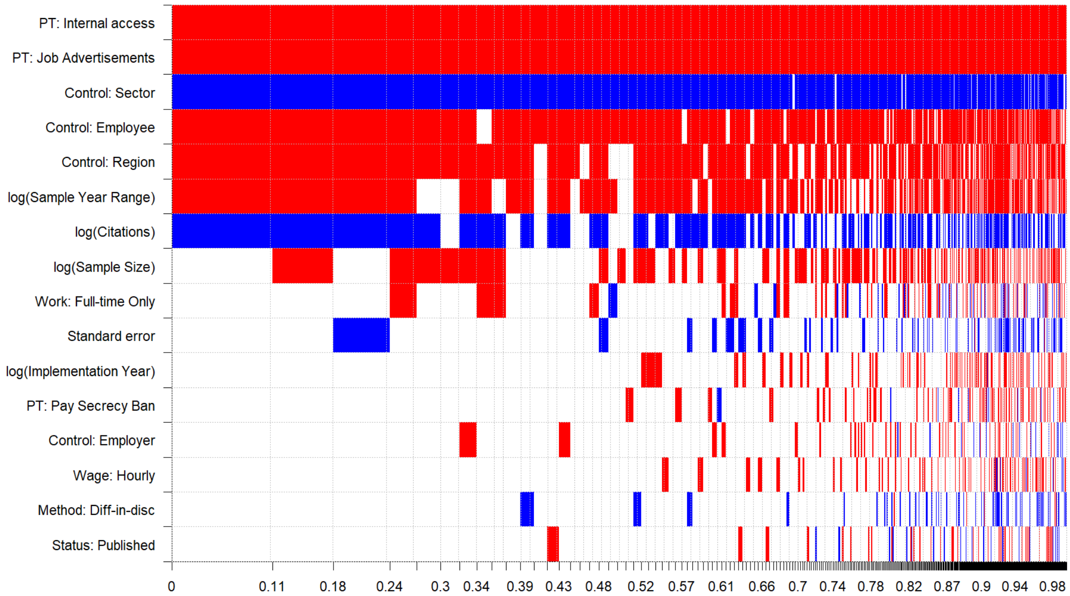
where y_{ij} denotes the log-wage change for women relative to men (higher values = larger gap reduction) and $X_{ij,k}$ is the vector of covariates.

Figure 5 visualizes model inclusion. Table 5 reports BMA (posterior means, SDs, PIPs) and FMA (coefficients, SEs, p-values). Key results are concise:

- **Publication bias proxy (SE).** The standard error has a low PIP, consistent with weak bias from Section 3.

- **PT design dominates.** *Internal access* and *job-ad disclosure* both achieve PIP = 1 with large negative posterior means relative to *public access*. This confirms that weaker or symbolic transparency regimes (e.g., sharing information only internally or posting minimum salaries in ads) are substantially less effective at narrowing the wage gap than full public reporting. The result is consistent across BMA and FMA, underscoring that policy design is the single most important driver of heterogeneity. By contrast, *pay-secrecy bans* remain imprecise and insignificant,

Figure 5: Graphical BMA results



Notes: The figure displays the results of BMA and the variables included in each of the individual models. Blue color indicates that the variable has a positive effect on the dependent variable, while red color indicates that it has a negative effect. The absence of color indicates that the variable was not included in the model. Descriptions of the variables are provided in Table 4.

largely due to the small sample (12 observations) and the influence of one outlier estimate (Chapko, 2024). Importantly, a "lower effect" here means closer to zero (a smaller narrowing), not necessarily a widening of the gap.

- **Data scope.** *Sample year range* emerges as relevant, with a relatively high PIP and a negative posterior mean. This finding is somewhat unexpected, as it implies that studies spanning longer periods tend to report smaller estimated effects of pay transparency on the gender wage gap. A plausible explanation is dilution. Longer windows accumulate time-varying influences (macroeconomic shocks, evolving gender norms, concurrent policy changes) that reduce the contrast between pre- and post-reform periods, attenuating estimated impacts even if transparency has a real effect.

- **Methods.** Difference-in-discontinuities does not differ meaningfully from difference-in-differences.

- **Controls matter.** *Region* and *employee* controls reduce the estimated effect, likely because they absorb variation in wages that might otherwise be attributed to transparency policies (e.g., differences in education or experience across workers, or regional wage differentials). In contrast, *sector* controls increase the effect, since they adjust for women's concentration in lower-paying industries and thereby highlight within-sector wage disparities where transparency is more influential. Put differently, geography and worker-composition controls "soak up" broad structural differences (attenuating the transparency coefficient), whereas sector controls sharpen within-industry comparisons where transparency plausibly bites most (amplifying the coefficient).

- **Study traits.** *Citations* exceed the 0.5 PIP threshold with a positive coefficient, meaning that more highly cited studies report larger effects. This may reflect genuine influence (e.g., higher-quality work or more impactful settings) but could also capture visibility and self-reinforcing citation dynamics. Importantly, publication status itself has low inclusion probability, suggesting that peer-reviewed and working papers do not systematically differ once other factors are controlled for.

Cross-country institutional context likely contributes to these patterns. For example, Nordic systems with strong collective bargaining and baseline transparency may leave less room for additional gains from new mandates, while Anglo-Saxon settings rely more on market discipline and public disclosure to induce change. Future research should explicitly test such institutional moderators (e.g., union coverage, enforcement intensity, legal remedies, cultural attitudes toward pay discussion) to explain remaining cross-study variation.

In Table 5, we present the results from BMA alongside those from FMA. The two approaches are broadly consistent, reinforcing the robustness of the findings. The only notable divergence concerns the *full-time only* variable, which is insignificant in BMA (low PIP) but marginally significant in FMA. This suggests that stronger gains may occur when part-time workers are included, though the evidence remains sensitive to specification. This is consistent with transparency affecting scheduling margins, job mobility, or the composition of hours worked, which are more salient when part-time work is present.

Overall, the heterogeneity analysis shows that the magnitude of transparency effects is shaped primarily by policy design and model specification, with secondary roles for data scope and study prominence. Importantly, our extensive publication bias diagnostics revealed no systematic distortion that would necessitate heavy correction. This means the challenge is not bias in the evidence base, but genuine variation across contexts and empirical choices. In this setting, reporting a single "best-practice" estimate is of limited value. It risks obscuring the conditions under which transparency is more or less effective. Accordingly, we emphasize design-contingent interpretation and transparent reporting of moderators, particularly policy type, controls, and time scope, over a single headline number.

Table 5: Model averaging results

	BMA			FMA		
	Post Mean	**Post SD**	**PIP**	**Coef.**	**SE**	**p-value**
Intercept	0.495	NA	**1.000**	7.487	4.899	0.126
Standard error	0.039	0.103	0.176	-0.030	0.162	0.854
Data characteristics						
log(Sample Size)	-0.001	0.001	0.460	-0.002	0.001	**0.029**
log(Sample Year Range)	-0.004	0.003	**0.758**	-0.006	0.002	**0.004**
Wage: Hourly	0.000	0.001	0.083	-0.004	0.004	0.351
Work: Full-time Only	-0.001	0.003	0.204	-0.009	0.004	**0.046**
Methodology						
Method: Diff-in-disc	0.000	0.002	0.078	0.005	0.006	0.393
PT characteristics						
PT: Internal access	-0.024	0.007	**1.000**	-0.035	0.006	**0.000**
PT: Job Advertisements	-0.037	0.005	**1.000**	-0.044	0.006	**0.000**
PT: Pay Secrecy Ban	0.000	0.002	0.093	0.000	0.009	0.955
log(Implementation Year)	-0.059	0.227	0.113	-0.974	0.644	0.130
Control variables						
Control: Region	-0.009	0.005	**0.845**	-0.015	0.004	**0.000**
Control: Sector	0.009	0.003	**0.983**	0.007	0.003	**0.021**
Control: Employer	0.000	0.001	0.093	0.000	0.003	0.930
Control: Employee	-0.007	0.003	**0.923**	-0.010	0.003	**0.003**
Study characteristics						
Status: Published	0.000	0.001	0.078	-0.005	0.004	0.250
log(Citations)	0.002	0.001	**0.707**	0.004	0.001	**0.001**

Notes: This table displays the results of BMA and FMA. PIP values above 0.5 and p-values below 0.05 are in bold for better clarity. Descriptions of the variables are provided in Table 4. PT = Pay Transparency.

4.1 Policy Implications and Comparative Context

Pay transparency policies appear to modestly narrow gender wage gaps, but their effectiveness depends critically on design and enforcement. Public, comparable, and enforced disclosure regimes produce the largest effects, whereas purely internal or symbolic measures have limited impact. This suggests that transparency works primarily through accountability and reputational pressure rather than individual bargaining alone.

Recent policy developments reinforce this interpretation. The European Parliament and Council (2023) represents a major legislative step toward standardized and enforceable disclosure requirements. It mandates that firms with at least 100 employees report gender pay gaps based on comparable metrics and provides employees with the right to request information about pay levels for equal work. Our findings suggest that such comprehensive and comparable reporting frameworks, when effectively enforced, are likely to yield stronger reductions in pay disparities than systems relying on voluntary or internal disclosure. However, implementation challenges remain substantial, including the harmonization of reporting templates, ensuring data comparability across firms and sectors, and monitoring strategic adaptation by employers (e.g., reclassifying bonuses or allowances to comply formally without addressing structural inequities).

As of late 2025, progress on transposing the Directive remains uneven across Member States. According to Eurofound (2025), only one Member State (Belgium's Fédération Wallonie-Bruxelles) has fully transposed the Directive, while others such as Czechia, Malta, and Poland have partially implemented measures covering pre-employment transparency, salary history bans, or pay-secrecy prohibitions. Many countries, including major economies like Germany, France, and Italy, are still in preparatory stages, drafting legislation or consulting social partners. This fragmented rollout underscores the scale of administrative and political challenges associated with harmonizing reporting standards and enforcement mechanisms across diverse institutional settings.

The new evidence also highlights what makes transparency effective: active disclosure by employers rather than employee-driven requests, public dissemination of pay data to invite external scrutiny, and sufficiently detailed reporting, including breakdowns by occupation, seniority, and variable pay components.These features, identified as most impactful in recent studies, align with our meta-analytic findings that public and comparable disclosure generates the strongest effects on narrowing wage gaps (Eurofound, 2025).

Enforcement mechanisms will determine whether visibility translates into real adjustment. Without credible oversight and sanctions, disclosure may lead to superficial compliance or temporary wage compression rather than structural change. Complementary measures such as pay-equity audits, standardized job evaluation frameworks, and oversight of variable pay components can enhance the effectiveness of transparency laws by converting disclosure into concrete action.

Finally, the external validity of our findings should be interpreted with caution. Nearly all available evidence comes from OECD and high-income countries with well-developed statistical systems and relatively strong legal enforcement. As such, the results may not generalize to lower-income or emerging economies, where labor markets are more informal and gender disparities stem from different institutional constraints. Future research should assess the transferability of transparency-based approaches beyond OECD settings, accounting for variations in data availability, institutional capacity, and social norms around pay communication.

Overall, transparency is not a silver bullet. Its success depends on institutional context, the scope of disclosure, and the credibility of enforcement. The evidence from this meta-analysis indicates that visibility can promote fairness, but only when it is coupled with mechanisms that ensure accountability and equitable adjustment.

5 Conclusion

The gender wage gap remains pervasive even after accounting for education, experience, and occupation (Bishu & Alkadry, 2017; Blau & Kahn, 2017; Boll *et al.*, 2016). Pay transparency has therefore emerged as a prominent policy tool to increase visibility of compensation practices and strengthen workers' bargaining positions (Duchini *et al.*, 2024), yet the literature has lacked a comprehensive quantitative synthesis. This meta-analysis synthesizes 268 estimates from 12 studies to answer a simple question: do pay transparency laws reduce the gender wage gap? We assess publication bias, estimate the underlying effect, and study sources of heterogeneity.

Across a broad suite of publication bias diagnostics, we find at most weak evidence of selective reporting, and the estimated effect beyond bias is small but positive. The pooled unweighted mean equals 0.012 log points (meaning $\approx 1.2\%$ increase in women's wages relative to men), implying a modest but genuine narrowing of the gap on average. Heterogeneity is substantive. Model averaging results show that policy design and specification controls are the dominant drivers of variation in reported effects. Public disclosure regimes are associated with larger reductions than internal access or minimum-salary job-advertisement rules, consistent with the role of sustained external scrutiny. Region and employee controls tend to attenuate estimated effects, while sector controls strengthen them, underscoring how contextual composition and model choices shape magnitudes.

The policy implications are direct. Transparency works best when it is genuinely public, comparable, and subject to continued scrutiny. Policymakers aiming to narrow the gap should prioritize robust public reporting (standardized templates, regular frequency, machine-readable formats), broad coverage (avoiding narrow thresholds that limit scope), and credible enforcement (audits and penalties). The recently adopted European Parliament and Council (2023) reflects this shift, mandating harmonized disclosure formats, employee rights to information, and stronger enforcement mechanisms. Its success will depend on how effectively member states ensure comparability and monitor employer compliance. At the same time, transparency alone is insufficient. Complementary measures, such as pay-equity audits, job-evaluation frameworks, and oversight of bonuses and allowances, are needed to ensure that visibility translates into genuine wage adjustments rather than simple compression. Without credible oversight, disclosure may produce symbolic compliance or temporary narrowing through male wage stagnation rather than sustained equity gains.

The analysis is not without limitations. The evidence base remains small and concentrated in a few institutional contexts, limiting the power of subgroup analyses and external validity. Estimates of pay-secrecy bans are especially imprecise due to few observations and outlier sensitivity. These limitations caution against overgeneralizing headline effects. Nearly all studies come from OECD or other high-income settings with relatively strong legal enforcement and data systems, so generalization to emerging economies remains uncertain. As additional studies accumulate, future meta-analyses will be able to deliver more precise estimates and richer insights into how transparency affects the gender wage gap. Future research should also expand geographic and sectoral coverage, evaluate transparency types separately, and compare enforcement intensity and compliance. Cross-country institutional moderators, such as collective bargaining strength, union coverage, or cultural attitudes toward pay discussion, offer promising directions for explaining residual heterogeneity. Tracing mechanisms, such as whether observed narrowing arises from women's wage growth, wage compression, or bargaining responses, will also be essential. Moreover, studies should report standardized outcomes and full precision statistics to facilitate cumulative evidence synthesis, and adopt designs that credibly address policy endogeneity, such as event-study difference-in-differences with pre-trend diagnostics, discontinuities at coverage thresholds, or randomized pilots where feasible.

In sum, the best available evidence indicates that pay transparency can modestly reduce the gender wage gap, and that design details matter: public disclosure with accountability outperforms limited or symbolic transparency. Building stronger enforcement, harmonized reporting, and more comparable evaluations will be essential for turning transparency from visibility into verifiable equality.

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Appendix

A Studies & Data Included in the Meta-analysis

Table A1: Studies included in the meta-analysis

Baker *et al.* (2023)	Bamieh & Ziegler (2025)	Bennedsen *et al.* (2022)
Blundell (2021)	Blundell *et al.* (2025)	Brütt & Yuan (2023)
Burn & Kettler (2019)	Chapko (2024)	Frimmel *et al.* (2024)
Gamage *et al.* (2024)	Gulyas *et al.* (2023)	Obloj & Zenger (2022)

Notes: This table lists the citations of all primary studies included in our meta-analysis.

Figure A1: PRISMA diagram

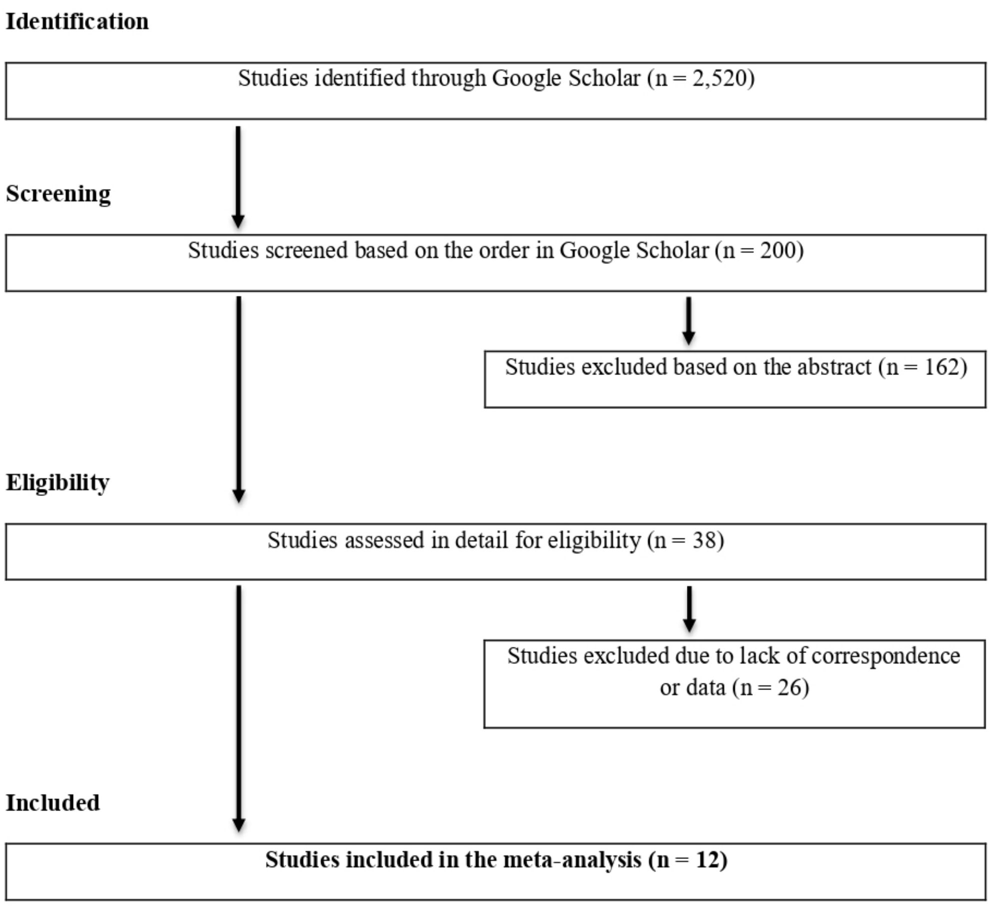


Table A2: Pay transparency law by country

Country	Internal access	Job advertisements	Pay secrecy ban	Public access	Total
Austria	20	41	0	0	61
Canada	0	0	0	64	64
Denmark	21	0	0	0	21
Germany	32	0	0	0	32
United Kingdom	0	0	0	60	60
United States	0	0	12	18	30
Total	**73**	**41**	**12**	**142**	**268**

Notes: Each cell reports the number of estimates drawn from studies examining the specified pay transparency type within each country. Public access includes mandatory disclosure of firm- or institution-level gender wage gaps; Internal access refers to reporting limited to employees or works councils; Job advertisements indicate mandatory pay ranges in postings; and Pay secrecy ban prohibits employers from restricting pay discussions.

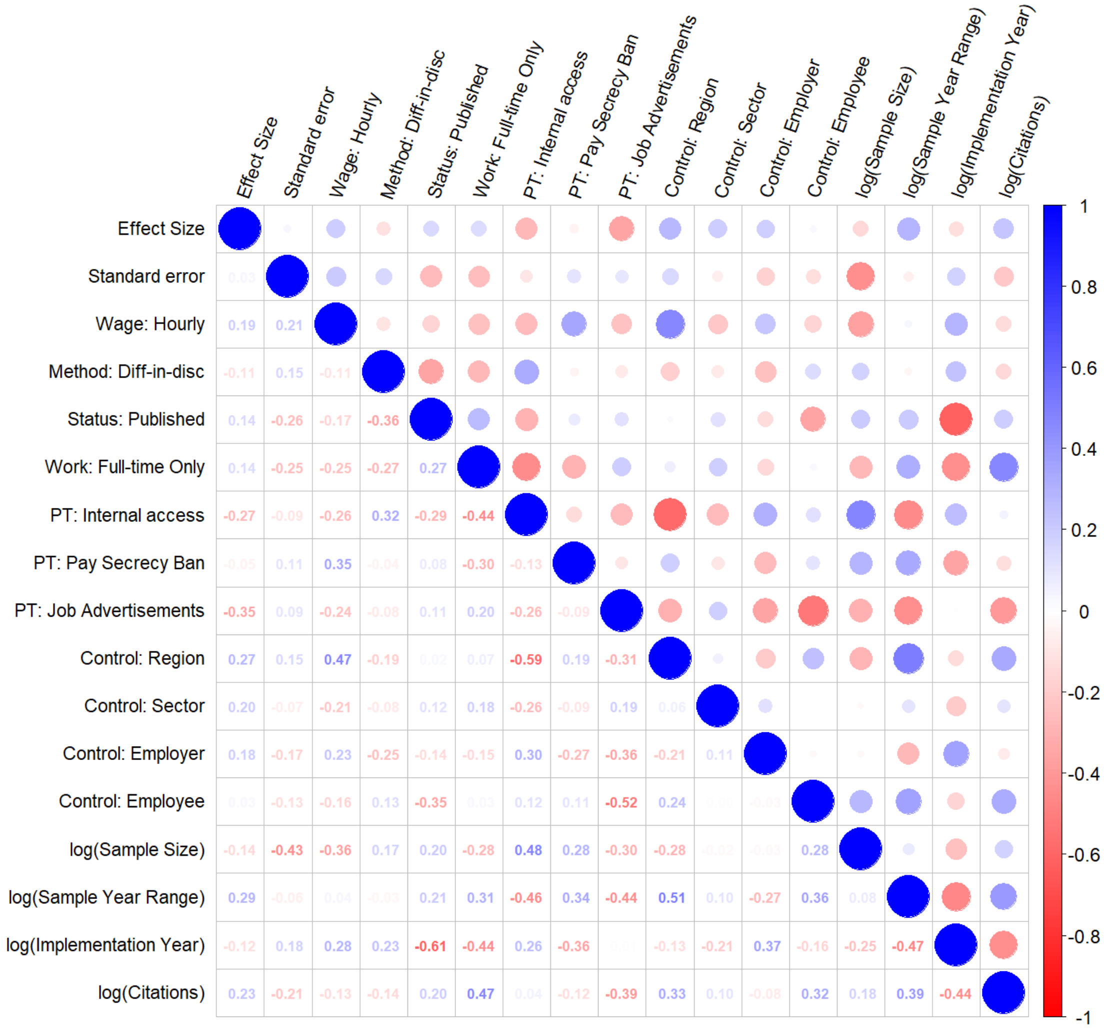
B Model Averaging Details and Robustness Checks

Table B1: VIFs of explanatory variables used in BMA

Variable	**VIF**
Standard Error	1.935
log(Sample Size)	4.310
Method: Diff-in-disc	2.006
Wage: Hourly	5.031
Published	4.290
log(Citations)	4.815
log(Sample Year Range)	4.971
log(Implementation Year)	5.313
Full-time Only	6.302
PT: Internal access	9.200
PT: Pay Secrecy Ban	4.956
PT: Job Advertisements	7.066
Control: Region	5.946
Control: Sector	1.957
Control: Employer	4.088
Control: Employee	3.196

Notes: This table displays the variance inflation factors (VIFs) for all explanatory variables used in the model averaging. Descriptions of the variables are provided in Table 4.

Figure B1: Correlation matrix of the variables used in BMA



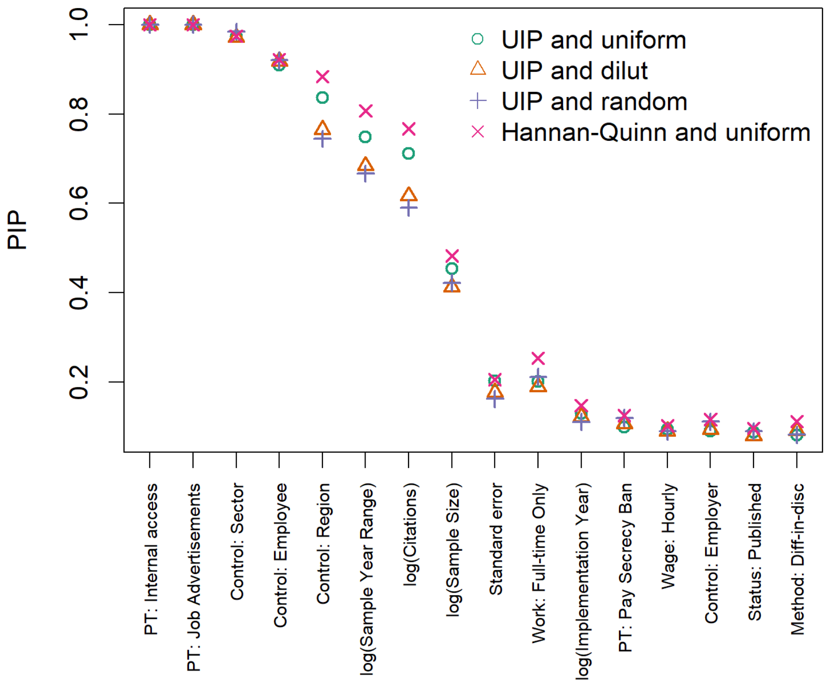
Notes: This figure shows the correlation coefficients of all pairs of variables used in BMA. Blue color indicates a positive correlation, while brown color indicates a negative correlation. The shade of the color depicts the strength of the correlation with darker shades indicating stronger correlation. The strongest correlation among them is only 0.61 in absolute value, which is only a mild correlation that should not pose a problem for model averaging. Descriptions of the variables are provided in Table 4.

Table B2: Results of different BMA specifications

	Dilut			Random			HQ		
	Mean	**SD**	**PIP**	**Mean**	**SD**	**PIP**	**Mean**	**SD**	**PIP**
Intercept	0.488	NA	1.000	0.488	NA	1.000	0.598	NA	1.000
Standard error	0.032	0.094	0.150	0.031	0.094	0.148	0.044	0.108	0.200
Data characteristics									
log(Sample Size)	-0.001	0.001	0.420	-0.001	0.001	0.422	-0.001	0.001	0.508
log(Sample Year Range)	-0.003	0.003	0.664	-0.003	0.003	0.664	-0.004	0.002	0.814
Wage: Hourly	0.000	0.001	0.081	0.000	0.001	0.081	0.000	0.001	0.093
Work: Full-time Only	-0.001	0.003	0.193	-0.001	0.003	0.197	-0.001	0.004	0.242
Methodology									
Method: Diff-in-disc	0.000	0.002	0.074	0.000	0.002	0.075	0.000	0.002	0.097
PT characteristics									
PT: Internal access	-0.023	0.007	1.000	-0.023	0.007	0.999	-0.025	0.007	1.000
PT: Job Advertisements	-0.036	0.006	1.000	-0.036	0.006	1.000	-0.038	0.005	1.000
PT: Pay Secrecy Ban	0.000	0.003	0.096	0.000	0.003	0.099	0.000	0.002	0.099
log(Implementation Year)	-0.059	0.227	0.110	-0.059	0.226	0.110	-0.073	0.252	0.137
Control variables									
Control: Region	-0.008	0.006	0.738	-0.008	0.006	0.737	-0.010	0.005	0.894
Control: Sector	0.009	0.003	0.981	0.009	0.003	0.983	0.009	0.003	0.984
Control: Employer	0.000	0.001	0.091	0.000	0.001	0.093	0.000	0.001	0.118
Control: Employee	-0.007	0.003	0.931	-0.007	0.003	0.929	-0.007	0.003	0.924
Study characteristics									
Status: Published	0.000	0.001	0.074	0.000	0.001	0.077	0.000	0.001	0.094
log(Citations)	0.002	0.002	0.599	0.002	0.002	0.597	0.002	0.001	0.776

Notes: This table displays the results of BMA models with different prior setups and serves as a robustness check of the original BMA model results. The columns labeled Dilut present the UIP g-prior and dilut model prior setup, the columns labeled Random present the UIP g-prior and random model prior setup, and the columns labeled HQ present the Hannan-Quinn g-prior and uniform model prior setup. Descriptions of the variables are provided in Table 4. PT = Pay Transparency.

Figure B2: Comparison of PIPs across different BMA specifications



Notes: This figure shows the PIPs of variables of all BMA models with distinct prior setups for clear comparison. The PIP values are on the vertical axis, while the columns correspond to each of the covariates included in the BMA. Each of the colored shapes represents a different BMA setup. The green circle represents the main setup, which is UIP g-prior and uniform model prior, the orange triangle represents the UIP g-prior and dilut model prior, the blue plus represents the UIP g-prior and random model prior, and the red cross represents the Hannan-Quinn g-prior and uniform model prior. Descriptions of the variables are provided in Table 4. PT = Pay Transparency.

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Univerzita Karlova v Praze, Fakulta sociálních věd
Institut ekonomických studií [UK FSV – IES] Praha 1, Opletalova 26
E-mail : ies@fsv.cuni.cz http://ies.fsv.cuni.cz