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The cyclicality of hiring discrimination*

A meta-reanalysis of correspondence experiments

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ABSTRACT I reanalyse over two decades' worth of correspondence experiments – representing over 1.4 million applications – to test whether hiring discrimination varies over the labour market cycle, for which groups, and where. To strengthen causal identification, I introduce the meta-analytic event study method, which integrates dynamic treatment effect estimation within a meta-regression framework. The results suggest that discrimination rises when unemployment is high. By group, meta-analytic empirical evidence of countercyclicality is consistent for racial and ethnic minorities in Western Europe and for older workers, but mixed for sexual and gender minorities. In contrast, I detect no systematic cyclicality in discrimination for racial and gender minorities in North America. The results reconcile mixed findings from the present empirical literature, often covering single countries, single groups, and restricted timeframes. From a policy perspective, I argue that anti-discrimination efforts are most needed in slack labour markets, when discriminatory hiring typically intensifies.

Keywords: Labour market cycle, Hiring discrimination, Correspondence experiments, Meta-analysis
JEL Codes: J23, J63, J64, J71

1 Introduction

Discrimination in hiring is a well-documented and enduring phenomenon (Batinovic et al., 2023; Flage, 2020; Galos & Coppock, 2023; Lippens et al., 2023; 2025; Quillian et al., 2017; Quillian & Lee, 2023; Schaerer et al., 2023). It occurs when employers treat equally productive applicants

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differently based on group characteristics such as race, gender, or age. Hiring discrimination reduces opportunities for affected groups and contributes to persistent penalties in employment outcomes (Kuhn & Chanci, 2024; Quillian et al., 2020; Zwysen et al., 2021). Several meta-studies of correspondence experiments concern the question of whether hiring discrimination has changed in the past decades (Lippens et al., 2023; 2025; Quillian et al., 2017; Quillian & Lee, 2023; Schaerer et al., 2023).[1] A linear approach to estimating such temporal change generally reveals neither improvement nor deterioration.[2] This paper evaluates the proposition that hiring discrimination follows a non-linear path tied to the labour market cycle.

The seminal theory of taste-based discrimination provides a rationale for the proposition that discrimination follows a countercyclical pattern (Becker, 1957; Lang & Spitzer, 2020). In Becker's model, prejudice inflates the perceived cost of hiring minority applicants. Improved labour market conditions can increase this cost. More specifically, in tight labour markets, when labour is scarce and competition for workers is fierce, employers are pushed to hire the best available candidate. In contrast, in slack labour markets, the cost of discrimination is negligible, and discrimination intensifies. Based on the empirical literature, the impact of the labour market cycle on hiring discrimination appears somewhat mixed. Most studies find a countercyclical effect, where a tight labour market with many vacancies and few job seekers leads to decreased discrimination (Challe, 2017; Challe et al., 2023; Dahl & Knepper, 2023; Drydakis, 2022; Inafuku, 2023; Kuhn & Chanci, 2024). Conversely, some evidence points towards a null effect or even a procyclical effect, mostly concerning ethnic and racial minorities (Bursell, 2014; Button & Walker, 2020; Carlsson et al., 2018; Kingston et al., 2015; Quillian et al., 2019; Zschirnt & Ruedin, 2016).

Unwrapping whether hiring discrimination is countercyclical is important for two related reasons. First, ignoring cyclical patterns in discrimination, e.g., by only measuring the phenomenon at the cross-section, can mask discriminatory tastes; I potentially overestimate discriminatory preferences of the average employer during busts and underestimate these during booms. Changes in the macroeconomic cycle can make it difficult to detect true discrimination (Button & Walker, 2020; Neumark & Button, 2014). The analytic approach can produce estimates of hiring discrimination net of labour market cyclicality. Second, if hiring discrimination is countercyclical, policymakers should increase efforts to counter it during busts. Following the Great Recession, for example, when hiring discrimination was likely at a peak, inequality bodies across the European Union reported considerable budget cuts, which undermined their anti-discrimination efforts (Equinet, 2012). Instead, if discrimination is countercyclical, policymakers should have ramped up anti-discrimination enforcement to attenuate discriminatory hiring practices.

[1]Correspondence experiments are experiments in which personal traits based on which disparate treatment is typically prohibited are randomly assigned to fictitious applicants and differences in employer callbacks are measured. They provide a way to causally identify discrimination in the first stage of the hiring process.

[2]Exceptions include (i) declining hiring discrimination against Latino applicants in the US (Quillian et al., 2017), (ii) heightened discrimination against applicants of Middle Eastern and Northern African origin in the 2000s compared to the 1990s across six OECD countries and declining racial and ethnic discrimination in France, while it rose in the Netherlands (Quillian & Lee, 2023), and (iii) increased discrimination against men in mixed-gender and male-stereotypical following the global financial crisis (Schaerer et al., 2023).

Prior standalone research has investigated the cyclicality of hiring discrimination through varying methods. For example, some empirical studies have examined whether hiring discrimination varies with vacancy tightness or posting duration by comparing group differences in callback rates for recently posted vacancies versus those open for longer (Baert et al., 2015; Carlsson et al., 2018). A different approach relies on linking changes in unemployment to micro-level differences in callback ratios and employment outcomes (Dahl & Knepper, 2023; Kuhn & Chanci, 2024). Others have exploited external market shocks, such as the Great Recession or the COVID-19 crisis, to assess how recessionary conditions, with relatively high numbers of jobseekers relative to outstanding job vacancies, affected hiring biases (Challe, 2017; Challe et al., 2023; Neumark & Button, 2014). While these studies provide valuable insights, they often rely on single-country analyses particular minority groups, or restricted time windows, limiting their generalisability.

I rely on a meta-reanalysis approach to examine the cyclical nature of hiring discrimination. I systematically identified conditional average treatment effects (CATEs) from existing correspondence audits and linked them to various labour market cycle indicators.[3] Through meta-regression techniques, I explain between-study variation in these conditional estimates in terms of labour market cyclicality (Stanley & Jarrell, 2005; Stanley, 2001). Specifically, I match administrative measures of cyclicality to hiring discrimination outcomes from correspondence experiments conducted in the past two decades.[4] These discrimination estimates represent roughly 1.4 million fictitious applications covering fifteen discrimination grounds (e.g., ethnicity, gender, age) across occupations, industries, and locations. The comprehensive evaluation enhances external validity and reconciles mixed findings in the literature. In addition, I leverage temporal variation in hiring discrimination to introduce the meta-analytic event study method, which calculates dynamic treatment effects within a meta-regression framework. This method allows us to establish a causal temporal link while simultaneously addressing publication selection bias, which is important in a meta-analytical context as the publication process overvalues often selectively reported just-significant results (Askarov et al., 2024; Brodeur et al., 2016).

The findings of the meta-reanalysis suggest that hiring discrimination moves countercyclically, especially for racial, ethnic, and age minority groups. Conversely, the meta-analytic evidence for sex and gender groups is weak, and hiring discrimination does not appear cyclical in North America. I predict that, in an efficient labour market, where jobseekers and vacancies are perfectly in balance, racial and ethnic hiring discrimination in Western Europe would, on average, almost disappear and be significantly reduced for older workers globally.

The remainder of the paper is structured as follows. Section 2 provides a brief overview of present empirical evidence on the cyclicality of hiring discrimination and a theoretical rationale for these findings. Section 3 outlines the data collection of hiring discrimination estimates and cyclicality measures, while Section 4 describes the methodological approach, paying specific attention to the identification strategy, the meta-regression estimation framework, and the

[3]I borrow the term *meta-reanalysis* and its meaning from Galos & Coppock (2023), who re-evaluated the relationship between occupational gender composition and hiring discrimination based on existing correspondence studies.

[4]The systematic review and meta-reanalysis were preregistered on the Open Science Framework (OSF) using the generalised systematic review registration template (van den Akker et al., 2023).

novel meta-analytic event study application. Section 5 answers the questions of whether hiring discrimination is responsive to labour market cycle fluctuations, whether these results differ by discrimination ground, treatment group, or region, and if hiring discrimination survives if the labour market operated efficiently, balancing jobseekers and vacancies. Section 6 concludes with a brief summary of the study's results and limitations, along with policy implications and ideas for future research.

2 Background

Economic theory provides a framework for understanding how employer discrimination could vary across the labour market cycle. Taste-based discrimination theory posits that employers with a taste or preference for discrimination incur costs that are more difficult to absorb in competitive, thriving markets (Becker, 1971). Their preferences lead them to overpay candidates of the desired group and forgo qualified candidates of the undesired group, increasing search costs and ultimately reducing firm profitability in a prosperous labour market (Lang & Lehmann, 2012). The search model of Biddle & Hamermesh (2013) supports the idea that the cost of prejudice is procyclical, leading to more discrimination in recessionary periods – in other words, discrimination is countercyclical. During economic booms, rejecting qualified workers from the undesired group is more costly because finding replacements is harder, and, therefore, more expensive. In contrast, during busts, the pool of job seekers is large and job openings are few, reducing the opportunity cost to discriminate. Employers more easily find alternative candidates from the desired group, so they can be 'picky' about whom to hire without raising search costs and reducing profitability as much.[5]

Conversely, statistical discrimination theory, which asserts that employers rely on group-level statistics as proxies for individual productivity in the absence of complete information, is less clear about the direction of the cyclicality effect. On the one hand, the theory suggests that discrimination can perpetuate or even intensify in spite of competition for scarce labour (Arrow, 1972; 1973; Phelps, 1972). Employers who at least believe their proxies are correct will be less inclined to hire from the (believed) least productive group as it makes economic sense to discriminate against applicants from a group with (perceived) subpar productivity potential (Bohren et al., 2023; Ruzzier & Woo, 2023). This tendency remains valid when employers face increased competition from other employers, i.e. when unemployment rates drop and vacancy rates rise. Statistical discrimination can even be distinctly procyclical if higher uncertainty about the productivity of a particular group leads employers to believe that only low-productive job seekers remain in that group when the talent pool dries up (Baert et al., 2015; Carlsson et al., 2018).[6] On the other hand, employers who do not update outdated group-based productivity beliefs in a timely manner following compositional changes among certain labour market groups (e.g., more educated women entering the labour market) may still hire from the 'wrong group' (Campos-

[5]When employers react to the preferences of their coworkers or customers, it can make economic sense to refrain from hiring minority candidates, even in recessionary periods, because, otherwise, employers could lose valuable personnel or business (Borjas, 2020; Coleman, 2004). Discrimination should not be cyclical in those cases.

[6]This premise aligns with employer screening models, which imply extended job seeking in tight labour markets signals low productivity (Kroft et al., 2013; Lockwood, 1991; Vishwanath, 1989).

Mercade & Mengel, 2024). This practice causes them to face stronger negative consequences from discriminating, especially when competition for scarce talent is fierce. Consequently, statistical discrimination can also be countercyclical.[7]

Several researchers find empirical evidence for countercyclical patterns of hiring discrimination. In earlier work based on a Belgian correspondence audit, Baert et al. (2015) uncovered that applicants with a foreign-sounding name faced more discrimination when applying for shortage occupations for which vacancies were difficult to fill.[8] Dahl & Knepper (2023) reused data from previous US correspondence studies and found that callback rates for older women responded more negatively to local unemployment rates than those for younger women.[9] Challe et al. (2023) examined hiring discrimination based on national origin and place of residence across five waves of correspondence audits in France. Their analysis revealed that discrimination sharply rose with worsening labour market conditions during the COVID-19 crisis. Similarly, the reanalysis of three correspondence experiments in the Greek labour market by Drydakis (2022) attributed the increased discrimination against gay men in later experiments to severe increases in unemployment rates. Finally, Chavez et al. (2022) find higher callbacks for Black and White women right after the COVID-19 pandemic's initial lockdown, when the labour market recovered and surged, compared to pre-pandemic figures.

The most notable example of empirical work finding procyclical hiring discrimination is that of Carlsson et al. (2018). Based on a Swedish audit study, they found that ethnic hiring discrimination increased with the occupation-specific female callback rate, a tightness measure they recycled from a different Swedish audit study that ran during a similar time. Yet, they rightfully note this measure is likely endogenous if the tested employers prefer women (without an ethnic background) over ethnic minorities. If the callback rate for women is high, it is then evident to observe a lower callback rate for Middle Eastern applicants. Alternatively, they found that as the vacancy–unemployment ratio increased, hiring discrimination against ethnic minorities decreased, albeit marginally significantly. The authors assert that a screening model, where the negative signal of the minority status increases with a tighter labour market, can best account for the procyclical pattern of discrimination.

A non-trivial part of the empirical literature does not detect robust links between measures of cyclicality and hiring discrimination. Most closely related to this study is the research of Quillian et al. (2019), who present a meta-regression analysis of ethnic hiring discrimination across nine OECD countries. Although not the focus of their analysis, they estimated a very small, insignificant coefficient for the local unemployment rate, which aligns with the results of the simple

[7]During economic busts, when the talent pool is broad, employers could have less incentive to screen individuals, relying on group-based productivity signals such as age instead and, thus, exert countercyclical discrimination (Dahl & Knepper, 2023).

[8]The shortage status of an occupation was based on (i) the vacancy-filling rate, which should have been lower than the median vacancy-filling rate for all occupations, and (ii) the median vacancy duration, which should have been higher than the median for all occupations.

[9]Relatedly, Kuhn & Chanci (2024) formally model the effect of unemployment on racial hiring discrimination using an average discrimination estimate derived from several US audit studies. They use a standard search-and-matching model à la Blanchard & Diamond (1994), which reproduces a widening of the Black–White unemployment gap to the disadvantage of Black workers as the local unemployment rate rises.

correlational approach of Zschirnt & Ruedin (2016). Furthermore, Kline et al. (2022) find no evidence that racial, gender, or age discrimination gaps varied significantly over their large-sample, year-long US audit study. Their experiment ran just before the first lockdown of the COVID-19 pandemic and resumed the following summer during a labour market uptake. Another example is the correspondence audit of Button & Walker (2020), which revealed very minimal evidence of ethnic hiring discrimination. Because their audit took place during a period of relatively low unemployment, they assessed a cross-sectional relationship between local unemployment and discrimination but found no evidence for this link.[10]

The study of the cyclicality of hiring discrimination relates to four tangent literatures. The first literature links labour market cyclicality to employment outcomes for different groups, such as racial minorities, immigrants, and obese workers (Boulware & Kuttner, 2024; Couch & Fairlie, 2010; Dustmann et al., 2010; Inafuku, 2023; Neumark & Button, 2014). Second, several studies consider competition in the product or service market (e.g. industry concentration, deregulation) and group employment differences (Ashenfelter & Hannan, 1986; Black & Strahan, 2001; Cooke et al., 2019; Hellerstein et al., 2002; Heyman et al., 2013; Heywood & Peoples, 1994; Popov & Zaharia, 2019; Weber & Zulehner, 2014). The third literature, and possibly the largest, looks at product or service market competition and group wage differences (Agesa & Hamilton, 2004; Berik et al., 2004; Black & Brainerd, 2004; Black & Strahan, 2001; Chattopadhyay & Bianchi, 2021; Coleman, 2004; Cooke et al., 2019; Cymrot, 1985; Deschamps & De Sousa, 2021; Dodini & Willén, 2025; Fays et al., 2020; Hellerstein et al., 2002; Heyman et al., 2013; Johnston & Lordan, 2016; Peoples & Saunders, 1993; Peoples & Talley, 2001; Weichselbaumer & Winter-Ebmer, 2007; Winter-Ebmer, 1995). The fourth, emerging literature evaluates the effect of product or service market competition on group differences in layoffs (Auer, 2022; Boulware & Kuttner, 2024; Couch & Fairlie, 2010; Dahl & Knepper, 2023; Neumark & Button, 2014). Most of these studies find evidence that, when markets expand or competition between employers increases, gaps in labour market outcomes narrow or discrimination weakens.

3 Data

3.1 Hiring discrimination

I gathered 3,418 conditional average treatment effects (CATEs) of hiring discrimination from 283 correspondence audit studies. These CATEs consist of discrimination estimates by various applicant and study characteristics, country, industry, and occupation. I retrieved the estimates based on a systematic and extensive search for field experiments that examine hiring discrimination through a correspondence audit approach. Following this search, I screened these studies for eligibility based on a predefined set of inclusion criteria and extracted the necessary metadata. I used these discrimination estimates as a dependent variable in the meta-regression analyses (see Section 4.2).

[10] In the same vein, Kingston et al. (2015) found no robust evidence that the recessionary conditions around 2010 in Ireland influenced self-reported work-based discrimination of non-Irish nationals relative to 2004, when labour market conditions were more favourable.

3.1.1 Study search

The literature search strategy involved systematically querying several academic databases and repositories to identify relevant audit studies. Specifically, I consulted the following databases: Web of Science (including the Web of Science Core Collection and ProQuest™ Dissertations & Theses Citation Index), SSRN, IZA (Discussion Paper Series), NBER (Working Papers), CEPR (Discussion Papers), ArXiv, PsyArXiv, and SocArXiv. Searching these databases ensured a comprehensive, multidisciplinary coverage of both peer-reviewed literature and grey literature, such as preprints, working papers, discussion papers, and theses.

I adhered to a structured inclusion and exclusion framework based on an adapted version of the commonly used PICO criteria (see Table A.1 in the appendix). Eligible studies were based on the correspondence experiment method, characterised by the experimental manipulation of applicant characteristics of interest (e.g., race), comparing employer responses of the treated group to those of an appropriate control group. Included studies specifically measured legally prohibited unequal treatment translatable into binary responses in a hiring and selection context (e.g., interview invitations, requests for additional information, general interest). I considered sixteen legally prohibited grounds and retrieved sufficient observations for fifteen of these to include in the analyses. The review period covered studies published between 2000 and 2024. Keywords comprised methodological terms (e.g., "correspondence test"), discrimination terms (e.g., "bias"), and ground-specific terms (e.g., "Arab").

To ensure comprehensiveness and assess the validity of the search strategy, I compared the initial search results against references listed in the recent meta-analysis of Lippens et al. (2023) covering the same discrimination grounds. I expected and found substantial overlap between the identified studies and those included in said meta-analysis – all studies from Lippens et al. (2023) appeared in the database searches. Additionally, I complemented the database searches by examining references cited in previous systematic reviews and meta-analyses on hiring discrimination relying on correspondence experiments (Bartkoski et al., 2018; Batinovic et al., 2023; Flage, 2020; Gaddis et al., 2021; Gallo et al., 2024; Galos & Coppock, 2023; Galvan et al., 2022; Habicht et al., 2025; Heath & Di Stasio, 2019; Park & Oh, 2025; Quillian et al., 2017; 2019; 2020; Rich, 2018; Schaerer et al., 2023; Schwitter et al., 2025; Thijssen et al., 2021; Zschirnt & Ruedin, 2016).

3.1.2 Study screening

Study screening was conducted through a systematic, multi-stage process. Initially, articles identified through the database searches underwent a first-round screening based primarily on titles and abstracts. At least two independent human reviewers conducted this step. Following this initial round, reports of eligible studies underwent full-text screening in a second stage. A similar approach applied here, with the lead screener verifying and resolving discrepancies between their classifications and those of other screeners.

During the screening process, I blinded inclusion decisions, but not bibliographic information. Instead, fields such as authors, publication years, journal titles, and abstracts remained visible to all screeners, facilitating decision-making and reducing needles extensive full-text assessments. I implemented automated deduplication via rayyan.ai at the outset of the screening stage, systematically removing identical records identified across different databases. Eventually, a total of 343 studies that successfully met the inclusion criteria were retained to extract their metadata.

3.1.3 Data extraction

Data extraction involved retrieving general study metadata, methodological information, and count data to establish hiring discrimination estimates. Extractors followed instructions and variable definitions detailed in a standardised data dictionary to ensure consistency. First, extractors independently retrieved relevant general study characteristics from the included reports. Extracted metadata encompassed bibliographic information, such as author names, digital object identifiers (DOI), and peer-review statuses. These metadata also comprised precise locations of relevant count data within each report (e.g., page numbers, table and figure identifiers, and appendices). Second, extractors captured study design and methodological information, including candidate demographics (i.e., gender, age, education level, migrant generation, and (un)employment status and duration), details of the experimental design, including occupation, industry, country, and applicant matching (i.e., whether authors tested employers multiple times with different applicant profiles). Last, extractors stored metadata concerning discriminatory treatment, including the specific grounds of discrimination, labels and descriptions of treatment and control groups, the number of applications sent for each group, and the number of callbacks. I also documented the definitions of callbacks; broad measures of positive employer responses were prioritised over narrow measures (such as interview invitations) to maximise informational value. Table A.2 , Table A.3 , Table A.4 , and Table A.5 provide summary statistics of the meta-data. I present hiring discrimination estimates by treatment group in Section 5.1

In several cases, multiple extractors verified each other's work, identifying and resolving any discrepancies. This verification process involved revisiting the original study sources to validate data accuracy, especially callback counts and classification decisions. Disagreements were resolved through discussion and mutual consensus between extractors. For 1,806 estimates (i.e., 54.2% of total), only one person extracted count data, although in 1,690 of those cases (i.e., 50.7% of total), the sole extractor relied on the authors' original data or a replication package to calculate positive response ratios. Two extractors verified the metadata for 870 estimates (i.e., 26.1% of total), three for 582 estimates (i.e., 17.5% of total), and four for 76 estimates (i.e., 2.3% of total).

3.2 Labour market cyclicality

I derived labour market cyclicality measures from several administrative data sources. Unadjusted quarterly country-level unemployment rates were taken from the International Labour Organization's (ILO) ILOSTAT database, focusing on individuals aged 15 and above to maximise the number of estimates and ensure comparability across countries. For the United States, I additionally built a state-level measure from the Bureau of Labor Statistics' (BLS) Local Area Unemployment Statistics (LAUS), which is restricted to people aged 16 and above. Unemployment rates are defined as the number of jobless labour force participants currently available and seeking work over the total number of labour force participants - see Equation (1) with u, jobseekers, and e, labour force participants (employed and unemployed). To assign unemployment rates to the hiring discrimination observations from the meta-dataset (see Section 3.1), I averaged across all quarterly unemployment rates that fall within each correspondence audit study's fieldwork window, i.e., a so-called fuzzy join.

$$\mathrm{U} = \frac{u}{e + u} \tag{1}$$

In parallel, I sourced unadjusted quarterly vacancy rates.[11] For the United States, I used the national vacancy rate series from the Job Openings and Labor Turnover Survey (JOLTS). For Canada, I used Statistics Canada's national job vacancy rates. Last, for European countries, I used Eurostat's job vacancy rate series for all Nomenclature statistique des activités économiques dans la Communauté européenne (NACE) activities under NACE's second revision, falling back to the closest aggregate under NACE's first revision if the former was unavailable. These statistics are calculated as the number of job opening (US) or vacancies (EU, Canada) divided by the number of labour force participants – see Equation (2) , with v, openings or vacancies, and e, employed individuals (US) or the number of occupied positions (EU, Canada). Where occupation information is available, I additionally linked Eurostat's vacancy rates by International Standard Classification of Occupations-08 (ISCO-08) major occupation group to the audit study's ISCO-coded occupations. Similar to the unemployment rates, I joined vacancy rates and hiring discrimination observations after averaging the former over each study's fieldwork window. After joining, the maximum sample size in the aggregate analyses was 3293.

$$\mathrm{V} = \frac{v}{e + v} \tag{2}$$

I complemented these raw cyclicality measures with two derivatives of the linked unemployment–vacancy pair computed over each study's fieldwork window: (i) labour market tightness – see Equation (3) and (ii) the deviation between the unemployment rate and the full-employment rate of unemployment (FERU) – see Equation (4) .[12] These measures were constructed based on the national series as well as the US state unemployment measure and the European occupation-specific vacancy rates.

$$\theta = \frac{\mathrm{V}}{\mathrm{U}} \tag{3}$$

$$\mathrm{U}^* = \mathrm{U} - \sqrt{\mathrm{UV}} \tag{4}$$

The deviation between the unemployment rate and the FERU in Equation (4) offers useful cross-country cyclicality comparisons (Michaillat & Saez, 2024). Assuming the Beveridge curve is approximately rectangular-hyperbolic, i.e. $\mathrm{UV} \approx \text{const}$, full employment corresponds to $\mathrm{U} = \mathrm{V}$. Thus, when $\mathrm{U} = \mathrm{U}^*$, the labour market is perfectly balanced or 'socially efficient' with labour supply more or less covering labour demand. Relatedly, the labour market is inefficiently slack when $\mathrm{U} - \mathrm{U}^* > 0$ and inefficiently tight when $\mathrm{U} - \mathrm{U}^* < 0$. In practice, the FERU is simple

[11] I limited vacancy rate data to North America and Europe, as these regions comprise the largest part of the hiring discrimination estimates (i.e., 92.22%) and statistics offices of the included countries readily provide these statistics on a quarterly basis.

[12] The FERU is the geometric mean of the vacancy and unemployment rates.

to compute, relatively stable over long periods, and policy-relevant because it provides a full-employment benchmark (Michaillat & Saez, 2024). I therefore use $U - U^*$, alongside V/U, V, and U as the preferred measures of labour market cyclicality. For reference, Figure 1 shows regional and country trends for each measure.

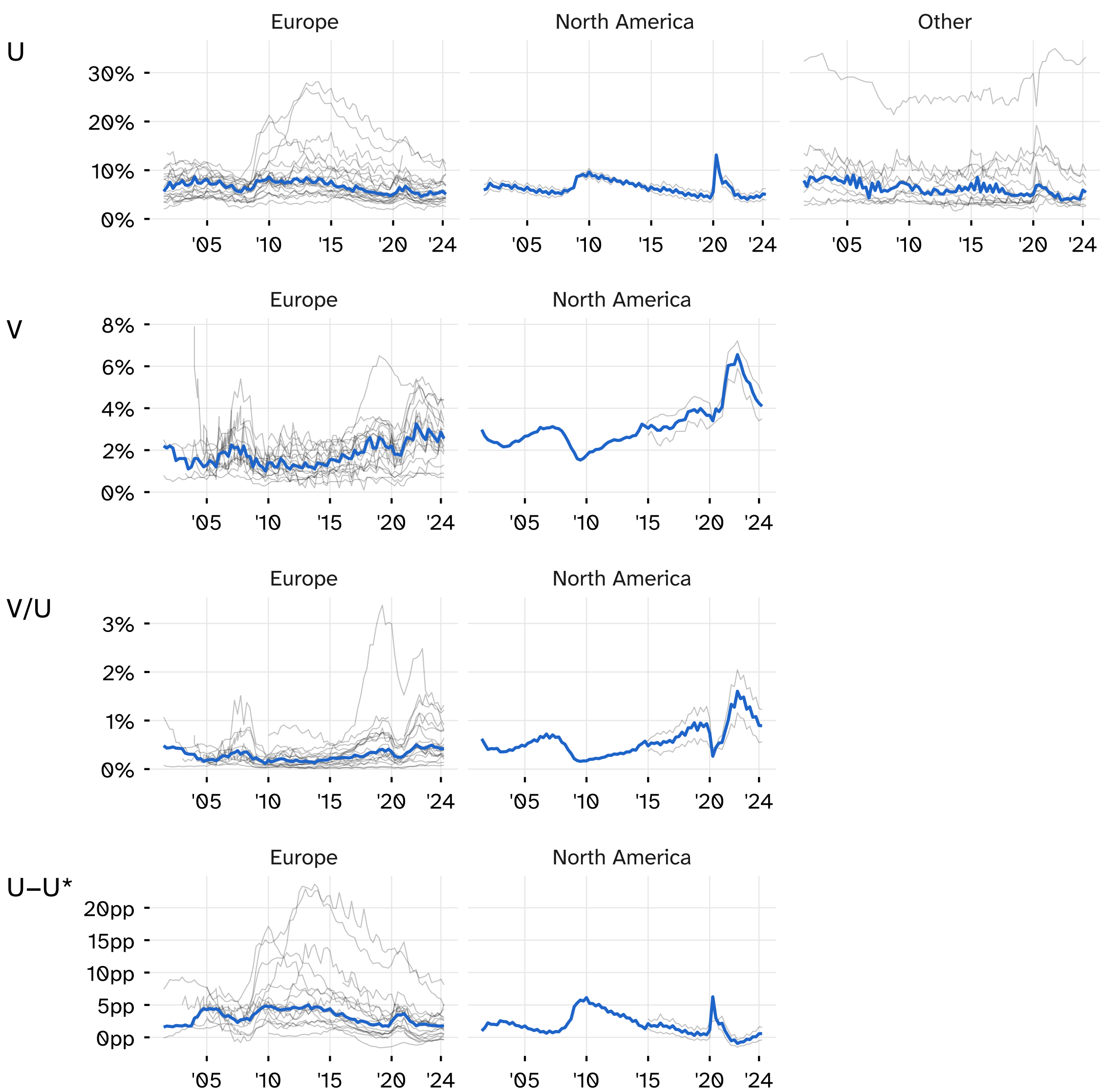

Figure 1: Unemployment and vacancy rates, tightness, and the full-employment rate of unemployment (FERU) gap by region. Each row plots a different cyclicality measure: U (unemployment rate), V (vacancy rate), V/U (vacancy-to-unemployment ratio), and $U - U^*$ (deviation between the unemployment rate and the FERU) or efficiency gap. The solid blue line depicts the cross-country median within each region. Thin grey lines are country series. Time series are restricted to the countries and quarters overlapping the fieldwork of the correspondence audit studies.

4 Methods

4.1 Identification

I study how labour market cyclicality shapes hiring discrimination. The causal identification of this relationship rests partly on the internal validity of the hiring discrimination estimates. Random allocation of the characteristics linked to the discrimination ground under study (e.g., ethnicity, gender, or age) to the fictitious applicants ensures said validity. Consequently, the discrimination estimates are causal in their interpretation regarding employer responses.

However, the primary challenge is to identify the relationship between measures of labour market cyclicality and hiring discrimination. While I can exploit two decades worth of temporal variation in hiring discrimination to estimate this relationship, the correspondence audits differ in where they run, which firms and occupations are sampled, and how callbacks are captured, for example. Because these shifts in experimental design can move with or against the labour market cycle, simple associations between cyclicality indicators and discrimination estimates are insufficiently informative. Previous meta-analytic evidence also showed that levels of hiring discrimination are heterogeneous across settings (Lippens et al., 2023; Quillian et al., 2017; Quillian & Lee, 2023; Schaerer et al., 2023). I therefore applied two complementary identification strategies.

First, I compared discrimination estimates across periods while holding constant observable features of the audits using meta-regression. Specifically, I controlled for (i) study design, such as whether researchers used a matched procedure and how callbacks were recorded; (ii) candidate characteristics, such as the specific treatment signal (e.g., race, gender, age), education level, and employment status; and (iii) treatment group, region, and occupation differences. This approach attributes remaining variation in hiring discrimination to changes in labour market conditions and some residual variation. A key identifying assumption is that, after conditioning on observed audit features, discrimination would have evolved similarly across markets absent labour market cycle variation. Equivalently, I assume that unobserved time-varying factors which affect hiring discrimination are independent of the cyclical measures.

Second, I introduced a novel meta-analytic event study approach centred around unemployment "shocks", which are defined as consecutive rises in country-level unemployment with varying thresholds (e.g., at least two quarters of ≥1% increases in the unemployment rate). By aligning the timing of the correspondence audits to these shocks, I aimed to eliminate relatively slow-moving confounds. This identification strategy supports a causal interpretation of how episodes of increasing unemployment affect discrimination. A key assumption here is that the shocks in unemployment are exogenous to the audit design choices and employer screening practices, with flat pre-shock trends. Because I average across many distinct audit studies, it is reasonable to assume the influence of these choices is mostly exogenous.

4.2 Estimation

4.2.1 Meta-regression

I first estimated the effect of cyclicality measures on hiring discrimination using an unrestricted weighted least squares meta-regression (UWLS-MRA) approach. UWLS-MRA offers several advantages over traditional random-effects meta-regression (RE-MRA) (Stanley & Doucouliagos, 2017). In random effects models, the weight multiplicative constant is fixed at one, and the between-study variance must be estimated. This procedure can be sensitive to publication bias, specifically small-sample bias. In contrast, UWLS-MRA estimates this multiplicative constant directly from the data via the mean squared error, typically yielding less biased estimates.[13]

I started with recalculating the response rates from the correspondence experiments, RR_k^T (treatment group) and RR_k^C (control group):

$$RR_k^T = y_k^T / n_k^T \tag{5}$$

$$RR_k^C = y_k^C / n_k^C \tag{6}$$

where y_k^T is the callback count for the treated (minority) group for observation k, n_k^T is the application count for the former group, y_k^C is the callback count for the control (majority) group, and n_k^C is the application count for the latter group. Observation k is a conditional average treatment effect defined by group (g), occupation (o), industry (i), country (c), time (t), and other study characteristics (s); $k = (g, o, i, c, t, s)$.

In the meta-regression specifications, the dependent variable is the natural logarithm of the positive response ratio, defined as:

$$\ln(PRR_k) = \ln\Big(\frac{RR_k^T}{RR_k^C}\Big) = \ln\Big(\frac{y_k^T / n_k^T}{y_k^C / n_k^C}\Big). \tag{7}$$

From Equation (7) , $\ln(PRR_k) < 0$ indicates unequal treatment of or discrimination against the treated group, while $\ln(PRR_k) > 0$ indicates discrimination against the control group. I applied the natural logarithm because it renders a quasi-normal distribution of the outcome variable, which is handled better by the least squares estimator than a right-skewed distribution. The standard error of the positive response ratio is:

$$SE_{\ln(PRR_k)} = \sqrt{\frac{1}{y_k^T} - \frac{1}{n_k^T} + \frac{1}{y_k^C} - \frac{1}{n_k^C}}. \tag{8}$$

I defined precision as:

$$p_k = \frac{1}{SE_{\ln(PRR_k)}} \tag{9}$$

and used its square as the weights w_k in the inverse-variance weighted meta-regression:

[13] Stanley & Doucouliagos (2017), Stanley et al. (2022), Stanley et al. (2023) show through simulation and empirically that UWLS-MRA often outperforms RE-MRA when excess heterogeneity or small-sample publication bias is present. This robustness is crucial in the analysis, given the variation in study-level precision and the potential for unobserved publication bias. By using UWLS-MRA, I ensure that the meta-regression estimates of hiring discrimination on the cyclicality measures are less prone to the distortions that can afflict conventional random effects approaches.

$$w_k = p_k^2 = \frac{1}{SE^2_{\ln(PRR_k)}}. \tag{10}$$

To obtain a small-sample bias-corrected pooled effect of hiring discrimination, I could retrieve the intercept, $\alpha = \widehat{PRR}_{\text{log-pooled}}$, from the following regression specification due to the properties of the UWLS-MRA estimator:

$$\ln(PRR_k) = \alpha + \beta^{SE}\, SE_{\ln(PRR_k)} + \epsilon_k \tag{11}$$

$$\epsilon_k \sim \mathcal{N}(0, \phi\, v_k) \tag{12}$$

where β^{SE} tests for small-sample publication bias and ϵ_k captures the sampling error, which is normally distributed with mean 0 and variance $\phi\, v_k$. This specification corresponds to the precision effect test (PET) variant of the unrestricted weighted least squares estimator (Stanley & Doucouliagos, 2017).

I added covariates and fixed effects to this specification to help explain or absorb variation in between-study estimates. The specification can then be written as:

$$\ln(PRR_k) = \beta^{SE}\, SE_{\ln(PRR_k)} + X_k\, B^X + \left(\kappa_o + \mu_r + \nu_g\right) + \epsilon_k \tag{13}$$

where $\ln(PRR_k)$ is the log positive response ratio, β^{SE} quantifies the small-sample publication bias, X_k a set of covariates consisting of study controls, and κ_o, μ_r, and ν_g absorb occupation, region, and treatment group variation, respectively.[14]

Finally, to assess the cyclicality of hiring discrimination, I added a cyclicality term CYC_{tc} at time t for country c to the equation:

$$\ln(PRR_k) = \beta^{C}\, CYC_{tc} + \beta^{SE}\, SE_{\ln(PRR_k)} + X_k\, B^X + \left(\kappa_o + \mu_r + \nu_g\right) + \epsilon_k \tag{14}$$

where β^C denotes the cyclicality effect of interest. I incorporated regression weights w_k from (10) into all of the meta-regressions. Standard errors are clustered at the study and country level, accounting for within-study dependence of the discrimination estimates. If the number of countries was small for a given subsample, typically below 10, I clustered standard errors by only study and not country.

I estimated meta-regressions for samples where the number of studies equalled or exceeded the number of continuous regressors plus the number of categorical dummies (excluding the reference category) plus one (accounting for the intercept). This restriction ensured that I had sufficient statistical power and precision to estimate the specified regressions (Borenstein et al., 2011). However, it has the disadvantage that I could only compute cyclicality effects separately for a handful of discrimination grounds within the scope of the meta-reanalysis.

[14] I only included the treatment group fixed effect when the analysis is done for the entire sample or for subsamples by discrimination ground, not by treatment group.

4.2.2 The meta-analytic event study

To assess the dynamic impact of cyclicality 'shocks' on hiring discrimination, I extended the meta-regression approach to a meta-analytic event study (MAES). This novel applied method integrates UWLS-MRA in an event-study framework. The MAES takes advantage of the temporal variability in between-study estimates in the meta-dataset and quantifies the effect of (exogenous) events on these estimates. In my specific case, the result is a set of bias-corrected, covariate-conditioned estimates of changes in the positive response ratio attributable to periods of rising unemployment.

To construct the event dataset, I matched the positive response ratios from Equation (7) alongside related study-level variables to country-level unemployment events via a fuzzy join. For each country, I flagged quarters in which the unemployment rate rose above a predetermined, absolute or relative threshold. In particular, an event encompassed any run of at least two consecutive quarterly rises in unemployment.[15] Around each event's zero mark, i.e., the quarter directly preceding the first rise of the two-quarter run, I constructed an event time window spanning two quarters before and four quarters after.[16] I then joined the discrimination data with the event data by country and by the temporal midpoint of the correspondence audit.[17] Although the fuzzy join introduces measurement error in event timing because correspondence audits often run for several quarters, pooling dynamic effects across many events should average out this idiosyncratic noise.

I estimated an event-study specification via the Sun & Abraham (2021) estimator using $\ln(PRR_k)$ as the outcome, the inverse-variance regression weights from Equation (10) , and cluster-robust standard errors at the study and country level, which is given by:

$$\begin{aligned}\ln(PRR_k) = \sum_{u}\sum_{\tau\neq-1} \delta_{u,\tau}\, \mathbf{1}\left\{t^s_{c(k)} = u\right\} \mathbf{1}\{t_k - u = \tau\} + X_k B^X \\ +\beta^{SE}\, SE_{\ln(PRR_k)} + \left(\ \kappa_o + \mu_r + \nu_g\right) + \gamma\, \mathrm{year}_k + \epsilon_k\end{aligned} \tag{15}$$

where u is the start cohort, i.e., the quarter when the two-quarter rise in unemployment began; $t^s_{c(k)}$ is the specific cohort start quarter for country c associated with observation k; t_k is the temporal midpoint of observation k in a given audit study expressed as a calendar quarter; $\delta_{u,\tau}$ is the cohort-specific effect at relative time τ for cohort u; vector X_k represents study-level characteristics; κ_o, μ_r, and ν_g are occupation, region, and treatment group fixed effects, respectively; $year_k$ is a linear trend over the years observed in the dataset; and ϵ_k is the residual error.[18]

[15] This approach aligns with how a recession is defined, i.e., a fall in GDP for two consecutive quarters.

[16] I restricted the time window to these seven quarters because I want to avoid contamination of overlapping events, where runs of rising unemployment are close together and discrimination estimates fall in multiple windows.

[17] For example, suppose the unemployment rate rose in Q1 of 2012 (relative to Q4 of 2011) and, again, in Q2 of 2012 (relative to Q1 of 2012) in a given country, constituting an event with the zero mark in Q4 of 2011 (i.e., 0Q). A correspondence audit that ran in that country between Q3 of 2012 and Q1 of 2013 would have its midpoint in Q4 of 2012. This point falls inside the two-quarter lead and four-quarter lag event window, and the study's observations are coded as occurring four quarters after the zero mark (i.e., +4Q) or two quarters after the event ended.

[18] I did not add calendar time or study fixed effects because they absorb too much of the identifying variation. The baseline specification therefore conditions on study characteristics, a linear time trend, and separate occupation,

Equation (15) can be rewritten after aggregation, retrieving the average dynamic treatment effect on the treated (ATT) for each relative period:

$$\begin{aligned}\ln(PRR_k) = \sum_{\tau \neq -1} \delta_\tau^{SA}\, D_{k,\tau}^{SA} + X_k B^X + \beta^{SE}\, SE_{\ln(PRR_k)} \\ + \left(\kappa_o + \mu_r + \nu_g \right) + \gamma\, \text{year}_k + \epsilon_k.\end{aligned} \tag{16}$$

where τ remains the event time relative to the start of the unemployment rise with reference period $\tau = -1$ and negative (positive) τ as leads (lags); δ_τ^{SA} is the Sun & Abraham (2021) interaction-weighted effect aggregated across cohorts at each relative period τ, while $D_{k,\tau}^{SA}$ is the event time regressor for observation k at τ; other terms are the same as in Equation (15) .[19] Importantly, the Sun & Abraham (2021) estimator does not use past events of heightened unemployment, i.e. so-called already-treated cohorts, as controls, avoiding contamination across event time coefficients. Moreover, including the specified controls and fixed effects improves comparability in event time effects across studies and enhances precision by accounting for or absorbing unobserved heterogeneity unrelated to the event timing.

5 Results

Here, I describe the results of the meta-reanalysis of how hiring discrimination estimates respond to labour market cyclicality. I start by presenting an overview of discrimination estimates (using positive response ratios) by treatment group based on the meta-dataset of correspondence experiments. This overview is followed by meta-regression results on the direct link between country-level measures of cyclicality (including unemployment rates, vacancy rates, vacancy-to-unemployment ratios, and deviations from full-employment rates of unemployment or 'efficiency gap') and the positive response ratios for the entire sample. These results also include the observations from the meta-analytic event studies. In addition, I assess heterogeneity by discrimination ground, region × ground, treatment group, and region × group for a select number of sufficiently powered subsamples. Last, I answer the counterfactual 'Would there still be hiring discrimination if the labour market were efficient and jobseekers and vacancies were perfectly balanced?' using model-implied predictions from the meta-regressions.

5.1 Hiring discrimination across groups

Figure 2 shows aggregate differences in positive employer responses for various groups compared to their respective control groups in the correspondence audit studies. I present three types of estimates based on unadjusted random effects (RE), bias-adjusted unrestricted weighted least squares (UWLS) and covariate- and bias-adjusted UWLS specifications. The RE specification most closely aligns with the approach in the meta-analysis of Lippens et al. (2023). Although the treatment group aggregation is not identical and I rely on a much larger meta-dataset, many of the

region, and treatment group fixed effects. I assumed unobserved time-invariant study characteristics are independent of the event timing after adjusting for these covariates and fixed effects.

[19]Following Sun & Abraham (2021), the interaction-weighted effect $\delta_\tau^{SA} = \sum_u w_{u,\tau}\, \delta_{u,\tau}$ has non-negative aggregation weights $w_{u,\tau} \geq 0,\ \sum_u w_{u,\tau} = 1$.

predicted differences approximate comparable estimates from their meta-study.[20] Discrimination estimates for applicants expressing a political affiliation or having a criminal record are new in this study. The most severe hiring discrimination seems to be targeted at various racial and ethnic groups, older workers, applicants with a religious affiliation, applicants with disabilities, less physically attractive applicants, and those with a criminal record. Table A.7 in the appendix provides precise numerical estimates and confidence intervals of the results shown in Figure 2 .

However, RE estimates are not adjusted for excess systematic heterogeneity or small-sample publication bias, in contrast to UWLS estimates. The latter incorporate the precision effect test (PET) (Stanley & Doucouliagos, 2017; 2014). After adjustment, I observe that many estimates shrink or even reverse sign, notably those for young jobseekers ($+11.47\%$, $CI_{95\%} = [+3.22\%, +20.38\%]$) and applicants signalling having a physical illness ($+9.73\%$, $CI_{95\%} = [-3.56\%, +24.87\%]$; see Figure 2).[21] Shrinkage is expected given the typical overselection of just-significant estimates in the hypothesised direction, particularly in small-sample studies (Askarov et al., 2024; Brodeur et al., 2016; 2020). These discrepancies empirically confirm the need for treatment effect precision adjustments to obtain more accurate aggregate hiring discrimination estimates.

Bias-adjusted UWLS estimates that also condition on study design characteristics, including the educational level of the fictitious applicants, and contextual variables, such as the region where the correspondence audits took place, provide additional gains in statistical precision. See Equation (13) for the regression specification and Table A.3 , Table A.4 , and Table A.5 for an overview of regressors and their possible values. The model-implied marginal means typically shrink the hiring discriminates further (particularly for the Asian treatment group; -6.96%; $CI_{95\%} = [-13.64\%, +0.23\%]$; see Figure 2). I rely on bias- and covariate adjusted estimates to answer the question of whether hiring discrimination is responsive to the labour market cycle in the next set of analyses (see Section 5.2 to Section 5.4).

[20]For example, Lippens et al. (2023) find a penalty of -40.63% ($CI_{95\%} = [-44.52\%, -36.47\%]$, $k = 31$) for fictitious applicants with an Arab, Maghrebi, or Middle Eastern background, while I find a penalty of -41.29% ($CI_{95\%} = [-47.74\%, -34.04\%]$, $k = 286$) for Middle Eastern and Northern African applicants based on the RE specification. Similarly, Lippens et al. (2023) report older applicants receive -33.54% fewer positive responses ($CI_{95\%} = [-37.08\%, -29.80\%]$, $k = 17$), while I observe -38.35% fewer positive responses ($CI_{95\%} = [-51.66\%, -21.39\%]$, $k = 233$).

[21]I evaluate predictions at $SE_{\ln(PRR_k)} = 0.1$, a small positive value within the range of observed standard errors. Evaluating $SE_{\ln(PRR_k)} = 0$ would imply infinite precision and forces out-of-sample extrapolation because it is never observed. Using a small positive value yields more stable predictions that are less sensitive to estimation noise in the precision slope of the UWLS-MRA specification and avoids inaccurate edge cases that appear with adjusting for additional covariates. For reference, Figure A.1 in the appendix shows the distribution of the observed standard errors in the sample.

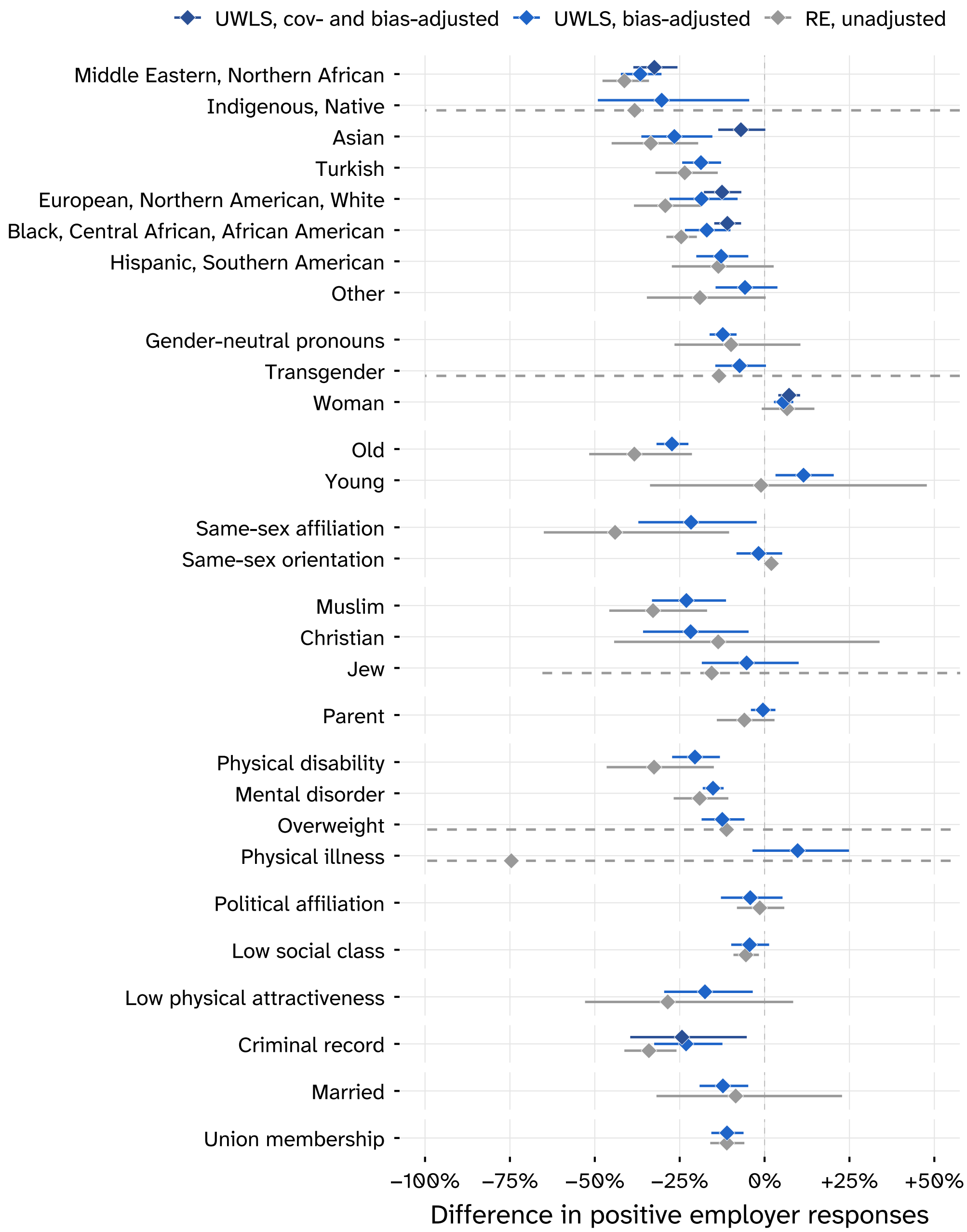

Figure 2: Predicted differences in positive employer responses by treatment group. Points show aggregate differences by treatment group, i.e. $\widehat{PRR}_g - 1$, expressed in percent. Horizontal lines are 95% confidence intervals. Unadjusted random effects (RE) predictions use restricted maximum likelihood with study-clustered standard errors and study random intercepts, inverse-variance weights, and Knapp–Hartung adjustments. Bias-adjusted UWLS predictions use inverse-variance WLS with precision effect test (PET) adjustment, evaluated at $SE_{\ln(PRR_k)} = 0.1$ – see Eq. (11) . Covariate- and bias-adjusted UWLS predictions condition on study design covariates, i.e. education, employment, gender, migrant generation (if ground is race, ethnic identity, and national origin), firm profit status, matched design, callback type, and occupation and region fixed effects – see Eq. (13) . Predictions are evaluated for subsamples where $N \geq param, N \geq 2, k \geq 10$, following Harrer et al. (2021), at the observed covariate and fixed effects values. UWLS standard errors are two-way clustered by study and country. Confidence intervals exceeding the figure limits are dashed.

5.2 The cyclicality of hiring discrimination

5.2.1 Aggregate cyclicality

Table 1 presents aggregate estimates by cyclicality measure for the entire meta-sample of correspondence audit studies. Panel A shows the estimates for the linear specification; Panel B shows the estimates using adapted piecewise cubic spline specifications at different breakpoints. Nearly all estimates are in the expected direction, suggesting a countercyclical effect, but are not statistically significant at the conventional $\alpha = 0.05$. In the log-log models, slopes represent elasticities, meaning that, e.g., a 1.00% increase in unemployment is associated with a decrease in positive responses in the minority group by about 0.14% ($\hat{\beta}^C = -0.14$; p = 0.098). Figure A.2 in the appendix visualises the relationship between country-level unemployment rates and positive response ratios. Although the results from Table 1 provide limited empirical evidence for the cyclicality of hiring discrimination, the meta-analytic event study estimates paint a different picture (see Section 5.2.2). Further analyses by ground, region, and treatment group also uncover substantial effect heterogeneity (see Section 5.3).

Table 1: UWLS-MRA of ln PRR on cyclicality measures

	ln U	**ln V**	**ln V/U**	**U–U***
Panel A: Linear specifications				
Coefficient	-0.14 (0.08)	0.03 (0.06)	0.03 (0.03)	-0.57 (0.98)
k	3,293	2,910	2,910	2,910
Adj. R^2	0.33	0.32	0.32	0.32
Panel B: Piecewise specification				
ln U [0, 0.10)	-0.45 (0.23)	—	—	—
ln U [0.10, +Inf)	-0.15 (0.29)	—	—	—
ln V [0, 0.03)	—	-0.13 (0.14)	—	—
ln V [0.03, +Inf)	—	0.22 (0.16)	—	—
k	3,293	2,910	—	—
Adj. R^2	0.33	0.33	—	—

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, region, and treatment group. Standard errors (between parentheses) are two-way clustered by study and country.

5.2.2 Meta-analytic event study estimates

Figure 3 shows dynamic treatment effects based on the meta-analytic event study (MAES) approach using events of rising unemployment matched to hiring discrimination estimates from the meta-sample. First of all, I observe no pre-trend or anticipation effect. Second, hiring discrimination responds heavily to increased unemployment in the first quarter (+1Q) following a relative rise across thresholds. The positive response ratio (PRR) decreases by about 15% to 22% relative to −1Q. This countercyclical effect is confirmed by an increase in the PPR by quarters 3 and 4, which suggests declining hiring discrimination when the labour market recovers. Notably,

$\Delta\text{PRR}/\text{PRR}_{-1} \approx 0$ after two quarters, while I still observe a rise in the unemployment rate relative to the preceding quarter – the 'event' has not yet ended, i.e., $\Delta\text{U}/\text{U}$ is the same in +1Q as in +2Q. I could attribute this finding to measurement error introduced by the fuzzy join or due to anticipation of recovering unemployment by the employer, although the unemployment rise is not strictly limited to two quarters and might continue after the second quarterly rise. Table A.8 in the appendix presents the numerical estimates of the dynamic treatment effects by threshold and quarter. In addition, the number of quarterly events by threshold is visualised in Figure A.4 .

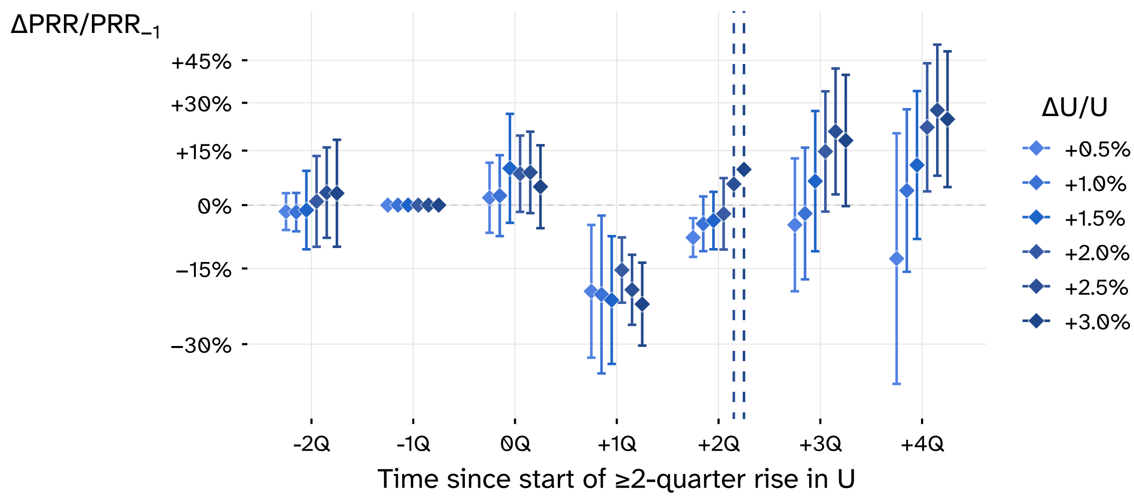

Figure 3: Meta-analytic event study (MAES) effects of rising unemployment on hiring discrimination. Points are estimates of proportional change in the positive response ratio (PRR) relative to the quarter preceding the ≥2 quarterly rises in the unemployment rate, i.e., $\Delta\text{PRR}_\tau/\text{PRR}_{\tau=-1} = \exp\left(\hat{\delta}^{SA}_\tau\right) - 1$. Estimates rely on Sun & Abraham (2021) aggregation and condition on study design covariates, i.e. education, employment, gender, firm profit status, matched design, callback type; occupation, region, and treatment group fixed effects; and a linear year trend – see Eq. (16) . Shades of blue correspond to quarter-over-quarter Δ U/U thresholds ranging from +0.5% to +3.0% in 0.5pp increments. Whiskered error bars represent 95% confidence intervals; intervals exceeding the figure limits are dashed.

5.3 Heterogeneity in the cyclicality of hiring discrimination

5.3.1 Ground heterogeneity

Table 2 shows heterogeneity in the cyclicality of hiring discrimination for four discrimination grounds for which I have sufficient observations. I observe no signs of cyclicality concerning race, ethnic identity, and national origin or criminal record. The piecewise unemployment rate estimates for sex and gender show some sign of countercyclicality (i.e., Panel B). Most notable is the countercyclicality of hiring discrimination based on age. Panels D, E, and F show a considerable response to the vacancy rate, the vacancy-to-unemployment ratio, and the deviation from the full-employment rate of unemployment (FERU). Focusing on the latter indicator, for every percentage point the unemployment rate exceeds the FERU, the positive response ratio drops by −6.62% (p = 0.001) for age minorities.

Table 2: UWLS-MRA of ln PRR on cyclicality measures by discrimination ground

	REN	SEG	AGE	CRI
Panel A: Unemployment rate				
ln U	-0.06 (0.07)	-0.11 (0.05)	-0.26 (0.29)	0.27 (0.16)
k	1,311	647	269	63
Adj. R^2	0.41	0.12	0.78	0.34
Panel B: Unemployment rate (piecewise)				
ln U [0, 0.10)	-0.20 (0.17)	-0.29* (0.14)	-0.72 (0.98)	0.57 (0.51)
ln U [0.10, +Inf)	0.10 (0.18)	-0.49* (0.19)	-0.07 (0.65)	0.43 (0.20)
k	1,311	647	269	63
Adj. R^2	0.41	0.13	0.78	0.33
Panel C: Vacancy rate				
ln V	0.03 (0.08)	-0.00 (0.05)	0.35 (0.16)	-0.30 (0.17)
k	1,130	559	261	62
Adj. R^2	0.36	0.11	0.78	0.29
Panel D: Vacancy rate (piecewise)				
ln V [0, 0.03)	-0.20 (0.13)	0.23 (0.18)	1.93** (0.45)	-0.85 (0.59)
ln V [0.03, +Inf)	0.33 (0.17)	-0.15 (0.08)	3.76*** (0.61)	-0.50 (0.25)
k	1,130	559	261	62
Adj. R^2	0.37	0.12	0.79	0.27
Panel E: Vacancy-to-unemployment ratio				
ln V/U	0.01 (0.04)	0.03 (0.02)	0.26*** (0.03)	-0.17 (0.09)
k	1,130	559	261	62
Adj. R^2	0.36	0.11	0.79	0.32
Panel F: Deviation from full-employment rate of unemployment				
U–U*	0.44 (0.87)	-1.17 (0.60)	-6.62*** (1.09)	2.74 (1.28)
k	1,130	559	261	62
Adj. R^2	0.36	0.12	0.79	0.32

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), REN (race, ethnic identity, national origin), SEG (sex and gender), AGE (age), CRI (criminal record), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, migrant generation (if ground is race, ethnic identity, and national origin) firm profit status, matched design, callback type, ISCO-08 major group occupation, region, and treatment group. Standard errors (between parentheses) are two-way clustered by study and country. * $p < .05$, ** $p < .01$, *** $p < .001$

However, this general approach ignores that age discrimination in hiring increases with age (differences), which is also suggested by Batinovic et al. (2023). The correspondence experiments included in the meta-dataset rely on various treatment and control groups. When I fix the baseline age level in the control group at 35 years, i.e., the median age in the control group, I see that predicted positive response ratios decline with increasing age and that younger applicants

(≤35y) face little hiring discrimination or even a hiring premium (see Figure A.3 and Table A.9). Applying moderation analysis, I find that the cyclicality of age discrimination is heterogeneous by age in the treatment group. Discrimination levels rise as unemployment rates (relative to the FERU) and age increase (see Figure 4). I also predict that increased country-level unemployment is associated with more positive responses to applications of young jobseekers, in particular as of ca. 4pp deviation. Model estimates used to derive these predictions are presented in Table A.9 in the appendix.

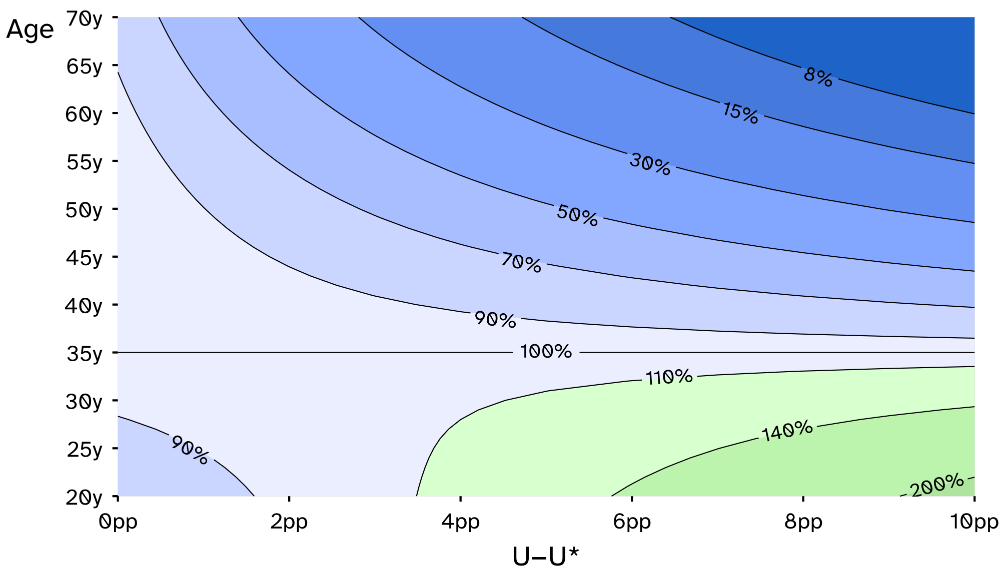

Figure 4: Predicted age discrimination in hiring by FERU gap. Filled contours plot the predicted positive response ratio (PRR) for applicants at age Y relative to age 35 as a function of the deviation between the unemployment rate and the full-employment rate of unemployment (FERU), $U - U^*$, where Y represents an age value on the y-axis. Shades of blue (green) indicate hiring discrimination against older (younger) applicants. Predictions are average comparisons derived from an UWLS-MRA specification based on Eq. (14) including moderators for the control group age and a piecewise-cubic spline in treatment age with a breakpoint at age 35 interacted with $U - U^*$. The predictions condition on study design covariates, i.e. education level, employment status, gender, firm profit status, matched design, callback type, and occupation and region fixed effects (all evaluated at the observed values). Standard errors are two-way clustered by study and country.

In contrast to results from the UWLS-MRA regression by discrimination ground, but in line with the aggregate MAES estimates for the full meta-sample, Figure 5 shows that race, ethnic identity, and national origin discrimination in hiring is countercyclical. Dynamic treatment effects at the first quarter of the ≥2-quarter rise range from −38% to −56% depending on the specific threshold. I observe a slight but statistically insignificant (at $\alpha = 0.05$) recovery in quarters 3 and 4 following the start of the rise. Table A.10 in the appendix presents the numerical estimates of the dynamic treatment effects by threshold and quarter. These findings raise the question of whether some regional hiring discrimination estimates might drive this effect, which I indeed observe in the heterogeneity analyses by region (see Section 5.3.2).

Figure 6 visualises MAES estimates concerning sex and gender. The event time coefficients suggest that sex and gender discrimination responds little to labour market cyclicality, which contradicts earlier ULWS-MRA estimates (see Table 2). If anything, sex and gender discrimination in hiring seems procyclical with dynamic treatment effects in the first quarter reaching +20% (p = 0.309) at the +2.0% threshold. Table A.11 in the appendix presents the numerical estimates of the dynamic treatment effects by threshold and quarter.[22]

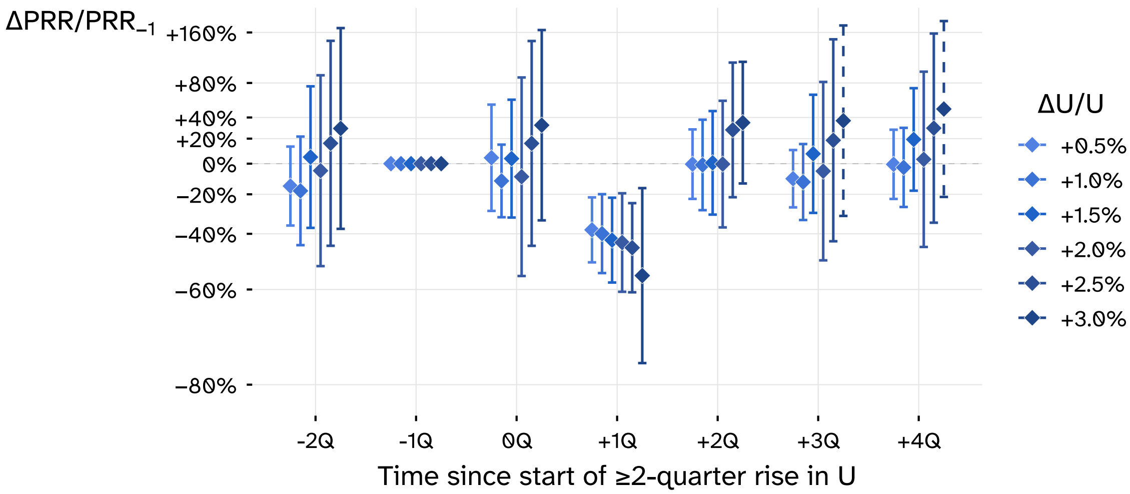

Figure 5: Meta-analytic event study (MAES) effects of rising unemployment on hiring discrimination for race, ethnic identity, and national origin. Points are estimates of proportional change in the positive response ratio (PRR) relative to the quarter preceding the ≥2 quarterly rises in the unemployment rate, i.e., $\Delta\mathrm{PRR}_\tau/\mathrm{PRR}_{\tau=-1} = \exp\left(\hat{\delta}_\tau^{SA}\right) - 1$. Estimates rely on Sun & Abraham (2021) aggregation and condition on study design covariates, i.e. education level, employment status, gender, firm profit status, matched design, callback type; occupation, region, and treatment group fixed effects; and a linear year trend – see Eq. (16) . Shades of blue correspond to quarter-over-quarter Δ U/U thresholds ranging from +0.5% to +3.0% in 0.5pp increments. Whiskered error bars represent 95% confidence intervals.

[22] I did not perform MAES for the discrimination grounds age and criminal record simply because I have insufficient hiring discrimination observations in the constructed event windows to calculate dynamic treatment effects.

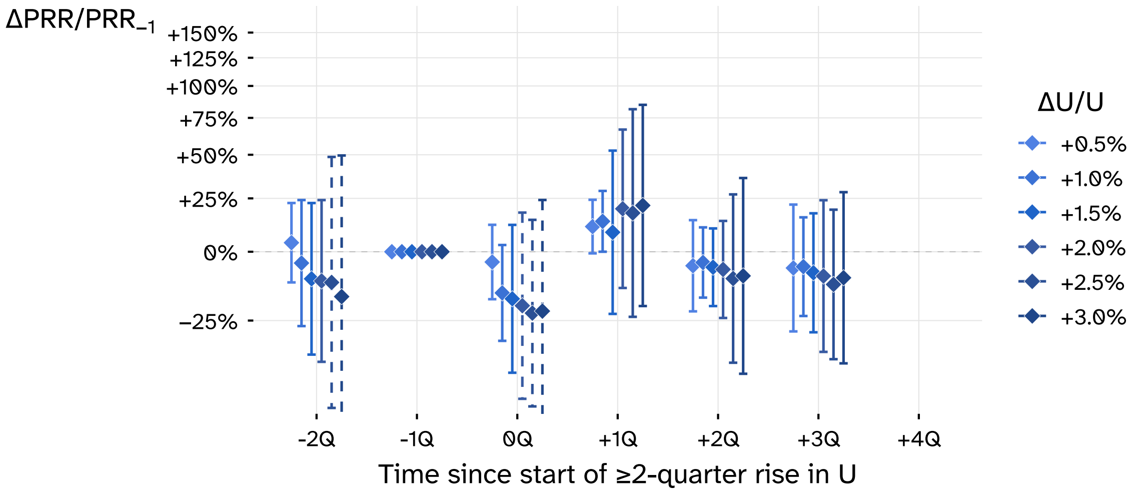

Figure 6: Meta-analytic event study (MAES) effects of rising unemployment on hiring discrimination for sex and gender. Points are estimates of proportional change in the positive response ratio (PRR) relative to the quarter preceding the ≥2 quarterly rises in the unemployment rate, i.e., $\Delta\mathrm{PRR}_\tau/\mathrm{PRR}_{\tau=-1} = \exp\left(\hat{\delta}_\tau^{SA}\right) - 1$. Estimates rely on Sun & Abraham (2021) aggregation and condition on study design covariates, i.e. education level, employment status, gender, firm profit status, matched design, callback type; occupation, region, and treatment group fixed effects; and a linear year trend – see Eq. (16) . Shades of blue correspond to quarter-over-quarter $\Delta\,\mathrm{U/U}$ thresholds ranging from +0.5% to +3.0% in 0.5pp increments. Whiskered error bars represent 95% confidence intervals. Estimates for +4Q are absent because of insufficient hiring discrimination estimates four quarters following the event onsets.

5.3.2 Region heterogeneity

Heterogeneity analyses of the cyclicality of hiring discrimination by region reveal a few interesting findings that were masked by prior aggregation. Table 3 shows regional heterogeneity in cyclicality for race, ethnic identity, and national origin. I find evidence for a countercyclical effect based on piecewise specifications for Europe (see Panels B and D). Following outlier analysis, Spain seems to be a severe outlier in Europe, sharply shrinking the estimates of the non-piecewise specifications. [The idea that Spain is an outlier in the European labour market is not new: https://www.jstor.org/stable/2117921, https://www.jstor.org/stable/1344700; structurally high unemployment rates. Also, OECD says Spain has very strongy anti-discrimination legislation.] I observe the strongest evidence for a countercyclical effect based on the results for Western Europe (see Panels A to F), which excludes Spain. Every percentage point increase in the unemployment rate relative to the FERU decreases the positive response ratio of ethnic and racial minorities by 7.09% (p = 0.005). Moreover, in line with Kline et al. (2022), i.a., I find no support for labour market cyclicality of racial hiring discrimination in North America.

Table 3: UWLS-MRA of ln PRR on cyclicality measures by region (race, ethnic identity, and national origin)

	EU	WEU	NA
Panel A: Unemployment rate			
ln U	-0.17 (0.17)	-0.56** (0.12)	0.11 (0.07)
k	900	485	356
Adj. R^2	0.44	0.45	0.30
Panel B: Unemployment rate (piecewise)			
ln U [0, 0.10)	-0.69** (0.19)	-1.04** (0.25)	0.17 (0.10)
ln U [0.10, +Inf)	0.12 (0.24)	-0.25* (0.09)	0.22 (0.13)
k	900	485	356
Adj. R^2	0.46	0.45	0.30
Panel C: Vacancy rate			
ln V	0.04 (0.11)	0.31*** (0.05)	0.10 (0.11)
k	788	479	342
Adj. R^2	0.41	0.43	0.28
Panel D: Vacancy rate (piecewise)			
ln V [0, 0.03)	-0.42** (0.10)	1.22* (0.41)	-0.32 (0.39)
ln V [0.03, +Inf)	0.48*** (0.08)	0.12 (0.10)	0.16 (0.18)
k	788	479	342
Adj. R^2	0.45	0.44	0.29
Panel E: Vacancy-to-unemployment ratio			
ln V/U	0.03 (0.07)	0.21** (0.04)	-0.05 (0.04)
k	788	479	342
Adj. R^2	0.41	0.43	0.28
Panel F: Deviation from full-employment rate of unemployment			
U–U*	0.20 (1.33)	-7.09** (1.73)	2.29 (1.50)
k	788	479	342
Adj. R^2	0.41	0.42	0.29

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), EU (Europe), WEU (Western Europe), NA (North America), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, migrant generation, firm profit status, matched design, callback type, ISCO-08 major group occupation, and treatment group. Standard errors (between parentheses) are two-way clustered by study and country, except for the regressions for North America, where I use study-level clustering. * $p < .05$, ** $p < .01$, *** $p < .001$

Table 4 shows regional heterogeneity in cyclicality for sex and gender. The countercyclical effect I observe for sex and gender discrimination in hiring in Europe seems to be mainly driven by

Northern European estimates (see Panels C, E, and F). I find no robust evidence for the cyclicality of hiring discrimination in Western Europe or North America.

Table 4: UWLS-MRA of ln PRR on cyclicality measures by region (sex and gender)

	EU	**WEU**	**NEU**	**NA**
Panel A: Unemployment rate				
ln U	-0.18** (0.05)	-0.15 (0.12)	-0.27 (0.15)	0.08 (0.12)
k	333	124	141	258
Adj. R^2	0.15	0.28	0.18	0.08
Panel B: Unemployment rate (piecewise)				
ln U [0, 0.10)	-0.39** (0.11)	-0.47* (0.12)	—	0.12 (0.17)
ln U [0.10, +Inf)	-0.35* (0.14)	0.18 (0.10)	—	0.26 (0.15)
k	333	124	—	258
Adj. R^2	0.15	0.31	—	0.09
Panel C: Vacancy rate				
ln V	0.03 (0.07)	-0.06 (0.14)	0.48*** (0.06)	-0.06 (0.11)
k	303	120	133	256
Adj. R^2	0.13	0.28	0.28	0.03
Panel D: Vacancy rate (piecewise)				
ln V [0, 0.03)	0.31 (0.23)	-0.44* (0.14)	—	0.05 (0.30)
ln V [0.03, +Inf)	-0.05 (0.11)	0.18* (0.06)	—	-0.06 (0.13)
k	303	120	—	256
Adj. R^2	0.14	0.33	—	0.02
Panel E: Vacancy-to-unemployment ratio				
ln V/U	0.04 (0.03)	0.03 (0.08)	0.31*** (0.05)	-0.04 (0.08)
k	303	120	133	256
Adj. R^2	0.14	0.28	0.26	0.03
Panel F: Deviation from full-employment rate of unemployment				
U–U*	-1.45* (0.54)	-1.00 (2.70)	-8.07** (2.10)	1.51 (2.59)
k	303	120	133	256
Adj. R^2	0.14	0.28	0.22	0.03

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), EU (Europe), WEU (Western Europe), NEU (Northern Europe), NA (North America), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and treatment group. Standard errors (between parentheses) are two-way clustered by study and country, except for the regressions for Northern Europe and North America, where I use study-level clustering. * $p < .05$, ** $p < .01$, *** $p < .001$

5.3.3 Treatment group heterogeneity

The last heterogeneity analysis comprises heterogeneity by treatment group. More specifically, I consider four ethnic groups for which I have sufficient observations to estimate bias- and covariate-adjusted cyclicality estimates. First, I find some evidence that hiring discrimination against Middle Eastern and Northern African applicants is countercyclical. Second, discrimination against Black, Central African, and African American applicants, who are primarily included in US correspondence audit studies, shows no signs of cyclicality. In contrast, discrimination against European, North American, White, and Asian minority groups appears largely procyclical. Table A.12 (Panels B and D), Table A.13 , Table A.14 (Panels A to F), and Table A.15 (Panels B to F) in the appendix present the ULWS-MRA estimates that support these conclusions.

5.4 Hiring discrimination if the labour market was "efficient"

Finally, I want to provide some pointers as to what would happen if the labour market was "efficient". As alluded to in Section 3.2, the deviation from the FERU, $\mathrm{U} - \mathrm{U}^*$, gives us an idea how far off a given country is from an ideal labour market. In other words, it yields the 'efficiency gap'. When $\mathrm{U} - \mathrm{U}^* = 0, \mathrm{U} = \mathrm{U}^*$, the unemployment rate equals the target full-employment rate of unemployment. In this scenario, there are, in theory, no excess jobseekers or vacancies. A deviation from this point constitutes a less efficient labour market. Of course, not all jobseekers will have the required qualifications to fill the outstanding vacancies, but the equilibrium implies a relatively tight, yet not overly tight, labour market.[23] I focus on three counterfactuals for which I have sufficient indications based on the UWLS-MRA specifications that moving (i.e., narrowing or widening) the efficiency gap matters for hiring discrimination.

Figure 7 visualises these counterfactuals. First, I see that minority sex and gender groups, typically women, would receive more positive employer responses in Europe, on average, if there was no FERU gap. In other words, I would observe a procyclical adjustment of hiring discrimination against men. Second, discrimination based on race and ethnicity would almost disappear in Western Europe, on average, if the labour market was efficient; minorities would only face an average penalty of −0.02% fewer positive employer responses compared to −0.17% without correction. A similar picture applies to age: comparing a 55-year-old to a 35-year-old jobseeker, the difference in positive employe response would rise from −0.28% to −0.15%, on average.

[23]For reference, $\mathrm{U} = \mathrm{U}^*, \mathrm{U} < \mathrm{U}^*$ has only occurred six times in the past hundred years in the US: during WWII, during the Korean War, during the Vietnam War, around the dot-com bubble, just before COVID-19, and right after COVID-19 (Michaillat & Saez, 2024).

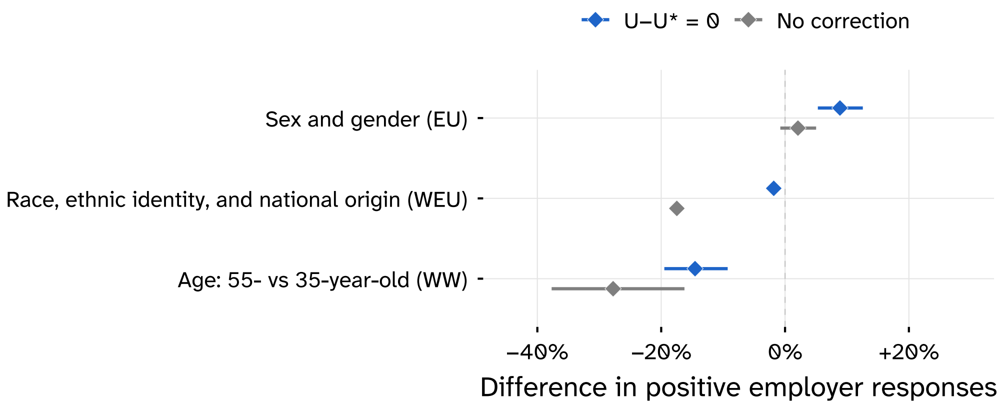

Figure 7: Counterfactual differences in positive employer responses. Points show model-implied UWLS-MRA predictions using inverse-variance weights with precision effect test (PET) adjustment. Horizontal lines are 95% confidence intervals. 'No correction' evaluates at the observed labour market conditions in the meta-sample; 'U–U* = 0' sets the gap between the unemployment rate and the full-employment rate of unemployment (FERU) to zero. Specifications include controls for education, employment status, gender, firm profitability, matched design, callback type, occupation, region (for age), and treatment group fixed effects (all evaluated at the observed values; the standard error of the positive response ratio is evaluated at 0.1). Standard errors are two-way clustered by study and country.

6 Conclusion

I assembled and reanalysed two decades of correspondence audit studies to evaluate whether hiring discrimination moves against the labour market cycle, for whom, and where. Methodologically, I linked positive response ratios to unemployment rates, vacancy rates, and derivative indicators and computed bias-adjusted meta-estimates of cyclicality. I also introduced the meta-analytic event study (MAES), which recovers dynamic effects around unemployment 'shocks'. The approach strengthened causal interpretation while addressing selective reporting and systematic between-study heterogeneity.

I find that hiring discrimination rises when labour markets slacken. In the aggregate, patterns are countercyclical: discrimination intensifies as unemployment rises, with MAES evidence showing deterioration in positive employer responses immediately after unemployment increases. By group, countercyclicality is most robust for racial and ethnic minorities in Western Europe and for older workers, while results are weaker or mixed for sex and gender. In addition, I identify no systematic cyclicality in North America. If the labour market were efficient, with no gap between the prevailing unemployment rate and the target rate at which jobseekers and vacancies are balanced, predicted racial and ethnic discrimination in Western Europe would, on average, nearly disappear, and age discrimination would be substantially attenuated.

The policy advice is straightforward: anti-discrimination enforcement is most needed in downturns, precisely when budgets often tighten. Monitoring and enforcement should therefore be countercyclical, ramping up as unemployment rises and outstanding vacancies diminish, and targeted to contexts where cyclicality bites most. Policymakers should maintain or expand anti-discrimination capacity during slack markets; otherwise, discrimination is likely to intensify when the opportunity cost of excluding minority candidates is lowest.

A few limitations remain. First, correspondence audits capture only the first stage (i.e., callbacks), not contract terms or on-the-job outcomes. Second, coverage is concentrated in so-called Western, Educated, Industrialised, Rich, and Democratic (WEIRD) contexts, in formal job searches, and among younger applicants (aside from the audit studies focusing on age), so external validity is bounded. Third, while the design mitigates confounding by using abundant controls and fixed effects in the meta-regressions and by relying on the novel MAES approach, some of the cyclicality evidence remains associational. Future work on the cyclicality of discrimination can hopefully evaluate this cyclicality (i) through later hiring stages, (ii) beyond WEIRD labour market settings, (iii) via informal recruitment channels, and (iv) for broader (age) groups at different demographic intersections.

Appendix

A Supplementary materials

A.1 Supplementary tables

Table A.1: PICO eligibility criteria for inclusion and exclusion

Criterion	**Definition**
Study type	Correspondence experiments in which applications of fictitious applicants, whose characteristic of interest (e.g., race, age, gender) are experimentally manipulated, are sent in response to genuine vacancies through (e-)mail or (online) job platforms.
Population	Fictitious applicants from minority groups and their majority counterparts.
Outcome	Unequal treatment, translatable into binary responses, forbidden by law in the hiring and selection process (i.e., hiring discrimination).
Comparison	Positive responses or callbacks or interview invitations of minority applicants compared with those of majority applicants.
Context	Hiring discrimination related to sixteen discrimination grounds upon which unequal treatment is forbidden (i.e., race, ethnic identity, and national origin, sex and gender, age, physical appearance, parenthood and fertility, health and disability, sexual orientation, religion, wealth, civil status, union affiliation, political orientation, military affiliation, genetic information, citizenship status, and criminal record).
Timing	Studies published from 2000 to 2024 (including).

Notes. The framework used to define the eligibility criteria is based on the PICO (Population, Intervention, Comparison, Outcome) framework first coined by (Richardson et al., 1995).

Table A.2: Treatment groups by discrimination ground

Ground	Treatment group	k	Percent
Race, ethnic identity, and national origin	Black, Central African, African American	407	11.91
	European, Northern American, White	297	8.69
	Middle Eastern, Northern African	275	8.05
	Asian	201	5.88
	Hispanic, Southern American	76	2.22
	Turkish	61	1.78
	Indigenous, Native	17	0.50
	Other	10	0.29
Sex and gender	Woman	543	15.89
	Gender-neutral pronouns	105	3.07
	Transgender	29	0.85
Age	Old	233	6.82
	Young	43	1.26
Sexual orientation	Same-sex affiliation	179	5.24
	Same-sex orientation and affiliation	20	0.59
	Same-sex orientation	13	0.38
	Queer	2	0.06
Religion	Muslim	115	3.36
	Buddhist	46	1.35
	Jew	14	0.41
	Christian	13	0.38
	Hindu	10	0.29
	Various or Other	6	0.18
Parenthood and fertility	Parent	177	5.18
	Pregnant	1	0.03
Health and disability	Overweight	56	1.64
	Physical disability	48	1.40
	Physical illness	25	0.73
	Mental disorder	24	0.70
	Other	7	0.20
Political orientation	Political affiliation	114	3.34
Wealth and class	Low social class	67	1.96
	Poor	4	0.12
Physical appearance	Low physical attractiveness	47	1.38
	Average physical attractiveness	20	0.59
	Tattoo	1	0.03

Ground	Treatment group	k	Percent
Criminal record	Criminal record	63	1.84
Civil status	Married	20	0.59
	Undisclosed civil status	3	0.09
Union affiliation	Union membership	10	0.29
	Union affiliation	7	0.20
Military affiliation	Military service	6	0.18
	Military affiliation	1	0.03
Citizenship status	Foreign-born, documented	1	0.03
	Foreign-born, undocumented	1	0.03

Notes. The ‘Percent’ column shows k, i.e., the number of conditional average treatment effects, as a percent of the total effects.

Table A.3: Audit study and design characteristics

Variable	Value	k	Percent
Gender	Male	1,307	38.24
	Female	1,259	36.83
	Unknown, Not applicable, or Missing	550	16.09
	Both	302	8.84
Education	Various	2,031	59.42
	Secondary	499	14.60
	Bachelor's	495	14.48
	Unknown	170	4.97
	Postsecondary	151	4.42
	Master's	70	2.05
	Primary	2	0.06
Employment	Employed	2,319	67.85
	Unknown	586	17.14
	Unemployed	376	11.00
	Mixed	137	4.01
Design	Matched	2,211	64.69
	Unmatched	1,207	35.31
Callback	Positive reaction	2,557	74.81
	Interview invitation	861	25.19

Notes. The 'Percent' column shows k, i.e., the number of conditional average treatment effects, as a percent of the total effects by variable.

Table A.4: Countries by region

Region	Country	k	Percent
Europe	Sweden	415	12.14
	Belgium	274	8.02
	Spain	233	6.82
	Germany	211	6.17
	Netherlands	184	5.38
	Norway	159	4.65
	United Kingdom	159	4.65
	France	116	3.39
	Greece	69	2.02
	Russia	66	1.93
	Italy	64	1.87
	Finland	55	1.61
	Denmark	43	1.26
	Switzerland	33	0.97
	Cyprus	12	0.35
	Austria	10	0.29
	Georgia	5	0.15
	Czechia	4	0.12
	Latvia	4	0.12
	Ireland	3	0.09
	Hungary	2	0.06
	Romania	1	0.03
North America	United States	1,004	29.37
	Canada	26	0.76
Asia	China	73	2.14
	Israel	28	0.82
	India	15	0.44
	Pakistan	8	0.23
	Malaysia	7	0.20
	Hong Kong	5	0.15
	Turkey	4	0.12
	Philippines	1	0.03
South America	Mexico	33	0.97
	Peru	15	0.44
	Brazil	4	0.12
	Jamaica	3	0.09

Region	Country	k	Percent
	Colombia	2	0.06
	Bolivia	1	0.03
Oceania	Australia	47	1.38
Various	Various	13	0.38
Africa	South Africa	5	0.15
	Algeria	1	0.03
	Gabon	1	0.03

Notes. The ‘Percent’ column shows k, i.e., the number of conditional average treatment effects, as a percent of the total effects.

Table A.5: Occupations by major and sub-major group

Occupation (ISCO-08 Major)	Occupation (ISCO-08 Sub-Major)	N	Percent
Various	Various	1,437	42.04
Service and sales workers	Sales workers	377	11.03
	Personal service workers	297	8.69
	Personal care workers	17	0.50
	Protective services workers	2	0.06
Professionals	Information and communications technology professionals	176	5.15
	Business and administration professionals	135	3.95
	Teaching professionals	59	1.73
	Health professionals	38	1.11
	Science and engineering professionals	36	1.05
	Legal, social and cultural professionals	25	0.73
Clerical support workers	Customer services clerks	133	3.89
	General and keyboard clerks	109	3.19
	Numerical and material recording clerks	109	3.19
	Other clerical support workers	48	1.40
Craft and related trades workers	Building and related trades workers (excluding electricians)	80	2.34
	Electrical and electronics trades workers	38	1.11
	Metal, machinery and related trades workers	29	0.85
	Food processing, woodworking, garment and other craft and related trades workers	1	0.03
Managers	Administrative and commercial managers	69	2.02
	Hospitality, retail and other services managers	6	0.18
	Production and specialized services managers	6	0.18
Elementary occupations	Cleaners and helpers	39	1.14
	Labourers in mining, construction, manufacturing and transport	31	0.91
Technicians and associate professionals	Business and administration associate professionals	31	0.91
	Health associate professionals	13	0.38
	Information and communications technicians	9	0.26
	Science and engineering associate professionals	6	0.18

Occupation (ISCO-08 Major)	**Occupation (ISCO-08 Sub-Major)**	**N**	**Percent**
	Legal, social, cultural and related associate professionals	2	0.06
Plant and machine operators, and assemblers	Drivers and mobile plant operators	33	0.97
	Stationary plant and machine operators	24	0.70
	Assemblers	2	0.06
Skilled agricultural, forestry and fishery workers	Market-oriented skilled agricultural workers	1	0.03

Notes. Acronyms used: ISCO (International Standard Classification of Occupations). The ‘Percent’ column shows k, i.e., the number of conditional average treatment effects, as a percent of the total effects. The ‘Various’ refer to observations that contain multiple occupations that can be categorised under different ISCO-08 major or sub-major groups.

Table A.6: Audit study and design characteristics

Region	Variable	Mean (SD)	Range
Europe	ln U	-2.69 (0.44)	[-3.82, -1.37]
	ln V	-3.99 (0.49)	[-5.30, -2.84]
	ln V/U	-1.30 (0.88)	[-3.62, 0.98]
	U–U*	0.04 (0.04)	[-0.01, 0.21]
North America	ln U	-2.81 (0.25)	[-3.40, -2.22]
	ln V	-3.20 (0.23)	[-4.16, -2.68]
	ln V/U	-0.39 (0.38)	[-1.81, 0.66]
	U–U*	0.01 (0.01)	[-0.01, 0.06]
Asia	ln U	-2.81 (0.37)	[-3.63, -2.07]
	ln V	—	—
	ln V/U	—	—
	U–U*	—	—
South America	ln U	-3.21 (0.37)	[-3.63, -2.37]
	ln V	—	—
	ln V/U	—	—
	U–U*	—	—
Oceania	ln U	-3.04 (0.14)	[-3.18, -2.70]
	ln V	—	—
	ln V/U	—	—
	U–U*	—	—
Africa	ln U	-1.50 (0.34)	[-2.19, -1.37]
	ln V	—	—
	ln V/U	—	—
	U–U*	—	—

Notes. Acronyms used: SD (standard deviation), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment).

Table A.7: Meta-analytic estimates by estimator, ground, and group

Treated	**k**	**Estimate [CI[“95%”]]**
Panel A: RE, unadjusted		
Race, ethnic identity, and national origin		
Middle Eastern, Northern African	286	-0.41 [-0.48, -0.34]
Indigenous, Native	17	-0.38 [-1.00, >1M]
Asian	201	-0.34 [-0.45, -0.20]
Turkish	50	-0.24 [-0.32, -0.14]
European, Northern American, White	297	-0.29 [-0.38, -0.19]
Black, Central African, African American	407	-0.25 [-0.29, -0.20]
Hispanic, Southern American	76	-0.14 [-0.27, 0.03]
Other	10	-0.19 [-0.35, 0.00]
Sex and gender		
Gender-neutral pronouns	105	-0.10 [-0.27, 0.11]
Transgender	29	-0.13 [-1.00, >1M]
Woman	543	0.07 [-0.01, 0.15]
Age		
Old	233	-0.38 [-0.52, -0.21]
Young	43	-0.01 [-0.34, 0.48]
Sexual orientation		
Same-sex affiliation	179	-0.44 [-0.65, -0.10]
Same-sex orientation	13	0.02 [0.00, 0.04]
Religion		
Muslim	115	-0.33 [-0.46, -0.17]
Christian	13	-0.14 [-0.44, 0.34]
Jew	14	-0.16 [-0.65, 1.06]
Parenthood and fertility		
Parent	177	-0.06 [-0.14, 0.03]
Health and disability		
Physical disability	48	-0.33 [-0.47, -0.15]
Mental disorder	24	-0.19 [-0.27, -0.11]
Overweight	56	-0.11 [-1.00, >1M]
Physical illness	25	-0.75 [-1.00, >1M]
Political orientation		
Political affiliation	114	-0.01 [-0.08, 0.06]
Wealth and class		
Low social class	67	-0.05 [-0.09, -0.02]
Physical appearance		

Treated	k	Estimate [CI["95%"]]
Low physical attractiveness	47	-0.29 [-0.53, 0.08]
Criminal record		
Criminal record	63	-0.34 [-0.41, -0.26]
Civil status		
Married	20	-0.09 [-0.32, 0.23]
Union affiliation		
Union membership	10	-0.11 [-0.16, -0.06]
Panel B: UWLS, bias-adjusted		
Race, ethnic identity, and national origin		
Middle Eastern, Northern African	286	-0.37 [-0.42, -0.30]
Indigenous, Native	17	-0.30 [-0.49, -0.05]
Asian	201	-0.27 [-0.36, -0.15]
Turkish	50	-0.19 [-0.24, -0.13]
European, Northern American, White	297	-0.19 [-0.28, -0.08]
Black, Central African, African American	407	-0.17 [-0.23, -0.10]
Hispanic, Southern American	76	-0.13 [-0.20, -0.05]
Other	10	-0.06 [-0.14, 0.04]
Sex and gender		
Gender-neutral pronouns	105	-0.12 [-0.16, -0.08]
Transgender	29	-0.07 [-0.15, 0.00]
Woman	543	0.06 [0.03, 0.08]
Age		
Old	233	-0.27 [-0.32, -0.22]
Young	43	0.11 [0.03, 0.20]
Sexual orientation		
Same-sex affiliation	179	-0.22 [-0.37, -0.02]
Same-sex orientation	13	-0.02 [-0.08, 0.05]
Religion		
Muslim	115	-0.23 [-0.33, -0.11]
Christian	13	-0.22 [-0.36, -0.05]
Jew	14	-0.05 [-0.18, 0.10]
Parenthood and fertility		
Parent	177	0.00 [-0.04, 0.03]
Health and disability		
Physical disability	48	-0.21 [-0.27, -0.13]
Mental disorder	24	-0.15 [-0.18, -0.12]

Treated	**k**	**Estimate [CI["95%"]]**
Overweight	56	-0.12 [-0.19, -0.06]
Physical illness	25	0.10 [-0.04, 0.25]
Political orientation		
Political affiliation	114	-0.04 [-0.13, 0.05]
Wealth and class		
Low social class	67	-0.04 [-0.10, 0.01]
Physical appearance		
Low physical attractiveness	47	-0.18 [-0.30, -0.03]
Criminal record		
Criminal record	63	-0.23 [-0.33, -0.12]
Civil status		
Married	20	-0.12 [-0.19, -0.05]
Union affiliation		
Union membership	10	-0.11 [-0.16, -0.06]
Panel C: UWLS, cov- and bias-adjusted		
Race, ethnic identity, and national origin		
Middle Eastern, Northern African	286	-0.32 [-0.39, -0.26]
Asian	201	-0.07 [-0.14, 0.00]
European, Northern American, White	297	-0.13 [-0.18, -0.07]
Black, Central African, African American	404	-0.11 [-0.15, -0.07]
Sex and gender		
Woman	543	0.07 [0.04, 0.10]
Criminal record		
Criminal record	63	-0.24 [-0.40, -0.05]

Notes. Acronyms used: CI (confidence interval), RE (random effects), UWLS (unrestriced weighted least squares). 'k' represents the number of conditional average treatment effects.

Table A.8: MAES estimates; all grounds

	+0.5%	**+1.0%**	**+1.5%**	**+2.0%**	**+2.5%**	**+3.0%**
τ = −2Q	-0.02 (0.02)	-0.02 (0.02)	-0.01 (0.05)	0.01 (0.06)	0.03 (0.06)	0.03 (0.07)
τ = +0Q	0.02 (0.04)	0.02 (0.05)	0.10 (0.07)	0.08 (0.05)	0.09 (0.06)	0.05 (0.05)
τ = +1Q	-0.20* (0.07)	-0.20* (0.08)	-0.22** (0.06)	-0.15*** (0.03)	-0.20*** (0.04)	-0.22*** (0.04)
τ = +2Q	-0.08** (0.02)	-0.05 (0.03)	-0.04 (0.03)	-0.02 (0.04)	0.06 (1,465.03)	0.10 (1,427.08)
τ = +3Q	-0.05 (0.08)	-0.02 (0.08)	0.06 (0.09)	0.15 (0.09)	0.21* (0.10)	0.18 (0.10)
τ = +4Q	-0.13 (0.14)	0.04 (0.11)	0.11 (0.10)	0.22* (0.10)	0.28** (0.10)	0.25* (0.11)
k	1,687	1,517	1,155	1,041	969	925
Adj. R^2	0.50	0.50	0.53	0.54	0.56	0.56

Notes. Acronyms used: MAES (meta-analytic event study). Estimates are aggregated dynamic treatment effects evaluated at different thresholds of ≥2 quarterly rises in the unemployment rate and represent proportional changes in the positive response ratio (PRR) relative to the quarter preceding a ≥2 quarterly rises in the unemployment rate. In the MAES specifications, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, region, treatment group, and a linear year trend. Standard errors (between parentheses) are two-way clustered by study and country. * $p < .05$, ** $p < .01$, *** $p < .001$

Table A.9: UWLS-MRA of ln PRR on cyclicality measures; age

	Age	**U–U* x Age**
Treated age [20, 35)	-0.57 (0.40)	0.44*** (0.05)
Treated age [35, 70]	-0.67** (0.16)	-0.11** (0.03)
U–U*	—	-234.36** (53.39)
U–U* x Treated age [20, 35)	—	-39.62** (10.79)
U–U* x Treated age [35, 70]	—	-40.21*** (2.91)
Control age	0.04** (0.01)	-0.17*** (0.03)
U–U* x Control age	—	2.14** (0.48)
k	276	261
Adj. R^2	0.63	0.79

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for (interactions of main regressors concerning age and cyclicality with) education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and region. Standard errors (between parentheses) are two-way clustered by study and country. * p < .05, ** p < .01, *** p < .001

Table A.10: MAES estimates; race, ethnic identity, and national origin

	+0.5%	**+1.0%**	**+1.5%**	**+2.0%**	**+2.5%**	**+3.0%**
τ = −2Q	-0.15 (0.12)	-0.18 (0.16)	0.05 (0.26)	-0.05 (0.31)	0.16 (0.41)	0.29 (0.45)
τ = +0Q	0.04 (0.19)	-0.12 (0.11)	0.04 (0.21)	-0.09 (0.31)	0.16 (0.41)	0.32 (0.44)
τ = +1Q	-0.38*** (0.07)	-0.40** (0.08)	-0.43** (0.08)	-0.44** (0.10)	-0.46*** (0.08)	-0.56* (0.13)
τ = +2Q	0.00 (0.12)	-0.01 (0.16)	0.01 (0.18)	0.00 (0.22)	0.28 (0.30)	0.35 (0.28)
τ = +3Q	-0.10 (0.09)	-0.13 (0.12)	0.07 (0.22)	-0.05 (0.29)	0.19 (0.41)	0.37 (0.45)
τ = +4Q	0.00 (0.12)	-0.03 (0.13)	0.19 (0.21)	0.03 (0.31)	0.30 (0.42)	0.49 (0.45)
k	561	506	363	348	325	316
Adj. R^2	0.46	0.45	0.56	0.58	0.60	0.60

Notes. Acronyms used: MAES (meta-analytic event study). Estimates are aggregated dynamic treatment effects evaluated at different thresholds of ≥2 quarterly rises in the unemployment rate and represent proportional changes in the positive response ratio (PRR) relative to the quarter preceding a ≥2 quarterly rises in the unemployment rate. In the MAES specifications, I control for education level, employment status, gender, migrant generation, firm profit status, matched design, callback type, ISCO-08 major group occupation, region, treatment group, and a linear year trend. Standard errors (between parentheses) are two-way clustered by study and country. * $p < .05$, ** $p < .01$, *** $p < .001$

Table A.11: MAES estimates; sex and gender

	+0.5%	**+1.0%**	**+1.5%**	**+2.0%**	**+2.5%**	**+3.0%**
$\tau = -2Q$	0.04 (0.08)	-0.05 (0.12)	-0.11 (0.14)	-0.11 (0.14)	-0.12 (0.22)	-0.17 (0.23)
$\tau = +0Q$	-0.04 (0.07)	-0.16 (0.08)	-0.18 (0.12)	-0.20 (0.15)	-0.23 (0.14)	-0.22 (0.17)
$\tau = +1Q$	0.11 (0.06)	0.14* (0.07)	0.08 (0.18)	0.20 (0.19)	0.18 (0.24)	0.21 (0.24)
$\tau = +2Q$	-0.06 (0.09)	-0.04 (0.07)	-0.06 (0.07)	-0.07 (0.09)	-0.11 (0.15)	-0.10 (0.18)
$\tau = +3Q$	-0.07 (0.12)	-0.06 (0.09)	-0.08 (0.11)	-0.10 (0.14)	-0.13 (0.13)	-0.10 (0.15)
k	321	311	258	230	206	195
Adj. R^2	0.10	0.10	0.11	0.14	0.11	0.12

Notes. Acronyms used: MAES (meta-analytic event study). Estimates are aggregated dynamic treatment effects evaluated at different thresholds of ≥2 quarterly rises in the unemployment rate and represent proportional changes in the positive response ratio (PRR) relative to the quarter preceding a ≥2 quarterly rises in the unemployment rate. In the MAES specifications, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, region, treatment group, and a linear year trend. Standard errors (between parentheses) are two-way clustered by study and country. * $p < .05$, ** $p < .01$, *** $p < .001$

Table A.12: UWLS-MRA of ln PRR on cyclicality measures; Middle Eastern and Northern African

	ALL	**EU**
Panel A: Unemployment rate		
ln U	-0.40 (0.25)	-0.40 (0.26)
k	282	267
Adj. R^2	0.40	0.41
Panel B: Unemployment rate (piecewise)		
ln U [0, 0.10)	-1.27** (0.31)	-1.35** (0.33)
ln U [0.10, +Inf)	0.21 (0.30)	0.25 (0.29)
k	282	267
Adj. R^2	0.46	0.47
Panel C: Vacancy rate		
ln V	0.16 (0.19)	0.16 (0.19)
k	237	227
Adj. R^2	0.43	0.44
Panel D: Vacancy rate (piecewise)		
ln V [0, 0.03)	-0.40 (0.26)	-0.78* (0.26)
ln V [0.03, +Inf)	0.84*** (0.17)	0.75*** (0.09)
k	237	227
Adj. R^2	0.48	0.52
Panel E: Vacancy-to-unemployment ratio		
ln V/U	0.10 (0.11)	0.10 (0.11)
k	237	227
Adj. R^2	0.43	0.44
Panel F: Deviation from full-employment rate of unemployment		
U–U*	-0.74 (2.24)	-0.76 (2.35)
k	237	227
Adj. R^2	0.41	0.43

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), ALL (worldwide), EU (Europe), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and region. Standard errors (between parentheses) are two-way clustered by study and country. * p < .05, ** p < .01, *** p < .001

Table A.13: UWLS-MRA of ln PRR on cyclicality measures; Black, Central African, African American

	ALL	**NA**
Panel A: Unemployment rate		
ln U	0.05 (0.04)	0.12 (0.07)
k	400	305
Adj. R^2	0.20	0.22
Panel B: Unemployment rate (piecewise)		
ln U [0, 0.10)	0.16 (0.11)	0.18 (0.11)
ln U [0.10, +Inf)	0.26 (0.16)	0.23 (0.14)
k	400	305
Adj. R^2	0.20	0.23
Panel C: Vacancy rate		
ln V	0.07 (0.11)	0.12 (0.13)
k	392	305
Adj. R^2	0.23	0.21
Panel D: Vacancy rate (piecewise)		
ln V [0, 0.03)	-0.97 (0.54)	-0.30 (0.45)
ln V [0.03, +Inf)	0.03 (0.13)	0.17 (0.20)
k	392	305
Adj. R^2	0.24	0.22
Panel E: Vacancy-to-unemployment ratio		
ln V/U	-0.04 (0.03)	-0.05 (0.05)
k	392	305
Adj. R^2	0.23	0.21
Panel F: Deviation from full-employment rate of unemployment		
U–U*	2.03 (0.92)	2.38 (1.60)
k	392	305
Adj. R^2	0.23	0.22

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), ALL (worldwide), NA (North America), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and region. Standard errors (between parentheses) are two-way clustered by study and country, except for the regressions for North America, where I use study-level clustering. * $p < .05$, ** $p < .01$, *** $p < .001$

Table A.14: UWLS-MRA of ln PRR on cyclicality measures; European, North American, White

	ALL	**EU**
Panel A: Unemployment rate		
ln U	0.06 (0.07)	0.10*** (0.02)
k	289	278
Adj. R^2	0.48	0.47
Panel B: Unemployment rate (piecewise)		
ln U [0, 0.10)	-0.83* (0.32)	-0.31* (0.12)
ln U [0.10, +Inf)	0.46*** (0.07)	0.47*** (0.06)
k	289	278
Adj. R^2	0.51	0.48
Panel C: Vacancy rate		
ln V	-0.18*** (0.02)	-0.18*** (0.02)
k	224	224
Adj. R^2	0.39	0.39
Panel D: Vacancy rate (piecewise)		
ln V [0, 0.03)	-0.45*** (0.01)	-0.45*** (0.01)
ln V [0.03, +Inf)	-0.21 (0.26)	-0.21 (0.26)
k	224	224
Adj. R^2	0.39	0.39
Panel E: Vacancy-to-unemployment ratio		
ln V/U	-0.07*** (0.00)	-0.07*** (0.00)
k	224	224
Adj. R^2	0.38	0.38
Panel F: Deviation from full-employment rate of unemployment		
U–U*	1.63*** (0.02)	1.63*** (0.02)
k	224	224
Adj. R^2	0.38	0.38

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), ALL (worldwide), EU (Europe), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and region. Standard errors (between parentheses) are two-way clustered by study and country. * p < .05, ** p < .01, *** p < .001

Table A.15: UWLS-MRA of ln PRR on cyclicality measures; Asian

	ALL
Panel A: Unemployment rate	
ln U	0.09 (0.13)
k	193
Adj. R^2	0.64
Panel B: Unemployment rate (piecewise)	
ln U [0, 0.10)	-0.56** (0.17)
ln U [0.10, +Inf)	0.61*** (0.13)
k	193
Adj. R^2	0.67
Panel C: Vacancy rate	
ln V	-0.23* (0.08)
k	155
Adj. R^2	0.37
Panel D: Vacancy rate (piecewise)	
ln V [0, 0.03)	-0.81*** (0.04)
ln V [0.03, +Inf)	0.78*** (0.01)
k	155
Adj. R^2	0.39
Panel E: Vacancy-to-unemployment ratio	
ln V/U	-0.12** (0.03)
k	155
Adj. R^2	0.37
Panel F: Deviation from full-employment rate of unemployment	
U–U*	3.13*** (0.23)
k	155
Adj. R^2	0.38

Notes. Acronyms used: UWLS-MRA (unrestricted weighted least squares meta-regression), PRR (positive response ratio), ALL (worldwide), U (unemployment rate), V (vacancy rate), U* (full-employment rate of unemployment). In the meta-regressions, I control for education level, employment status, gender, firm profit status, matched design, callback type, ISCO-08 major group occupation, and region. Standard errors (between parentheses) are two-way clustered by study and country. * p < .05, ** p < .01, *** p < .001

A.2 Supplementary figures

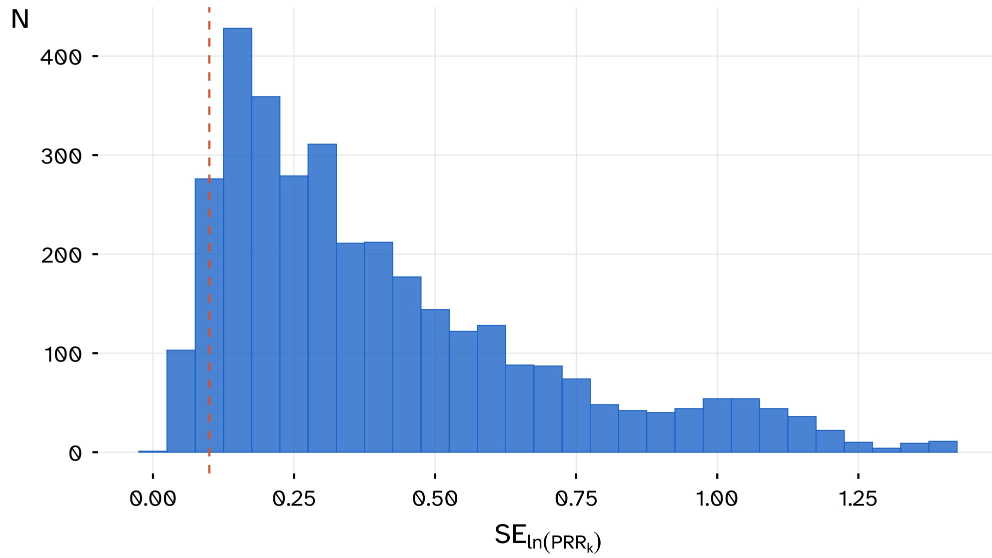

Figure A.1: Histogram of standard errors of the log positive response ratios. The dashed vertical line shows a standard error value of 0.1, which is used as baseline to correct for small-study publication bias in the predictions based on the UWLS meta-regressions.

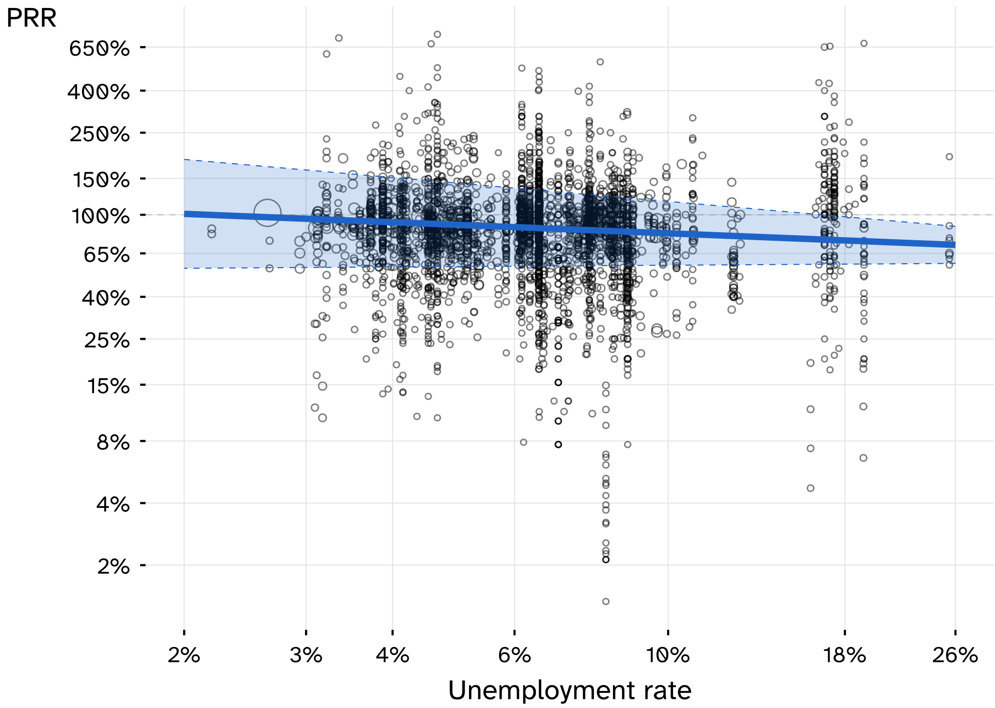

Figure A.2: Unemployment and hiring discrimination. Points represent conditional average treatment effects or positive response ratios (PRR) for the entire sample (i.e., across discrimination grounds). Point sizes are proportional to the ratios' inverse variances, which are equivalent to the regression weights. The solid blue line shows model-implied UWLS-MRA predictions, evaluated at $SE_{\ln(PRR_k)} = 0.1$, based on the specification in Eq. (14) . These predictions condition on study design covariates, i.e. education level, employment status, gender, firm profit status, matched design, callback type, and occupation, region, and treatment group fixed effects (all evaluated at the observed values). Standard errors are two-way clustered by study and country. The shaded area depicts the 95% confidence band. Axes are log-transformed with back-transformed tick labels.

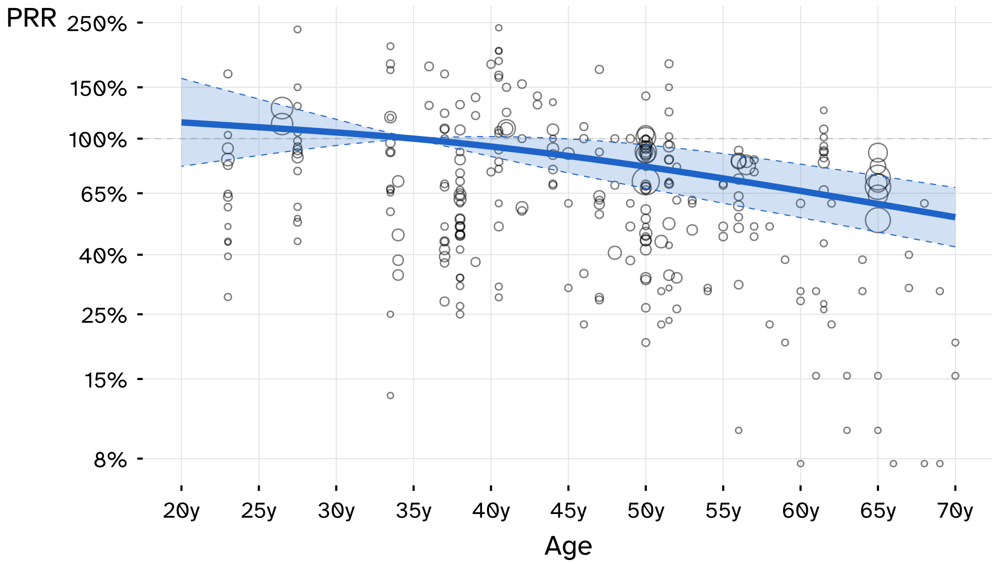

Figure A.3: Age discrimination in hiring by age. Points represent positive response ratios (PRR). Point sizes are proportional to the ratios' inverse variances, which are equivalent to the regression weights. The solid blue line shows model-implied average comparisons, evaluated at $SE_{\ln(PRR_k)} = 0.1$, between applicants at age X and applicants aged 35 (i.e., the median control age), where X represents an age value on the x-axis. The shaded area depicts the 95% confidence band. Predictions are derived from an UWLS-MRA specification based on Eq. (13) including a linear term for the control group age and a piecewise-cubic spline in treatment age with a breakpoint at age 35. The comparisons condition on study design covariates, i.e. education level, employment status, gender, firm profit status, matched design, callback type, and occupation and region fixed effects (all evaluated at the observed values). Standard errors are two-way clustered by study and country. Y-axis is log-transformed with back-transformed tick labels.

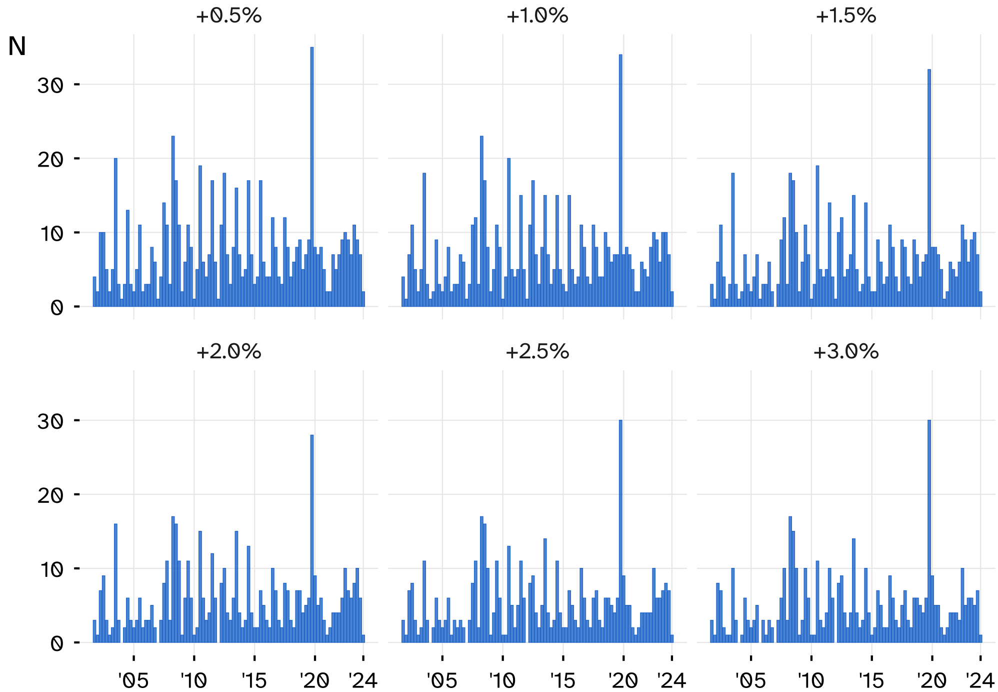

Figure A.4: Frequencies of unemployment rate 'event' onsets by relative threshold. Bars plot the number of quarters by country in which the unemployment rate begins a relative quarter-over-quarter increase of at least $\Delta U/U$ for two consecutive periods. Facets correspond to different thresholds with $\Delta U/U \in \{+0.5\%, +1.0\%, +1.5\%, +2.0\%, +2.5\%, +3.0\%\}$.

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