On the Allocation of People, Ideas, and Talent

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Preface

In this dissertation, I analyze different dimensions of the allocation of people, ideas, and talent. Two themes recur throughout: on the one hand, the sorting and selection of individuals; on the other hand, the measurement of underlying drivers of behavior. Such drivers include information on individuals' performance, but also their ideas (such as attitudes).

Chapter 1 investigates the relationship between superiors' gender attitudes and women's career progression. I introduce a novel measure of gender attitudes based on individuals' use of gender-inclusive language. I construct gender inclusivity scores for German ministers and link them to self-collected data on civil servants in German ministries over four decades. Thus, for each civil servant at each point in time, I observe the score for the gender attitudes of the minister in charge. I estimate the effect of ministers with different gender attitudes on women's promotion probabilities. Ministers with higher gender attitudes promote more women, thereby affecting the allocation of talent.

Chapter 2, which is based on co-authored work with Carlo Schwarz and Fabian Waldinger, analyzes the impact of performance metrics on the allocation of talent in science. The introduction of the Science Citation Index, the first citation database, made scientists' citation counts visible. Soon universities and academics started to use citation counts as performance metrics. Our empirical strategy uses quasi-random variation in the visibility of individual scientists' citation counts. This allows us to estimate how the visibility of citations affects scientists' careers. We find that citation metrics affect the assortative matching between scientists and universities, and that disadvantaged groups benefit from revealing performance metrics. Performance metrics also affect promotions and receiving research grants.

Chapter 3, which is based on co-authored work with Emilio Esguerra, studies how war grievances that individuals associate with their former enemy affect whether individuals comply with oppressive post-conquest policy. We focus on the German-speaking region of South Tyrol, which after World War I was annexed by Italy and subjected to severe assimilation policy. Using self-digitized archival data, we analyze whether war grievances directed at Italy made South Tyroleans more likely to avoid assimilation—either by emigrating or by giving their children more Germanic names. Overall, we find little evidence that these Italy-specific war grievances made individuals avoid assimilation.

This dissertation makes methodological and thematic contributions. In each chapter, I propose a new way of measuring an underlying driver of decision-making and then study its effects on the allocation of people, ideas, and talent in society.

Chapter 1

Speaking of Gender: Superiors' Gender Attitudes and Women's Careers

This chapter is based on single-authored work (see Hager, 2024). I am grateful for financial support from the Nationalökonomische Gesellschaft and from the Evidence-Based Economics program of the Elite Network of Bavaria.

1.1 Introduction

When society recruits its leaders from a limited pool of candidates, it loses efficiency (Bertrand, 2018; Hsieh et al., 2019). Despite the progress of the past decades, women are still underrepresented in leadership positions. In Germany in 2023, only 24% of DAX executives were women (Ernst and Young, 2024). In the German public sector, only one in every three civil servants with a leadership role was female (Destatis, 2024). To address the underrepresentation of women in the leadership of organizations, understanding its causes is crucial. Recent research has emphasized the role of superiors in differentially shaping the careers of women and men (e.g., Cullen and Perez-Truglia, 2023; Haegele, 2024). Yet, apart from a few exceptions (e.g., Ronchi and Smith, 2024), little is known about how superiors' gender attitudes affect women's career progression within organizations.

In this paper, I investigate the relationship between superiors' gender attitudes and women's career outcomes. Studying the effects of gender attitudes on labor market outcomes is empirically challenging: gender attitudes are generally unobservable. I address this challenge by introducing a novel measure of gender attitudes based on speech data. I construct this measure for all German ministers and link it to self-collected data on civil servants in leadership positions in German ministries. This allows me to estimate the effect of ministers with different gender attitudes on the probability that female civil servants in the respective ministry get promoted.

In the first part of this paper, I develop a new method for measuring gender attitudes. I construct a score that captures the degree to which a person speaks gender-inclusively. This score relies on the gender-marked nature of the German language: almost every word referring to a person has a female and a male form. While in English, words with gender-specific forms (such as waitress and waiter) are the exception, in German they are the rule. For example, the word for a female doctor (*Ärztin*) is different from the word for a male doctor (*Arzt*). Traditionally, male-specific words are considered generic, i.e., women can be referred to using the male form of a person-specific noun. In recent decades, however, gender-inclusive language has emerged with the explicit goal of making women more visible in language. Instead of referring to a group of male and female doctors by the generic masculine plural (*Ärzte*), the gender-inclusive way of denoting such a mixed-gender group involves both forms (e.g., *Ärztinnen und Ärzte*).

I define the gender inclusivity score as the ratio between the female-specific words and all person-specific words an individual uses. Next, I apply this method to data on German parliamentary speeches and calculate individual-level scores for over two thousand politicians. In several validation checks, I show that the gender inclusivity score is a meaningful way to measure gender attitudes.

I present a series of facts: first, gender-inclusive language in the German parliament has increased by 50% between 1993 and 2018. This is driven by members of all parties and genders. Second, women across the political spectrum use more gender-inclusive language than men. Moreover, male parliamentarians in the 2010s speak as gender-inclusively as did women in the 1990s. Third, left-wing politicians use more gender-inclusive language than right-wing politicians. For example, Greens thirty years ago spoke more gender-inclusively than did Conservatives in 2018. Fourth, there is considerable variation in individuals' gender inclusivity scores. This holds within genders, within parties, and over time.

Further, I show that politicians with a higher gender inclusivity score are more likely to vote in female-friendly ways. In 1997, several Conservative and Liberal members of parliament voted against the criminalization of marital rape. Politicians who voted against the law had a markedly lower gender inclusivity score than those voting in favor. This holds also within parties. These findings add to the validity of using the gender inclusivity score as a measure of gender attitudes.

In the second part of the paper, I use this measure to analyze whether female employees benefit from superiors with higher gender attitudes. I digitize data on civil servants in leadership positions in all German federal ministries over the past four decades. Since I observe civil servants' leadership ranks over time, I can infer whether an individual was promoted in a given year. This allows me to reconstruct around five thousand employees' career trajectories through the German federal administration. In particular, I can observe gender differences in promotion outcomes.

I link the data on civil servants with ministers and their individual gender inclusivity scores. Thus, for each civil servant at each point in time, I observe the score for the gender attitudes of the minister in charge. Regular changes in ministers introduce variation within ministries over time in ministers' gender attitudes. My empirical strategy estimates how female employees' probability of being promoted is affected by ministers who differ in their gender attitudes.

I find that ministers with higher gender attitudes promote more women. At the same time, I find no such effect on men's promotion probabilities. A minister with one standard deviation higher gender attitudes increases women's promotion probability by around two percentage points (around 30% relative to the mean). In my analysis, I hold constant potential selection of ministers into specific ministries and potential effects of governments by including individual and time fixed effects. Across specifications, the result is stable.

Last, I explore heterogeneities of this result. I find that the effect is driven by ministers at the high end and the low end of the gender attitudes distribution. Ministers in the upper quartile in the gender attitudes distribution increase women's promotion probability by four percentage points relative to ministers in the middle half of the distribution. Further, I investigate whether there are differences in the effect between male and female ministers. I find that, if anything, the effect is driven by male ministers with high gender attitudes.

My study contributes to research at the intersection of three strands of literature. The first literature focuses on careers within organizations, and especially reasons for the underrepresentation of specific groups in leadership. Various channels have been suggested for promotion and representation gaps in organizations, ranging from social interactions with superiors (e.g., Cullen and Perez-Truglia, 2023) to outright discrimination (e.g., Aneja and Xu, 2022). Since gender quotas have become a common policy tool, increased attention has been paid to the effects of female leaders. Overall, the literature finds limited effects of female leaders on the careers of women in their organizations (e.g., Bertrand et al., 2019; Maida and Weber, 2022). However, other characteristics of superiors—such as their attitudes—might be more important for closing an organization's representation gap. For example, Ronchi and Smith (2024) use the birth of a daughter as a positive shock to gender attitudes and thereby show that superiors' gender attitudes impact their female employees' careers. This is where I contribute to the literature: rather than relying on a categorical characteristic of a superior (such as gender or having a daughter), I measure superiors' gender attitudes on a continuum and relate them to rich personnel data on German civil servants over time.

Second, I contribute to a literature that seeks to identify the contribution of taste-based discrimination to observed employment gaps. Becker (1957) modeled discrimination in the labor market as a result of biased employers who differ in their discriminatory preferences. Empirical analyses of Becker's model are complicated by the challenge that measures of such discriminatory preferences are often not available, especially at the firm or manager level (e.g., Charles and Guryan, 2008). An experimental strand in this literature manipulates employers' perceptions of applicants by randomly assigning different characteristics to job applications (e.g., Bertrand and Mullainathan, 2004; Oreopoulos, 2011). While these audit studies can measure discriminatory outcomes, they do not measure discriminatory preferences. To address this limitation, Kline et al. (2022) sent applications to multiple jobs at the same firms and, thus, measured discriminatory bias at the company level. I contribute to the literature on taste-based discrimination by measuring the distribution of gender attitudes of individual superiors. I then relate these individual-level scores to women's career outcomes.

Last, I contribute to a literature that develops methods to measure attitudes and investigates the effects of attitudes on decision-making. Since people can misrepresent their views in surveys, psychological measures for attitudes such as implicit association tests (IATs) have become prevalent in economics and other social sciences. For example, Glover et al. (2017) use IATs to study the influence of attitudes towards minorities on performance in the workplace, and Carlana (2019) shows that teachers with stereotypical gender attitudes are harmful to girls' school achievements. However, IAT scores have been criticized for being noisy, easy to manipulate, and unstable over time within an individual (e.g. Gawronski et al., 2017; Schimmack, 2021). Thus, other work has focused on individuals' observed behavior to measure revealed attitudes. For example, Ash et al. (2024) construct a score of judges' gender-stereotyped language. They find that more stereotyping judges interact differently with female judges. While I also measure attitudes based on text data over many years, my method is more tractable and relies on no assumptions about word meanings. Moreover, I document a series of facts on the evolution of gender-inclusive language.[1]

1.2 Measuring Gender Attitudes

1.2.1 The Gender Inclusivity Score

I develop a new measure of gender attitudes based on individual-level text data. Given the salience of gender in the grammar of many languages, attitudes are likely to be revealed in people's speech (Sczesny et al., 2015). I measure how German speakers choose to represent women in their language.

Languages differ in the way gender enters their grammar. For example, German and French assign grammatical gender to every noun, including inanimate objects such as sausage or croissant. English, in contrast, does not have grammatical gender, but only gender-specific pronouns (she and he). As a result, gender-specific words for people (such as *waitress* and *waiter*) are the exception. In German they are the rule: there is a gender-specific noun for a female doctor (*Ärztin*) and for a male doctor (*Arzt*), for

[1]In studying the social norm of gender-inclusive language over time, I also contribute to a literature on the evolution of culture and social norms (e.g., Young, 2015; Giuliano and Nunn, 2020).

a female colleague (*Kollegin*) and for a male colleague (*Kollege*), and so on. Almost every noun denoting a person has a female-specific and a male-specific form.[2]

Traditionally, male-specific nouns for people are considered generic. The so-called generic masculine means that the male-specific term can also be used for women. For example, the word for protester (*Demonstrant*) can refer to both men and women, even though there is a female-specific form (*Demonstrantin*). Especially in the plural, this leads to an oddity: when 99 women gather in a protest, they are *99 Demonstrantinnen*; should only one man join, they suddenly become *100 Demonstranten*.

Feminist linguists and cognitive scientists have criticized the generic masculine as a sexist convention (e.g., Hofstadter, 1985; Saul et al., 2022). Thus, feminists have proposed that equality of the genders must not stop at grammar: women, too, should be visible in language. They have suggested gender-inclusive language as an alternative to the convention of the generic masculine (Journalistinnenbund, 2024b). From the feminist or gender-inclusive point of view, a mixed-gender group of protesters should be denoted as what they are: *Demonstrantinnen und Demonstranten* (i.e., female and male protesters).[3] Thus, gender inclusivity is a salient feature in German; more so than it is in English, for example. Over the past decades, gender-inclusive language has become widely used in public discourse. Politicians, in particular, have started to use gender-inclusive language—even among the conservative end of the spectrum (see Figure 1.3).

Using individual-level speech data, I can measure the extent to which a person chooses gender-inclusive terms. This is a continuous measure since speaking gender-inclusively is not a binary choice. One person might constantly stick to gender-inclusive language, while another person only occasionally uses gender-inclusive terms. I define the gender inclusivity score as the ratio of female-specific words to all person-specific words an individual uses:

$$Inclusivity_i = \frac{\#(\text{female-specific nouns})_i}{\#(\text{female-specific nouns})_i + \#(\text{male-specific nouns})_i}. \tag{1.1}$$

Drawing on a list of female-specific and male-specific words (see Appendix Section 1.A.2), I count how many times individual i uses female and male forms for words referring to people; i.e., this excludes other nouns such as apple or philosophy. I then calculate the proportion of female-specific nouns relative to all person-specific nouns

[2] A few person-specific words in German are grammatically neutral, and thus can refer to both men and women. For example, a member of parliament is always a *Mitglied* (member) of the Bundestag.

[3] Alternative gender-inclusive ways to refer to this mixed-gender group in writing are, for example, *DemonstrantInnen*, *Demonstrant:innen*, and *Demonstrant*innen* (see Journalistinnenbund, 2024a).

(i.e., the sum of female-specific and male-specific nouns). Thus, for each individual i, I construct the ratio $Inclusivity_i$, ranging from zero to one. For example, consider a sentence referring to citizens, voters, and taxpayers; it uses the gender-inclusive form only for citizens but not for voters and taxpayers. Out of the four person-specific words in this sentence only one is female-specific; the ratio is thus 1/4.[4]

This measure has some important advantages: first, it is tractable and intuitive. While other text-based measures of attitudes rely on assumptions about word meanings to capture the association between gender and certain stereotypes (e.g. Ash et al., 2024), my score does not. Instead, it counts gender-specific words. This makes the score easy to interpret: a person speaking gender-inclusively all the time has a score of 0.5, whereas someone who exclusively speaks in the generic masculine has a score of zero. Thus, I can compare individuals' scores, both qualitatively and quantitatively. Hypothetically, a person who speaks in the unconventional generic feminine would have a score of one. However, in practice, this does not happen and thus the score is capped at around 0.5 (see Table 1.1).

Second, with a large corpus of text data spanning many years, the gender inclusivity score is constructed from many speeches over time. Thus, it is a less noisy measure than other scores, such as IATs. Yet, this already hints at the major practical challenge of constructing this score: the availability of comprehensive speech data on individuals.

1.2.2 Gender Inclusivity Among German Politicians

I illustrate my gender inclusivity score using data from Rauh and Schwalbach (2020) on all speeches in the German parliament, the Bundestag, between March 1991 and December 2018. To make the score more tractable, I compile a list of the 100 most used person-specific words in German parliamentary speeches (see Appendix Table 1.A.1 for the ten most used words). These words amount to 95% of all instances a person-specific word was used in parliamentary speeches. I count for each individual or group how often they use the female-specific and male-specific versions of these words. I then calculate the ratio between the female-specific words and all person-specific words (see Appendix Section 1.A.2 for more details on the construction of the score). I report summary statistics on the politicians in Table 1.1.

[4]See Appendix Section 1.A.1 for an example from a German parliamentary speech.

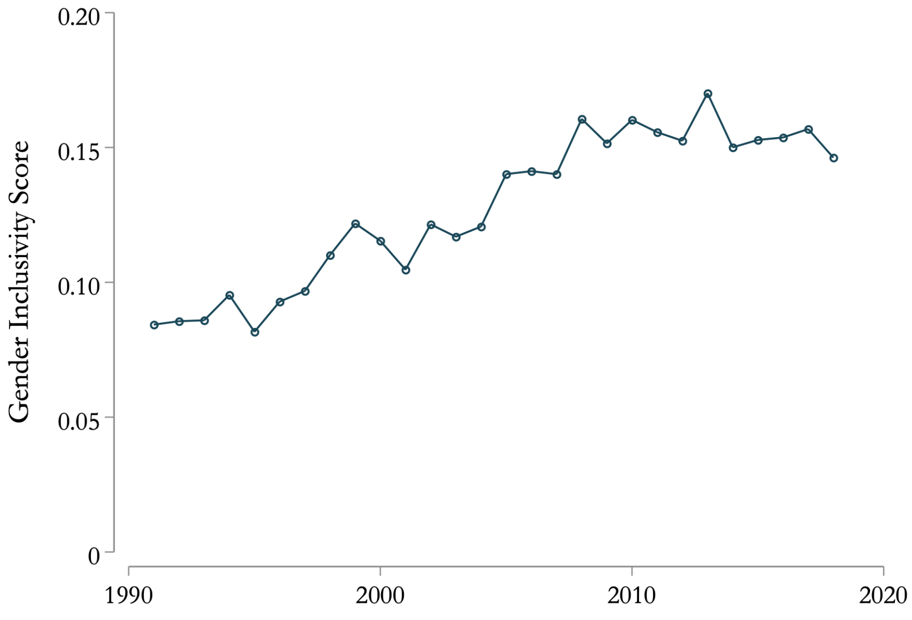
Table 1.1: Summary Statistics on German MPs

Variable	Mean	Std. Dev.	Median	Min.	Max.	Obs.
Gender Inclusivity Score	0.11	0.10	0.09	0	.54	2,144
Years in Data	8.45	6.26	7.00	1	28	2,156
Share Female	0.31	0.46				2,156
Share Linke	0.08	0.28				2,156
Share Grüne	0.08	0.27				2,156
Share SPD	0.30	0.46				2,156
Share FDP	0.11	0.31				2,156
Share CDU/CSU	0.37	0.48				2,156
Share AfD	0.04	0.20				2,156

Notes: The table reports descriptive statistics on German MPs between 1991 and 2018. *Years in Data* reports the number of years in which an individual politician has given a speech. *Gender Inclusivity Score* reports the measure of gender attitudes as suggested in Equation (1.1). To account for a small fraction of MPs who switch parties over time, I assign each MP their modal party affiliation.

In this first set of findings, I present four facts about gender-inclusive language in the German parliament. The first fact is that gender inclusivity has increased over time (see Figure 1.1). The average level of gender inclusivity in the Bundestag has almost doubled from 0.08 in 1991 to around 0.15 in 2018. This means that in 2018 out of 100 person-specific nouns used in all speeches in the German parliament 15 are female-specific. This is in line with the evolution of feminism and the social norm of using gender-inclusive language in the public sphere.

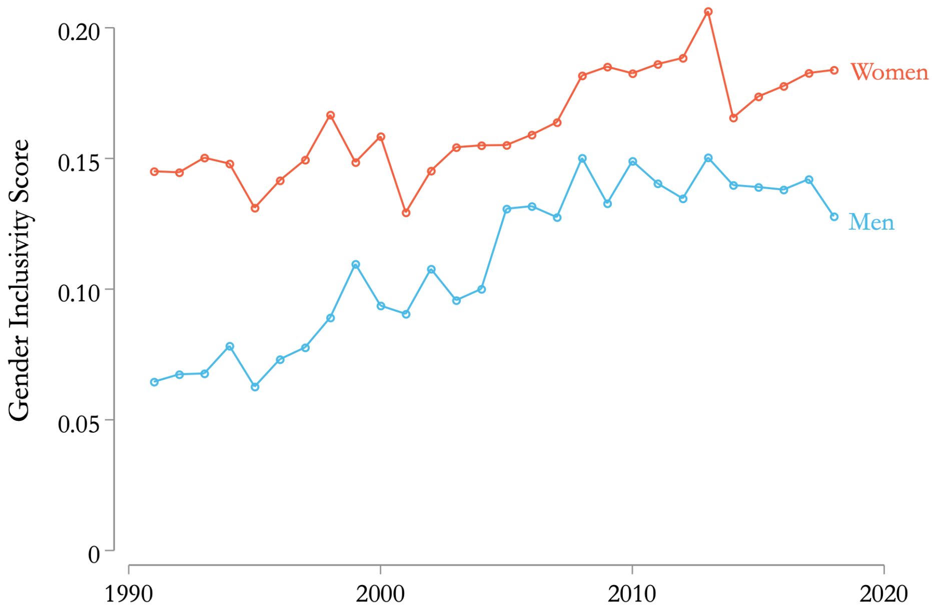
Figure 1.1: Gender Inclusivity of German MPs Over Time



Notes: This figure plots the yearly average of the gender inclusivity score over all speeches in the German parliament.

The second fact is that women speak more gender-inclusively than men (see Figure 1.2). While politicians of both genders have increased their use of gender-inclusive language, men have consistently been lagging behind women. Male MPs in the 2010s spoke as gender-inclusively as did female MPs in the 1990s.

Figure 1.2: Gender Inclusivity of German MPs Over Time, by Gender

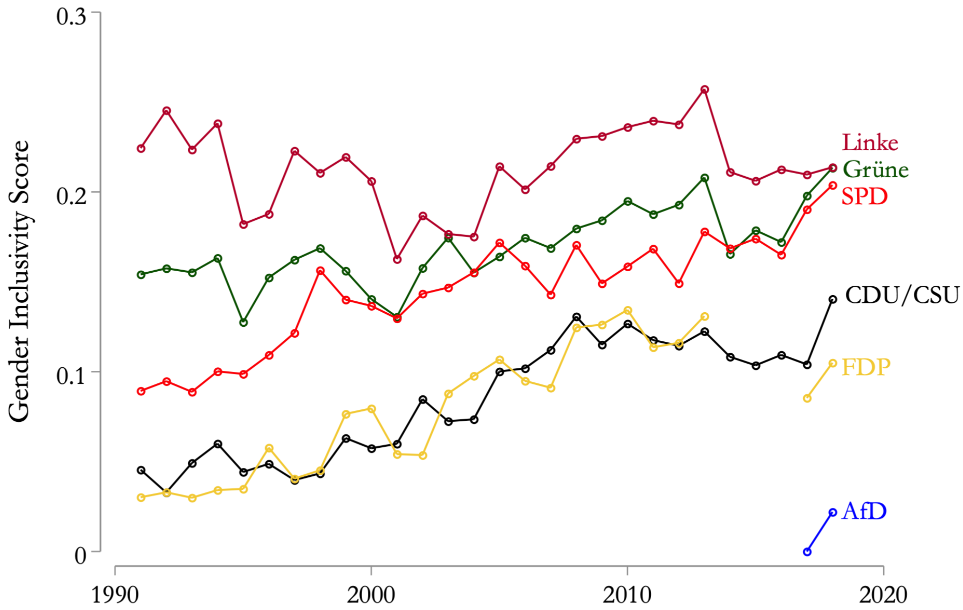


Notes: This figure plots the yearly average of the gender inclusivity score for men (blue) and for women (orange) in the German parliament.

Third, politicians from left-wing parties speak more gender-inclusively than politicians from right-wing parties (see Figure 1.3).[5] The conservative CDU/CSU and the liberal FDP have continuously increased their use of gender-inclusive language. Nevertheless, conservative MPs in 2018 spoke less gender-inclusively than the Greens in 1991. Moreover, while the center-left SPD in the early 1990s was closer to the right-wing parties, it is nowadays indistinguishable from the other left-wing parties in its use of gender-inclusive language. The far-right and reactionary AfD almost never speak gender-inclusively. In fact, they speak less gender-inclusively than did Conservatives over three decades ago. These findings are not driven by more women being in left-wing parties. I also show that these cross-party patterns hold for men and women separately (see Appendix Figures 1.B.1 and 1.B.2.)

[5]Throughout this paper, I denote the Greens (Grüne), the Left (Linke), and the Social Democrats (SPD) as left-wing parties, and the Liberals (FDP), the Conservatives (CDU/CSU), and the Alternative for Germany (AfD) as right-wing parties. I exclude the AfD, a xenophobic far-right party, from most analyses, because they only entered parliament in 2017.

Figure 1.3: Gender Inclusivity of German MPs Over Time, by Party

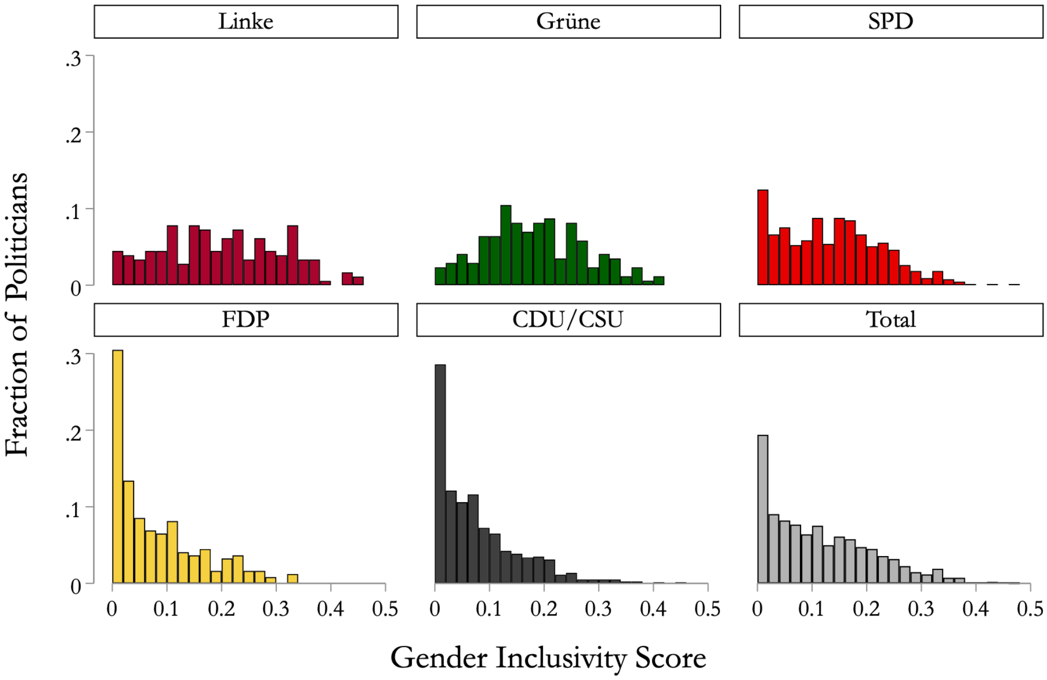


Notes: This figure plots the yearly average of the gender inclusivity score for each party in the German parliament: the Left (Linke, dark red), the Greens (Grüne, green), the Social Democrats (SPD, red), the Liberals (FDP, yellow), the Conservatives (CDU/CSU, black), and the Alternative for Germany (AfD, blue).

Fourth, there is considerable variation in individuals' gender inclusivity scores.[6] Above, I have shown that gender inclusivity meaningfully tracks political ideology. Yet, Figure 1.3 only shows the party-level means of politicians' gender inclusivity. Instead, I now construct a time-invariant politician-level score and plot the distribution of scores for each party (see Figure 1.4). The further one moves to the right on the political spectrum, the more skewed is the distribution toward gender-uninclusive language. While the Left party has an almost uniform distribution, the Greens and the SPD exhibit more bell-like distributions. The CDU/CSU and the FDP have a high proportion of individuals who virtually never use female-specific words in their speech. Yet, even within political parties, there is considerable variation in individuals' gender inclusivity scores. Regardless of the shape of the distribution, in every party there are some gender-inclusive and some gender-uninclusive politicians.

[6] I plot the distribution of gender inclusivity scores for all politicians in Appendix Figure 1.B.3. I also show that this distribution persists over time in Appendix Figure 1.B.5.

Figure 1.4: Distribution of Gender Inclusivity Scores, by Party



Notes: This figure plots the distribution of individual politicians' gender inclusivity scores within the five main parties. From left to right, these are: the Left (Linke, dark red), the Greens (Grüne, green), the Social Democrats (SPD, red), the Liberals (FDP, yellow), the Conservatives (CDU/CSU, black). I also show the distribution over all politicians in the bottom right panel. I construct the politician-level gender inclusivity score based on all speeches of a politician.

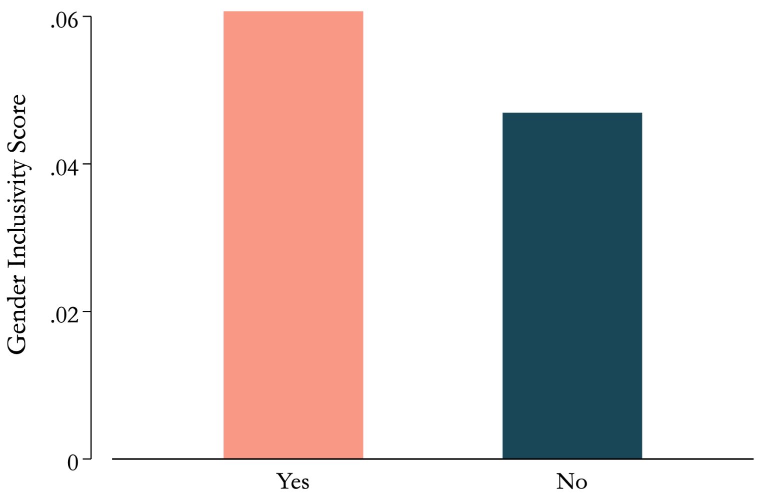
1.2.3 Validity of the Measure

These findings suggest that my gender inclusivity score is a proxy for gender attitudes. This interpretation is in line with studies in social psychology that show that attitudes affect gender-inclusive language (e.g., Sczesny et al., 2015). To further strengthen the validity of my score as a measure for gender attitudes, I show that politicians who speak more gender-inclusively also vote more female-friendly.

In the German political system, MPs usually vote in line with the party leadership. In rare cases, however, MPs within a party are split and can vote against party lines. This was the case in 1997 when the German parliament voted on a law criminalizing marital rape. While the left-wing parties unanimously voted in favor of this law, the Conservatives and the Liberals voted both ways. Thus, I can use politicians' voting decisions on this law as a validation check for my measure of gender attitudes.

I collect data from plenary protocols of the German parliament, which list MPs' voting decisions (Deutscher Bundestag, 1997). I then link politicians' voting decisions and their gender inclusivity scores. I show that those voting in favor of criminalizing marital rape also speak more gender-inclusively (see Figure 1.5). Among the CDU/CSU and the FDP, those voting in favor of the law had around 30% higher gender inclusivity scores than those voting against it. I also show that this pattern persists within parties (see Appendix Figure 1.B.6). This finding adds to the validity of using the gender inclusivity score as a proxy for gender attitudes.

Figure 1.5: Voting on the Criminalization of Marital Rape



Notes: This table shows the average gender inclusivity score of MPs from the Conservatives (CDU/CSU) and the Liberals (FDP) by whether they voted in favor of a 1997 law criminalizing marital rape.

1.3 Data on German Civil Servants

In the second part of my paper, I use this measure to investigate the role of superiors' gender attitudes in shaping women's careers. To study whether ministers with high gender attitudes are more likely to promote women, I collect employment data on civil servants in German ministries and I construct gender inclusivity scores for all ministers.

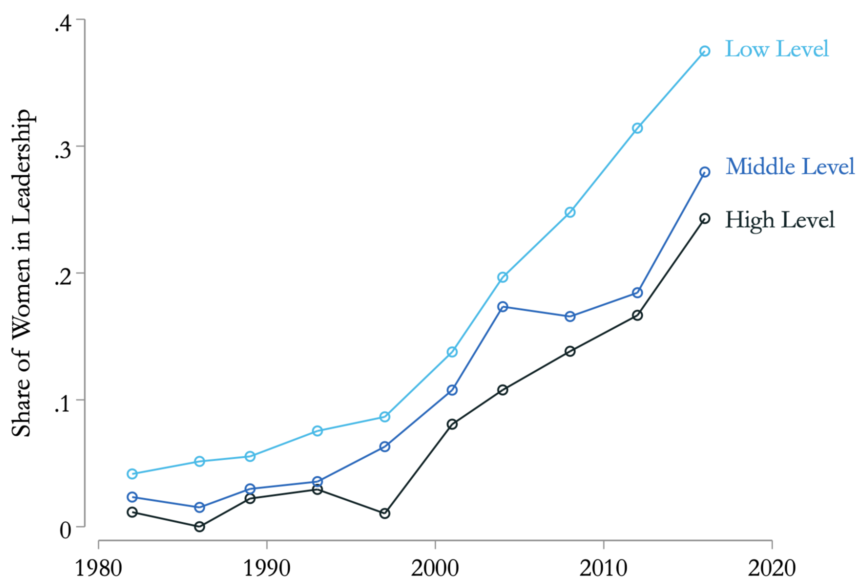
Ministerial Employment Data

I digitize data on the population of civil servants in leadership positions in German federal ministries. At least once a year, every German ministry publishes an official organization chart, listing all civil servants who hold a leadership position (see Appendix Figure 1.B.8 for an example). These charts detail the organizational structure of the ministry: each section (*Referat*; henceforth: low level leadership) belongs to a subdivision (*Unterabteilung*; middle level), which in turn belongs to a division (*Abteilung*; high level). At the top level, ministries are led by a small number of state secretaries (*Staatssekretär:in*) and the minister. Every civil servant above and including low level leadership is listed in these organization charts.

I collect organization charts for all ministries from records at the German Federal Archives (*Bundesarchiv*). I digitize a total of 138 organization charts at ten points in time from 1982 to 2016. I always choose the year before an election, so I observe civil servants' career outcomes at the end of a minister's appointment. These data contain 20,103 observations at the individual-year level.[7]

One advantage of working with these data is that the organizational structure within a ministry remains stable over time. Moreover, all ministries have the same hierarchy of state secretaries, high level, middle level, and low level leaders. This institutional setting is crucial for my main analysis because it allows me to systematically track female employees' career trajectories over time. Moreover, using these data I can compare female representation in different leadership levels across ministries and over time.

Figure 1.6: Share of Women in Different Leadership Levels



Notes: This figure plots the share of women in different leadership ranks in German ministries: low level (light blue), middle level (medium blue), and high level (dark blue).

[7]My final dataset excludes civil servants at the Ministry of Defence. The Ministry of Defence does not indicate the names of its civil servants in organization charts.

My study period coincides with the entry of women into higher leadership ranks of the German civil service. The share of women among low level leaders rose from 5% in 1982 to over 35% in 2016 (see Figure 1.6). The share of female leaders is lower in higher ranks. Yet, there was a considerable increase of women in high level leadership: from virtually no women in 1997 to around 23% in 2016. This increase was not uniform across ministries (see Appendix Figure 1.B.7). Some ministries (e.g., the Ministry for International Cooperation and Development) have appointed many women to leadership ranks. At the same time, other ministries have been lagging behind. For example, in the Finance Ministry in 2016, 23% of its low level leaders were women. At higher levels, this share is even lower.

Final Dataset on Civil Servants and Ministers' Gender Attitudes

My dataset contains 4,973 civil servants who appear at least twice. For each civil servant in each year, I observe their name, gender, their position, their pay band, and who their direct superior is. I then link individuals over time and thus construct for each civil servant a history of their career steps. Thus, I can observe, at the individual level, which civil servant was promoted in a given year. Importantly for my analysis, I construct an indicator variable $\mathbb{1}[Promotion]_{imt}$ that is equal to one if individual i working in ministry m was promoted from low level leadership to middle level leadership between $t-1$ and t (usually a period of four years).[8]

Among the civil servants who I link over time, 16% are female (see Table 1.2). 79% of all civil servants in my data were at some point low level leaders, whereas only nine percent were ever promoted to high level leadership. 28% of all civil servants were promoted at least once.

For each employee-year observation, I observe the minister in charge of the ministry at the time. I calculate the lifetime gender inclusivity score for nearly all ministers who appear in the parliamentary speech dataset. For 79 of these ministers, I calculate their gender inclusivity score, covering 127 ministry-year pairs.[9] Just as individual politicians vary in their gender inclusivity scores, so do ministers. For example, among the cabinet in 2020, ministers' gender inclusivity scores ranged from 0.04—i.e., four in every one hundred person-specific words were female-specific—to 0.35 (see Appendix Table 1.B.1). This introduces variation within ministries over time in ministers' gender

[8] I can only construct this variable for the second time I observe an individual in the data. Hence, I lose 4,973 observations from my dataset.

[9] Only for 14 ministers, covering 18 ministry-year pairs, I cannot construct the gender inclusivity ratio. All but one of these ministers served in the 1980s, which is unsurprising since the parliamentary speech data covers speeches only after 1991.

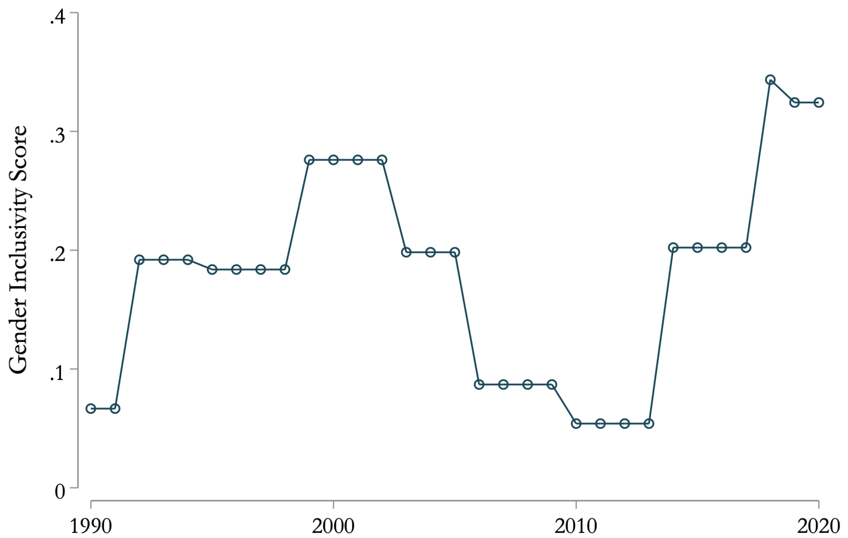
attitudes (see Figure 1.7 for an example). Thus, for each civil servant at each point in time, I observe the score for the gender attitudes of the minister in charge.

Table 1.2: Summary Statistics

Variable	Mean	Std. Dev.	Median	Min.	Max.	Obs.
Panel A: Civil Servants						
Number Times in Data	3.25	1.30	3	2	10	4,973
Share Female	0.16	0.36				4,973
Share Ever Promoted	0.28	0.45				4,973
Share Ever State Secretary	0.01	0.12				4,973
Share Ever High Level	0.09	0.28				4,973
Share Ever Mid Level	0.18	0.39				4,973
Share Ever Lower Level	0.79	0.41				4,973
Panel B: Ministers						
Number Times in Data	1.61	0.93	1	1	6	79
Gender Inclusivity Score	0.11	0.08	0.10	0	.34	79
Share Female	0.28	0.45				79
Share Grüne	0.04	0.19				79
Share SPD	0.33	0.47				79
Share FDP	0.16	0.37				79
Share CDU/CSU	0.46	0.50				79

Notes: Panel A reports summary statistics at the employee level in the ministerial employment data. I include only those employees who appear more than once. Panel B reports summary statistics for all ministers for whom I have speech data and can thus construct the gender inclusivity score.

Figure 1.7: Example: Gender Inclusivity in a Ministry Over Time



Notes: This figure plots the variation in the gender inclusivity score within a ministry over time using the example of the Ministry for Family Affairs, Senior Citizens, Women, and Youth (BMFSFJ). For each year, it shows the gender inclusivity score of the minister in charge of the ministry. Thus, the level of the score changes when a new minister is appointed. The gender inclusivity score for each minister is calculated using all their speeches in the German parliament between 1991 and 2018.

1.4 Ministers' Gender Attitudes and Women's Careers

1.4.1 Empirical Strategy

I study whether ministers with different gender attitudes affect female civil servants' careers.[10] Over time, civil servants in the same ministry experience ministers with different gender attitudes. I use this variation at the ministry level to estimate the effect of ministers with different gender attitudes on the probability that low level leaders are promoted to a higher position in the ministerial hierarchy.

Using my panel dataset on civil servants, I estimate the regression:

$$\begin{aligned}\mathbb{1}[Promotion]_{imt} = \beta_1 \cdot Inclusivity_{mt} + \beta_2 \cdot Inclusivity_{mt} \times FemaleEmployee_i \\ + X'_{mt} \cdot \theta + \alpha_t + \lambda_i + \epsilon_{imt}\end{aligned} \tag{1.2}$$

where the dependent variable $\mathbb{1}[Promotion]_{imt}$ is an indicator variable equal to one if individual i in ministry m has been promoted from the low level to middle level leadership between $t-1$ and t. $Inclusivity_{mt}$ is the gender attitudes score of the minister in ministry m at time t; its value changes when a new minister gets appointed to ministry m. $FemaleEmployee_i$ is an indicator variable equal to one if i is female. X_{mt} is a vector of control variables for the minister in charge (e.g., minister's gender and party), α_t is a full set of time fixed effects, and λ_i is a full set of individual fixed effects. To account for potential correlation of shocks within ministries across time, I cluster standard errors at the ministry level.[11]

The coefficient on $Inclusivity_{mt}$ captures the effect of ministers' gender attitudes on male civil servants' promotion probability. Gender attitudes are unlikely to predict men's promotion probabilities. Thus, I expect this coefficient to be near zero. The coefficient on the interaction term $Inclusivity_{mt} \times FemaleEmployee_i$ captures the differential effect of ministers with high gender attitudes on female civil servants' promotion probability. If the estimate for this coefficient is positive, this would indicate that, indeed, women's careers benefit from female-friendly ministers. Under the identifying assumption that ministers with higher gender attitudes do not get appointed

[10] Ministers cannot fire civil servants at will, since civil servants have a high degree of job protection. Ministers do, however, have discretionary powers in the appointment of civil servants to leadership positions (see Goetz, 2007; Jann and Veit, 2010).

[11] To avoid a bias in estimated standard errors due to the small number of clusters (16), I implement a cluster-bootstrap to calculate standard errors.

to ministries in which women were already more likely to be promoted, this effect is causally estimated.

1.4.2 Main Findings

I report estimates of Equation (1.2) in Table 1.3. The first column includes individual fixed effects and time fixed effects. The point estimate on $Inclusivity_{mt}$ is 0.02 and is not statistically significant. This is in line with the idea that gender attitudes do not affect the promotion probability of men in a ministry. The point estimate on $Inclusivity_{mt} \times FemaleEmployee_i$ is 0.23 and is statistically significant at the 10%-level. An increase in the gender inclusivity score by 0.01 is associated with a 0.23 percentage point increase in women's promotion probability. In other words, ministers with one standard deviation higher gender attitudes increase the promotion probability of women by around two percentage points ($2 \approx 0.23 \times 8.3$; i.e., the point estimate times the standard deviation of ministers' gender inclusivity scores, see Table 1.2).

Table 1.3: Main Results

	Dependent Variable: Promotion			
	(1)	(2)	(3)	(4)
Gender Inclusivity	0.02	0.01	0.03	
	(0.06)	(0.06)	(0.06)	
Gender Inclusivity × Female Employee	0.23*	0.23*	0.25**	0.28**
	(0.13)	(0.13)	(0.13)	(0.13)
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	
Female Minister Control		Yes	Yes	
Party Minister Controls			Yes	
Ministry × Time Fixed Effects				Yes
Observations	7,751	7,751	7,751	7,751
R^2	0.706	0.706	0.706	0.715
Dependent Variable Mean	6.54	6.54	6.54	6.54

Notes: This table reports estimates of Equation (1.2). The dependent variable is an indicator equal to one if individual i working in ministry m was promoted from a low level leadership position to middle level leadership at time t. The explanatory variable *Gender Inclusivity* is the gender inclusivity score of the minister in charge of ministry m at time t (on a scale from 0 to 1). The explanatory variable *Gender Inclusivity* × *Female Employee* is the interaction between the variable *Gender Inclusivity* and an indicator equal to one if individual i is female. Column (1) reports estimates from a regression on these two explanatory variables, and individual and time fixed effects. Column (2) reports estimates from a regression which additionally controls for the gender of the minister in charge of ministry m at time t. Column (3) additionally controls for the party of the minister in charge of ministry m at time t. Column (4) includes ministry×time fixed effects. Standard errors are clustered at the ministry level and calculated using the cluster-bootstrap. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

One might be concerned that the gender inclusivity score of the minister is correlated with other characteristics of the minister. Thus, I control for the minister's gender (column (2)) and additionally for the minister's party (column (3)).[12] The estimated coefficients remain stable and statistically significant at the 5% or 10%-level. In column (4), I control for ministry-year fixed effects. These hold fixed all aspects within a ministry at a specific point in time, e.g., characteristics of a minister. Thus, I can no longer estimate the baseline effect of the gender inclusivity score. Yet, I can still estimate the coefficient of interest: the differential effect of ministers with higher gender attitudes on women. The estimated coefficient is 0.28 and significant at the 5%-level.

Across specifications, the effect of ministers with one standard deviation higher gender attitudes on women's promotion probability is around two percentage points. This is a meaningful and sizeable effect, considering that the overall promotion probability from low level to middle level leadership is around 6.5%.

Identification of the impact on women's careers of ministers who differ in their gender attitudes relies on the assumption that female-friendly ministers do not get appointed to ministries in which women were already more likely to be promoted. Of course, ministers are not randomly appointed to lead specific ministries. Yet, ministers are generally appointed as a result of political considerations. To address potential government-level effects (e.g., a shift of the government along the left-right spectrum or a potential shift to an explicitly female-friendly government), I control for time fixed effects. Moreover, ministry fixed effects would control for selection of ministers with higher gender attitudes into specific ministries. Since I observe individual civil servants only within ministries, individual fixed effects absorb ministry fixed effects. Thus, my specification controls for time-invariant ministry-level confounders.

A limitation of my analysis is that I cannot identify the precise mechanism by which superiors with higher gender attitudes affect women's career outcomes. The estimated effect could be explained by both demand and supply forces. For example, a sexist minister might be less likely to choose a woman from the pool of candidates. But, at the same time, female employees might also be less likely to apply for a promotion under a sexist minister. Hence, my empirical strategy estimates the reduced-form effect of ministers with higher gender attitudes on women's promotions.

[12] In Appendix Table 1.C.1, I show that this result is stable to the inclusion of further control variables.

1.4.3 Heterogeneous Impact of Ministers

I now turn to an additional analysis and ask: which ministers matter? I study the heterogeneous effects (1) of ministers with particularly high or low gender attitudes scores, and (2) of male relative to female ministers.

Ministers With High and Low Gender Attitudes

First, I investigate if ministers with higher or lower gender attitudes drive the effect. I estimate a non-parametric version of my main regression:

$$\begin{aligned} \mathbb{1}[Promotion]_{imt} = {} & \beta_1 \cdot HighIncl_{mt} + \beta_2 \cdot HighIncl_{mt} \times FemaleEmployee_i \\ & + \gamma_1 \cdot LowIncl_{mt} + \gamma_2 \cdot LowIncl_{mt} \times FemaleEmployee_i \\ & + \alpha_t + \lambda_i + \epsilon_{imt} \end{aligned} \tag{1.3}$$

where $HighIncl_{mt}$ and $LowIncl_{mt}$ are indicator variables equal to one if the minister in ministry m at time t is in the highest quarter or the lowest quarter in the distribution of ministers' gender inclusivity scores. I interact these indicator variables with $FemaleEmployee_i$. The remaining variable definitions are identical to Equation (1.2).

I report estimates of this specification in Table 1.4. In column (1), I report estimates from a regression where I only estimate the effects of ministers who are in the top quartile of the gender inclusivity score distribution, relative to ministers in the lowest 75 percent of the distribution. I again find that the baseline effect of a minister with high gender attitudes on men is insignificantly different from zero. However, the differential effect on women, i.e., the coefficient on the interaction $HighIncl_{mt} \times FemaleEmployee_i$, is positive and statistically significant at the 10%-level. The differential effect of a minister with high gender attitudes on the probability that a woman gets promoted is around four percentage points. In column (2), I report estimates from an analogous regression in which I estimate the effect of ministers with low gender attitudes. The reported estimates are symmetric: the baseline effect on male employees is near zero, but the differential effect on female employees is negative. However, this effect is not statistically significant.

In column (3), I report estimates from a regression with indicator variables on both ministers with high gender attitudes and ministers with low gender attitudes, i.e., of Equation (1.2). The estimated effects of these variables are relative to ministers between the 26$^{\text{th}}$ and 75$^{\text{th}}$ percentiles. For both sets of coefficients, I find similar results to the ones in columns (1) and (2). I find null effects on the baseline promotion

Table 1.4: Non-Linear Effect of Gender Attitudes

	Dependent Variable: Promotion			
	(1)	(2)	(3)	(4)
Top Quarter Inclusivity	0.007		0.007)	
	(0.009)		(0.009)	
Top Quarter Inclusivity × Female Employee	0.041*		0.039*	0.045*
	(0.022)		(0.022)	(0.024)
Bottom Quarter Inclusivity		0.005	0.005	
		(0.031)	(0.031)	
Bottom Quarter Inclusivity × Female Employee		-0.060	-0.045	-0.047
		(0.048)	(0.049)	(0.045)
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	
Ministry × Time Fixed Effects				Yes
Observations	7,751	7,751	7,751	7,751
R^2	0.706	0.705	0.706	0.715
Dependent Variable Mean	6.54	6.54	6.54	6.54

Notes: This table reports estimates of Equation (1.3). The dependent variable is an indicator equal to one if individual i working in ministry m was promoted from a low level leadership position to middle level leadership at time t. The explanatory variable *Top Quarter Inclusivity* is an indicator equal to one if the minister in charge of ministry m at time t is in the top quartile in the distribution of ministers' gender inclusivity scores. The explanatory variable *Bottom Quarter Inclusivity* is an indicator equal to one if the minister in charge of ministry m at time t is in the bottom quartile in the distribution of ministers' gender inclusivity scores. *Top Quarter Inclusivity × Female Employee* and *Bottom Quarter Inclusivity × Female Employee* are the interactions between these indicator variables and the indicator $FemaleEmployee$, which is equal to one if individual i is female. Columns (1)-(3) include individual and time fixed effects. Column (1) reports estimates from a regression on the variable *Bottom Quarter Inclusivity* and its interaction with *Female Employee.* Column (2) reports estimates from a regression on the variable *Top Quarter Inclusivity* and its interaction with *Female Employee.* Column (3) reports estimates from a regression with both explanatory variables, *Bottom Quarter Inclusivity* and *Top Quarter Inclusivity*, and their interactions with the indicator *Female Employee.* Column (4) reports estimates from a regression with the same explanatory variables as in column (3), except that it includes individual and ministry×time fixed effects. Standard errors are clustered at the ministry level and calculated using the cluster-bootstrap. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

probability, indicating that under ministers with higher gender attitudes promotions are not more likely for men. They are, however, more likely for women: I find that the differential effect of a minister with high gender attitudes on women's promotion probability is around 3.9 percentage points, which is statistically significant at the 10-% level. The differential effect of ministers with low gender attitudes is -4.5 percentage points. This would indicate that the effect is symmetric, i.e., driven by both ministers with high and low gender attitudes. However, this effect is not statistically significant.

I replicate these results using ministry-time fixed effects, which control for party and gender of the minister as well as potential government effects over time. In this regression, I can no longer estimate the baseline coefficients β_1 and γ_1 because they vary at the ministry-time level. Estimated coefficients remain broadly unchanged.

Male and Female Ministers

Second, I analyze if the effect of ministers with high gender attitudes is driven by male or female ministers. In this specification, I estimate the effect of gender attitudes on female employees separately for male and female ministers:

$$\begin{aligned}\mathbb{1}[Promotion]_{imt} = \beta_1 &\cdot Inclusivity_{mt} \\ &+ \beta_2 \cdot Inclusivity_{mt} \times FemEmployee_i \\ &+ \beta_3 \cdot Inclusivity_{mt} \times FemMinister_{mt} \\ &+ \beta_4 \cdot Inclusivity_{mt} \times FemMinister_{mt} \times FemEmployee_i \\ &+ \beta_5 \cdot FemMinister_{mt} \times FemEmployee_i \\ &+ \beta_6 \cdot FemMinister_{mt} + \alpha_t + \lambda_i + \epsilon_{imt}\end{aligned} \tag{1.4}$$

where $FemMinister_{mt}$ is an indicator equal to one if the minister in ministry m at time t is a woman. As in the main analysis, I include the variables $Inclusivity_{mt}$ and $Inclusivity_{mt} \times FemEmployee_i$. Additionally, I estimate the differential effect of female ministers with high gender attitudes on female employees, i.e., I include $Inclusivity_{mt} \times FemMinister_{mt} \times FemEmployee_i$. I control for ministry and time fixed effects, and cluster standard errors at the ministry level.

I report the results of this regression in Table 1.5. Column (1) reports estimates from the main regression, i.e., without the interactions for female ministers, and is included for reference. In column (2), I report estimates of Equation (1.4). The differential effect of ministers with high gender attitudes on the baseline probability of promotion is near zero (0.04), as is the differential effect of female ministers (-0.02). This again shows that gender attitudes are not driving men's promotions in a ministry, under both male and female ministers. Also, the differential effect of ministers with high gender attitudes on female employees is positive (0.37) and significant at the 10%-level.

If female ministers with high gender attitudes promote more women than male ministers with high gender attitudes, I expect the differential effect of female ministers with high attitudes to be positive. I find that this is not the case. The point estimate is -0.20, which is large relative to the estimated coefficient on $Inclusivity_{mt} \times FemEmployee_i$ (0.37). However, this effect is not statistically significant. I replicate the findings in column (3), where I include ministry-year fixed effects. I conclude that the effect of ministers with high gender attitudes is not driven by female ministers but by ministers of both genders. If anything, the point estimate (-0.20) indicates that male ministers

Table 1.5: Female and Male Ministers

	Dep. Var.: Promotion		
	(1)	(2)	(3)
Gender Inclusivity	0.03	0.04	
	(0.06)	(0.08)	
Gender Inclusivity × Female Employee	0.25**	0.37*	0.38*
	(0.13)	(0.22)	(0.22)
Gender Inclusivity × Female Minister		-0.02	
		(0.17)	
Gender Inclusivity × Female Minister × Female Employee		-0.20	-0.23
		(0.25)	(0.26)
Individual Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	
Party Minister	Yes	Yes	
Female Minister	Yes	Yes	
Female Minister × Female Employee Control		Yes	Yes
Ministry × Time Fixed Effects			Yes
Observations	7,751	7,751	7,751
R^2	0.706	0.706	0.715
Dependent Variable Mean	6.541	6.541	6.541

Notes: This table reports estimates of Equation (1.4). The dependent variable is an indicator equal to one if individual i working in ministry m was promoted from a low level leadership position to middle level leadership at time t. The explanatory variable *Gender Inclusivity* is the gender inclusivity score of the minister in charge of ministry m at time t (on a scale from 0 to 1). The explanatory variable *Gender Inclusivity × Female Employee* is the interaction between the variable *Gender Inclusivity* and indicator equal to one if i is female. The explanatory variable *Gender Inclusivity × Female Minister* is the interaction between the variable *Gender Inclusivity* and an indicator equal to one if the minister in charge of ministry m at time t is female. The explanatory variable *Gender Inclusivity × Female Minister × Female Employee* is the interaction between the variable *Gender Inclusivity*, and an indicator equal to one if the minister in charge of ministry m at time t is female, and an indicator equal to one if individual i is female. Column (1) reports estimates from a regression on the first two explanatory variables, individual and time fixed effects, and indicator variables for the party and gender of the minister in charge of ministry m at time t. It is equivalent to column (3) in Table 1.3 and is included for reference. Column (2) reports estimates from a regression which additionally includes *Gender Inclusivity × Female Minister*, *Gender Inclusivity × Female Minister × Female Employee*, and *Female Minister × Female Employee*. Column (4) additionally includes ministry×time fixed effects and therefore omits time fixed effects and minister controls. Standard errors are clustered at the ministry level and calculated using the cluster-bootstrap. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

with high gender attitudes are more likely to promote women than female ministers with high gender attitudes.

1.5 Conclusion

In this paper, I introduced a new method to measure gender attitudes: the extent to which a person speaks gender-inclusively. I then presented a series of facts about German politicians' use of gender-inclusive language and argued that my score is a meaningful measure of gender attitudes. I used this measure to estimate the effect of ministers who differ in their gender attitudes on female employment outcomes. I find that ministers with one standard deviation higher gender attitudes increase the promotion probability of women by about two percentage points. This effect holds both for male and female ministers.

This finding is important for policy: while many countries have introduced quotas for women in leadership (e.g., boards of advisors or public bodies), research has shown that the representation of women at higher levels often does not trickle down to lower levels (e.g., Bertrand et al., 2019; Maida and Weber, 2022). My findings indicate that female-friendly superiors—as opposed to female superiors—might be more beneficial for women's careers.

This paper leaves two aspects of the role of superiors' gender attitudes unanswered. First, I estimate a reduced-form effect, which could be explained by both demand and supply. Second, my findings do not show that an intervention that makes superiors speak more gender-inclusively or have more female-friendly gender attitudes (e.g., diversity training) would have a causal effect on their likelihood of promoting women. Yet, my findings do suggest that in the absence of female-friendly superiors women's underrepresentation in leadership may persist.

Appendix to Chapter 1

This appendix presents details on data collection and additional results:

- Section 1.A provides details on the construction of the gender inclusivity score.
- Section 1.B provides additional figures.
- Section 1.C reports additional findings.

1.A Further Details on the Gender Inclusivity Score

1.A.1 Example

This example illustrates the construction of the gender inclusivity score using a speech by Angela Merkel in the German parliament on 29 October 2020 (Bundesregierung, 2020). Below is a part of the speech in its German original. Female-specific words are colored in orange and male-specific words in light blue. In the English translation of the text, I indicated the use of gender-specific words by adding these terms in the respective colors.

> "Liebe **Kolleginnen** und **Kollegen**! ... Am 18.10. waren es 769 betreute **Patienten**, am 28.10., zehn Tage später, 1 569. ... Deshalb haben sich die **Regierungschefinnen** und **Regierungschefs** des Bundes und der Länder gestern zu einer weiteren Konferenz getroffen und weitere Vereinbarungen beschlossen. ... Wir haben also gemeinsam mit den **Ministerpräsidentinnen** und **Ministerpräsidenten** Folgendes beschlossen ..."

> "Dear **colleagues** and **colleagues**! ... On the 10th of October, 769 **patients** were in intensive care; on the 29^{th} of October, 10 days later, this number has reached 1,569. ... Hence, the federal and state **heads of government** and **heads of government** have met yesterday and made further agreements. ... Together with the **minister-presidents** and **minister-presidents** we have come to the following conclusions ..."

In this example, Angela Merkel uses four different person-specific words: colleagues, patients, heads of government, and minister-presidents. For three of these words she uses the gender-inclusive form, i.e., she uses a male-specifc and a female-specific word to describe these people. For one of these words, patients, she only uses the male word. Hence, she has used 7 person-specific words, of which 3 were female-specific. Her gender inclusivity score in this example is thus 3/7.

1.A.2 Details on Constructing the Gender Inclusivity Score

I calculate the gender inclusivity score for all German politicians in my dataset by counting the number of female-specific and male-specific words they use in their speeches. Hence, I need a list of person-specific words (e.g., baker, terrorist, expert) with their female and male forms. Instead of relying on an arbitrary selection of person-specific words, I draw these person-specific words from the data itself. I construct the gender inclusivity score in three steps:

(1) I compile a list of person-specific words with gender-specific forms. I parse through all speeches and extract the word after the substring “innen und.” For example, the word for pilot in German is “Pilot” for men and “Pilotin” for women. The gender-inclusive plural is “Pilotinnen und Piloten.” The term after “innen und” is “Piloten,” the plural for the male-specific word for pilot. This process gives me a list of 6,000 person-specific words that were used at least once in their gender-inclusive form. (This list contains some mistakes which I manually clean. For example, in the case of the words “gewinnen und verlieren” (in English: “win and lose”), I would extract the string “verlieren,” which of course is not a noun.)

(2) For each of these terms, I count how often they appear in all speeches. I rank terms by their frequency and extract a list of the 100 most used person-specific words. Out of all instances a person-specific word was used in a speech (based on my list compiled in step 1), in 95% of these instances the word was among the top 100 words. (I report the ten most used person-specific nouns in speeches in the German parliament in Table 1.A.1.) I compile a list of the respective female and male forms of these 100 words.

(3) For all speeched, I count the number of times each of these 100 words was used in their male form and in their female form.

(4) I then calculate, within a politician, the sum of all female-specific words and the sum of all male-specific words they used in all their speeches. Then I construct the ratio, as defined in Equation (1.1).

When I construct scores for genders or parties, I do not perform step 4 at the level of the politician, but rather I count the sum of all male-specific and all female-specific words within all speeches of politicians of that specific gender or party. Likewise, when I construct time-varying gender inclusivity scores I count the sum of these words at the gender-year or the party-year level.

Table 1.A.1: Ten Most Used Person-Specific Words

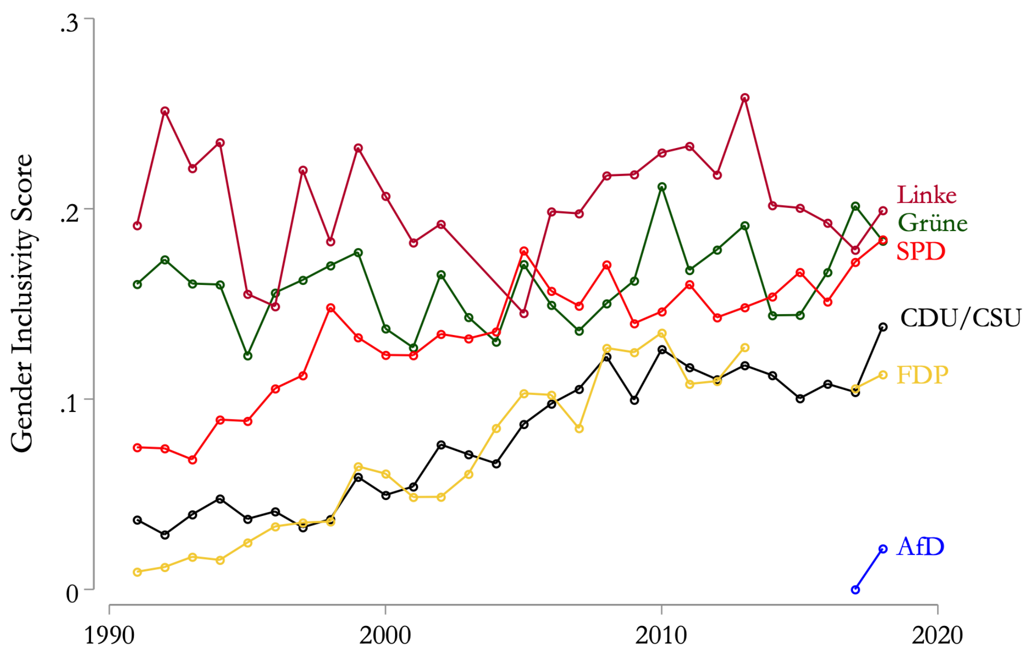
Rank	Male-specific word	Female-specific word	English translation
1	Kollege	Kollegin	Colleague
2	Bürger	Bürgerin	Citizen
3	Soldat	Soldatin	Soldier
4	Arbeitnehmer	Arbeitnehmerin	Employee
5	Mitarbeiter	Mitarbeiterin	Co-worker
6	Verbraucher	Verbraucherin	Consumer
7	Schriftführer	Schriftführerin	Clerk/minute taker
8	Patient	Patientin	Patient
9	Rentner	Rentnerin	Pensioner
10	Wähler	Wählerin	Voter

Notes: This table reports the ten most used person-specific words in speeches in the German parliament.

1.B Additional Figures

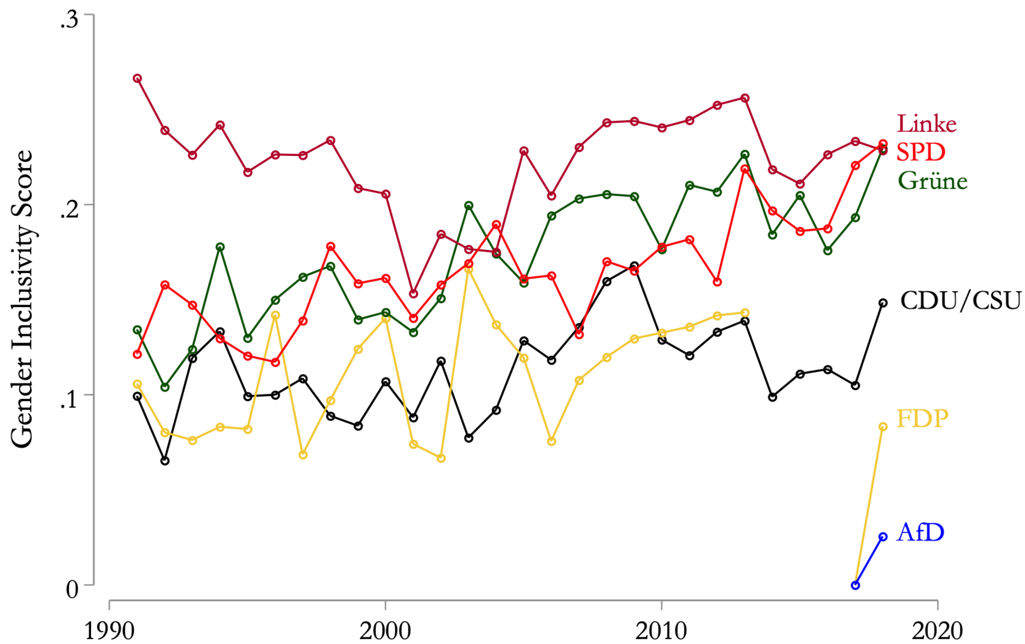
1.B.1 Gender Inclusivity Among German Politicians

Figure 1.B.1: Gender Inclusivity of Male MPs Over Time, by Party



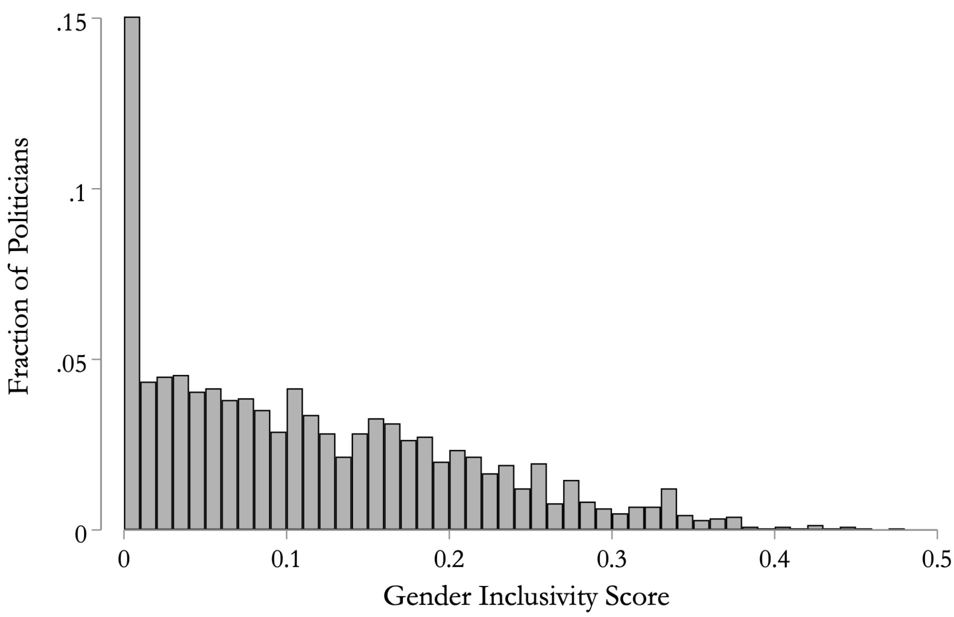
Notes: This figure plots the yearly average of the gender inclusivity score for male MPs in each party: the Left (Linke, dark red), the Greens (Grüne, green), the Social Democrats (SPD, red), the Liberals (FDP, yellow), the Conservatives (CDU/CSU, black), and the Alternative for Germany (AfD, blue).

Figure 1.B.2: Gender Inclusivity of Female MPs Over Time, by Party



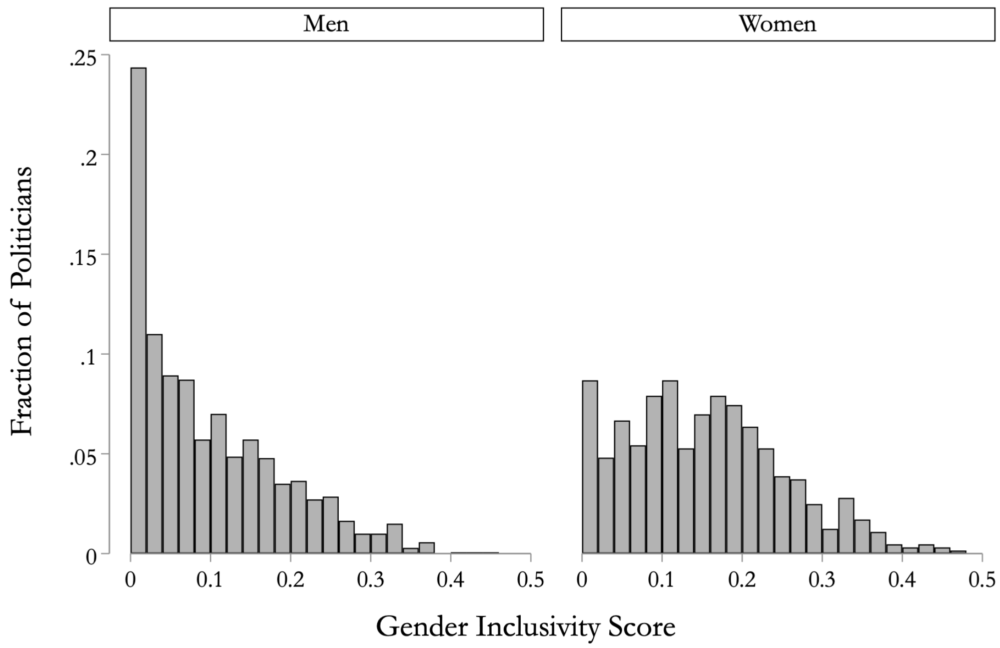
Notes: This figure plots the yearly average of the gender inclusivity score for female MPs in each party: the Left (Linke, dark red), the Greens (Grüne, green), the Social Democrats (SPD, red), the Liberals (FDP, yellow), the Conservatives (CDU/CSU, black), and the Alternative for Germany (AfD, blue).

Figure 1.B.3: Distribution of Gender Inclusivity Scores



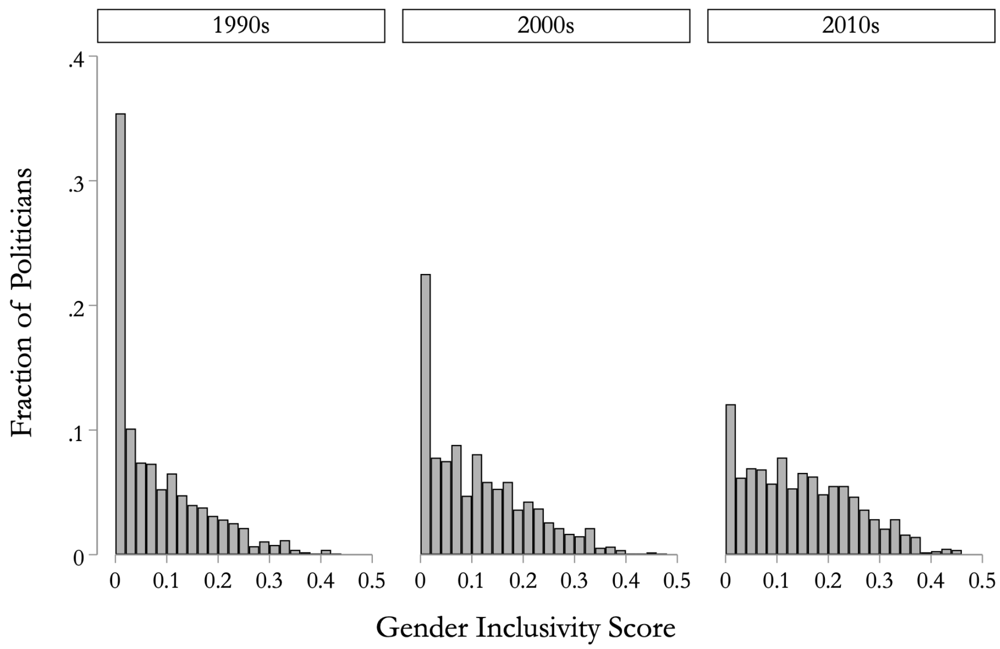
Notes: This figure plots the distribution of individual politicians' gender inclusivity scores.

Figure 1.B.4: Distribution of Gender Inclusivity Scores, by Gender



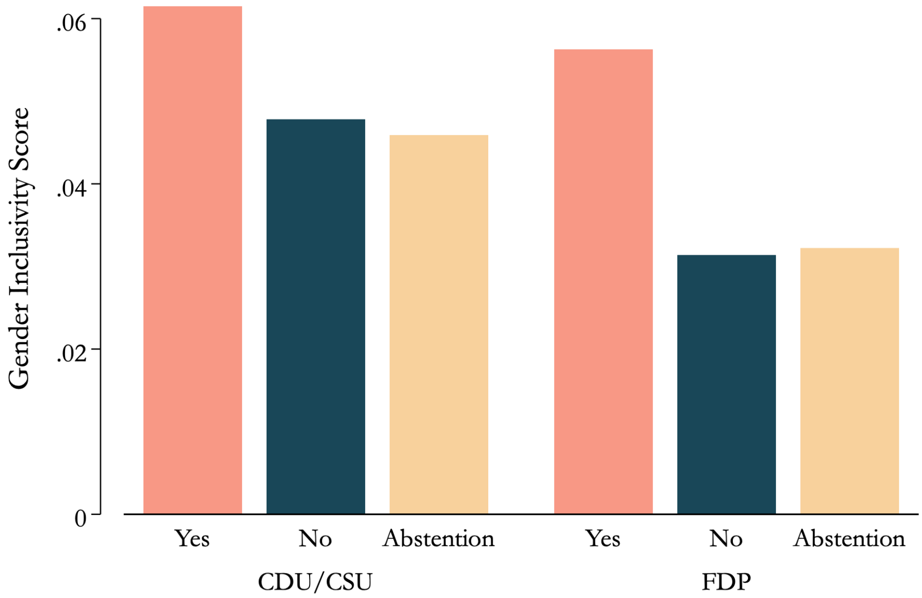
Notes: This figure plots the distribution of male and female politicians' gender inclusivity scores.

Figure 1.B.5: Distribution of Gender Inclusivity Scores, By Decade



Notes: This figure plots the distribution of individual politicians' gender inclusivity scores by decade (1990s, 2000s, 2010s).

Figure 1.B.6: Voting on the Criminalization of Marital Rape, by Party



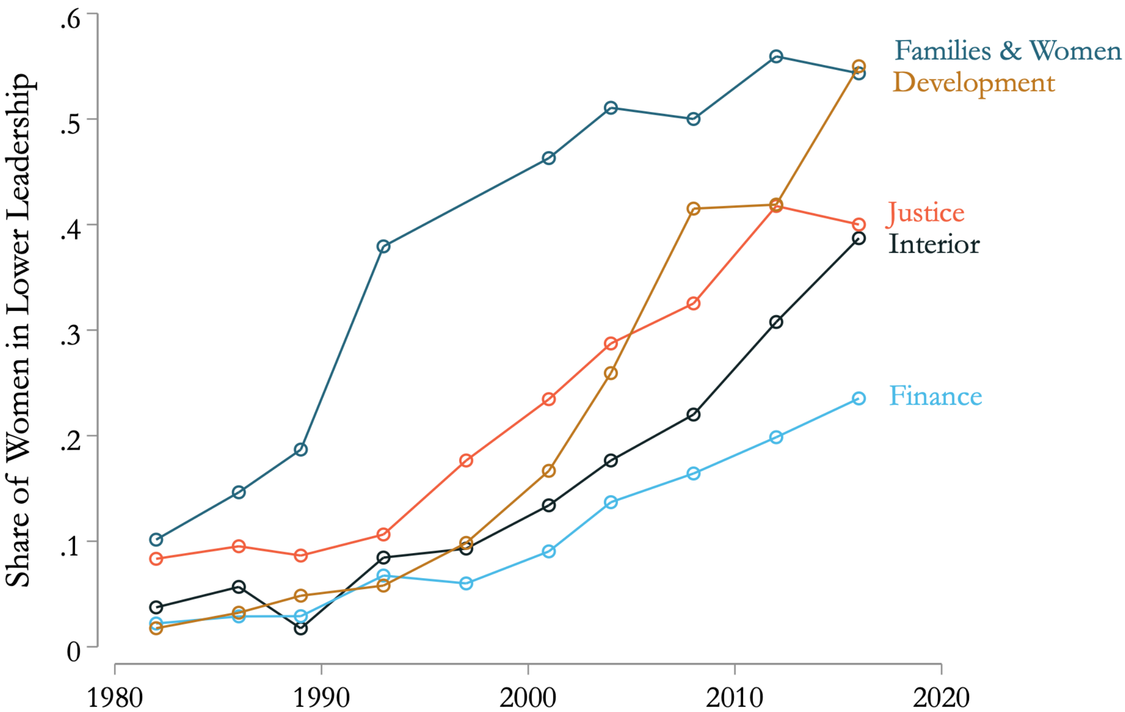
Notes: This table shows, separately for the Conservatives (CDU/CSU) and the Liberals (FDP), the average gender inclusivity score for politicians who voted in favor of a 1997 law criminalizing marital rape, for those who voted against the law, and for those who abstained.

Table 1.B.1: Gender Inclusivity Score Among Ministers in 2020

Minister	Ministry	Gender	Party	Inclusivity
Svenja Schulze	Environment	F	SPD	0.35
Franziska Giffey	Family & Women	F	SPD	0.32
Andreas Scheuer	Transport	M	CSU	0.27
Olaf Scholz	Finance	M	SPD	0.26
Hubertus Heil	Labour	M	SPD	0.23
Julia Klöckner	Agriculture	F	CDU	0.19
Angela Merkel	Chancellor	F	CDU	0.19
Peter Altmaier	Economy	M	CDU	0.19
Heiko Maas	Foreign Affairs	M	SPD	0.18
Gerd Müller	Development	M	CSU	0.14
Christine Lambrecht	Justice	F	SPD	0.13
Helge Braun	Chancellery	M	CDU	0.10
Anja Karliczek	Education	F	CDU	0.07
Jens Spahn	Health	M	CDU	0.06
Horst Seehofer	Interior	M	CSU	0.04
Average				0.18

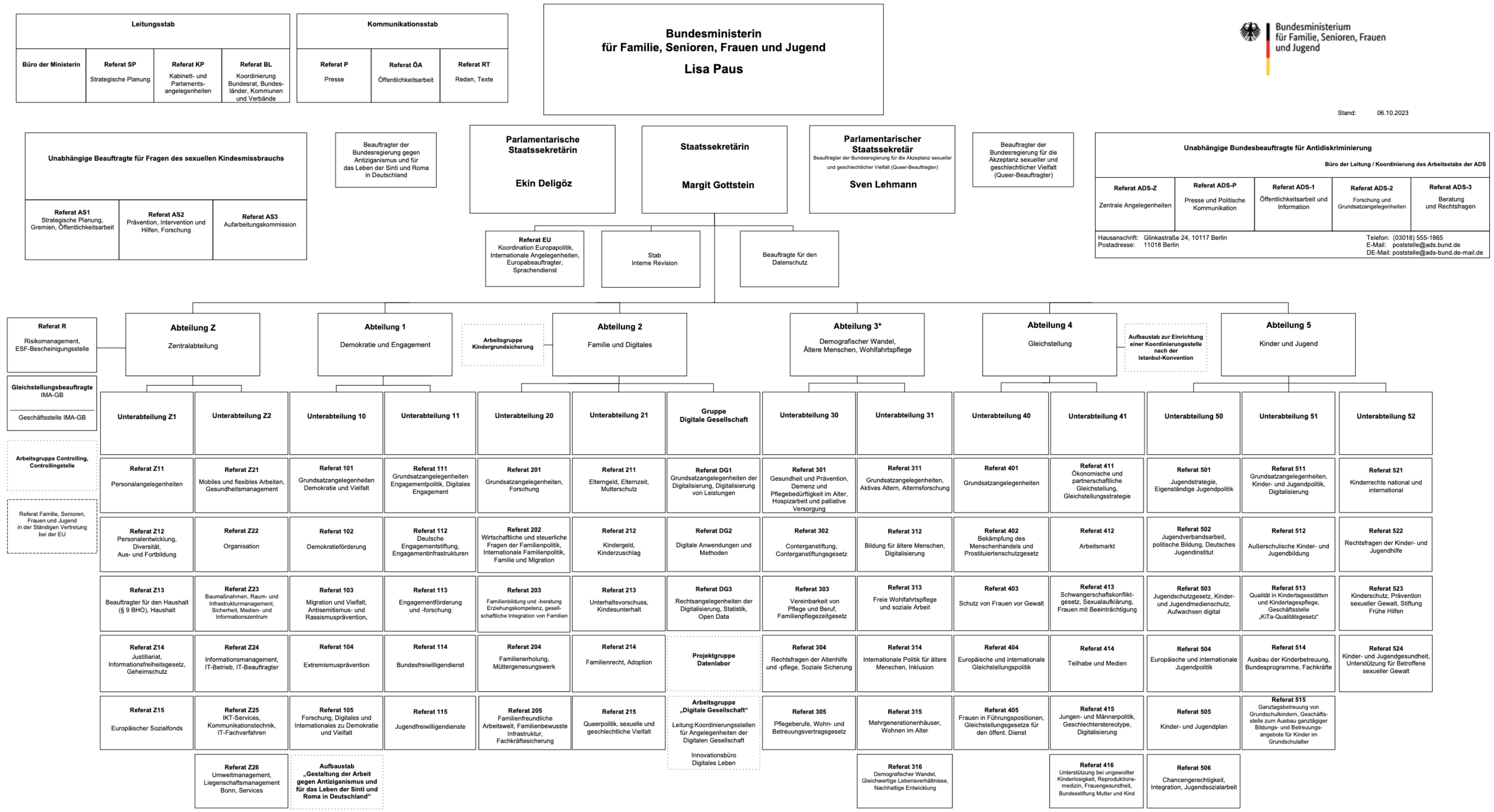
Notes: This table lists the individual-level gender inclusivity score for each minister in 2020, with information on their ministry, their gender, and party. The Minister of Defence in 2020, Annegret Kramp-Karrenbauer, is omitted from this table due to missing speech data.

Figure 1.B.7: Share of Women in Low Level Leadership



Notes: This figure plots the share of women in low leadership ranks between 1982 and 2016 in five ministries: the German Federal Ministry of Family Affairs, Senior Citizens, Women, and Youth; the Ministry of Finance; the Ministry for International Cooperation and Development; the Ministry of the Interior; and the Ministry of Justice.

Figure 1.B.8: Example Organization Chart



Notes: This figure shows the organization chart of the German Federal Ministry of Family Affairs, Senior Citizens, Women, and Youth in 2023. For data protection, I show a chart without names (unlike the ones I have digitized). Source: BMFSFJ (2024).

1.C Additional Findings

Table 1.C.1 reports estimates from regressions using additional ministry-level and individual-level controls. The results in the main analysis (here columns (1) and (4), included for reference) are robust to controlling for the share of lower-level leaders who are female at $t-1$ (column (2)). The results are also robust to controlling for indicator variables that capture individual i's years of experience, i.e., how many times I observe them in the data (columns (3) and (5)).

Table 1.C.1: Robustness: Adding Additional Controls

	Dependent Variable: Promotion				
	(1)	(2)	(3)	(4)	(5)
Gender Inclusivity	0.03	0.03	0.00		
	(0.06)	(0.07)	(0.07)		
Gender Inclusivity × Female Employee	0.25**	0.25*	0.30*	0.28**	0.33**
	(0.13)	(0.14)	(0.16)	(0.13)	(0.15)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes		
Minister Female Control	Yes	Yes	Yes		
Minister Party Controls	Yes	Yes	Yes		
Share of Lower-Level Leaders Control		Yes	Yes		
Years of Experience Controls			Yes		Yes
Ministry × Time Fixed Effects				Yes	Yes
Observations	7,751	7,707	6,042	7,751	6,086
R^2	0.706	0.707	0.720	0.715	0.728
Dependent Variable Mean	6.54	6.55	6.17	6.54	6.16

Notes: This table reports estimates of Equation (1.2). The dependent variable is an indicator equal to one if individual i working in ministry m was promoted from a low level leadership position to a middle level leadership at time t. The explanatory variable *Gender Inclusivity* is the gender inclusivity score of the minister in charge of ministry m at time t (on a scale from 0 to 1). The explanatory variable *Gender Inclusivity × Female Employee* is the interaction between the variable *Gender Inclusivity* and whether individual i is female, i.e., it is equal to zero for men and equal to *Gender Inclusivity* for women. Column (1) reports estimates from a regression on these two explanatory variables, individual and time fixed effects, and controls for the gender and party of the minister in charge of ministry m at time t. This column is equivalent to column (3) of Table 1.3 and is included for reference. Column (2) additionally controls for the share of women among low level leaders in ministry m at time $t-1$. Column (3) additionally controls for a set of indicator variables for the years of experience of individual i at time t. Column (4) is equivalent to column (1) except that it includes ministry×time fixed effects. This column is equivalent to column (4) of Table 1.3 and is included for reference. Column (5) additionally controls for a set of indicator variables for the years of experience of individual i at time t. Standard errors are clustered at the ministry level and calculated using the cluster-bootstrap. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

Chapter 2

Measuring Science: Performance Metrics and the Allocation of Talent

This chapter is based on co-authored work with Carlo Schwarz and Fabian Waldinger (see Hager et al., 2023).

2.1 Introduction

The allocation of talent to productive positions in society is of utmost importance for the creation of new ideas, technological progress, and economic growth (e.g., Romer, 1986, 1990; Murphy et al., 1991; Jones, 1995; Weitzman, 1998; Hsieh et al., 2019). As talent is scarce, private sector firms and universities increasingly rely on performance metrics to identify talented individuals (e.g., Bersin, 2013; Hoffman et al., 2018). In academia, performance metrics based on citations and publications affect hiring, promotions, wages, research funding, and the prestige of academics (e.g., Hamermesh and Schmidt, 2003; Ellison, 2013). Due to their increasing use, concerns have been raised about a potential overreliance on performance metrics in science (CoARA, 2024; DORA, 2024). Despite the importance of such metrics, as well as the recent discussions, there is virtually no evidence that quantifies how performance metrics affect the organization of science.

In this article, we provide the first systematic evidence of the impact of performance metrics on the allocation of talent and on scientific careers. Specifically, we study how citation metrics affect the assortative matching between scientists and universities, which groups benefit most from citation metrics, and how citation metrics affect career outcomes, such as promotions and research funding.

Our empirical strategy exploits the introduction of the *Science Citation Index* (SCI), which led to quasi-random variation in the visibility of individual scientists' citation counts. While researchers always had a rough sense of the influence of scientific work, it was impossible to systematically measure citations until the 1960s. This changed fundamentally in 1963 when Eugene Garfield published the first *Science Citation Index* (SCI). For the first time, it became possible to identify the highest-cited papers and researchers. The Nobel laureate and molecular biologist Joshua Lederberg lauded the invention of the SCI with the words: "I think you're making history, Gene!" (Wouters, 2017). Scientists, funding bodies, and university administrators immediately started to use citation counts in hiring, promotion, and funding decisions. The sociologist Harriet Zuckerman remarked in the *New York Times* that there are "cases of people who have been asked to go count their own citations, and also of deans and administrations who have asked for citation counts" (Charlton, 1981).

In the first part of the article, we investigate how the availability of citation metrics affects the assortative matching between scientists and departments. We document that the correlation between scientists' citation counts and the rank of their department increased by 61%. At the same time, scientists' publication counts became 46%

less predictive of their department rank. These over-time changes suggest that hiring committees started to attach more weight to citation counts and less weight to other observable characteristics such as publications when evaluating candidates. The increased correlation between scientists' citations and the ranking of their departments may be spurious for various reasons. For example, the increasing importance of expensive research labs and of federal research funding (e.g., Kantor and Whalley, 2023) could disproportionately favor leading departments and allow them to attract star scientists, who turn out to be highly cited. Similarly, increases in team production (e.g., Wuchty et al., 2007; Jones, 2009) may have spurred collaborations within departments and, hence, made department quality more critical for citations of individual scientists.

We estimate the causal effect of citation metrics by exploiting that, for technical reasons, the SCI only covered citations in a subset of years and journals. Only these citations became *visible* to the scientific community. In contrast, other citations remained *invisible* to contemporaries, yet are observable in modern citation data. The variation in the visibility of citations stems from two sources: variation in the coverage of citations (1) over time and (2) across journals. First, citations appearing in cit*ing* articles until 1960 were invisible. With the first edition of the SCI, citations from citing articles in 1961 became visible. Due to technological constraints, the coverage of the SCI was interrupted for two years. Hence, citations appearing in citing articles in 1962 and 1963 remained invisible at the time. After 1964, the SCI was published yearly, and thus citations appearing in citing articles after 1964 became visible. Second, due to a lack of computing power, the SCI only covered citations in certain journals. As a result, some citations appearing in covered years (1961 and from 1964 onwards) remained invisible if they came from citing articles published in journals not indexed by the SCI. Crucially, in the early years, the selection of citing journals was somewhat arbitrary because the lack of citation data meant that journal rankings did not exist.[1]

Importantly, our empirical strategy exploits when and where a scientist's papers were *cited*, not when and where they were published. The cit*ed* papers could be published in any journal and in any earlier year. The following example of two hypothetical scientists illustrates our identification strategy: suppose that both scientists published a paper in 1957 (in any journal). One of the papers was cited in *Nature* in 1961, while the other one was cited in *Nature* in 1962. As the SCI covered citations in 1961 but not in 1962, the first citation became visible to contemporaries, while the second remained invisible. Using modern citation data, we can, however, observe both visible and invisible citations.

[1] In fact, the impact factor, which nowadays is used to rank academic journals, was invented by the creators of the SCI (Garfield, 1979, p. 150).

For our analysis, we combine new data on historical faculty rosters of U.S. universities from the *World of Academia Database* (Iaria et al., 2022) with extensive publication and citation data from *Clarivate Web of Science.* These data enable us to construct the most comprehensive individual and department-level rankings for the 1960s. In addition, we digitize lists from historical volumes of the SCI, which specify the exact citing journals that were indexed in each volume of the SCI. This allows us to measure which citations were visible and, thus, to reconstruct the information set available to scientists in the 1960s.

We estimate the effect of citation metrics on the match between scientists and departments by comparing the relative importance of visible to invisible citations. We find that visible citations are four times as predictive of scientists' department rank than invisible citations. Specifically, scientists with a 10 percentile higher visible citation count were, on average, placed at a 2.5 percentiles higher ranked department in 1969. For instance, a mathematician would be placed at Princeton or Chicago as opposed to Columbia or Brandeis. In contrast, scientists with a 10 percentile higher invisible citation count were on average only placed at a 0.6 percentiles higher ranked department. This pattern holds even if we control for detailed publication records, i.e., for the number of publications in each journal (e.g., two *Nature*, one *Science*, and one *PNAS* publication) and in each year (e.g., one publication in 1956, two in 1960, and one in 1964). Note that it is not surprising that even invisible citations affect the matching between scientists and departments since the academic community always had some knowledge of the quality of scientists' research, even if precise citation counts were not available.

Despite the somewhat arbitrary nature of the SCI coverage, two main concerns could potentially invalidate this identification strategy. First, visible citations may come from articles in higher-quality journals. Second, as the SCI was introduced in 1961, visible citations occur in later years, on average, and may have a larger impact on career outcomes in 1969. As a consequence, the impact of visible citations on scientists' careers would be overestimated.

To address the quality concern, we compute measures of the quality of citing journals. We find that visible and invisible citations come from journals of similar quality. We also provide further evidence that differences in the quality of citing journals do not bias our results. For this test, we estimate regressions that only consider citations from the set of citing journals that were indexed in the first edition of the SCI. This analysis compares scientists whose paper was cited, for example, in *Science* in 1961, and was therefore visible, to scientists whose paper was cited in *Science* in 1963, and was therefore invisible.

To address the timing concern, we confirm that the results hold in specifications that exclusively rely on across-journal variation in the visibility of citations. This analysis compares scientists whose paper was cited in the same year (e.g., 1961), but one citation occurred in the *Journal of the American Chemical Society*, and was thus visible in the SCI, while the other citation occurred in *Chemical Reviews*, and was thus invisible.

The quality of citing journals and the timing of citations could interact to make visible citations more predictive for assortative matching. To address this concern, we introduce an additional specification. For this test, we partition the citation space into four mutually exclusive sets depending on where and when a scientist was cited: (1) *visible citations*: citations from journals that were indexed in the SCI in years when the SCI was published; (2) *pseudo-visible citations*: citations from journals that were indexed in the SCI in 1961 but from years when the SCI was not published; (3) *invisible citations (SCI years)*: citations from journals that were not indexed in the SCI in years when the SCI was published; and (4) *invisible citations (non-SCI years)*: citations from journals that were not indexed in the SCI in 1961 and from years when the SCI was not published.

We find that the coefficient on visible citations is almost identical to the baseline specification. Moreover, the coefficient on pseudo-visible citations is considerably smaller and very similar to the two coefficients on invisible citations in SCI years and in non-SCI years. This indicates that citations in journals that were indexed by the SCI only had a differential impact in years in which the SCI was actually available. These results support the validity of our identification strategy.

Next, we shed light on two potential mechanisms that could underlie the increase in assortative matching based on citation metrics. First, scientists with few citations may have disproportionately left academia. We find that scientists with a 10 percentile higher visible citation count were 3.0 percentage points (or 4.3 percent) less likely to leave academia between 1956 and 1969. In contrast, invisible citations did not affect the probability of leaving academia. Second, highly cited scientists may have moved to higher-ranked departments. We show that scientists with a 10 percentile higher visible citation count were 0.8 percentage points (or 17.6 percent) more likely to move to a higher-ranked department between 1956 and 1969. Invisible citations had no effect on moving to a higher-ranked department. Overall, these results indicate that both mechanisms increased assortative matching.

Citation metrics may matter more in situations where peers did not have good information on the quality of a potential hire. We, therefore, explore whether citation metrics reduced information frictions across geographic and intellectual distance. We find that citation metrics only impacted moves to higher-ranked departments that were geographically far but not to departments that were geographically close. Similarly, we find that citation metrics only impacted moves to higher-ranked departments where the moving scientist had not been cited before the move. These results suggest that citation metrics helped overcome information frictions. Reducing these frictions may have enabled departments to discover scientists in lower-ranked departments, even if they had not interacted before.

In the second part of the article, we investigate the heterogeneous effects of citation metrics. First, we show that scientists in higher percentiles of the individual-level citation distribution, and especially those above the 90th percentile, benefited disproportionately from the availability of citation metrics. Second, we find that the availability of citation metrics particularly benefited highly cited academics who were originally placed in lower-ranked departments. Thus, citation metrics enabled the discovery of these "hidden stars." This suggests that the introduction of the SCI helped to overcome misallocation by helping the highest-cited scientists move to higher-ranked departments. We also investigate the characteristics of these hidden stars. We provide evidence that these scientists, on average, obtained their Ph.D. from worse universities and that they were more likely to be female. Third, we investigate whether minority scientists (female, Jewish, Hispanic or Asian) differentially benefited from the introduction of the SCI. While we do not find evidence that minority scientists, on average, benefited more from citation metrics than majority scientists, we find evidence that among star scientists, minority scientists benefit slightly more. Overall, these results suggest that the availability of more "objective" performance metrics helped highly cited scientists in lower-ranked departments and highly cited scientists from minority groups.

In the last part of the article, we study the impact of citation metrics on other career outcomes: promotions and receiving research grants. In particular, we analyze whether scientists who were assistant or associate professors in 1956 were promoted to full professors by 1969. The probability of promotion increased by 4.2 percentage points (or 6.0 percent) for scientists with a 10 percentile higher visible citation rank. In contrast, invisible citations did not affect promotions. Similarly, we find that scientists with a 10 percentile higher visible citation rank were 18.2 percent more likely to receive an NSF grant. These results indicate that citation metrics not only affected assortative matching but also had direct impacts on the careers of scientists and changed the allocation of resources. Scientists with many visible citations accrued additional rewards and recognition, suggesting the presence of Matthew effects (Merton, 1968).

This paper contributes to three different strands of the literature. First, our paper contributes to the body of literature on the economics of science and the creation of knowledge. The existing literature has shown that scientists have to process increasing amounts of knowledge to advance the scientific frontier (Jones, 2009) and that access to the knowledge frontier is crucial for producing science (Iaria et al., 2018). Additional contributions have studied the importance of superstar scientists (Azoulay et al., 2010), peer-effects and scientific productivity (e.g., Waldinger, 2010; Borjas and Doran, 2012; Waldinger, 2012), and the role of editors (e.g., Card and DellaVigna, 2020). More recently, increased attention has been paid to inefficiencies in the scientific process such as the Matthew Effect (Azoulay et al., 2014; Jin et al., 2019), gatekeepers (Azoulay et al., 2019), or discrimination (e.g., Card et al., 2020; Koffi, 2021; Card et al., 2022; Hengel, 2022; Iaria et al., 2022).

Despite all these papers making use of publication and citation data, and a long-standing sociological debate on this fundamental aspect of modern science (e.g., Lotka, 1926; Merton, 1968; Zuckerman and Merton, 1971; Wouters, 1999a, 2014; Muller and Peres, 2019; Biagioli and Lippman, 2020; Pardo-Guerra, 2022), there is no causal evidence on how performance metrics affect scientific careers.[2] Our paper is the first to provide causal evidence that citation metrics fundamentally impact the organization of science.

Second, our findings contribute to the literature on performance metrics in the labor market. As highlighted by the theoretical models of Holmstrom and Milgrom (1991) and Feltham and Xie (1994), the use of performance metrics shapes incentives of agents in the labor market. The key empirical challenge to estimating the impact of performance metrics is that, in most cases, it is impossible to measure performance before the introduction of a specific performance metric. As a result, researchers often lack a valid counterfactual. This makes empirical evidence on how performance metrics affect the allocation of talent exceedingly rare. A few notable exceptions study the effect of performance metrics in the teacher labor market (Rockoff et al., 2012) and on first placements of MBA graduates (Floyd et al., 2023). The unique advantage of our setting is that we observe the information set available at the time and, importantly, what was not part of that information set.[3]

[2]Some papers document that citation metrics, such as the h-index or citation counts, are correlated with career outcomes (e.g., Jensen et al., 2009; Ellison, 2013; Hilmer et al., 2015).

[3]Since we measure the information set of contemporaries in the 1960s, our analysis allows us to identify the effects of revealing new information on labor market outcomes. In this, we add to the

Last, we contribute to research on assortative matching in labor markets (e.g., Abowd et al., 1999; Andrews et al., 2008; Card et al., 2013; Song et al., 2019). We show that performance metrics can increase assortative matching by lowering information frictions.

2.2 The Science Citation Index: Background and Data

2.2.1 The Creation of the Science Citation Index

The SCI was the first systematic international and interdisciplinary citation index. During the 1950s, Eugene Garfield and his newly founded *Institute for Scientific Information* (ISI) developed the technology to construct a citation index. By the early 1960s, this endeavor was supported by grants from the National Institutes of Health and the National Science Foundation. In November 1963, these efforts came to fruition, and the first edition of the SCI was published, covering citations in 1961 (Garfield, 1963b, see Figure 2.A.1 for a picture of the first SCI). The SCI quickly became the "most widely used and authoritative database of research publications and citations" (Birkle et al., 2020).[4]

To construct the SCI, Garfield and his team selected 613 cit*ing* journals from the physical and life sciences and collected all citations appearing in articles in these journals in 1961 (Garfield, 1963a). This enabled them to identify all papers that were cited by these articles in 1961. The cit*ed* papers could have been published in any previous year (i.e., not only in 1961) and in any journal (i.e., not only in the set of citing journals but in any journal or book).

This information was stored on punch cards and converted to magnetic tapes, which were processed by IBM computers Garfield (1963b, p. x (sic)). Entries were ordered by last names and initials of scientists (see Figure 2.A.1). Figure 2.1 shows the 1961 entry for the medical scientist Murray Abell. His entry covers five cited papers: a 1950 paper

literature on how information disclosure and new information technologies affect market efficiency (e.g., Jensen, 2007; Koudijs, 2015; Tadelis and Zettelmeyer, 2015; Steinwender, 2018; Bernstein et al., 2023).

[4]The SCI was revolutionary because it created a novel metric of scientific productivity that individuals were unable to compile for themselves. No scientist would have had the capacity to count citations to their own work, because it would have required sifting through hundreds of thousands of potentially citing articles. In contrast, earlier metrics of scientific productivity, such as publication catalogs, aggregated information that was already individually available (for example, the *Catalogue of Scientific Papers* (Csiszar, 2017)).

in *Archives of Pathology* (vol. 50, p. 1), another 1950 paper in *Archives of Pathology* (vol. 50, p. 23), a 1956 paper in *Archives of Pathology* (vol. 61, p. 360), a 1957 paper in the *American Journal of Clinical Pathology* (vol. 28, p. 272), and a 1961 paper in *Cancer* (vol. 14, p. 318). Each of these papers was cited at least once in 1961; e.g., the 1956 *Archives of Pathology* paper was cited by one article in 1961 in the *Journal of Pathology and Bacteriology* (vol. 82, p. 281). Overall, these five papers received six citations in 1961.

Figure 2.1: Entry in the Science Citation Index

ABELL MR		*50*	ARCH PATHOL		50	1
	EMERY GN		CAN J BIOCH	61	39	977
		50	ARCH PATH		50	23
	HRSTKA V		ARCH I PHAR	61	130	304
		56	ARCH PATH		61	360
	WILLIAMS GE		J PATH BACT	61	82	281
		57	AMER J CLIN PATH		28	272
	INKLEY SR		ARCH IN MED	61	108	903
	LAUFER A		PATH MICROB	61	24	72
		61	CANCER		14	318
	GOSLING JR		CANCER	61	14	330

Notes: This figure shows a sample entry of the 1961 volume of the SCI. It lists five cited papers for "Abell MR." Murray R. Abell was Professor of Pathology (Medicine) at the University of Michigan. The cited papers could have been published in any year until 1961 (here: 1950 (twice), 1956, 1957, and 1961). The five papers are cited by six citing articles. Because this example is from the 1961 volume of the SCI, all citations are from 1961.

For technical reasons, the SCI did not collect citations for 1962 and 1963. As "[t]he 1961 SCI was the result of an experimental research program," its preparation took more than two years Garfield (1965). After releasing the 1961 SCI in November 1963, the ISI moved on to preparing the 1964 SCI.[5] From then on, the SCI was published quarterly. The set of indexed cit*ing* journals quickly expanded from 613 in 1961 to 2,180 in 1969.

The SCI was an immediate success. By the late 1960s, every major university had a subscription (Garfield, 1972, p. 4). For example, in 1965 chemists at Ohio State University lobbied the library administration to subscribe to a second copy of the SCI, in addition to the copy that was already available in the medical library (see Appendix Figure 2.A.3).[6]

[5] The 1962 and 1963 SCIs were released only in 1972 (Garfield, 1972). For this reason, we measure outcomes in 1969 and, hence, before the ISI had begun to fill in gaps in coverage.

[6] By 1966, the SCI was not only available as printed volumes, but could also be purchased on magnetic tapes. The magnetic tapes provided the raw data for constructing citation counts and for conducting quantitative citation analyses (Garfield, 1966). Furthermore, the ISI published five-year cumulations of the SCI. For example, the 1965-1969 compilation included all citations between 1965 and 1969 (Garfield, 1971).

2.2.2 Data

Reconstructing SCI Coverage from the Web of Science

For contemporaries, citations were only visible if they came from citing articles in journals that were indexed by the SCI. This means that only an incomplete set of citations was visible at the time. Citations before the SCI's introduction in 1961, as well as those from 1962 and 1963, and from journals that were not indexed by the SCI remained invisible. In the 1970s and 1980s, the SCI was backward expanded to cover additional years and journals, and later became part of the *Web of Science*. As a result, the *Web of Science* covers both citations that were visible to contemporaries and citations that were invisible at the time, but became available during the backward expansions.

We reconstruct the sets of citations that were visible and invisible to contemporaries. For this purpose, we hand-collect yearly lists of citing journals from the printed historical SCI volumes. We digitize these lists and hand-link them to the *Web of Science*. Appendix Figure 2.A.2 shows a sample journal list. Using this linking procedure, we can identify which citations were part of the information set of the 1960s, and which ones were not.

Faculty Rosters

To study how the introduction of citation metrics affects the careers of academics, we use data containing faculty rosters for nearly all universities in the United States from the *World of Academia Database* Iaria et al. (see 2022). The data contain almost comprehensive cross-sections of all U.S. academics for the years 1956 and 1969. Because the SCI only counted citations for the natural and biomedical sciences, we focus on all academics who worked in either biology, biochemistry, chemistry, physics, mathematics, or medicine. For the period of our analysis, the database provides the most comprehensive data on academics in the United States (see Iaria et al. (2022) for details). For the 1969 cross-section, the data contain 27,315 scientists at 1,477 departments in 384 universities (Table 2.1, Panel B).

The *World of Academia Database* has two unique advantages for our purpose. First, it enables us to identify the department (e.g., physics at Berkeley) of each academic. Second, it contains complete faculty rosters, which allows us to observe both academics who received citations and, importantly, academics who did not receive any citations.

This enables us to construct comprehensive individual and department rankings based on *all* academics and not only based on those who published and were cited.

Linking Scientists with Publications and Citations

To count scientists' publications and citations, we link the *World of Academia Database* with publication and citation data from the *Web of Science*. We use the cascading linking algorithm developed in Iaria et al. (2022) (see Section 2.B.1.1 for details).

For the 1969 cohort of scientists, we link their publications and citations from 1956 to 1969. This enables us to measure the number of papers that each scientist published in this period and to count the citations that these papers received from the time they were published until 1969. Importantly, for our identification strategy, we observe the complete citation network and thus the exact journal in which a certain paper was cited. This allows us to measure whether the citations were covered in the SCI and were thus visible to contemporaries.

The average scientist in our data published 8.75 papers between 1956 and 1969 (Table 2.1, Panel A). These papers received 47 citations that were visible to contemporaries and 19 citations that were invisible to contemporaries but can be observed today.[7] As has been documented by a large literature in the sociology of science, citations of academics are highly skewed (e.g., Lotka, 1926). The most highly cited scientists in our data received more than 3,000 visible and more than 2,000 invisible citations between 1956 and 1969.

Constructing Scientist Rankings

Using our scientist-publication-citation-linked data, we can construct rankings based on citations and publications. Within each subject, we rank scientists according to their citation (or publication) counts between 1956 and 1969. We then calculate each scientist's percentile rank in the subject-specific distribution of citations (or publications), assigning 100 to the best and 1 to the worst scientist. This variable transformation allows us to compare the scientists' relative positions in the citation distributions, even if these distributions differ across subjects. For example, the median biologist received 2 citations, while the median chemist received 9 citations. If percentiles cannot be uniquely assigned because too many scientists have the same number of citations or

[7] We show below that the different distributions of visible and invisible citations do not drive our results.

Table 2.1: Descriptive Statistics

Panel A: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Publications	8.75	16.65	0	405
Visible Citations	46.99	128.05	0	3,346
Invisible Citations	18.93	57.95	0	2,010
Full Professor Share	0.40	0.49		
Female Share	0.10	0.30		

Panel B: Number of Observations

Dataset includes:	Observations
Citations	1,800,669
Publications	239,124
Scientists	27,315
Departments	1,477
Universities	384

Notes: Panel A reports summary statistics at the scientist level for the cross-section of scientists observed in 1969. Publications are the number of papers a scientist published between 1956 and 1969; visible citations are the number of citations these papers received between 1956 and 1969 that were visible in the SCI; invisible citations are the number of citations these papers received between 1956 and 1969 that were not visible in the SCI. Panel B reports the number of observations at the citation, publication, scientist, department, and university level.

publications, we assign the mid-point of the corresponding percentiles.[8] This is particularly important for scientists with zero citations. Alternative assignments of percentile ranks to scientists with zero citations do not affect our findings (see Section 2.C.2.3).

Constructing Department Rankings

Our data also enable us to construct the most comprehensive department rankings for this time period. These are the first rankings for this period that are based on scientific output, as opposed to reputational surveys. In addition, our rankings cover a much larger number of departments than previously available survey-based rankings. In fact, the practice of ranking departments by their research output only developed as a result of citation indexing.

[8] For example, in physics 30.37% of observations have zero citations. For the main results, we assign the mid-point between the 1st percentile and the 31st percentile, i.e., a percentile rank of 15.5, to each of these observations.

We rank all 1,477 departments in 384 universities on the basis of the average total citations received by scientists in each department. As outlined above, the rankings avoid systematic error because the *World of Academia* database also lists all scientists who have not published and/or were not cited in our study period. In our main department ranking, we construct the leave-out mean of the number of citations received by scientists in a given department, i.e., the average citation count of scientist i's colleagues. We then assign the percentile rank in the subject-specific distribution of leave-out mean citation counts, assigning 100 to the best and 1 to the worst department. We use the percentile rank because it allows us to compare the relative position of departments in different subjects (physics, chemistry, and so on), which have different numbers of departments, scientists, and average citations per scientist.

In robustness checks, we show that our findings are robust to using several alternative department rankings. First, we construct analogous department percentile ranks based on publications. Second, we construct department percentile ranks using reputation-based rankings from Roose and Andersen (1970) and Cartter (1966). As highlighted above, the reputation-based rankings cover far fewer universities.[9] In Section 2.B.2, we list the top 20 departments in each subject, as measured by the various rankings.

2.2.3 How Was the SCI Used in Hiring and Promotions?

While the SCI was predominantly designed to facilitate literature research, it was immediately used to evaluate scientists. For example, Eugene Garfield remembered:

> "The SCI's success did not stem from its primary function as a search engine, but from its use as an instrument for measuring scientific productivity." (Garfield, 2007, p. 65)

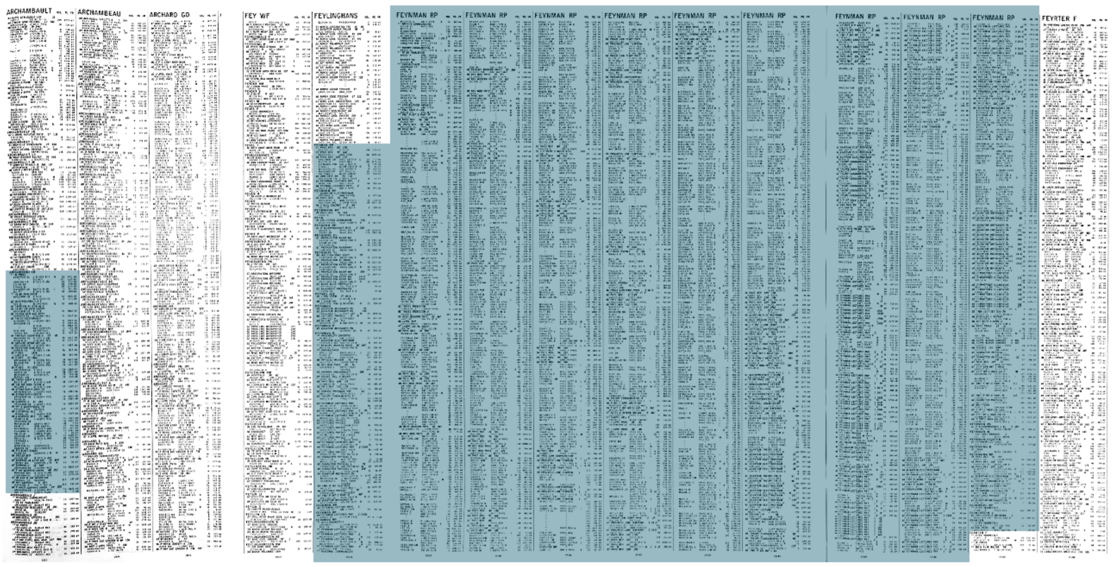
The eminent biologist Richard Dawkins described the SCI as a publication that:

> "is intended as an aid to tracking down the literature on a given topic. University appointments committees have picked up the habit of using it as a rough and ready (too rough and ready) way of comparing the scientific achievements of applicants for jobs." (Dawkins, 2016, p. 427)

[9]The Cartter ranking contains 106 universities, and the Roose-Andersen ranking contains 130, while our baseline ranking contains 384 universities. The alternative rankings strongly correlate with our main citation-based ranking. The correlation between the Cartter ranking and our citation-based ranking is 0.68, while the correlation between the Roose-Andersen ranking and our citation-based ranking is 0.70.

The SCI made scientists' citations visible and readily accessible for the first time. Because the SCI was organized by cited authors, it was easy to measure and compare the citation counts of scientists. Figure 2.2 shows one such comparison for two scientists working at Caltech. The box on the left shows citations of the physicist Charles Archambeau. The box on the right shows the citations of the 1965 physics Nobel laureate Richard Feynman. As one contemporary remarked, "[a]n early form of research evaluation of individuals made use of a ruler to measure column inches of citations!" (Birkle et al., 2020, p. 364).

Figure 2.2: Comparison of SCI Entries



Notes: This figure compares the entries in the 1965-1969 cumulation of the SCI (Garfield, 1971) for two physicists at Caltech: Charles Archambeau on the left, and Nobel laureate Richard Feynman on the right.

Quickly, scientists, funding bodies, and university administrators started to use citation counts in hiring, promotion, and funding decisions. Some universities even made citations a mandatory metric in the evaluation of applicants' portfolios (Wade, 1975, p. 429). The importance of newly available citation metrics is exemplified in the court case *Johnson v. University of Pittsburgh*.[10] In 1973, Sharon Johnson sued the biochemistry department at the University of Pittsburgh for sex discrimination. Her legal case argued that she was overlooked for tenure even though her papers had received more citations (as measured in the SCI) than those of two recently tenured male colleagues.

[10] *Dr. Sharon Johnson v. The University of Pittsburgh*, W.Da. PA., 1977.

The SCI's Impact on Assortative Matching: Suggestive Evidence

We first provide suggestive evidence of the impact of the citation metrics on the assortative matching of academics and departments. If departments began to use the SCI to evaluate scientists, we would expect that the correlation between a scientist's citations and their department rank increased after the introduction of the SCI. We find that the correlation between a scientist's individual citation rank and their department rank increased by 61% between 1956 and 1969 (Figure 2.3, panels (a) and (b)). In contrast, the correlation between the individual publication rank and the department rank decreased by 46% (Figure 2.3, panels (c) and (d)).

This evidence is in line with the hypothesis that the introduction of citation metrics increased the reliance of hiring decisions on citations, and decreased the reliance on other observable characteristics such as publications. However, the increasing correlation between scientists' citation rank and their department rank may have been caused by other factors. For example, the increasing importance of expensive research labs or federal research funding (e.g., Kantor and Whalley, 2023) could disproportionately favor leading departments and allow them to attract highly cited scientists. Similarly, increases in team production (e.g., Wuchty et al., 2007; Jones, 2009) may have spurred within-department collaborations and, hence, may have made department quality more important for scientists' citations. To overcome these challenges, we introduce a novel identification strategy that allows us to isolate the causal effect of citation metrics on assortative matching in academia.

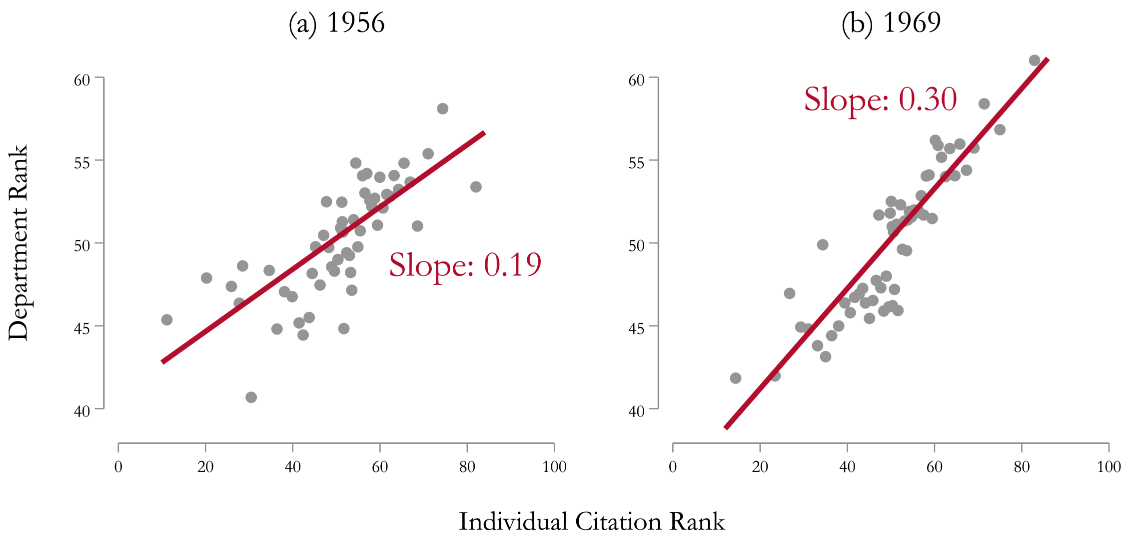
2.3 The Effect of Citation Metrics on Assortative Matching

2.3.1 Empirical Strategy

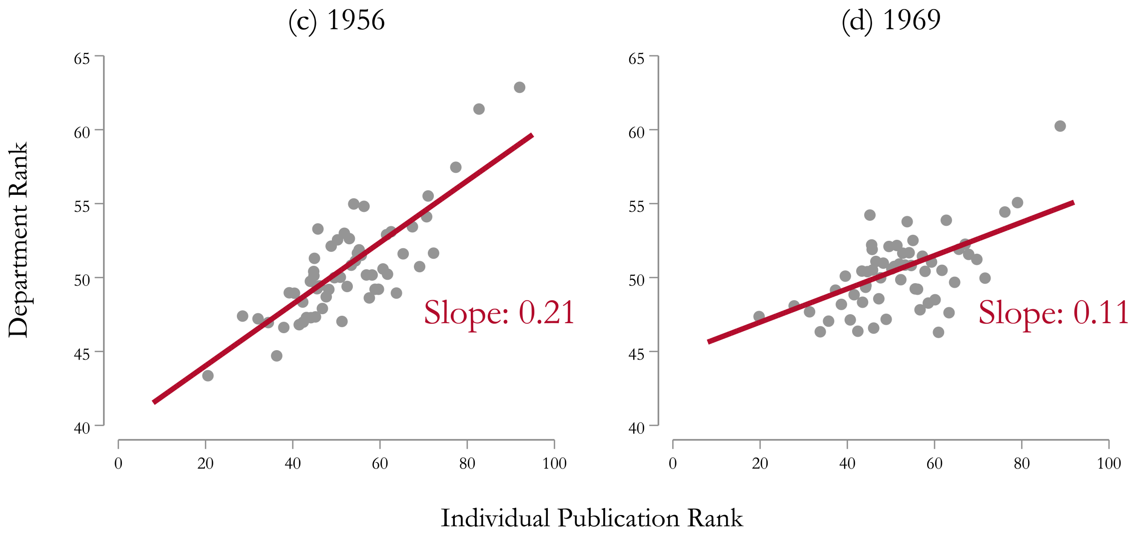
We identify the causal effect of citation metrics by comparing the effect of citations that were *visible* in the SCI to the effect of citations that remained *invisible*. For technical reasons, the SCI only covered citations from cit*ing* articles in a subset of journals and years. Hence, only citations from citing articles in this subset were visible to the scientific community. In contrast, other citations remained invisible because they were not covered in the SCI. Importantly, the cit*ed* papers could have been published in any journal and in any previous year. Therefore, scientists' visible citation counts were

Figure 2.3: Assortative Matching Before and After Citation Metrics

Assortative Matching by Citations



Assortative Matching by Publications



Notes: Panels (a) and (b) show the correlation of scientists' citation rank and their department rank for two cross-sections: 1956 and 1969. Panel (a) shows a binned scatter plot for 1956 and, thus, before the introduction of the SCI. While we can now measure these citations, they were not observable at the time. Panel (b) shows a binned scatter plot for 1969 and, thus, after the introduction of the SCI. The regression coefficient in both panels is conditional on an individual's publication rank. The p-value of the test that the slope coefficients in panels (a) and (b) are equal is 0.008. Panels (c) and (d) show the correlation between scientists' publication rank and their department rank. Publications were observable to contemporaries in both 1956 and 1969. The regression coefficient in both panels is conditional on an individual's citation rank. The p-value of the test that the slope coefficients in panels (c) and (d) are equal is 0.007.

not determined by the journals in which their papers were published but only by the journals in which their papers were cited.

As described above, the first volume of the SCI covered citations from 1961 in any of the 613 citing journals. As a result, all 1961 citations in those 613 journals became visible in the SCI, while citations before 1961 and in other journals remained invisible. Due to limited computing power, the collection of citation data was interrupted in 1962 and 1963. By 1964, data collection resumed. The set of indexed citing journals quickly expanded from 613 in 1961 to 2,180 in 1969. As a result, the visibility of citations was affected by two sources of variation: first, in which *year* a paper was cited, and second, in which *journal* it was cited.[11]

Our data enable us to reconstruct which citations were part of the information set of the 1960s, i.e., we measure citations that were *visible* in the SCI. Crucially, we can also reconstruct which citations were not part of that information set, i.e., citations that were *invisible*. Invisible citations can be measured today because citation databases were expanded to include citations for additional years and for a larger set of citing journals.

Table 2.2 illustrates the identifying variation for a hypothetical scientist. It reports citations to the scientist's papers, which were published in any journal and in any year. These papers were cited in articles from journals A, B, and C between 1956 and 1969. Journal A was in the initial set of 613 citing journals indexed by the SCI in 1961. Journal B was added to the SCI in 1966, whereas journal C was not indexed in the 1960s. The dark blue cells indicate citations that were visible to contemporaries because the SCI collected citations for these years and citing journals. The light blue cells indicate citations that were invisible because the SCI did not collect data for these years and citing journals. In other words, citations in dark blue cells were part of contemporaries' information set, while citations in light blue cells were not.

In the example, the hypothetical scientist's papers were cited in articles published in journal A in 1959, in 1961, in 1963, and twice in 1967. The citations in 1959 and 1963 were invisible because the SCI did not exist for those years. In contrast, the citations in 1961 and 1967 were visible in the SCI. Similarly, the scientist's papers were cited in articles in journal B in 1957, 1961, 1965, and three times in 1966. Because journal B was added to the SCI only in 1966, the citations in 1957, 1961, and 1964 were invisible. In contrast, the three citations in 1966 were visible. Finally, the scientist's

[11]Below, we provide evidence that the quality of citing journals or differences in the timing of citations does not drive our findings.

Table 2.2: Identifying Variation for Specification 1

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

Notes: This table reports citations of a hypothetical scientist's papers. Numbers in dark blue cells show citations that were visible in the SCI because the citation occurred in a journal and year (1961, or 1964-69) that was covered by the SCI. Numbers in light blue cells show citations that were invisible in the SCI, but are observable today.

papers were cited in articles in journal C in 1959, 1961, and 1969. As journal C was not indexed in our study period, all of these citations were invisible to contemporaries.

Hence, if contemporaries had looked up the scientist's total citations in the SCI in 1969, they would have observed six citations, i.e., the scientist had six *visible* citations. In addition, the scientist had eight citations that were *invisible* at the time. Using modern citation data, we can observe both visible and invisible citations. For each scientist i, we separately count the number of visible and invisible citations between 1956 and 1969 to i's papers published between 1956 and 1969.

2.3.2 Specification 1: Visible vs. Invisible Citations

Our identification strategy exploits the differential visibility of scientists' citations. If the measurement of citations affects the assortativeness of the match between academics and universities, visible citations should be more predictive of career outcomes than invisible ones.[12] The identifying assumption underlying this new empirical strategy is that the effect of visible and invisible citations would be the same if both had been covered in the SCI. Given the arbitrary timing of the introduction of the SCI and the lack of coverage for the years 1962 and 1963, this seems plausible. Nonetheless, there may be concerns that any effect might be driven by differences in the quality of the citing journals or the timing of citations, i.e., by the two sources of variation in the visibility of citations. We address these concerns with alternative specifications outlined below.

We estimate the following regression:

$$\begin{aligned} \textit{Dep. Rank}_i = \delta \cdot \textit{Visible Citations}_i + \theta \cdot \textit{Invisible Citations}_i \\ + \pi \cdot \textit{Publications}_i + \textit{Subject FE} + \epsilon_i \end{aligned} \tag{2.1}$$

where $\textit{Dep. Rank}_i$ is the department rank of scientist i in 1969, where 100 is the best and 1 the worst department.[13] $\textit{Visible Citations}_i$ measure scientist i's visible citations. $\textit{Invisible Citations}_i$ measure scientist i's invisible citations. In the baseline specification, we measure citations as the percentiles in the distributions of visible and invisible citations.[14] $\textit{Publications}_i$ flexibly control for scientists i's publications. $\textit{Subject FE}$ control for differences between academic subjects. To account for potential correlations of regression residuals in a certain department, e.g., in chemistry at Berkeley, we cluster all standard errors at the department level.

To study how citation metrics affect assortative matching, we compare the magnitudes of the estimated coefficients $\hat{\delta}$ and $\hat{\theta}$. If the visibility of citations in the SCI increased the assortativeness of the match between scientists and departments, we would expect that $\delta > \theta$. For example, the difference between δ and θ captures whether citations that occurred in 1961 instead of 1962 had a larger impact on the match between scientists and departments. Note that we would not expect θ to be zero because, even in the absence of the SCI, scientists will have an approximate idea about the importance and quality of other scientists' papers.

We report estimates of Equation (2.1) in the first panel of Table 2.3. In column (1), we report a specification that controls for subject fixed effects. The coefficient

[12] Invisible citations may still correlate with outcomes, because scientists have always had a rough idea of the quality, and thus citation potential, of their peers' papers.

[13] In the main specification, we use the department ranking based on the leave-out mean of citations. All results are robust to using different measures of the department rank, e.g., based on citations, publications, or alternative department rankings based on contemporaneous reputation-based surveys (Tables 2.C.1 and 2.C.2).

[14] We explore alternative transformations of citation counts in Table 2.C.3, e.g., standardizing citation counts or using the inverse hyperbolic sine of citations.

Table 2.3: Citations and Assortative Matching

	Dependent Variable: Department Rank				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Visible Citations	0.299	0.320	0.280	0.252	0.244
	(0.034)	(0.031)	(0.035)	(0.035)	(0.036)
Invisible Citations	0.103	0.068	0.062	0.057	0.056
	(0.023)	(0.020)	(0.021)	(0.023)	(0.023)
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
R^2	0.138	0.140	0.153	0.229	0.256
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	0.305	0.327	0.284	0.256	0.247
	(0.035)	(0.032)	(0.036)	(0.036)	(0.037)
Pseudo-Visible Citations	0.033	0.012	0.013	0.025	0.023
	(0.021)	(0.020)	(0.020)	(0.022)	(0.023)
Invisible Citations (SCI years)	0.030	0.029	0.030	0.021	0.021
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Invisible Citations (non-SCI years)	0.057	0.044	0.037	0.025	0.029
	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)
P-value (Visible = Pseudo-Visible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Visible = Invisible (SCI years))	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Visible = Invisible (non-SCI years))	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.451	0.551	0.676	0.972	0.939
R^2	0.138	0.141	0.154	0.229	0.256
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	27,315	27,315	27,315	27,315	27,315
Dependent Variable Mean	50.40	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.1) in the first panel and of Equation (2.2) in the second panel. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

for visible citations is around three times larger than the coefficient for invisible citations. Scientists with a 10 percentiles higher visible citation count were, on average, placed at a 3.0 percentiles higher-ranked department in 1969. For example, a chemist would be placed at Harvard or Stanford as opposed to Northwestern University or the University of Southern California. In contrast, scientists with a 10 percentiles higher invisible citation count were, on average, only placed at a 1.0 percentiles higher-ranked department.[15] We also report the p-value of a two-sided t-test for the equality of the two citation coefficients. We reject the equality of the two coefficients at the 0.1%-level.

To rule out that these differences could potentially be explained by scientists' publication records, we include fine-grained controls for publications in columns (2)-(5). In column (2), we show that the results are robust to controlling for the number of publications by year, i.e., controlling separately for the number of publications in 1956, 1957, and so on.[16] One might be concerned that differences in publication and citation patterns across the sciences could explain our findings. For example, mathematicians publish fewer papers and receive fewer citations than chemists or medical researchers. To address this concern, we show that the results are robust to separately controlling for the number of publications by year and subject (column (3)).

Naturally, not only the number of publications but also the journal in which a paper was published may be correlated with citation counts and thus might bias our estimates. To overcome this challenge, we additionally control for the number of publications in each individual journal. That is, we add a variable that counts the number of papers in *Science*, another variable that counts the number of papers in *Nature*, and so on. In total, we add 1,714 variables that control for the number of publications in each journal (column (4)). We also allow the effect of these controls to differ by subject, so that a publication in *Science* may have a different effect on the career of a physicist than on the career of a chemist (column (5)). The results are robust to the inclusion of these fine-grained controls for scientists' publication records. In fact, the difference in the impact of visible and invisible citations increases with the inclusion of additional controls. With all controls (column (5)), visible citations have a four times larger effect on the department rank than invisible citations. Appendix Figure 2.C.1 illustrates these results graphically.

We show that these findings are robust to using alternative ways of ranking departments (Section 2.C.2.1), to using alternative transformations of individual citation counts (Section 2.C.2.2 and 2.C.2.3), and to imposing additional sample restrictions (Section 2.C.2.4).

[15] As discussed above, it is not surprising that invisible citations are positively correlated with the department rank because they proxy for wider recognition by the scientific community.

[16] Since the number of scientists' publications takes many fewer values than the number of citations (see Table 2.1), especially when measuring publications separately by years (columns (2)-(5) in Table 2.3) and journals (columns (4)-(5) in Table 2.3), we do not use the percentile rank transformation of publications.

Alternative Explanation 1: Quality of Citing Journals

Despite the somewhat arbitrary nature of the SCI coverage, the results would be biased if the visibility of citations in the SCI were correlated with other characteristics that impacted a scientist's department rank in 1969.

The first concern is that visible citations may come from citing articles in higher quality journals (e.g., *Nature* or *Science*) and therefore have a larger impact on a scientist's career. It is important to note that this concern is somewhat mitigated because it was difficult to assess journal quality before the introduction of the SCI. Some of the citing journals initially indexed in the SCI turned out to be of relatively lower quality. Similarly, many journals that were, in fact, of high quality were not indexed during the first years of the SCI.

While it was not possible to quantitatively measure journal quality at the time, we can retrospectively compute measures of the quality of the citing journal and thereby assess whether visible citations came from better journals. For this test, we compute the impact factors for all citing journals in the pre-SCI period.[17] Journals which were indexed in the 1961 SCI had an average impact factor of 0.83, while journals which were not indexed had an average impact factor of 0.86 (p-value of test of equal means: 0.618). We also plot the distributions of the average impact factors for both types of journal in Figure 2.4. This analysis indicates that journals indexed in the 1961 volume of the SCI were not of higher quality than journals that were not indexed.

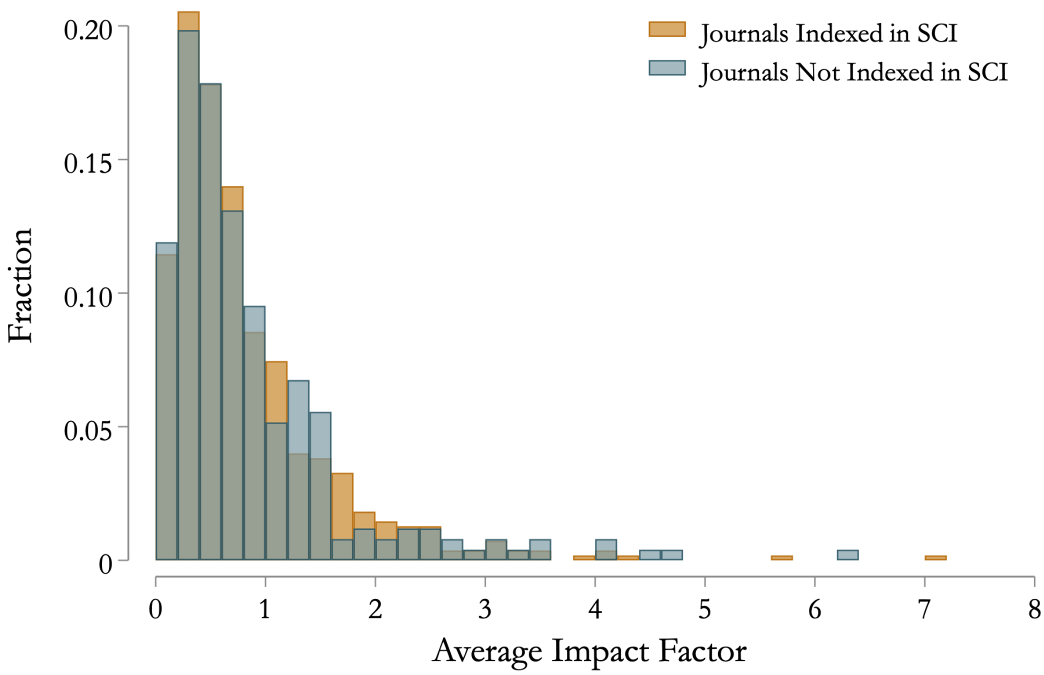
To provide additional evidence that differences in the quality of citing journals are not driving the results, we estimate regressions that only consider citations from a fixed set of journals. For this test, we only rely on over-time variation in the visibility of citations. This allows us to abstract from potential differences in journal quality. In particular, we estimate regressions that only use visible and invisible citations from the set of journals that were included in the first edition of the SCI in 1961 (i.e., only using over-time variation in citations from type A journals in Table 2.2).[18]

For example, the test compares scientists who were cited in *Nature* in 1961 and therefore these citations were visible in the SCI, to scientists who were cited in *Nature* in 1962 and therefore these citations were invisible. The hypothetical scientist presented in Table 2.2 would have three visible citations: one in 1961 and two in 1967; and two

[17]Because the 1961 volume of the SCI was published in November 1963, we define the pre-SCI period as 1956-1963. The impact factor is calculated as the average number of citations in year t to articles published in that journal in the years $t-1$ and $t-2$.

[18]We visualize the underlying variation of this robustness check in panel (b) of Appendix Figure 2.C.2.

Figure 2.4: Quality of Journals Indexed and Not Indexed in SCI



Notes: The figure shows histograms of impact factors for two sets of journals: journals indexed in the SCI in 1961 (orange) and journals not indexed in the SCI in 1961 (blue). For each journal, we average the impact factors over the pre-period (1956-1963).

invisible citations: one in 1959 and one in 1963. For this test, we do not consider citations in type B or C journals, i.e., journals not indexed in the first SCI in 1961. The results that use only citations from type A citing journals are almost identical to the main results (see Appendix Table 2.C.6), indicating that differences in the quality of citing journals do not drive our findings.

Alternative Explanation 2: Timing of Citations

The second concern stems from the differential timing of visible and invisible citations. As the SCI was introduced in 1961, visible citations, on average, occurred in later years than invisible ones. If more recent citations had more predictive power for career outcomes in 1969, the larger effect of visible citations may be spurious.

We address this concern by fixing the timing of citations and exclusively relying on across-journal variation in visibility. In particular, we estimate regressions that only use visible and invisible citations from years in which the SCI was available (i.e., 1961 and 1964-1969). This exercise compares scientists with the same publication record who were cited in similar years but in different journals, only some of which were covered in the SCI.[19]

For our hypothetical scientist presented in Table 2.2, this test considers six visible citations: one from journal A in 1961, two from journal A in 1967, and three from journal B in 1966. It also considers three invisible citations: one each from journal B in 1961 and 1965, and one from journal C in 1969.[20]

The results that use only citations from years in which the SCI was published are very similar to the main results (Appendix Table 2.C.7). The point estimates are almost identical, and the p-values for the difference in coefficients remain below the 0.1%-level. These results strongly suggest that the differential timing of visible and invisible citations does not drive our findings.[21]

2.3.3 Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations

The quality of citing journals and the timing of citations might interact to make visible citations more predictive for assortative matching. To address such concerns, we introduce a second specification, which includes a placebo test that compares the predictiveness of different types of invisible citations. For this specification, we partition the citation space into four mutually exclusive sets depending on where and when a scientist was cited (see Table 2.4):

1. *Visible citations*: citations from journals that were indexed in the SCI in years when the SCI was published (1961 and 1964-1969),
2. *Pseudo-visible citations*: citations from journals that were indexed in the SCI in 1961 but from years when the SCI was not published (1956-1960 and 1962-1963),
3. *Invisible citations (SCI years)*: citations from journals that were not indexed in the SCI in years when the SCI was published (1961 and 1964-1969),

[19]As outlined above, in the early years, limited funding and computing power prevented the Institute for Scientific Information from covering a large number of journals in the SCI (Garfield, 1963b, p. xvii). As a result, citations in many reputable journals remained invisible.

[20]See also panel (c) of Appendix Figure 2.C.2.

[21]As more journals were indexed in later years, even in this test, visible citations may, on average, come from later years. We address this concern by restricting the years for which we measure visible and invisible citations to even smaller windows (see Appendix Table 2.C.8).

4. *Invisible citations (non-SCI years)*: citations from journals that were not indexed in the SCI in 1961 and from years when the SCI was not published (1956-1960 and 1962-1963).

Table 2.4: Identifying Variation for Specification 2

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

Notes: This table reports citations to a hypothetical scientist's papers. We partition the citation space along two dimensions: (i) years covered by the SCI (blue) or not (red) and (ii) journals covered by the SCI (dark) or not (light). Dark blue cells show citations that were visible in the SCI. Dark red cells show pseudo-visible citations, i.e., citations that were invisible (because they came from years not covered by the SCI) but would have been visible had the SCI been published for those years. Light blue cells show invisible citations for years in which the SCI was published, i.e., citations that came from journals not covered by the SCI in years when the SCI was published. Light red cells show invisible citations for years in which the SCI was not published, i.e., citations that came from journals not covered by the SCI in years when the SCI was not published.

For our hypothetical scientist, this test considers six visible citations (dark blue in Table 2.4). It also considers two pseudo-visible citations (dark red). Furthermore, it considers three invisible citations in SCI years (light blue). Finally, it considers three invisible citations in non-SCI years (light red).

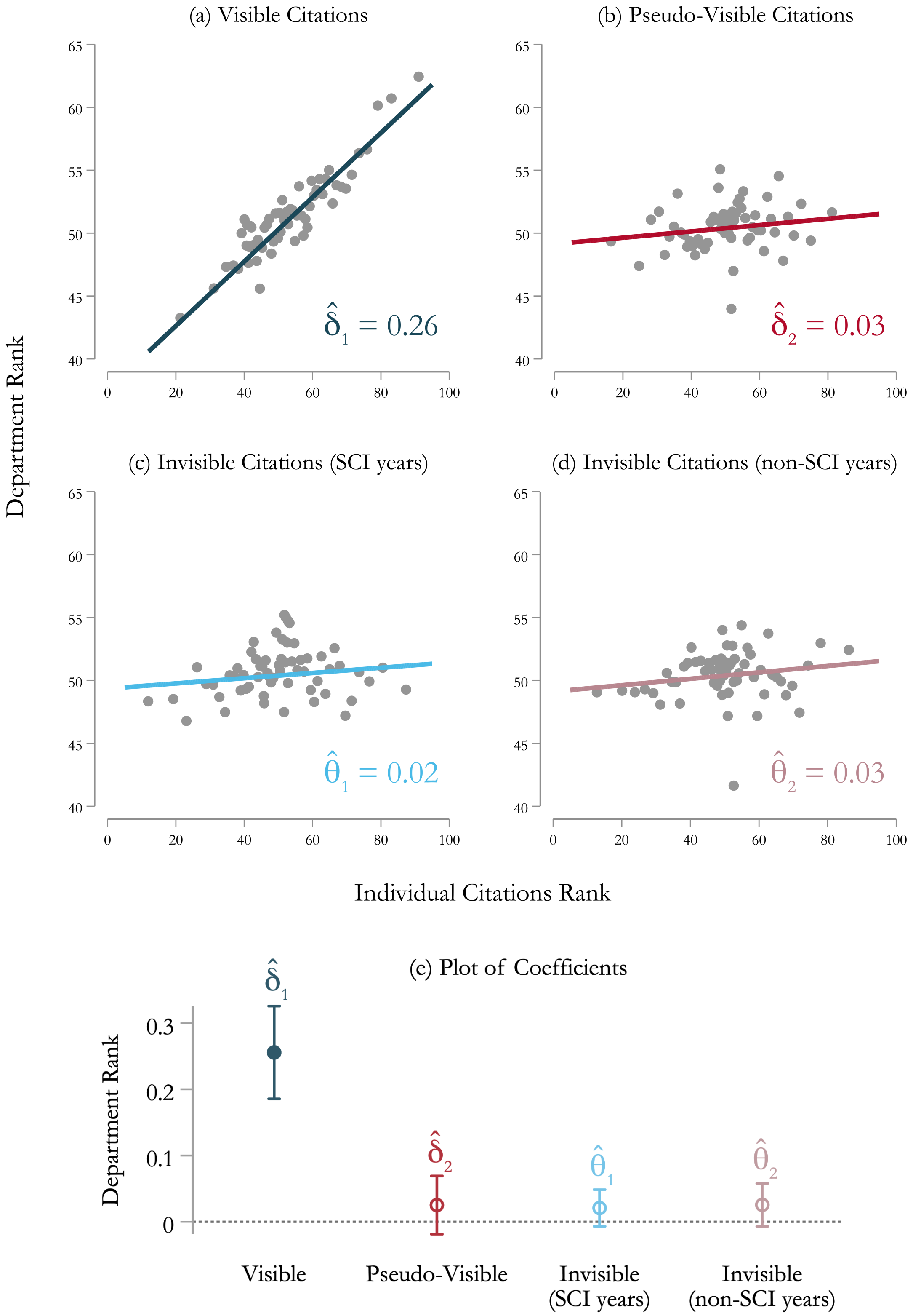
For each scientist, we count the number of citations in these four sets and construct the corresponding percentile ranks. Using these measures, we estimate the following regression:

$$\begin{aligned} \textit{Dep. Rank}_i = {} & \delta_1 \cdot \textit{Visible Citations}_i \\ & + \delta_2 \cdot \textit{Pseudo-Visible Citations}_i \\ & + \theta_1 \cdot \textit{Invisible Citations (SCI years)}_i \\ & + \theta_2 \cdot \textit{Invisible Citations (non-SCI years)}_i \\ & + \pi \cdot \textit{Publications}_i + \textit{Subject FE} + \epsilon_i \end{aligned} \tag{2.2}$$

As pseudo-visible citations were not visible to contemporaries, we would expect them to matter similarly to the invisible ones, i.e., we would expect $\delta_1 \gg \delta_2 \approx \theta_1 \approx \theta_2$. Note that the comparison between visible and pseudo-visible citations allows us to estimate the causal effect of citation metrics even if journals indexed in the SCI differed in quality from journals not indexed in the SCI.

We find that the coefficient on visible citations (Table 2.3, Specification 2) is almost identical to the baseline specification (Table 2.3, Specification 1). Strikingly, the coefficient on pseudo-visible citations is a lot smaller and very similar to the coefficients on invisible citations. This indicates that citations in journals that were indexed by the SCI only had a differential impact in years in which the SCI was actually available. The coefficients on invisible citations from SCI years and non-SCI years are also very similar and not distinguishable from the coefficient on pseudo-visible citations (p-value of test $\delta_2 = \theta_1 = \theta_2$: 0.972). Figure 2.5 visualizes the results of Specification 2. This confirms that citations from journals indexed by the SCI only mattered in years in which the SCI was available. In addition, in years when the SCI was not available, citations from journals indexed by the SCI (pseudo-visible citations) did not differ from other invisible citations.

Figure 2.5: Assortative Matching, Specification 2



Notes: The figure illustrates the results from Equation (2.2), see Table 2.3, Specification 2. Panels (a) to (d) report bin-scatter plots illustrating the relationship between citation ranks and the department rank. Panel (e) plots the coefficients and 95 percent confidence intervals.

2.3.4 Mechanisms

In the next subsection, we shed light on two potential mechanisms that could underlie the increased assortative matching. First, scientists with few citations may have disproportionately left academia. Second, highly cited scientists may have moved up to better departments. We investigate these explanations in turn by comparing the impact of visible and invisible citations on these individual-level career outcomes.

Effect on Leaving Academia

We start by estimating the impact of citation metrics on the probability of leaving academia. For these regressions, we study scientists who we observe in the 1956 cross-section of academics. We exclude scientists who were already full professors in 1956 to avoid picking up retirements.[22] We then check whether these scientists had left academia by 1969. We estimate the following regressions:

Specification 1:

$$\begin{aligned}\mathbb{1}[\mathit{Leaving\ Academia}]_i = {} & \delta \cdot \mathit{Visible\,Citations}_i + \theta \cdot \mathit{Invisible\,Citations}_i \\ & + \pi \cdot \mathit{Publications}_i + \mathit{Subject\,FE} + \epsilon_i \end{aligned} \tag{2.3}$$

Specification 2:

$$\begin{aligned}\mathbb{1}[\mathit{Leaving\ Academia}]_i = {} & \delta_1 \cdot \mathit{Visible\,Citations}_i \\ & + \delta_2 \cdot \mathit{Pseudo\text{-}Visible\,Citations}_i \\ & + \theta_1 \cdot \mathit{Invisible\,Citations\ (SCI\ years)}_i \\ & + \theta_2 \cdot \mathit{Invisible\,Citations\ (non\text{-}SCI\ years)}_i \\ & + \pi \cdot \mathit{Publications}_i + \mathit{Subject\,FE} + \epsilon_i \end{aligned} \tag{2.4}$$

where $\mathbb{1}[\mathit{Leaving\ Academia}]_i$ is an indicator variable equal to one if a scientist left academia between 1956 and 1969. The remaining variable definitions are identical to the definitions in Equations (2.1) and (2.2).

The probability of leaving academia was lower for academics with a higher visible citation count (Table 2.5, Specification 1). Scientists with a 10 percentile higher visible citation count were around 3.0 percentage points (or 4.3 percent relative to the mean) less likely to leave academia between 1956 and 1969. Strikingly, invisible citations did

[22]The results are similar if we include full professors in this analysis.

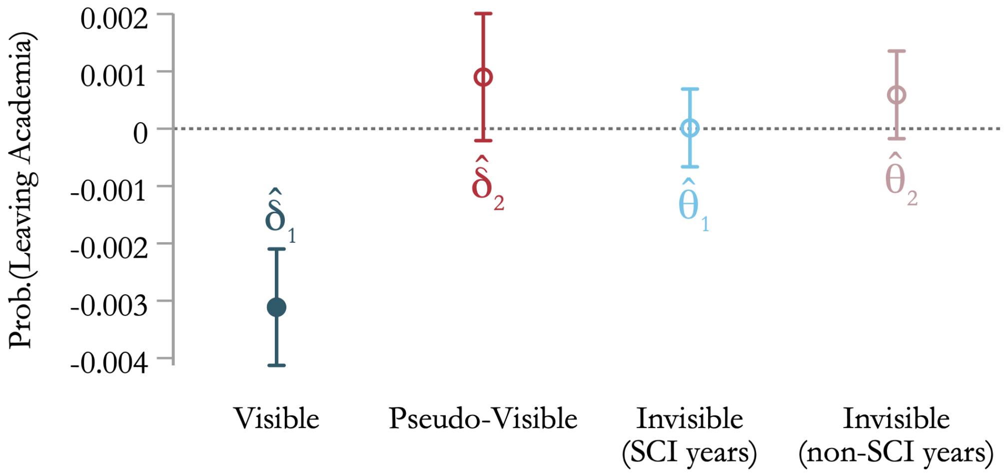
Table 2.5: Mechanism 1: Leaving Academia

	Dependent Variable: Leaving Academia				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Citations Visible	-0.0038	-0.0042	-0.0038	-0.0030	-0.0029
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Citations Invisible	0.0001	0.0008	0.0009	0.0008	0.0009
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0005)
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
R^2	0.088	0.092	0.105	0.254	0.301
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	-0.0037	-0.0039	-0.0035	-0.0031	-0.0031
	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Pseudo-Visible Citations	0.0002	0.0006	0.0006	0.0009	0.0009
	(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0006)
Invisible Citations (SCI years)	-0.0002	-0.0000	0.0000	0.0000	-0.0000
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Invisible Citations (non-SCI years)	-0.0000	0.0001	0.0001	0.0006	0.0008
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
P-value (Visible = Pseudo-Visible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Visible = Invisible (SCI years))	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Visible = Invisible (non-SCI years))	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.718	0.510	0.579	0.298	0.218
R^2	0.089	0.092	0.105	0.253	0.300
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	12,368	12,368	12,368	12,368	12,368
Dependent Variable Mean	0.691	0.691	0.691	0.691	0.691

Notes: The table reports the estimates of Equation (2.3) in the first panel and of Equation (2.4) in the second panel. The dependent variable is an indicator equal to one if scientist i left academia, i.e., i was observed in 1956, but not in 1969. These regressions use the 1956 cross-section of scientists who were not full professors. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

not have a significant impact on the probability of leaving academia. The p-values for the tests that the coefficients on visible and invisible citations are equal are lower than 0.001. The estimates from Specification 2 confirm these findings (Table 2.5, Specification 2; and Figure 2.6). These results suggest that the increased assortative matching of academics was, in part, driven by scientists with fewer visible citations leaving academia.

Figure 2.6: Leaving Academia, Specification 2



Notes: The figure plots the coefficients and 95 percent confidence intervals from Equation (2.4), see Table 2.5, Specification 2.

Effect on Moving to a Higher-Ranked Department

As a second mechanism for increased assortative matching, we investigate the moves of scientists between departments. More specifically, we estimate variants of Equation (2.3) and Equation (2.4) in which we replace the dependent variable with an indicator that equals one if a scientist moved to a higher-ranked department between 1956 and 1969.

We find that scientists with a 10 percentile higher visible citation count were around 0.8 percentage points more likely to move to a higher-ranked department (Table 2.6, Specification 1). This relatively small point estimate nevertheless represents a 17.6 percent increase relative to the mean. Invisible citations did not affect the probability of moving to a higher-ranked department. The results are similar if we estimate Specification 2 (Table 2.6, Specification 2; and Figure 2.7).

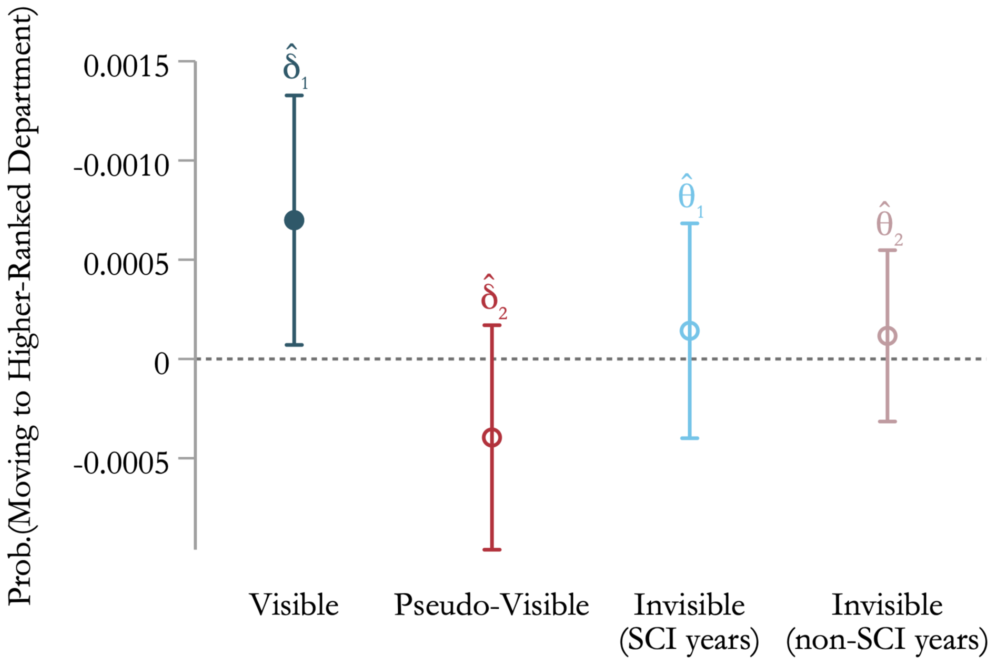
Only 4.6 percent of academics managed to move to a higher-ranked department between 1956 and 1969. Hence, some of the differences between the coefficients on visible and (the various) invisible citations are not significant at conventional levels. However, the results suggest that assortative matching also increased because scientists with many visible citations moved to higher-ranked departments.

Table 2.6: Mechanism 2: Moving to Higher-Ranked Department

	Dep. Var.: Moving to Higher-Ranked Department				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Visible Citations	0.0008	0.0007	0.0006	0.0008	0.0007
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Invisible Citations	-0.0001	0.0001	0.0000	-0.0004	-0.0003
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
P-value (Visible = Invisible)	0.101	0.254	0.238	0.069	0.173
R^2	0.014	0.018	0.037	0.331	0.398
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	0.0008	0.0007	0.0006	0.0007	0.0006
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Pseudo-Visible Citations	-0.0002	-0.0001	-0.0002	-0.0004	-0.0003
	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0004)
Invisible Citations (SCI years)	0.0002	0.0002	0.0002	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Invisible Citations (non-SCI years)	-0.0000	0.0000	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
P-value (Visible = Pseudo-Visible)	0.027	0.076	0.076	0.048	0.151
P-value (Visible = Invisible (SCI years))	0.113	0.189	0.252	0.269	0.401
P-value (Visible = Invisible (non-SCI years))	0.015	0.050	0.102	0.130	0.283
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.498	0.625	0.519	0.321	0.544
R^2	0.014	0.018	0.037	0.331	0.398
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	6,478	6,478	6,478	6,478	6,478
Dependent Variable Mean	0.046	0.046	0.046	0.046	0.046

Notes: The table reports the estimates of variants of Equations (2.3) and (2.4) with a different dependent variable: an indicator equal to one if scientist i moved to a higher-ranked department between 1956 and 1969. These regressions use the sample of scientists observed in 1956 and 1969. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

Figure 2.7: Moving to Higher-Ranked Department, Specification 2



Notes: The figure plots the coefficients and 95 percent confidence intervals from a variant of Equation (2.4) with an alternative dependent variable: an indicator for moving to a higher-ranked department, see Table 2.6, Specification 2.

2.3.5 Overcoming Information Frictions Across Geographic and Intellectual Distance

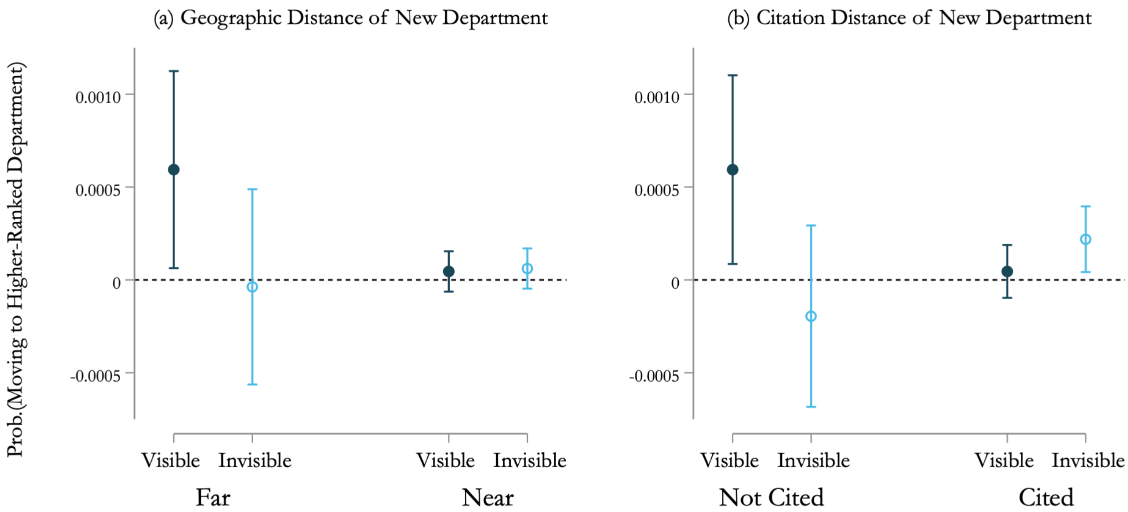
The results on scientists who move up the department quality ladder also enable us to explore how citation metrics reduced information frictions. We would expect that citation metrics would matter more in situations where peers did not have good information on the quality of a potential hire.

We first investigate whether citation metrics help to overcome information frictions due to geographic distance. Specifically, we estimate two regressions with different dependent variables: (1) an indicator equal to 1 if scientist i moved to a higher-ranked department that was geographically far, and (2) an indicator equal to 1 if scientist i moved to a higher-ranked department that was geographically close. We define departments to be geographically far if they are more than 100km apart.[23] The results suggest that citation metrics only impacted moves to higher-ranked departments

[23]Results are similar if we define departments as geographically close using alternative cutoffs (see Figure 2.C.3).

that were geographically far but not to departments that were geographically close (Figure 2.8, panel (a); and Table 2.C.10).

Figure 2.8: Moving to Higher-Ranked Departments by Geographic and Intellectual Distance



Notes: The figure plots coefficients and 95 percent confidence intervals from variants of Equation (2.3). Panel (a) reports results from two regressions with alternative dependent variables: (i) an indicator for moving to a higher-ranked department that was far from scientist i's department; (ii) an indicator for moving to a higher-ranked department that was close to scientist i's department. Panel (b) reports results from two regressions with alternative dependent variables: (i) an indicator for moving to a higher-ranked department where scientist i's papers were not cited before 1963; (ii) an indicator for moving to a higher-ranked department where scientist i's papers were cited before 1963. For detailed results, see Appendix Tables 2.C.9 and 2.C.10.

We also investigate whether citation metrics helped to overcome information frictions due to intellectual distance. We measure intellectual distance using cross-department citations before the move of the scientist. Specifically, we measure whether scientist i's papers had been cited in the receiving department before the introduction of the SCI in 1963. We estimate two regressions with alternative dependent variables: (1) an indicator equal to 1 if scientist i moved to a higher-ranked department where i's research was not cited before the move, and (2) an indicator equal to one if scientist i moved to a higher-ranked department where i's research was cited at least once before the move.[24] The results suggest that citation metrics only impacted moves to higher-ranked departments where scientist i had not been cited before the move (Figure 2.8, Panel B; and Table 2.C.10).

[24] Around a quarter of all moves to higher-ranked departments were to departments where scientists were cited before.

Overall, these findings show that citation metrics helped overcome information frictions due to geographic and intellectual distance. Reducing these frictions may have enabled departments to discover scientists in lower-ranked departments, even if they had not interacted before.

2.4 Heterogeneous Impact of Performance Metrics

As the next step of our analysis, we investigate the heterogeneous impact of the SCI depending on the scientists' citation rank and the rank of their department. Furthermore, we investigate if minorities disproportionately profited from the availability of citation metrics.

2.4.1 Heterogeneous Effects by Individual-Level Citation Rank

First, we investigate if scientists in different percentiles benefited differentially from the visibility of their citations. Specifically, we estimate a non-parametric variant of our main regression:

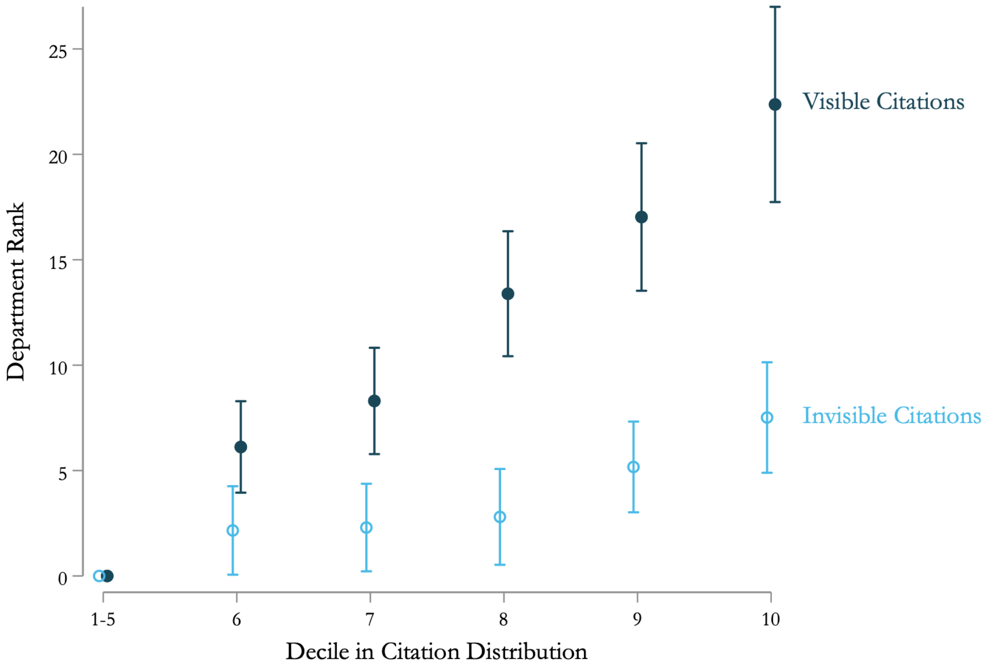
$$\begin{aligned} \textit{Dep. Rank}_i = & \sum_q \delta_q \cdot \mathbb{1}(\textit{Visible Cit Decile}_i = q) \\ & + \sum_q \theta_q \cdot \mathbb{1}(\textit{Invisible Cit Decile}_i = q) \\ & + \pi \cdot \textit{Publications}_i + \textit{Subject FE} + \epsilon_i \end{aligned} \tag{2.5}$$

$\mathbb{1}(\textit{Visible Cit Decile}_i = q)$ and $\mathbb{1}(\textit{Invisible Cit Decile}_i = q)$ are indicator variables for i's decile in the visible and invisible citations distributions. We visualize the estimates relative to the bottom half of the visible and invisible individual-level citation distribution (Figure 2.9).[25]

Over the upper half of the citation distribution, an increase in visible citations increases the assortativeness of the match between the rank of scientist i and the rank of her department. Furthermore, the gap between visible and invisible citations widens for higher deciles of the citation distribution. A scientist in the top decile of the visible citation distribution was, on average, placed in a department that was 22.4 percentiles

[25]To save space, we report results for the specification that controls for the number of publications by year and subject, equivalent to column (3) of Table 2.3. The results for the other specifications are almost identical. Because in some subjects, e.g., mathematics, a relatively high fraction of scientists have zero citations, we do not separately estimate effects for lower deciles.

Figure 2.9: Heterogenous Effects by Individual-Level Citation Rank



Notes: The figure plots coefficients $\hat{\delta}_q$ (dark blue) and $\hat{\theta}_q$ (light blue) and 95 percent confidence intervals from Equation (2.5).

higher in the department ranking, compared to scientists in the bottom half of the visible citation distribution. This is equivalent to a physicist being placed at Harvard as opposed to Case Western Reserve University. In contrast, a scientist in the top decile of the invisible citation distribution was, on average, placed in a department that was only seven percentiles higher ranked, compared to a scientist in the bottom half of the invisible citation distribution. In Appendix Figure 2.D.1, we further split up the top decile and show that scientists in the highest percentiles of the visible citation distribution are placed in even higher-ranked departments. These results suggest that scientists at the upper end of the citation distribution had a particularly large benefit from the availability of citation metrics.

2.4.2 Heterogeneous Effects for Peripheral Scientists

Second, we analyze if scientists who were placed in lower-ranked departments (peripheral scientists) in 1956 differentially benefited from the availability of citation metrics. For this test, we restrict the sample to scientists who we observe both in 1956 and in 1969. The outcome variable is their department rank in 1969:

$$
\begin{aligned}
Dep.\ Rank_i = &\sum_q \delta_q^H \cdot \mathbb{1}(Visible\ Cit\ Decile_i = q) \times High\text{-}Ranked\ (1956)_i \\
&+ \sum_q \delta_q^L \cdot \mathbb{1}(Visible\ Cit\ Decile_i = q) \times Low\text{-}Ranked\ (1956)_i \\
&+ \sum_q \theta_q^H \cdot \mathbb{1}(Invisible\ Cit\ Decile_i = q) \times High\text{-}Ranked\ (1956)_i \\
&+ \sum_q \theta_q^L \cdot \mathbb{1}(Invisible\ Cit\ Decile_i = q) \times Low\text{-}Ranked\ (1956)_i \\
&+ \omega \cdot Low\text{-}Ranked\ (1956)_i + \pi \cdot Publications_i + Subject\ FE + \epsilon_i
\end{aligned}
\tag{2.6}
$$

Variable definitions are identical to Equation (2.5). We add interactions between the deciles of the individual-level citation distributions with indicator variables that equal one if the scientist was working in either a high-ranked or a low-ranked department in 1956. We also control for the main effect of working in a low-ranked department in 1956. We define low-ranked departments as those below the 75th percentile of the department ranking.[26] In physics, for example, low-ranked departments are all departments that were ranked lower than the University of Wisconsin, Madison.

We show estimates for the deciles of the visible citation distribution for scientists in high-ranked and low-ranked departments in Figure 2.10.[27] Estimates for scientists in low-ranked departments are consistently larger than for scientists in high-ranked departments. The p-values for the tests that coefficients for the top two deciles are the same in low-ranked and high-ranked departments are below 0.001. This indicates that scientists who were in lower-ranked departments in 1956 benefited disproportionately from the availability of citation metrics.[28]

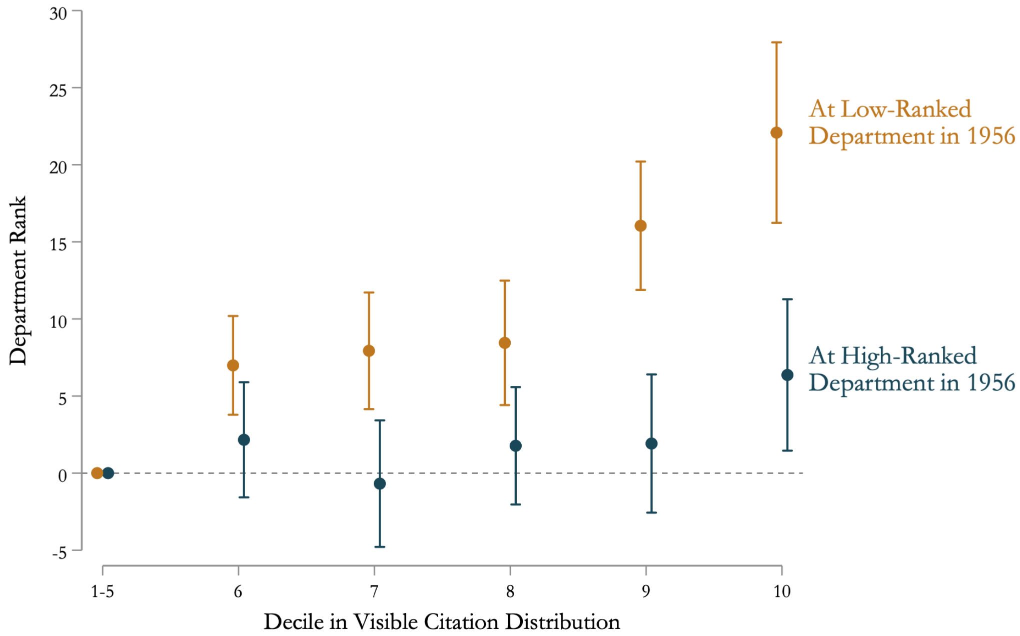
In other words, citation metrics enabled the discovery of “hidden stars.” This may have reduced misallocation by helping the highest-cited scientists in low-ranked departments to move to high-ranked departments. This finding is consistent with anecdotal evidence; for example, a contemporary scientist remarked that “[t]he SCI

[26] Results are qualitatively similar if we use alternative cutoffs (e.g., 60th, 70th, 80th, or 90th percentile, see Appendix Figure 2.D.2).

[27] To improve clarity, the figure does not report the estimates for the invisible citation deciles. As in Figure 2.9, the estimates for invisible citations are consistently smaller than for visible citations. We also find no difference in the impact of invisible citations depending on the department rank.

[28] These effects may be interpreted as mechanical because scientists in low-ranked departments in 1956 have more scope to move to a higher-ranked department. Nevertheless, it is important to quantify how ”hidden stars” may benefit from the availability of performance metrics.

Figure 2.10: Heterogenous Effect of Citation Rank for Peripheral Scientists



Notes: The figure plots coefficients $\hat{\delta}_q^H$ (orange) and $\hat{\delta}_q^L$ (blue) and 95 percent confidence intervals from Equation (2.6).

was especially useful to find people who would otherwise be overlooked" (as cited in Wouters, 1999b, p. 138).

One example, of such a "hidden star" is the medical scientist Hans Hecht. Swiss-born, he obtained his M.D. in Germany in 1936. He escaped the Nazi regime in 1938 and emigrated to the United States.[29] He started his U.S. career as an "Instructor of Medicine at the Wayne University School of Medicine, following which he moved to the University of Utah, where, in 1946, he earned a second M.D. degree" (Katz, 1971) and became a professor there. Arnold Katz of the Mount Sinai School of Medicine described that his: "breadth of scientific interests [...] was always based on an extraordinarily high level of scientific excellence [...] he was never taken in by the investigator with a long list of unoriginal or superficial papers, but saw clearly the essential quality of a man's work" (Katz, 1971). In the mid-1960s, Hans Hecht was hired by the University of Chicago.

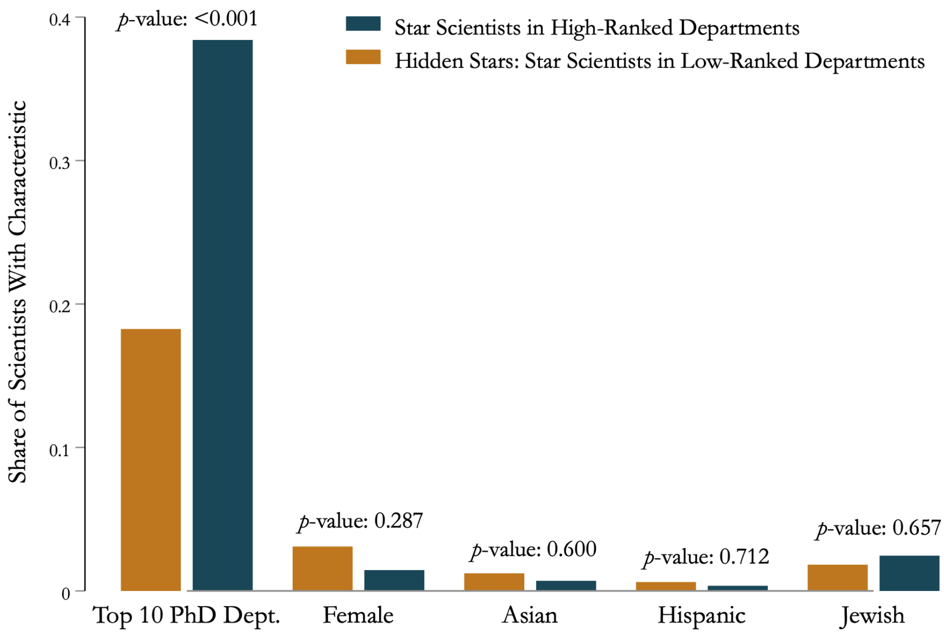
We explore whether the example of Hans Hecht indeed provides more general insights into the characteristics of "hidden stars." That is, we investigate which characteristics are correlated with being underplaced before the availability of citation

[29]See Becker et al. (2023) for the emigration of scientists from Nazi Germany.

metrics. For this analysis, we define star scientists as scientists whose total citations (both visible and invisible) place them in the top five percent of the subject-level citation distribution in 1969. For these 450 scientists we can infer some characteristics from our data, e.g., whether they were female, but also whether they were of Asian, Hispanic, or Jewish origin. We measure these characteristics based on the names of academics (for more details, see Section 2.B.1). In addition, we collect information on where these star scientists obtained their Ph.D. through an extensive web search.[30]

We then report the average characteristics of star scientists in high-ranked departments and of star scientists who worked in low-ranked departments in 1956 ("hidden stars"). 38% of star scientists in high-ranked departments had received a Ph.D. from a top-10 department in the United States. In contrast, only 18% of "hidden stars" had received a Ph.D. from a top-10 department (Figure 2.11). We also find that there were twice as many women among "hidden stars." Since there were very few women in academia at the time (Iaria et al., 2022), the difference is not statistically significant. Overall, this evidence suggests that "hidden stars" had, on average, obtained their Ph.D. from worse universities and that they were more likely to be female.

Figure 2.11: Characteristics of "Hidden Stars" and Other Star Scientists



Notes: The figure reports characteristics of star scientists who were in high-ranked departments (blue) and low-ranked departments ("hidden stars," orange) in 1956. As before, low-ranked departments are those below the 75th percentile of the department ranking in 1956. For this figure, we define star scientists as all scientists in the top five percent of the subject-level citation distribution.

[30] We obtain the Ph.D. university for 400 out of the 450 star scientists.

2.4.3 Heterogeneous Effects for Minority Scientists

In the last part of this section, we investigate the heterogeneous impacts of citation metrics on minority scientists. Specifically, we analyze whether women, Hispanics, Asians, and Jews disproportionately benefited from the availability of citation metrics. As outlined above, we identify these groups based on the names of academics. As the proportion of minorities among academics was low in the 1960s (e.g., Card et al. (2023), Iaria et al. (2022)), we pool all minorities to gain power. We then estimate the following regression:

$$\begin{aligned}
Dep.\ Rank_i = &\sum_q \delta_q^M \cdot \mathbb{1}(Visible\, Cit\, Decile_i = q) \times Majority_i \\
&+ \sum_q \delta_q^m \cdot \mathbb{1}(Visible\, Cit\, Decile_i = q) \times Minority_i \\
&+ \sum_q \theta_q^M \cdot \mathbb{1}(Invisible\, Cit\, Decile_i = q) \times Majority_i \\
&+ \sum_q \theta_q^m \cdot \mathbb{1}(Invisible\, Cit\, Decile_i = q) \times Minority_i \\
&+ \omega \cdot Minority_i + \pi \cdot Publications_i + Subject\, FE + \epsilon_i
\end{aligned} \tag{2.7}$$

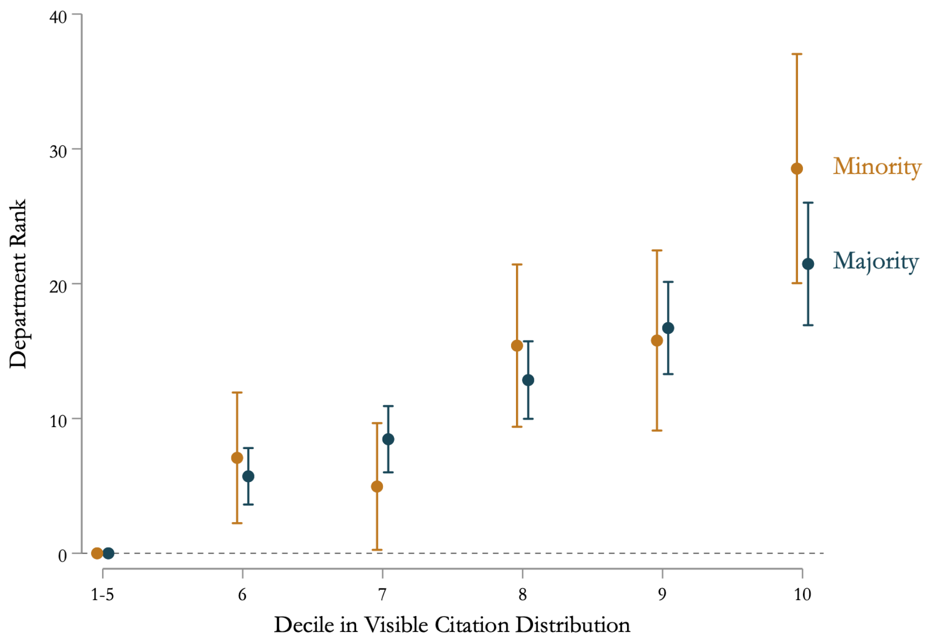
Variables are defined as before, but we add interactions with indicator variables that equal one if the scientist belonged either to the majority or to the minority. We also control for an indicator that equals one if the scientists belonged to a minority.

While we do not find evidence that minority scientists, on average, benefited more from citation metrics than majority scientists (Appendix Table 2.D.2), the evidence in Figure 2.12 suggests that among star scientists (top decile) minority scientists benefit slightly more than majority scientists.[31] The p-value for the test that the coefficients for the tenth decile are the same for minority and majority scientists is 0.051.

Taken together, these results suggest that the availability of more "objective" performance metrics helped disadvantaged high-quality scientists. In particular, highly cited scientists in lower-ranked departments ("hidden stars") and highly cited minority scientists benefited from the availability of citation metrics.

[31] The democratizing effect of citation metrics is driven by larger effects of citation metrics for women and Jews (see Figure 2.D.3). These results are robust to adding a control for the department rank of scientist i in 1956 (Appendix Figure 2.D.4).

Figure 2.12: Heterogenous Effects for Majority and Minority Scientists



Notes: The figure plots coefficients $\hat{\delta}_q^M$ (blue) and $\hat{\delta}_q^m$ (orange) and 95 percent confidence intervals from Equation (2.7).

2.5 Impact of Performance Metrics on Careers

As shown above, citation metrics increased assortative matching between scientists and departments. In the last part of the paper, we study whether scientists with more visible citations also accrued additional benefits. We investigate such benefits by studying the impact of citation metrics on promotions and receiving NSF grants. This analysis also speaks to whether citation metrics increased recognition by peers and the wider scientific community, suggesting Matthew effects (Merton, 1968). We estimate the following regressions:

Specification 1:

$$\begin{aligned}\mathbb{1}[CareerOutcome]_i = \delta \cdot Visible\,Citations_i + \theta \cdot Invisible\,Citations_i \\ + \pi \cdot Publications_i + Subject\,FE + \epsilon_i\end{aligned} \tag{2.8}$$

Specification 2:

$$\begin{aligned}\mathbb{1}[CareerOutcome]_i = &\ \delta_1 \cdot Visible\,Citations_i \\ &+ \delta_2 \cdot Pseudo\text{-}Visible\,Citations_i \\ &+ \theta_1 \cdot Invisible\,Citations\,(SCI\ years)_i \\ &+ \theta_2 \cdot Invisible\,Citations\,(non\text{-}SCI\ years)_i \\ &+ \pi \cdot Publications_i + Subject\,FE + \epsilon_i\end{aligned} \tag{2.9}$$

where $\mathbb{1}[CareerOutcome]_i$ is an indicator that equals one if the scientist was promoted or received an NSF grant. The remaining variable definitions are identical to Equations (2.1) and (2.2).

2.5.1 Effect on Promotions

We investigate if scientists who we observe as assistant or associate professors in 1956 were promoted to full professors by 1969. This allows us to directly study how the introduction of performance metrics influenced academic careers and peer recognition. We estimate Equations (2.8) and (2.9), where the dependent variable equals one if scientist i was promoted to full professor between 1956 and 1969.

We find that the visible citation rank has a significant positive impact on promotions (Table 2.7). The probability of promotion increased by 4.2 percentage points (or 6.0 percent relative to the mean) for scientists with a 10 percentile higher visible citation rank.[32] The estimates for invisible citations are close to zero and statistically insignificant. The estimates from Specification 2 confirm these findings (Table 2.7 and Figure 2.13, panel (a)).

The results indicate that departments indeed used citation metrics in promotion decisions. As full professor positions come with many advantages such as prestige, job security, and research funds, these findings suggest that citation metrics affected individual careers and the allocation of resources in the sciences.

[32]The effect of citation metrics on promotions is estimated within the set of academics who we observe in 1956 and who have not left academia by 1969. Since the probability of leaving academia decreases with visible citations (see Section 2.3.4), we likely estimate a lower-bound of the effect of citation metrics on promotions.

Table 2.7: Promotion to Full Professor

	Dependent Variable: Promotion to Full Professor				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Visible Citations	0.0042	0.0046	0.0047	0.0042	0.0041
	(0.0006)	(0.0007)	(0.0007)	(0.0010)	(0.0013)
Invisible Citations	0.0009	0.0003	0.0004	-0.0004	-0.0003
	(0.0005)	(0.0006)	(0.0006)	(0.0010)	(0.0012)
P-value (Visible = Invisible)	0.002	< 0.001	< 0.001	0.010	0.045
R^2	0.140	0.145	0.154	0.363	0.393
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	0.0043	0.0048	0.0048	0.0043	0.0042
	(0.0006)	(0.0006)	(0.0007)	(0.0010)	(0.0013)
Pseudo-Visible Citations	0.0000	-0.0004	-0.0003	-0.0004	-0.0001
	(0.0006)	(0.0006)	(0.0006)	(0.0011)	(0.0012)
Invisible Citations (SCI years)	0.0006	0.0005	0.0005	0.0005	0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0009)	(0.0011)
Invisible Citations (non-SCI years)	0.0003	0.0001	0.0002	-0.0008	-0.0011
	(0.0005)	(0.0005)	(0.0005)	(0.0009)	(0.0011)
P-value (Visible = Pseudo-Visible)	< 0.001	< 0.001	< 0.001	0.010	0.050
P-value (Visible = Invisible (SCI years))	< 0.001	< 0.001	< 0.001	0.010	0.051
P-value (Visible = Invisible (non-SCI years))	< 0.001	< 0.001	< 0.001	< 0.001	0.001
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.755	0.541	0.663	0.682	0.632
R^2	0.140	0.146	0.154	0.363	0.394
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	3,364	3,364	3,364	3,364	3,364
Dependent Variable Mean	0.707	0.707	0.707	0.707	0.707

Notes: The table reports the estimates of Equation (2.8) in the first panel and of Equation (2.9) in the second panel. The dependent variable is an indicator equal to one if scientist i was promoted to full professor between 1956 and 1969. These regressions use the sample of scientists observed in 1956 and 1969, who were not full professors in 1956. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

2.5.2 Effect on Research Grants

Finally, we investigate the effect of citation metrics on receiving research grants. This analysis examines whether citation metrics affect the allocation of resources and recognition by the wider scientific community. We collect data on all National Science Foundation (NSF) grants between 1964 and 1972 and match them to the scientists in our faculty rosters (see Section 2.B.1.3). We estimate Equations (2.8) and (2.9), where the dependent variable equals one if scientist i received at least one NSF grant.[33]

Table 2.8: Receiving an NSF Grant

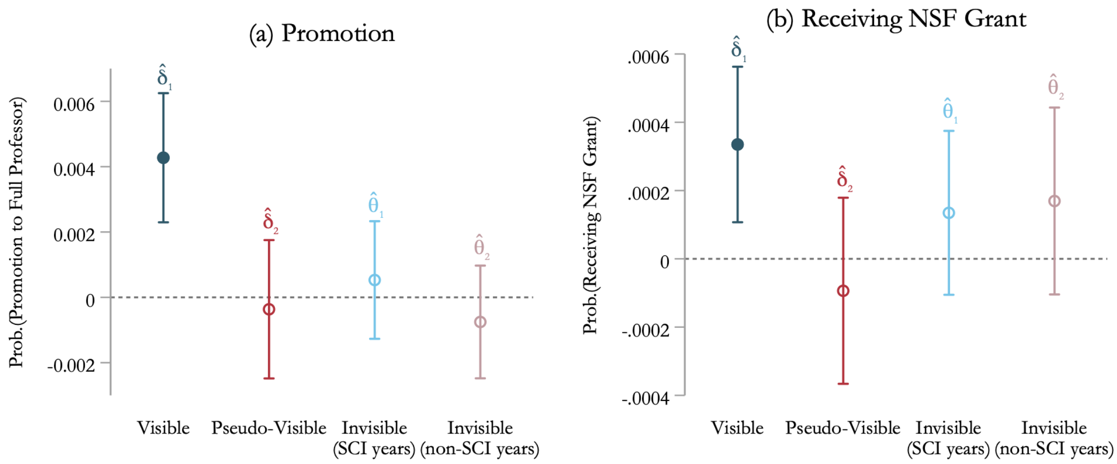
	Dependent Variable: Receiving NSF Grant				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Visible Citations	0.0007	0.0006	0.0005	0.0004	0.0004
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations	0.0001	0.0001	-0.0000	-0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Invisible)	0.003	0.014	0.022	0.061	0.066
R^2	0.026	0.030	0.042	0.168	0.203
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	0.0006	0.0005	0.0004	0.0003	0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pseudo-Visible Citations	-0.0001	-0.0001	-0.0002	-0.0001	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations (SCI years)	0.0002	0.0002	0.0002	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations (non-SCI years)	0.0003	0.0002	0.0002	0.0002	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Pseudo-Visible)	< 0.001	0.002	0.006	0.053	0.110
P-value (Visible = Invisible (SCI))	0.024	0.051	0.388	0.273	0.163
P-value (Visible = Invisible (non-SCI))	0.026	0.096	0.411	0.379	0.226
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.108	0.268	0.095	0.429	0.830
R^2	0.028	0.031	0.043	0.168	0.203
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	15,582	15,582	15,582	15,582	15,582
Dependent Variable Mean	0.023	0.023	0.023	0.023	0.023

Notes: The table reports the estimates of Equation (2.8) in the first panel and of Equation (2.9) in the second panel. The dependent variable is an indicator equal to one if scientist i received an NSF grant between 1961 and 1972. These regressions use the sample of scientists observed in 1969, excluding medicine. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

[33] We exclude medical scientists from this analysis because the NSF does not fund research in medicine. If we include medical researchers, the results are similar (see Appendix Table 2.E.1).

The visible citation rank has a significant positive impact on receiving NSF grants (Table 2.8). The probability of receiving a grant increased by 0.4 percentage points (or 18.2 percent relative to the mean) for scientists with a 10 percentile higher visible citation rank. The estimates for invisible citations are close to zero and statistically insignificant. The estimates from Specification 2 confirm these findings (Table 2.8 and Figure 2.13, panel (b)).

Figure 2.13: Effect on Career Outcomes, Specification 2



Notes: The figure plots coefficients and 95 percent confidence intervals from variants of Equation (2.9), see Tables 2.7 and 2.8, Specification 2.

These results highlight that the effects of citation metrics go beyond the allocation of talent: they affect whether scientists are promoted and whether they receive research grants. Thus, recognition through citations enables high-performing scientists to accrue additional rewards and resources, contributing to Matthew effects in the sciences (Merton, 1968).

2.6 Conclusion

The evaluation of scientists based on performance metrics, and in particular citations, has become ubiquitous in modern science. Scientists are highly aware of the number of citations their papers have received, and standard metrics like the impact factor or the h-index are not only used to evaluate scientists and papers but also influence hiring and promotion decisions. Equally, departments and scientific journals are frequently ranked based on citation measures. This widespread reliance on citation metrics has been criticized, as citations only capture one dimension of an academic's contribution to knowledge (CoARA, 2024; DORA, 2024). Despite these concerns, little is known about the consequences of measuring citations for scientific careers, and the allocation of talent and resources.

In this paper, we use the introduction of the *Science Citation Index* to provide the first causal estimates of how citation metrics affect the organization of science. We collect new data and develop a new identification strategy to show that systematically measuring and revealing citations had a large and immediate impact on the careers of scientists. First, we show that the introduction of citation metrics increased assortative matching between scientists and departments based on citations by reducing information frictions. Second, we show that the effect was particularly pronounced for scientists in the top end of the citation distribution, and especially for "hidden stars" (highly cited scientists in lower-ranked departments), as well as for highly cited minority scientists. Finally, we show that measuring citations increased the reliance on citation metrics in promotion decisions and in allocating research grants. Overall, our findings demonstrate that citation metrics have a profound impact on the organization of modern science.

Appendix to Chapter 2

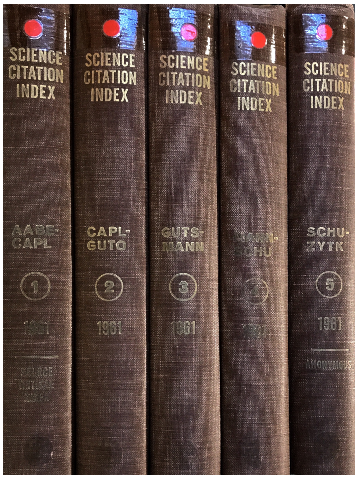
This appendix presents details on data collection and additional results:

- Section 2.A provides further background on the Science Citation Index.
- Section 2.B provides details on data collection.
- Section 2.C reports robustness checks and additional findings on the analysis of assortative matching in Section 2.3.
- Section 2.D reports additional findings on the heterogeneity analysis in Section 2.4.
- Section 2.E reports additional findings on the analysis of career outcomes in Section 2.5.

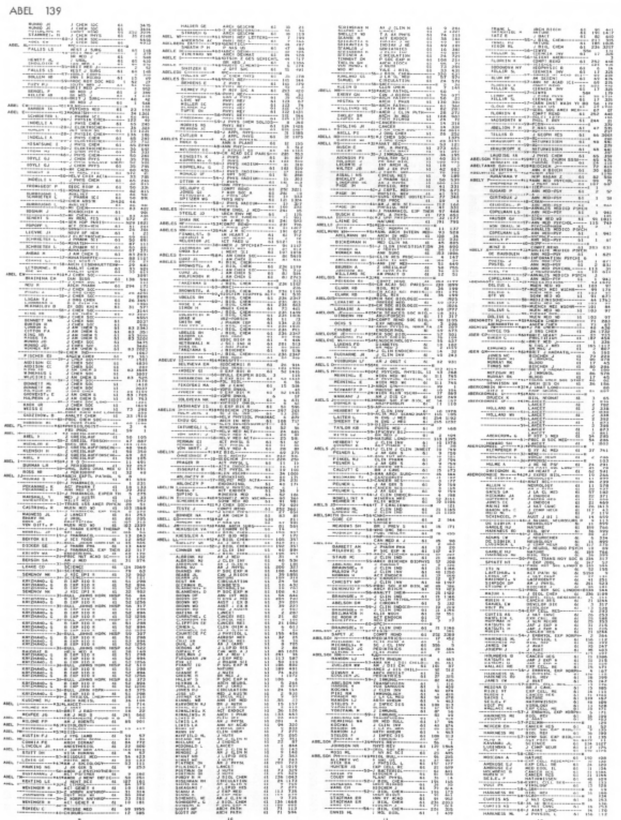
2.A Background on the Science Citation Index

Figure 2.A.1: Entry in Science Citation Index

(a) The 1961 SCI volume



(b) A page in the 1961 SCI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Notes: Panel (a) shows the five books of the 1961 SCI. Panel (b) shows a sample page in the 1961 volume of the SCI.

Figure 2.A.2: Example of Citing Journal List

Science Citation Index - 1961
Source Journals
Arranged by Full Title

ACTA ALLERGOLOGICA	ACT ALLERG
ACTA ANAESTHESIOLOGICA SCANDINAVICA	ACT ANAE SC
ACTA ANATOMICA	ACT ANATOM
ACTA BIOCHIMICA POLONICA	ACT BIOCH P
ACTA BIOLOGICA ACADEMIAE SCIENTIARUM HUNGARICAE	ACT BIOL H
ACTA BIOLOGICA ET MEDICA GERMANICA	ACT BIO MED
ACTA CHEMICA SCANDINAVICA	ACT CHEM SC
ACTA CHIMICA ACADEMIAE SCIENTIARUM HUNGARICAE	ACT CHIM H
ACTA CHIRURGICA ACADEMIAE SCIENTIARUM HUNGARICAE	ACT CHIR H
ACTA CIENTIFICA VENEZOLANA	ACT CIENT V
ACTA CRYSTALLOGRAPHICA	ACT CRYST
ACTA CYTOLOGICA	ACT CYTOL
ACTA DERMATO-VENEREOLOGICA	ACT DER-VEN
ACTA ENDOCRINOLOGICA	ACT ENDOCR
ACTA ENDOCRINOLOGICA SUPPLEMENTUM	ACT ENDOCR
ACTA GENETICA ET STATISTICA MEDICA	ACT GENET S
ACTA GENETICAE MEDICAE ET GEMELLOLOGIAE	ACT GENET M
ACTA HAEMATOLOGICA	ACT HAEMAT
ACTA HEPATO-SPLENOLOGICA	ACT HEP-SPL
ACTA HISTOCHEMICA	ACT HISTOCH
ACTA MEDICA ACADEMIAE SCIENTIARUM HUNGARICAE	ACT MED H
AGRICULTURAL AND BIOLOGICAL CHEMISTRY	AGR BIOL CH
AGRONOMY JOURNAL	AGRON J
AMERICAN DOCUMENTATION	AM DOCUMENT
AMERICAN HEART JOURNAL	AM HEART J
AMERICAN JOURNAL OF ANATOMY	AM J ANAT
AMERICAN JOURNAL OF BOTANY	AM J BOTANY
AMERICAN JOURNAL OF CARDIOLOGY	AM J CARD
AMERICAN JOURNAL OF CLINICAL NUTRITION	AM J CLIN N
AMERICAN JOURNAL OF CLINICAL PATHOLOGY	AM J CLIN P
AMERICAN JOURNAL OF DIGESTIVE DISEASES	AM J DIG DI
AMERICAN JOURNAL OF DISEASES OF CHILDREN	AM J DIS CH
AMERICAN JOURNAL OF GASTROENTEROLOGY	AM J GASTRO
AMERICAN JOURNAL OF HUMAN GENETICS	AM J HU GEN
AMERICAN JOURNAL OF HYGIENE	AM J HYG
AMERICAN JOURNAL OF MATHEMATICS	AM J MATH
AMERICAN JOURNAL OF MEDICINE	AM J MED
AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY	AM J OBST G
AMERICAN JOURNAL OF OPHTHALMOLOGY	AM J OPHTH
AMERICAN JOURNAL OF ORTHODONTICS	AM J ORTHOD
AMERICAN JOURNAL OF PATHOLOGY	AM J PATH
AMERICAN JOURNAL OF PHARMACEUTICAL EDUCATION	AM J PHA ED

Notes: This figure shows the first page of the "Source Journal List" of the 1961 SCI (Garfield, 1963b). This is a complete list of all 613 citing journals, from which citations were indexed for the 1961 SCI. We construct visible citations based on this list and the analogous lists from the 1964 to 1969 SCIs (see Section 2.2.2).

Figure 2.A.3: Internal Correspondence at Ohio State University

THE OHIO STATE UNIVERSITY LIBRARIES
Chemistry Library
COLUMBUS 10, OHIO

LEWIS C. BRANSCOMB
Director of Libraries

September 21, 1965

Professor Hyman W. Kritzer
Assistant Director, Public Services
Main Library, 1858 Neil Avenue
Campus

Dear Professor Kritzer:

I would like to place a subscription for the Science Citation Index for Chemistry Library. In view of the scarcity of funds for periodical subscriptions, I am sending the order to you for your approval and assistance.

A poll of some of the faculty in the Chemistry Department revealed that, without exception, all the younger men would like to have SCI available for use in Chemistry Library. The senior faculty are not as emphatic but would like to have it available also.

Since SCI definitely complements the Chemical Abstracts approach to the literature and since placing it in Chemistry Library would mean fairly close access for other science departments in the area, I think special consideration should be given to our request for a subscription.

Sincerely,

Virginia E. Yagello

Virginia E. Yagello
Chemistry Librarian

VEY:kk

Enclosures: Correspondence to Chemistry faculty with comments attached.

Notes: In this letter, the chemistry librarian at Ohio State University requested a second copy of the SCI to be placed in the library of the chemistry department, in addition to the existing copy at the medical library. It shows that as early as 1965 there was large demand by chemists at Ohio State University to use the SCI. We thank archivists at Ohio State University Library for sharing this document.

2.B Further Details on Data

2.B.1 Data on Scientists

2.B.1.1 Linking Faculty Rosters with Publication and Citation Data

As described in the main text, we link scientists with their publications and citations using the linking algorithm developed in Iaria et al. (2022). The links are based on the academic's surname, first name or initials (depending on whether first names are available), country, city, and subject. The matching is based on the primary subject of each academic (e.g., physics) to reduce the number of false positives. To harmonize affiliations across the faculty rosters and the *Web of Science*, we rely on *Google Maps API*.

2.B.1.2 Coding Minority Status

In Section 2.4, we report results on the heterogeneous effect of citation metrics. In particular, in Section 2.4.3, we report differential results for women and for people with Asian, Hispanic, and Jewish names.

We use information in the faculty rosters to tag scientists as members of one of these groups. Gender coding relies on information on gender that can be directly observed in the faculty rosters (e.g., Miss in front of the first name) and the first names of scientists (see Iaria et al. (2022)).

We code Jewish names based on the approach in Benetti et al. (2023). Using their classification of Jewish names results in an overly conservative classification of Jewish scientists. We therefore lower the cut-off for classifying names as distinctively Jewish to 5 (instead of 10). However, results remain similar when using the cut-off used in Benetti et al. (2023).

The coding of Hispanic names is based on data from the U.S. Census. We draw a list of Hispanic names from Name Census (2023b). From this list, we select all surnames with a conditional probability of self-identifying as Hispanic of more than 25%. We then tag all academics who have one of these names as Hispanic.

Similarly, we use data from the U.S. Census to code Asian names. We draw a list of the most common Asian names from Name Census (2023a). From this list, we select all surnames with a conditional probability of self-identifying as Asian or Pacific Islander of more than 50%.[34] We then tag all academics who have one of these names as Asian.

2.B.1.3 Data on NSF Grants

For the analysis in Section 2.5.2, we match scientists in our faculty rosters with historical records on grants by the National Science Foundation (NSF). We retrieve all NSF grants between 1964 (the first year after the SCI was introduced) and 1972 (the year before additional volumes of the SCI were published for the years 1962 and 1963, making many previously invisible citations visible to the scientific community) from the National Science Foundation (2023). To reduce false positives, we assign each grant a subject based on its title, using a multinomial logit classifier that was pre-trained on the *Web of Science* (see Iaria et al. (2022)). We then match principal investigators from the grants to the scientists in our data based on first names and last names and subject.

2.B.2 Department Rankings

The following six tables list the top 20 departments according to our self-constructed rankings (by average citations and by average publications in a department) and according to survey-based rankings from the 1960s and 1970s. Across all rankings similar departments are ranked among the top 20 departments.

[34] The different cutoffs for Asian and Hispanic names reflect different assimilation patterns of the various immigrant groups. Results are similar if we impose the same cutoffs for both groups.

Table 2.B.1: Top 20 Departments: Biochemistry

Rank	Citations Ranking	Publications Ranking	Cartter Ranking	Roose-Andersen Ranking
1	Stanford	Washington	Harvard	Harvard
2	Rockefeller	Harvard	U.C. Berkeley	Stanford [2]
3	Johns Hopkins	Stanford	Stanford	U.C. Berkeley [2]
4	Washington	U.C. Berkeley	Rockefeller	Rockefeller
5	Harvard	Dartmouth	Wisconsin	Wisconsin
6	Kentucky	Wisconsin	M.I.T.	Cal. Tech.
7	U.C. Berkeley	Michigan	Cal. Tech.	M.I.T.
8	Dartmouth	Kentucky	Johns Hopkins	Brandeis [8]
9	Wisconsin	Johns Hopkins	Brandeis	Cornell [8]
10	Michigan	Virginia Polytechnic Institute	Illinois	Johns Hopkins [8]
11	U.C. Davis	U.C. Davis	Columbia	Duke [11]
12	Brandeis	Kansas [12]	Case Western Reserve	U.C.L.A. [11]
13	Case Western Reserve	Saint Louis [12]	N.Y.U.	U.C. San Diego [13]
14	Utah	Rockefeller	Washington	Washington [13]
15	Duke	Duke	Duke	Yeshiva University [13]
16	U.C.L.A.	U.C.L.A.	Michigan	Chicago [16]
17	Columbia	Columbia	Pennsylvania [17]	Illinois [16]
18	Pennsylvania	Case Western Reserve	Yeshiva University [17]	Princeton [16]
19	Chicago	Rice	Chicago	Case Western Reserve [19]
20	Rochester	Brandeis	U.C.L.A.	N.Y.U. [19]

Notes: This table lists the top 20 biochemistry departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cartter (1966). The fourth column reports the ranking from Roose and Andersen (1970). Where departments are ranked equally (in any of the four rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

Table 2.B.2: Top 20 Departments: Biology

Rank	Citations Ranking	Publications Ranking	Cartter Ranking	Roose-Andersen Ranking
1	Rockefeller	Albion College	U.C. Berkeley	Harvard
2	Albion College	Millikin	Harvard	U.C. Berkeley
3	Harvard	Texas	Cal. Tech.	M.I.T.
4	Princeton	Georgetown College	Johns Hopkins	Cal. Tech.
5	U.C. San Diego	Rockefeller [5]	Rockefeller	Rockefeller
6	Stanford	U.C. San Diego [5]	Wisconsin	Wisconsin
7	Cal. Tech.	U.C. Riverside	Illinois	Stanford
8	Texas	Wisconsin	Michigan	Washington
9	U.C. Berkeley	U.C. Berkeley	Stanford	U.C. San Diego [9]
10	Syracuse	Stanford	Minnesota	Yale [9]
11	Brandeis	U.C. Davis	Indiana [11]	Chicago
12	Yale	Brandeis	Princeton [11]	Illinois
13	Chicago	Princeton	Cornell	Cornell
14	M.I.T.	Notre Dame	Yale	U.C. Davis
15	U.C. Santa Barbara	Whitman College	Purdue [15]	Michigan
16	Notre Dame	Mount Holyoke College	U.C.L.A. [15]	Duke
17	Johns Hopkins	Alma College	Case Western Reserve	U.C.L.A.
18	Whitman College	U.C. Santa Barbara	Washington	Johns Hopkins
19	Washington	Central College Pella [19]	Chicago	Brandeis
20	U.C. Davis	Harvard [19]	Pennsylvania	Indiana

Notes: This table lists the top 20 biology departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cartter (1966). While the Cartter ranking does not report rankings for biology overall, it does report rankings for five subfields of biology (Bacteriology/Microbiology, Botany, Entomology, Physiology, and Zoology). Based on these rankings, we construct an overall score for biology by taking the average rank of a department in the five reported subfields of biology. The fourth column reports the ranking from Roose and Andersen (1970). While the Roose-Andersen ranking does not report results for biology overall, it does report rankings for eight subfields of biology (Botany, Developmental Biology, Entomology, Microbiology, Molecular Biology, Physiology, Population Biology, and Zoology). Based on these rankings, we construct an overall score for biology by taking the average rank of a department in the eight reported subfields of biology. Where departments are ranked equally (in any of the four rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

Table 2.B.3: Top 20 Departments: Chemistry

Rank	Citations Ranking	Publications Ranking	Cartter Ranking	Roose-Andersen Ranking
1	U.C. Irvine	U.C. Santa Barbara	Harvard	Harvard
2	Stanford	Thiel College	Cal. Tech.	Cal. Tech.
3	Harvard	Stanford	U.C. Berkeley	Stanford [3]
4	U.C. Santa Barbara	U.C. Riverside	M.I.T.	U.C. Berkeley [3]
5	U.C.L.A.	U.C. Irvine	Stanford	M.I.T.
6	U.C. Riverside	Southern California	Illinois	Illinois
7	Cal. Tech.	College of Forestry at Syracuse	Columbia [7]	U.C.L.A.
8	Northwestern	Iowa State	Wisconsin [7]	Chicago [8]
9	Southern California	Utah	U.C.L.A.	Columbia [8]
10	College of Forestry at Syracuse	U.C. Davis	Chicago	Cornell [8]
11	Thiel College	Northwestern	Cornell	Wisconsin [8]
12	U.C. Berkeley	Texas	Yale	Yale
13	Iowa State	U.C.L.A.	Princeton	Princeton
14	Rice	Case Western Reserve	Northwestern	Northwestern
15	Illinois	Pennsylvania	Minnesota	Iowa State [15]
16	Utah	Illinois	Iowa State	Purdue [15]
17	Notre Dame	Johns Hopkins	Ohio State [17]	Ohio State [17]
18	U.C. Santa Cruz	Iowa State	Purdue [17]	Texas [17]
19	Columbia	Michigan	Michigan	U.C. San Diego [17]
20	Texas	Harvard	Indiana	Indiana

Notes: This table lists the top 20 chemistry departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cartter (1966). The fourth column reports the ranking from Roose and Andersen (1970). Where departments are ranked equally (in any of the four rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

Table 2.B.4: Top 20 Departments: Mathematics

Rank	Citations Ranking	Publications Ranking	Cartter Ranking	Roose-Andersen Ranking
1	Princeton	U.C. Santa Barbara	Harvard	Harvard [1]
2	Chicago	U.C. Riverside	U.C. Berkeley	U.C. Berkeley [1]
3	Stanford	Harvard	Princeton	Princeton
4	Institute for Advanced Study	Princeton	Chicago	Chicago
5	Harvard	Carnegie-Mellon	M.I.T.	M.I.T.
6	Columbia	Washington	Stanford	Stanford
7	Johns Hopkins	Chicago	Yale	Yale
8	Brandeis	Johns Hopkins	N.Y.U.	N.Y.U.
9	U.C. Berkeley	Rockefeller	Columbia	Wisconsin
10	Virginia Polytechnic Institute	Stanford	Wisconsin	Columbia [10]
11	Rockefeller	Washington Saint Louis	Michigan	Michigan [10]
12	U.C. San Diego	Columbia	Illinois	Cornell [12]
13	Washington	Virginia	Cornell	Illinois [12]
14	Carnegie-Mellon	U.C. San Diego	Cal. Tech.	U.C.L.A.
15	Wisconsin	Wisconsin	Minnesota	Brandeis [15]
16	Yale	Brandeis	U.C.L.A.	Brown [15]
17	Washington Saint Louis	Yale	Washington	Cal. Tech. [15]
18	Case Institute of Technology	Institute for Advanced Study	Brown	Minnesota [18]
19	Brown	Minnesota	Brandeis	Pennsylvania [18]
20	Cornell	Michigan	Johns Hopkins	Washington [18]

Notes: This table lists the top 20 mathematics departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cartter (1966). The fourth column reports the ranking from Roose and Andersen (1970). Where departments are ranked equally (in any of the four rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

Table 2.B.5: Top 20 Departments: Medicine

Rank	Citations Ranking	Publications Ranking	Cole-Lipton Ranking
1	Rockefeller	New Mexico	Harvard
2	Harvard	Minnesota Rochester	Johns Hopkins [2]
3	Utah	Rutgers	Stanford [2]
4	U.C. San Diego	U.C. San Diego	U.C. San Francisco
5	Minnesota Rochester	Harvard	Yale
6	Rutgers	Amherst College	Columbia
7	Washington	Loretto Heights College	Duke
8	M.I.T.	Medical College of Virginia	Michigan
9	Texas	M.I.T.	Cornell
10	U.C. San Francisco	Washington	Washington Saint Luis
11	Johns Hopkins	U.C.L.A.	Pennsylvania
12	Minnesota	Johns Hopkins	Minnesota
13	U.C.L.A.	Utah	U.C.L.A.
14	Florida	Minnesota	Albert Einstein College
15	New Mexico	Florida [15]	Chicago Pritzker [15]
16	Kansas	Rockefeller [15]	Washington [15]
17	Medical College of Virginia	U.C. San Francisco	Case Western Reserve
18	Washington Saint Louis	Southern California	Rochester
19	Stanford	Mississippi	Colorado
20	Columbia	Wagner College	U.C. San Diego

Notes: This table lists the top 20 biochemistry departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cole and Lipton (1977). Since Cartter (1966) and Roose and Andersen (1970) do not report rankings for medical schools, we use the ranking by Cole and Lipton (1977) for medicine. Where departments are ranked equally (in any of the three rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

Table 2.B.6: Top 20 Departments: Physics

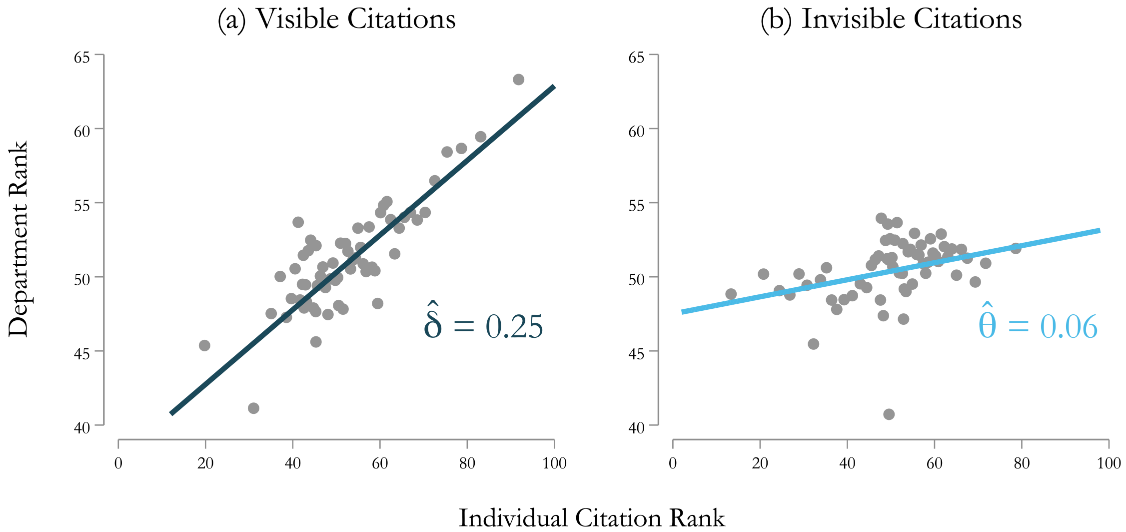
Rank	Citations Ranking	Publications Ranking	Cartter Ranking	Roose-Andersen Ranking
1	U.C. San Diego	U.C. Riverside	U.C. Berkeley	Cal. Tech. [1]
2	U.C. Riverside	U.C. San Diego	Cal. Tech.	Harvard [1]
3	U.C. Berkeley	Lycoming College	Harvard	U.C. Berkeley [1]
4	Chicago	U.C. Santa Barbara	Princeton	Princeton
5	Rockefeller	Kentucky Wesleyan College	Stanford	M.I.T. [5]
6	Stanford	Goshen College	M.I.T.	Stanford [5]
7	Princeton	Chicago	Columbia	Columbia [7]
8	Columbia	Harvard	Illinois	Illinois [7]
9	U.C. Santa Barbara	Rockefeller	Cornell	Chicago [9]
10	Harvard	U.C. Irvine	Chicago	Cornell [9]
11	Pennsylvania	Columbia	Yale	U.C. San Diego [11]
12	U.C. Irvine	Stanford	Wisconsin	Yale [11]
13	Brown	Princeton	Michigan [13]	Wisconsin
14	Carnegie-Mellon	Pennsylvania	Rochester [13]	Michigan [14]
15	Cal. Tech.	Pittsburgh	Pennsylvania	Pennsylvania [14]
16	Pittsburgh	Brown	Maryland	Maryland [16]
17	State University of New York	U.C. Berkeley	Minnesota	Rockefeller [16]
18	Washington	Iowa State	Washington	Rochester
19	Illinois	Washington	Johns Hopkins [19]	U.C.L.A.
20	Johns Hopkins	Notre Dame	U.C.L.A. [19]	Minnesota

Notes: This table lists the top 20 physics departments based on four different department rankings. The first column reports our self-constructed ranking based on the average number of citations (between 1956 and 1969, to publications between 1956 and 1969) of all scientists employed at the department in 1969. The second column reports our self-constructed ranking based on the average number of publications (between 1956 and 1969) of all scientists employed at the department in 1969. The third column reports the ranking from Cartter (1966). The fourth column reports the ranking from Roose and Andersen (1970). Where departments are ranked equally (in any of the four rankings), a superscript indicates their rank. In the analysis, they are given the same rank.

2.C Additional Findings: Assortative Matching

2.C.1 Graphical Representation of Specification 1

Figure 2.C.1: Illustration of Results, Specification 1



Notes: The figure illustrates the results from Equation (2.1), see Table 2.3, Specification 1. Panel (a) shows a bin-scatter plot with the visible citation percentile rank on the horizontal axis and the department rank on the vertical axis, conditional on invisible citations and publication controls. Panel (b) shows a binned scatter plot with the invisible citation percentile rank on the horizontal axis and the department rank on the vertical axis, conditional on visible citations and publication controls. The slopes are significantly different from each other; the p-value from a t-test of no difference is < 0.001.

2.C.2 Robustness Checks

In this section, we show that the main results are robust to various changes to the analysis. First, in Section 2.C.2.1, we show that results are similar for alternative measures of the department rank. Second, in Section 2.C.2.2, we show results are similar for alternative performance measures of individual scientists. Third, in Section 2.C.2.3, we show that the results are robust to different ways of assigning percentile ranks to scientists and departments. Last, in Section 2.C.2.4, we show that the results hold in different subsamples. To reduce the number of tables, we report all robustness checks using the specification equivalent to column (3) in Table 2.3, Specification 1. The results are similar across specifications using alternative control variables, corresponding to columns (1), (2), (4), and (5) in Table 2.3.

2.C.2.1 Alternative Department Rankings

First, we consider alternative department rankings. The main results (Table 2.3) are estimated with department ranks based on the leave-out mean of citations as the dependent variable. The results are robust to using rankings based on the mean of citations, i.e., including citations of the focal scientist (Table 2.C.1, Panel A, column (2)). Instead of using department rankings based on citations, we can use scientists' publication counts to construct department rankings. This leaves the results almost unchanged (Table 2.C.1, Panel A, columns (3) and (4)).

Our results also hold if we construct department rankings based on the scientific output of departments in the 1956 cross-section (Table 2.C.1, Panel B). While 1956 rankings have the advantage that they are determined before the introduction of the SCI, they are not available for universities that only enter the data after 1956. Moreover, the 1956 rankings may suffer from higher measurement error, because we measure department composition before hiring and moving decisions were actually made. Ranking departments on the basis of 1956 rankings results in a 25 percent smaller sample. Nevertheless, the results remain qualitatively unchanged.

Our results are also robust to using external department rankings, which do not rely on citation or publication data. We draw on subject-specific reputational rankings from Roose and Andersen (1970) and Cartter (1966) to construct analogous department percentile ranks. To avoid unnecessary sample selection for this robustness check, departments that are not listed in these rankings are assigned the percentile rank

Table 2.C.1: Robustness Check: Alternative Measures of Department Quality

	Dependent Variable: Department Rank			
	(1)	(2)	(3)	(4)
Department Ranking Based on:	Leave-Out Mean of Citations	Mean of Citations	Leave-Out Mean of Publications	Mean of Publications
Panel A: Department Rankings From 1969				
Visible Citations	0.280	0.320	0.286	0.318
	(0.035)	(0.030)	(0.034)	(0.028)
Invisible Citations	0.062	0.078	0.047	0.053
	(0.021)	(0.020)	(0.020)	(0.019)
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001
Observations	27,315	27,315	27,315	27,315
R^2	0.153	0.207	0.150	0.210
Dependent Variable Mean	50.40	50.20	50.37	50.16
Panel B: Department Rankings From 1956				
Visible Citations	0.169	0.178	0.158	0.175
	(0.038)	(0.039)	(0.037)	(0.039)
Invisible Citations	0.027	0.028	0.006	0.009
	(0.026)	(0.027)	(0.026)	(0.027)
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001
Observations	21,269	21,269	21,269	21,269
R^2	0.066	0.066	0.061	0.063
Dependent Variable Mean	50.29	55.59	50.26	56.27
Subject Fixed Effects	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes

Notes: The table reports the estimates of Equation (2.1) with alternative department rankings as dependent variables. In Panel A, department rankings are based on the 1969 cross-section of scientists; in Panel B, they are based on the 1956 cross-section. For departments that did not exist in 1956, the 1956 ranking cannot be computed. This results in a smaller sample size in Panel B. In column (1), the dependent variable is the department rank, based on the leave-out mean of citations in the department of scientist i (as in Table 2.3). In column (2), the department rank is based on the mean of citations in the department. In column (3), the department rank is based on the leave-out mean of publications in the department. In column (4), the department rank is based on the mean of publications in the department. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

between 1 and the lowest-ranked department.[35] As these rankings do not cover medical schools, we supplement these rankings with the first comprehensive ranking of medical schools by Cole and Lipton (1977). We report the results of these tests in Table 2.C.2, column (4). The estimates show that our results are similar if we use independently compiled reputation-based rankings.

Instead of percentile ranks, we can also use the reputational rankings from Cartter (1966) and Roose and Andersen (1970) to construct indicators for being in a top-ranked department. According to both rankings, we assign each scientist an indicator for whether they worked in a top-five, top-ten, or top-twenty department. In line with our main results, a scientist with a higher visible citation rank was more likely to work in a top department in 1969. For example, a ten-percentile increase in visible citations increased the probability of being affiliated with a top-twenty department by 2.94 percentage points (i.e., a 13.5 percent increase). In contrast, invisible citations had a much smaller effect on the assortativeness of the match to a top department (Table 2.C.2, columns (1)-(3)).

[35] This is necessary because these external rankings cover fewer departments than our data. Furthermore, Roose and Andersen (1970) and Cartter (1966) do not contain rankings for biology as a whole but for specific subfields of biology (Botany, Developmental Biology, Entomology, Microbiology, Molecular Biology, Physiology, Population Biology, and Zoology in the Roose-Andersen ranking; Botany, Entomology, Microbiology, Physiology, and Zoology in the Cartter ranking). For both the Roose-Andersen ranking and the Cartter ranking, we construct an overall ranking for biology by calculating the average rank of a department in the subfields of biology.

Table 2.C.2: Robustness Check: External Department Ranking

	Dependent Variable: Indicator			*Dep. Rank*
	(1) Top 5	(2) Top 10	(3) Top 20	(4)
Panel A: Cartter Ranking				
Visible Citations	0.00077	0.00156	0.00294	0.224
	(0.00037)	(0.00039)	(0.00044)	(0.031)
Invisible Citations	0.00023	0.00059	0.00083	0.046
	(0.00018)	(0.00025)	(0.00032)	(0.022)
P-value (Visible = Invisible)	0.282	0.066	0.001	< 0.001
Observations	27,315	27,315	27,315	27,315
R^2	0.050	0.061	0.097	0.104
Dependent Variable Mean	0.04	0.12	0.22	50.15
Panel B: Roose-Andersen Ranking				
Visible Citations	0.00084	0.00166	0.00282	0.249
	(0.00037)	(0.00040)	(0.00043)	(0.032)
Invisible Citations	0.00025	0.00067	0.00096	0.039
	(0.00019)	(0.00025)	(0.00032)	(0.022)
P-value (Visible = Invisible)	0.234	0.061	0.004	< 0.001
Observations	27,315	27,315	27,315	27,315
R^2	0.053	0.065	0.099	0.116
Dependent Variable Mean	0.05	0.12	0.22	50.15
Subject Fixed Effects	Yes	Yes	Yes	Yes
Publications by Year $\times$ Subject	Yes	Yes	Yes	Yes

Notes: The table reports the estimates of Equation (2.1), where the dependent variable is based on the reputation-based department rankings by Cartter (1966) and Roose and Andersen (1970). Since these rankings do not cover medical schools, for medicine we supplement them with the ranking of medical schools by Cole and Lipton (1977). In columns (1)-(3), the dependent variable is an indicator for whether scientist i was employed at a top-5, top-10, or top-20 department. In column (4), the dependent variable is the rank of scientist i's department. To avoid unnecessary sample selection, we assign departments that are not listed in these rankings to the average rank between 1 and the lowest-ranked department. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

2.C.2.2 Alternative Transformations of Individual Citation Counts

We also show that results are robust to using alternative ways of measuring the performance of individual scientists.

For the main results, we count citations independently of the number of co-authors on the cited papers. In Table 2.C.3, column (2), we report results of Specification 1, where citations to each paper are divided by the number of authors of the paper. The results are very similar.

Another concern could be that the results are driven by differences in the distributions of visible and invisible citations. Larger measurement error for invisible citations could potentially explain the smaller and insignificant coefficient for invisible citations. We address this concern with a robustness check in which we only use citations from 1956 to 1965 to construct visible and invisible citation ranks. This leads to similar distributions of visible and invisible citations.[36] For these alternative variables, measurement error concerns would, if anything, disproportionately downward bias the coefficient on visible citations. Using these alternative individual citation ranks leaves our results qualitatively unchanged (Table 2.C.3, column (3)).

A further concern is that one "superstar" paper may place a scientist at the top of the citation distribution. However, having many moderately cited papers might be a better signal of quality than having very few highly cited papers. To account for both the number of cited papers and for the citations they receive, we use the h-index (e.g., Hirsch, 2005; Ellison, 2013) as an alternative performance metric. A scientist has an h-index of h if h of their papers have at least h citations each. We calculate the h-index of visible and invisible citations for each scientist. We then transform the h-index into the percentile rank for two reasons: first, this makes the coefficient directly comparable to the main results. Second, different scientific subjects have different publication and citation patterns. An h-index of three (i.e., having at least three publications with at least three citations) therefore indicates very different quality percentiles in each subject. For example, in medicine, a subject where scientists publish many papers and receive many citations, an h-index of three indicates poorer performance than in mathematics, a subject where scientists publish relatively few papers and receive a lot fewer citations. When we use percentiles of the visible and invisible h-indices as the explanatory variable, we confirm our main results (Table 2.C.3, column (4)).

[36]For citations measured in 1956-1965 the summary statistics are as follows. Visible citations: mean 14.3, standard deviation 41.4; invisible citations: mean 17.3, standard deviation 52.1.

We also show that the results are similar if we standardize visible and invisible citations at the subject level (Table 2.C.3, column (5)). As standardized citations contain large outliers, we show that the results are also robust to winsorizing citation counts at the 99th percentile and then standardizing citation counts (Table 2.C.3, column (6)). Further, the results are also similar if we use the inverse hyperbolic sine transformation of citations (Table 2.C.3, column (7)).

Table 2.C.3: Robustness Check: Alternative Transformations of Citation Counts

	Dependent Variable: Department Rank						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable Transformation:	Main Specification	Co-Author Weighted Citations	Only 1956-65 Citations	H-Index	Standard-ized	Winsorized & Std.	Inverse Hyperbolic Sine
Visible Citations	0.280	0.288	0.208	0.278	2.484	4.631	3.294
	(0.035)	(0.034)	(0.029)	(0.033)	(0.693)	(0.543)	(0.567)
Invisible Citations	0.062	0.062	0.117	0.074	0.367	1.461	1.268
	(0.021)	(0.022)	(0.025)	(0.021)	(0.545)	(0.416)	(0.309)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	0.007	< 0.001	0.063	< 0.001	0.002
Observations	27,315	27,315	27,315	27,315	27,315	27,315	27,315
R^2	0.153	0.157	0.143	0.150	0.105	0.116	0.149
Dependent Variable Mean	50.40	50.40	50.40	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.1) for alternative transformations of visible and invisible citations. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. In column (1), the explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. In column (2), citation counts are divided by the number of authors of a paper and then transformed as in column (1). In column (3), citation counts are based only on citations from 1956-1965 (instead of 1956-1969), and then transformed as in column (1). In column (4), the explanatory variables are scientist i's h-index values based on visible and invisible citations, which are then transformed into the percentile rank. In column (5), we standardize citations by subject. In column (6), we standardize citations by subject, but to reduce the weight of outliers, we winsorize citation counts at the 99th percentile before standardizing them. In column (7), we transform citations using the inverse hyperbolic sine. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

2.C.2.3 Scientists and Departments with Zero Citations

When more than one percent of scientists have zero citations, a unique percentile rank cannot be assigned to these scientists. For example, in physics, 30.37% of observations have zero citations. For these scientists, there is no unique percentile in the distribution of citations. In our main analysis, we assign the mid-point between the 1st and the 31st percentile, i.e., a percentile rank of 15.5, to each of these observations. Alternatively, we can assign all of these observations to the 1st percentile (Min.-Point in Table 2.C.4) or to the 31st percentile (Max.-Point). Reassuringly, the exact construction of percentile ranks of scientists with zero citations has no qualitative impact on the findings (Table 2.C.4, columns (2) and (3)). A similar issue can occur for scientists with very low citation counts, e.g., one citation. We treat them accordingly.

Another way of assigning the percentile rank to scientists with zero citations is to spread the specific percentile rank randomly within the group of scientists with zero citations. In the above example of physicists with zero citations, this means that each of these scientists' percentile rank is independently drawn from a uniform distribution from 1 to 31. The results using this alternative transformation are similar to the main results column (4).

Table 2.C.4: Robustness Check: Alternative Percentile Rank Definititions

	Dependent Variable: Department Rank			
	(1)	(2)	(3)	(4)
Variable Transformation:	Mid-Point (Main Spec.)	Min.-Point	Max.-Point	Random For 0 Cit.
Visible Citations	0.280	0.211	0.361	0.238
	(0.035)	(0.022)	(0.059)	(0.028)
Invisible Citations	0.062	0.048	0.107	0.069
	(0.021)	(0.014)	(0.036)	(0.015)
Subject Fixed Effects	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001
Observations	27,315	27,315	27,315	27,315
R^2	0.153	0.155	0.148	0.148
Dependent Variable Mean	50.40	50.03	50.76	50.40

Notes: The table reports the estimates of Equation (2.1) for alternative constructions of the percentile rank transformation. In all columns, the dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. The columns differ in how percentile ranks are assigned to brackets that consist of multiple percentiles. In column (1), departments and individuals without citations are assigned a percentile according to the midpoint between 1 and the lowest percentile with positive citations. In column (2), departments and individuals without citations are assigned to the first percentile. In column (3), departments and individuals without citations are assigned to the lowest percentile with positive citations. In column (4), individuals without citations are randomly assigned to a percentile rank within the bracket of zero citations. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

2.C.2.4 Alternative Sample Restrictions

We also show that the results are robust to restricting the sample in various ways. In particular, the findings are robust to excluding scientists with zero citations (Table 2.C.5, column (2)). This test shows that our findings are not driven by scientists without citations. We also show that the results are robust to excluding scientists in small departments because department ranks may be less precisely calculated in small departments. For this test, we restrict the sample to all scientists in departments with more than 10 scientists (Table 2.C.5, column (3)).

Table 2.C.5: Robustness Check: Alternative Sample Restrictions

	Dep. Var.: Department Rank		
	(1)	(2)	(3)
Sample Restriction:	Full Sample	Num. of Cit. > 0	Size of Dept. > 10
Visible Citations	0.280	0.314	0.212
	(0.035)	(0.039)	(0.035)
Invisible Citations	0.062	0.085	0.060
	(0.021)	(0.020)	(0.021)
Subject Fixed Effects	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001
Observations	27,315	17,066	22,753
R^2	0.153	0.136	0.135
Dependent Variable Mean	50.40	56.56	54.97

Notes: The table reports the estimates of Equation (2.1) for alternative subsamples. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. In column (1), we use the full sample, i.e., it is equivalent to column (3) in of Table 2.3, Specification 1. Column (2) reports results for the subsample of scientists who have more than zero citations. Column (3) reports results for the subsample of scientists who are employed at departments with at least ten scientists. Standard errors are clustered at the department level.

2.C.3 Ruling out Alternative Explanations

In this section, we show that neither differences in the quality of citing journals nor differential timing of citations biases our findings (Tables 2.C.6 and 2.C.7). Figure 2.C.2 illustrates the variation used in these tests.

Figure 2.C.2: Illustration of Variation Used in Additional Tests

(a) Specification 1

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

(b) Alternative Explanation 1

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

(c) Alternative Explanation 2

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

(d) Specification 2

	Citations in Journal A	Citations in Journal B	Citations in Journal C
1956			
1957		1	
1958			
1959	1		1
1960			
1961	1	1	
1962			1
1963	1		
1964			
1965		1	
1966		3	
1967	2		
1968			
1969			1

Notes: The four panels illustrate the sets of citations used for testing the alternative explanations in Section 2.C.3 and for Specification 2 in Section 2.3.3. As in Table 2.2, these tables illustrate citations for a hypothetical scientist. Panel (a) illustrates the variation used in Specification 1, see Table 2.3). Numbers in dark blue cells indicate citations that were visible in the SCI because the citation occurred in a journal and year (1961, or 1964-69) that was indexed by the SCI. Numbers in light blue cells indicate citations that were invisible, but are observable today. Panel (b) illustrates the variation used in testing Alternative Explanation 1, i.e., where citations are counted from a consistent set of journals (see Table 2.C.6). We disregard citations in journals that were not indexed by the first SCI in 1961 (here: journals B and C), and focus only on citations in journals that were indexed by the 1961 SCI (here: journal A). Numbers in dark blue cells indicate citations that were visible in the SCI, i.e., citations from 1961, or 1964-69. Numbers in light blue cells indicate citations that were invisible because they came from years not covered by the SCI. Panel (c) illustrates the variation used in testing Alternative Explanation 2, i.e., where citation are counted in years in which the SCI was published (see Table 2.C.7). We disregard citations from years in which the SCI was not published, and focus only on citations in years that were covered by the SCI, i.e., citations from 1961, or 1964-69. Numbers in dark blue cells indicate citations that were visible in the SCI, because they came from journals indexed by the SCI. Numbers in light blue cells indicate citations that were invisible because they came from journals not indexed by the SCI. Panel (d) illustrates the variation used in Specification 2, equivalent to Table 2.4 in the main paper.

Table 2.C.6: Alternative Explanation 1: Citations From Consistent Set of Journals

	Dependent Variable: Department Rank				
	(1)	(2)	(3)	(4)	(5)
Visible Citations	0.289	0.299	0.260	0.232	0.223
	(0.034)	(0.030)	(0.033)	(0.034)	(0.035)
Invisible Citations	0.109	0.075	0.067	0.065	0.065
	(0.022)	(0.020)	(0.021)	(0.023)	(0.023)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Observations	27,315	27,315	27,315	27,315	27,315
R^2	0.129	0.131	0.147	0.225	0.252
Dependent Variable Mean	50.40	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.1), where individual citation counts are based only on the restricted set of citing journals that were indexed in the 1961 edition of the SCI. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations in the restricted set of citing journals. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations in the restricted set of citing journals. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

Table 2.C.7: Alternative Explanation 2: Citations Only From Years With SCI

	Dependent Variable: Department Rank				
	(1)	(2)	(3)	(4)	(5)
Visible Citations	0.342	0.347	0.302	0.277	0.267
	(0.039)	(0.035)	(0.040)	(0.041)	(0.042)
Invisible Citations	0.066	0.047	0.046	0.034	0.035
	(0.017)	(0.014)	(0.014)	(0.015)	(0.015)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Observations	27,315	27,315	27,315	27,315	27,315
R^2	0.137	0.140	0.153	0.229	0.255
Dependent Variable Mean	50.40	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.1), where individual citation counts are based only the restricted set of citations from years when the SCI was available, i.e., 1961 and 1964-1969. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations in the restricted citation years. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations in the restricted citation years. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

While the test for Alternative Explanation 2 in Table 2.C.7 considers only citations in years in which the SCI was published, one might still be concerned that even in this subset of citations, visible citations, on average, come from later years. If later citations are more important for career outcomes in 1969, this might still bias the results.

We address this concern by repeating the robustness test for smaller time windows within the years covered by the SCI. In Table 2.C.8, we present the results for five different regressions in which we only count visible and invisible citations within five three-year windows (1961 and 1964-1965, 1964-1966, 1965-1968, 1966-1968, and 1967-1969). This enables us to abstract from the timing of citations and consider almost exclusively across-journal variation in visibility. We show that the difference between visible and invisible citations remains unchanged. Furthermore, the actual time window of measuring visible and invisible citations only has a small impact on the estimates. This corroborates the finding in Table 2.C.7, that the timing of visible and invisible citations does not drive our results.

Table 2.C.8: Alternative Explanation 2: Restricted Time Windows

	Dependent Variable: Department Rank				
	(1)	(2)	(3)	(4)	(5)
Citation Years:	1961, 1964-65	1964-66	1965-67	1966-68	1967-69
Visible Citations	0.278	0.293	0.302	0.305	0.302
	(0.038)	(0.039)	(0.039)	(0.039)	(0.039)
Invisible Citations	0.050	0.040	0.054	0.072	0.085
	(0.013)	(0.013)	(0.015)	(0.016)	(0.016)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes	Yes
P-value (Visible = Invisible)	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Observations	27,315	27,315	27,315	27,315	27,315
R^2	0.141	0.145	0.147	0.149	0.150
Dependent Variable Mean	50.40	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.1), where individual citation counts are based on restricted sets of citations from years when the SCI was available. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations in the restricted citation years. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations in the restricted citation years. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. In column (1), visible and invisible citation counts are based on the years 1961 and 1964-65; in column (2) 1964-66; in column (3) 1965-67; in column (4) 1966-68; and in column (5) 1967-69. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

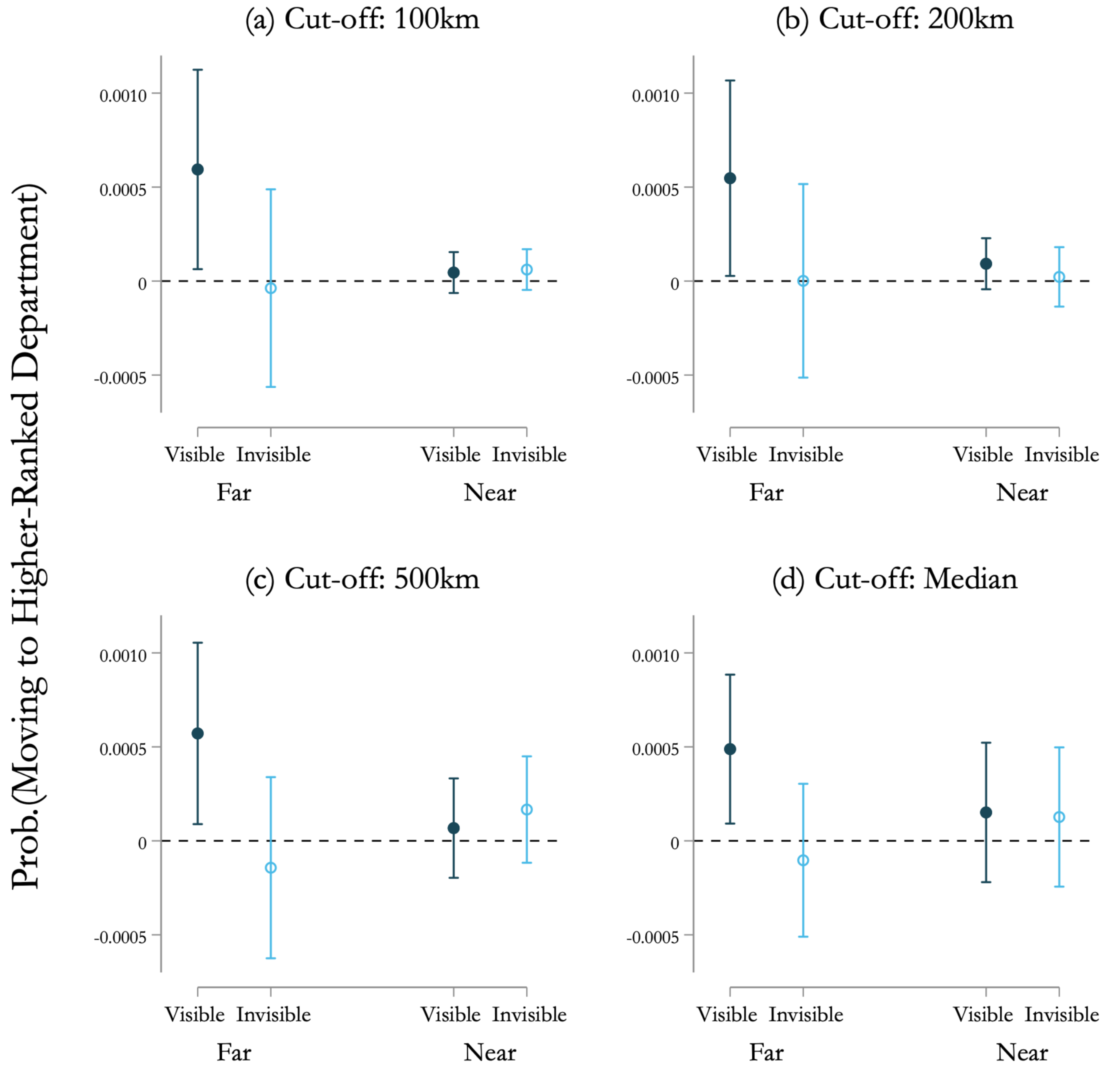
2.C.4 Additional Findings

Table 2.C.9: Moving to Higher-Ranked Department by Geographic Distance

Dependent Variable: Moving to Higher-Ranked Department by Geographic Distance					
	(1)	(2)	(3)	(4)	(5)
Panel A: New Department Far					
Visible Citations	0.0007	0.0006	0.0006	0.0008	0.0006
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Invisible Citations	-0.0001	0.0000	-0.0000	-0.0004	-0.0003
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
P-value (Visible = Invisible)	0.097	0.227	0.220	0.066	0.172
Observations	6,478	6,478	6,478	6,478	6,478
R^2	0.013	0.017	0.036	0.328	0.390
Dependent Variable Mean	0.042	0.042	0.042	0.042	0.042
Panel A: New Department Near					
Visible Citations	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations	0.0000	0.0000	0.0001	0.0000	-0.0000
	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Invisible)	0.952	0.797	0.873	0.871	0.778
Observations	6,478	6,478	6,478	6,478	6,478
R^2	0.001	0.003	0.021	0.311	0.445
Dependent Variable Mean	0.004	0.004	0.004	0.004	0.004
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes

Notes: The table reports the estimates from variants of Equation (2.3) with different dependent variables: in Panel A, an indicator for moving to a higher-ranked department that was far from scientist i's department; in Panel B, an indicator for moving to a higher-ranked department that was close to scientist i's department. The cut-off between near and far departments is 100km. These regressions use the sample of scientists observed in 1956 and 1969. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

Figure 2.C.3: Moving to Higher-Ranked Departments by Geographic Distance - Alternative Cutoffs



Notes: The figure plots coefficients and 95 percent confidence intervals from variants of Equation (2.3). Each panel reports results from two regressions with alternative dependent variables: (i) an indicator for moving to a higher-ranked department that was far from scientist i's department; (ii) an indicator for moving to a higher-ranked department that was close to scientist i's department. In panel (a) the cut-off between near and far departments is 100km; in panel (b) 200km; in panel (c) 300km; and in panel (d) 837km, which is the median distance of moves.

Table 2.C.10: Moving to Higher-Ranked Department by Citation Distance

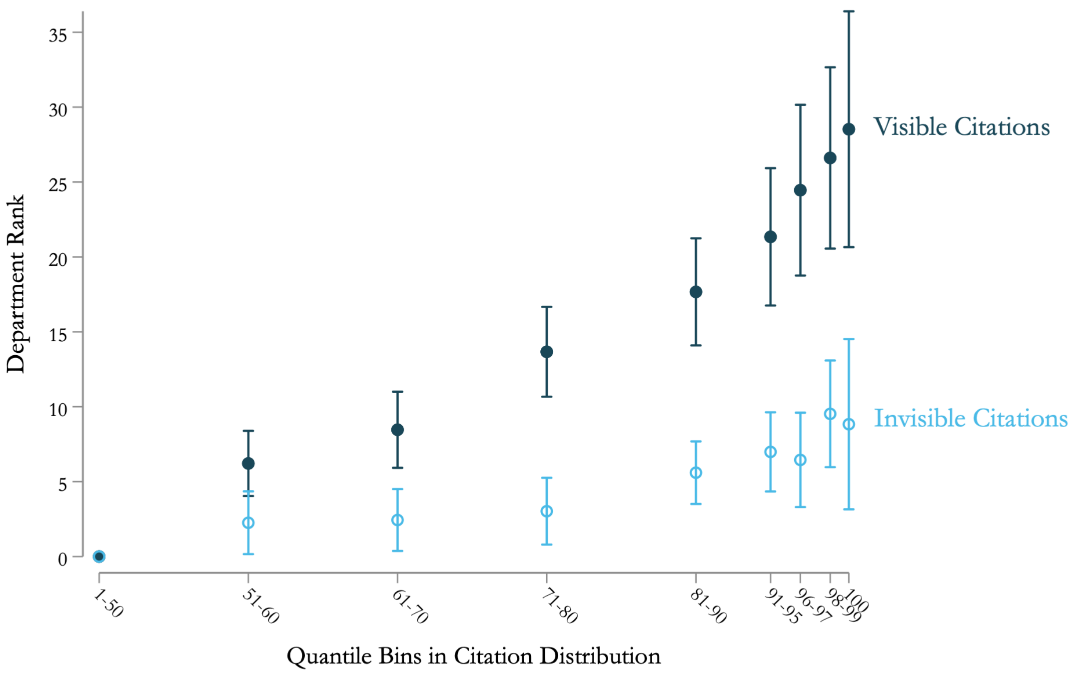
	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Moving to Higher-Ranked Department by Citation Distance				
Panel A: Not Cited In New Department Before SCI					
Visible Citations	0.0007	0.0007	0.0006	0.0008	0.0007
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Invisible Citations	-0.0004	-0.0002	-0.0002	-0.0005	-0.0005
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0003)
P-value (Visible = Invisible)	0.027	0.082	0.110	0.030	0.058
Observations	6,478	6,478	6,478	6,478	6,478
R^2	0.008	0.012	0.026	0.290	0.354
Dependent Variable Mean	0.035	0.035	0.035	0.035	0.035
Panel B: Cited In New Department Before SCI					
Visible Citations	0.0001	-0.0000	0.0000	-0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations	0.0004	0.0003	0.0002	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Invisible)	0.019	0.051	0.209	0.357	0.194
Observations	6,478	6,478	6,478	6,478	6,478
R^2	0.020	0.030	0.060	0.432	0.525
Dependent Variable Mean	0.011	0.011	0.011	0.011	0.011
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes

Notes: The table reports the estimates from variants of Equation (2.3) with different dependent variables: in Panel A, an indicator for moving to a higher-ranked department where scientist i's papers were not cited before 1963; in Panel B, an indicator for moving to a higher-ranked department where scientist i's papers were cited before 1963. These regressions use the sample of scientists observed in 1956 and 1969. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

2.D Additional Findings: Heterogeneity Analysis

2.D.1 Heterogeneous Effect in Non-Parametric Analysis

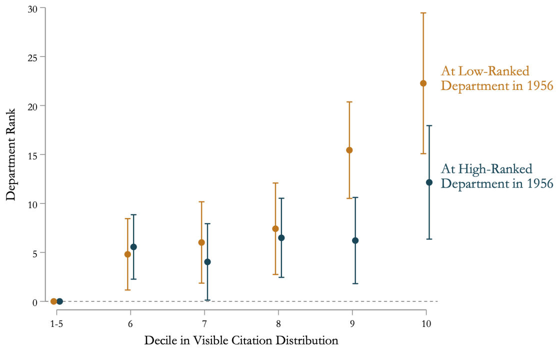
Figure 2.D.1: Heterogeneous Effects by Percentile Rank



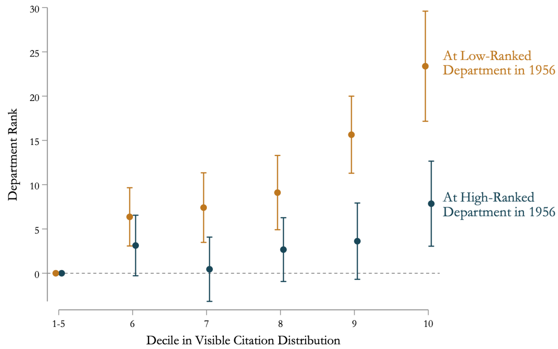
Notes: The figure plots coefficients $\hat{\delta}_q$ (dark blue) and $\hat{\theta}_q$ (light blue) and 95 percent confidence intervals from a variant of Equation (2.5). It differs from Figure 2.9 in that it splits up the 10th decile into smaller percentile bins.

Figure 2.D.2: Heterogeneous Effects for Peripheral Scientists

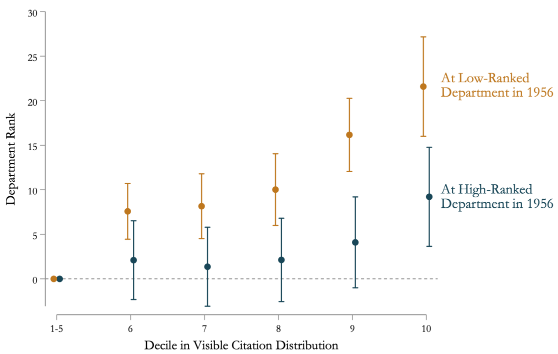
(a) Cutoff: 60th percentile



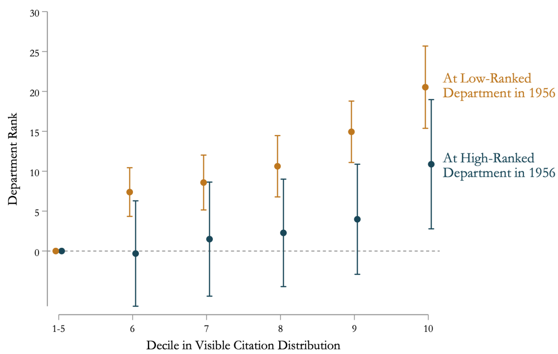
(b) Cutoff: 70th percentile



(c) Cutoff: 80th percentile



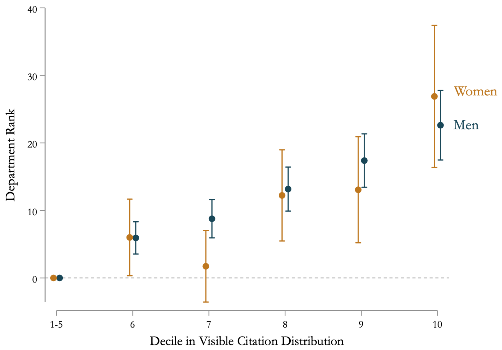
(d) Cutoff: 90th percentile



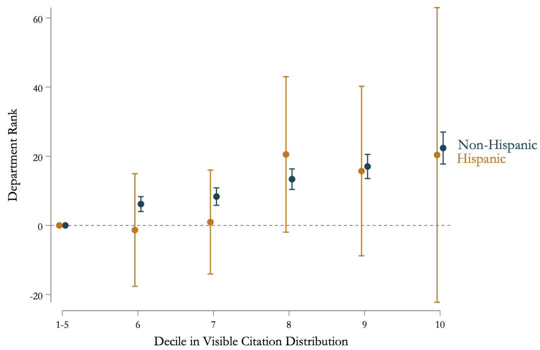
Notes: The figure plots coefficients $\hat{\delta}_q^H$ (orange) and $\hat{\delta}_q^L$ (blue) and 95 percent confidence intervals from Equation (2.6) for alternative cutoffs of high and low-ranked departments. In panel (a) we define low-ranked departments as those below the 60th percentile of the department ranking in 1956. In panel (b) we define low-ranked departments as those below the 70th percentile of the department ranking in 1956. In panel (c) we define low-ranked departments as those below the 80th percentile of the department ranking in 1956. In panel (d) we define low-ranked departments as those below the 90th percentile of the department ranking in 1956.

Figure 2.D.3: Heterogenous Effects for Minority Scientists

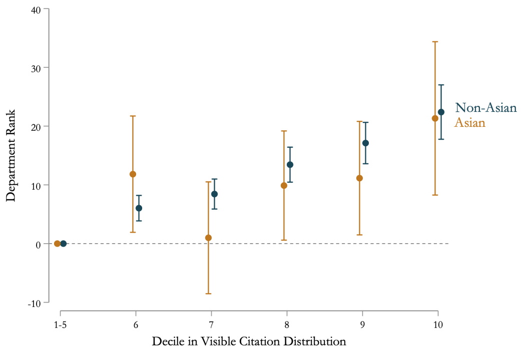
(a) Female Academics



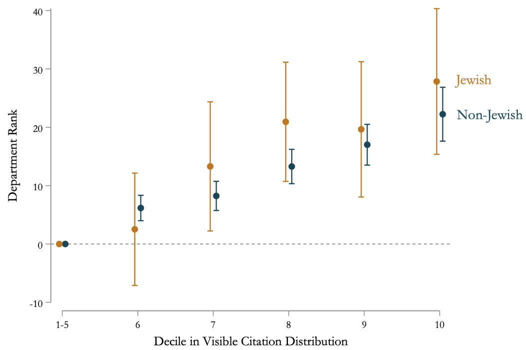
(b) Academics with Hispanic Names



(c) Academics with Asian Names

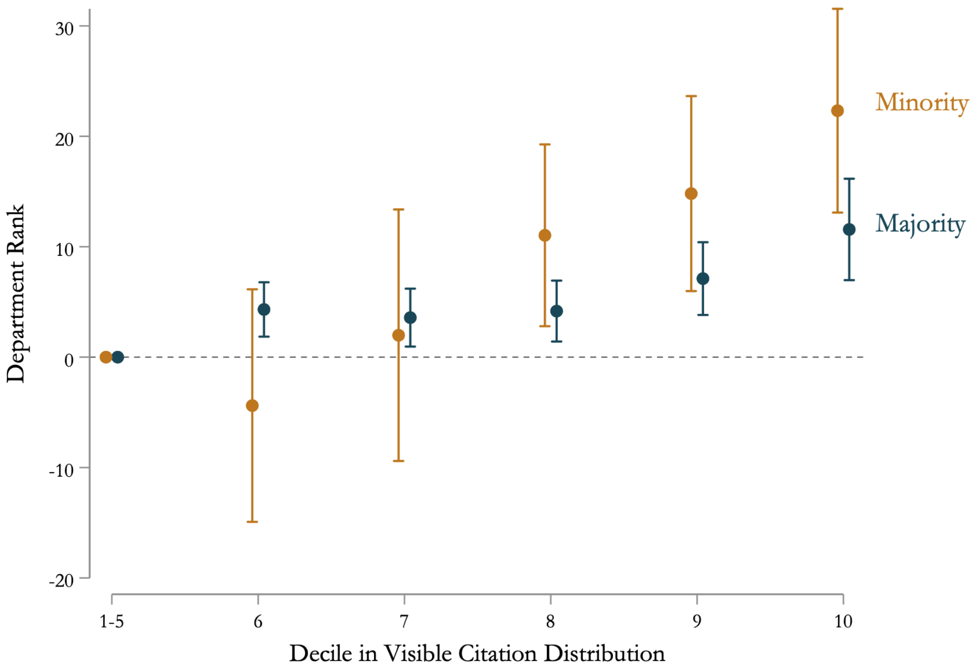


(d) Academics with Jewish Names



Notes: The figure plots coefficients $\hat{\delta}_q^M$ (blue) and $\hat{\delta}_q^m$ (orange) and 95 percent confidence intervals from Equation (2.7). Panel (a) plots separate sets of coefficients for women (orange) and men (blue). Panel (b) plots separate sets of coefficients for Hispanics (orange) and Non-Hispanics (blue). Panel (c) plots separate sets of coefficients for Asians (orange) and Non-Asians (blue). Panel (d) plots separate sets of coefficients for Jewish (orange) and Non-Jewish scientists (blue).

Figure 2.D.4: Heterogenous Effects for Minority and Majority Scientists (Controlling For Department Rank in 1956)



Notes: The figure plots coefficients $\hat{\delta}_q^M$ (blue) and $\hat{\delta}_q^m$ (orange) and 95 percent confidence intervals from a variant of Equation (2.7), while controlling for the department rank of scientist in 1956. As a result, the sample is restricted to scientists who appear in both 1956 and 1969. The p-value for the test that the coefficients for the tenth decile are the same among minority and majority scientists is 0.034.

2.D.2 Heterogeneous Effect on Assortative Matching

In Sections 2.4.2 and 2.4.3, we perform heterogeneity analyses for scientists at low-ranked departments and for minority scientists. These are based on a non-parametric regression as outlined in Equations (2.6) and (2.7). Below, we report additional results on the heterogeneous effect of citation metrics on assortative matching based on a variant of the main specification (Equation (2.1)):

$$\begin{aligned} \textit{Dep. Rank}_i = {} & \delta \cdot Visible\,Citations_i + \delta^I \cdot Visible\,Citations_i \times Indicator_i \\ & + \theta \cdot Invisible\,Citations_i + \theta^I \cdot Invisible\,Citations_i \times Indicator_i \\ & + \omega \cdot Indicator_i + \pi \cdot Publications_i + Subject\,FE + \epsilon_i \end{aligned} \tag{2.10}$$

$Indicator_i$ takes value 1 if scientist i is a member of a specific subgroup of scientists. In Table 2.D.1, we report results for peripheral scientists, i.e., where the indicator captures whether a scientist was working at a low-ranked department in 1956. In Table 2.D.2, we report results for minority scientists, i.e., where the indicator captures whether the scientist was part of a minority group.

Table 2.D.1: Heterogeneous Effect on Assortative Matching for Peripheral Scientists

	Dependent Variable: Department Rank				
	(1)	(2)	(3)	(4)	(5)
Definition of Low-Ranked Department:	Below 60	Below 70	Below 75	Below 80	Below 90
Visible Citations	0.168	0.112	0.088	0.119	0.176
	(0.043)	(0.038)	(0.040)	(0.047)	(0.070)
Invisible Citations	-0.001	-0.011	-0.008	-0.025	-0.074
	(0.035)	(0.035)	(0.036)	(0.042)	(0.058)
Visible Citations × Indicator	0.075	0.138	0.169	0.151	0.100
	(0.059)	(0.050)	(0.052)	(0.057)	(0.076)
Invisible Citations × Indicator	0.071	0.097	0.099	0.121	0.191
	(0.054)	(0.052)	(0.051)	(0.053)	(0.064)
Indicator	-36.700	-41.744	-43.410	-42.901	-40.917
	(3.488)	(3.273)	(3.368)	(3.688)	(5.275)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes	Yes
Observations	6,374	6,374	6,374	6,374	6,374
R^2	0.394	0.367	0.351	0.319	0.240
Dependent Variable Mean	59.47	59.47	59.47	59.47	59.47

Notes: The table reports the estimates of Equation (2.10), where the indicator captures whether scientist i was working at a low-ranked department in 1956. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Indicator* is equal to one if scientist i worked at a low-ranked department in 1956. Thus, the sample used in this analysis is all scientists who appear in our data in both 1956 and 1969. We define low-ranked departments as those below a specific percentile in the 1956 department ranking. The different columns report estimates using different definitions of low-ranked department: 60th percentile in column (1), 70th percentile in (2), 75th percentile in column (3), 80th percentile in column (4), and 90th percentile in column (5). *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

Table 2.D.2: Heterogeneous Effect on Assortative Matching for Minority Scientists

	Dependent Variable: Department Rank					
	(1)	(2)	(3)	(4)	(5)	(6)
Group Indicator:	Main	Female	Asian	Hispanic	Jewish	Any Minority
Visible Citations	0.280	0.285	0.281	0.280	0.279	0.270
	(0.035)	(0.040)	(0.035)	(0.035)	(0.035)	(0.033)
Invisible Citations	0.062	0.049	0.063	0.062	0.063	0.064
	(0.021)	(0.022)	(0.021)	(0.021)	(0.021)	(0.021)
Visible Citations × Indicator		-0.053	-0.050	0.068	0.049	0.020
		(0.050)	(0.076)	(0.181)	(0.088)	(0.044)
Invisible Citations × Indicator		-0.050	-0.043	0.035	-0.050	-0.039
		(0.055)	(0.084)	(0.179)	(0.087)	(0.043)
Indicator		-2.871	2.452	-5.042	5.754	-5.772
		(2.472)	(3.262)	(5.556)	(3.352)	(2.632)
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Publications by Year × Subject	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,315	24,529	27,315	27,315	27,315	27,315
R^2	0.153	0.162	0.153	0.153	0.154	0.159
Dependent Variable Mean	50.40	48.08	50.40	50.40	50.40	50.40

Notes: The table reports the estimates of Equation (2.10), where the indicator captures whether scientist i is part of a minority group. The dependent variable is the department rank in 1969, based on the leave-out mean of citations in the department of scientist i. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. We transform ranks into percentiles, where 100 is the best and 1 the worst department/scientist. *Indicator* is equal to one if scientist i is part of a minority group. Column (1) reports estimates of the main specification for reference (see column (3) in Table 2.3, Specification 1). Columns (2)-(5) report estimates from regressions where the indicator captures if scientist i is part of a minority group: female in column (2), Asian in column (3), Hispanic in column (4), and Jewish in column (5). Column (6) reports the estimates from a regression where the indicator equals one if scientist i is part of any one of these subgroups. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. Standard errors are clustered at the department level.

2.E Additional Findings: Career Outcomes

Table 2.E.1: Receiving an NSF Grant

	Dependent Variable: Receiving NSF Grant				
	(1)	(2)	(3)	(4)	(5)
Specification 1: Visible vs. Invisible Citations					
Visible Citations	0.0005	0.0005	0.0003	0.0003	0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations	-0.0000	0.0000	-0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Invisible)	< 0.001	0.001	0.012	0.033	0.041
R^2	0.026	0.026	0.049	0.160	0.210
Specification 2: Visible vs. Pseudo-Visible vs. Invisible Citations					
Visible Citations	0.0004	0.0004	0.0002	0.0002	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Pseudo-Visible Citations	-0.0002	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations (SCI years)	0.0001	0.0001	0.0001	0.0001	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Invisible Citations (non-SCI years)	0.0001	0.0001	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
P-value (Visible = Pseudo-Visible)	< 0.001	< 0.001	0.003	0.027	0.067
P-value (Visible = Invisible (SCI))	0.001	0.003	0.184	0.125	0.092
P-value (Visible = Invisible (non-SCI))	0.001	0.003	0.202	0.235	0.138
P-value (Pseudo-Vis. = Invis. (SCI) = Invis. (non-SCI))	0.032	0.058	0.044	0.226	0.630
R^2	0.026	0.027	0.049	0.160	0.210
Subject Fixed Effects	Yes	Yes	Yes	Yes	Yes
Publications by Year		Yes			
Publications by Year × Subject			Yes	Yes	Yes
Publications by Journal				Yes	
Publications by Journal × Subject					Yes
Observations	15,582	15,582	15,582	15,582	15,582
Dependent Variable Mean	0.013	0.013	0.013	0.013	0.013

Notes: The table reports the estimates of Equation (2.8) in the first panel and of Equation (2.9) in the second panel. The dependent variable is an indicator equal to one if scientist i received an NSF grant between 1964 and 1972. These regressions use the sample of scientists observed in 1969, including medicine. The explanatory variable *Visible Citations* measures scientist i's individual rank in the distribution of visible citations. *Invisible Citations* measures scientist i's individual rank in the distribution of *in*visible citations. *Pseudo-Visible Citations* measures scientist i's individual rank in the distribution of pseudo-visible citations (citations in journals indexed in the SCI in 1961, but for years not covered in the SCI, i.e., 1956-1960 and 1962-1963). *Invisible Citations (SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in SCI years (1961 and 1964-1969). *Invisible Citations (non-SCI years)* measures scientist i's individual rank in the distribution of *in*visible citations in non-SCI years (citations in journals not indexed in the SCI in 1961 and in years that were not covered, i.e., 1956-1960 and 1962-1963). We transform ranks into percentiles, where 100 is the best and 1 the worst scientist. *Publications by Year* separately measure the number of scientist i's publications in each year between 1956 and 1969. *Publications by Journal* separately measure the number of scientist i's publications in each journal (e.g., *Nature*). Standard errors are clustered at the department level.

Chapter 3

Identity Under Attack: How War Grievances Impact Behavior Under Oppressive Post-War Policy

This chapter is based on co-authored work with Emilio Esguerra (see Esguerra, 2023). I am grateful for financial support from the Nationalökonomische Gesellschaft.

3.1 Introduction

War and conquest can cast a long shadow over societies. Beyond the immediate loss of life, post-war societies can suffer from support for authoritarian politics (Acemoglu et al., 2022; Cagé et al., 2023; Koenig, 2023; De Juan et al., 2024), weakened trust in the state (Conzo and Salustri, 2019; Vlachos, 2022), and the discrimination of minorities (Fouka, 2019; Ferrara and Fishback, 2022). While post-war policy often centers on stability and reconciliation (e.g., Blouin and Mukand, 2019), ethnic minorities can be left vulnerable to oppression and assimilation policies (e.g., Dehdari and Gehring, 2022). Individuals faced with post-conquest policy might behave differently depending on their experiences with the former enemy. Yet, there is little evidence on the role of war grievances, i.e., violent experiences of war in an individual's family, on behavior under oppressive post-conquest policy.

In this article, we study how war grievances that individuals associate with their former enemy—and current ruler—affect whether individuals comply with assimilation policy. We focus on the German-speaking region of South Tyrol, which after World War I (WWI) was annexed by Italy. During WWI, many South Tyroleans fought against Italy. After the war, South Tyroleans became a German-speaking minority in Italy, which faced severe assimilation policy. Using self-digitized archival data, we study whether individual-level war grievances directed at Italy made South Tyroleans more likely to refuse assimilation.

After its annexation, South Tyrol was subjected to a so-called Italianization campaign. For example, Italian became the sole language in schools, and villages and streets were given Italian names. These efforts at assimilation culminated in the South Tyrol Option Agreement of 1939: all South Tyroleans were forced to choose between being either German or Italian citizens. If they chose German citizenship, they had to emigrate to Germany; if they chose Italian citizenship, they were able to remain in their homeland but had to assimilate into Italian culture. While most individuals would have preferred to stay in South Tyrol and keep their culture and language, this was no longer possible after the Option Agreement. In essence, the *Option* forced South Tyroleans to make a momentous choice: either emigrate and avoid assimilation, or stay and be subjected to Italianization.

We study whether war grievances directed at Italy—the former enemy—made individuals more strongly resist Italian assimilation efforts. Social identity theory suggests that negative experiences with an out-group may lead to in-group cohesion and a strengthening of identity (e.g., Tajfel and Turner, 2004; Bisin et al., 2011). War experiences, in particular, have been shown to affect individuals' engagement with their community but also their attitudes towards out-groups (e.g., Bellows and Miguel, 2009; Grossman et al., 2015). Building on these insights, we hypothesize that war grievances that South Tyroleans associate with Italy may intensify individuals' identity and thereby make them resist Italian assimilation efforts. We study the effects of such Italy-specific war grievances on two behaviors that seek to avoid assimilation: emigrating to Germany and giving children more Germanic names.

To measure individuals' war grievances, we combine novel individual-level data from two main sources. First, we collect war records on all casualties of South Tyrolean soldiers during WWI. From these casualty lists, we can infer soldiers' place of origin, which army unit they served in, and, crucially, on which front they were fighting. Second, we digitize individual-level archival data on South Tyroleans' behavior around the time of the *Option*. We observe who applied for emigration and who eventually migrated to Germany. We obtain detailed socio-economic and biographic information on these individuals and their children. Based on these data, we match option files to the casualty lists and thereby measure individuals' war grievances.

We use information on the enemy associated with a war grievance to investigate the hypothesis that Italy-specific war grievances made individuals less willing to assimilate. For causal identification, we exploit Italy's unexpected declaration of war on Austria-Hungary in the summer of 1915 as a natural experiment. While nearly all South Tyrolean soldiers initially fought on the Eastern front, they were suddenly redeployed to fight on the Italian front. This historical event led to exogenous variation in the front to which soldiers were deployed, even though they continued to fight in the same army units. This empirical strategy identifies an effect within the set of individuals who experienced any form of war grievance. This strategy holds fixed the effects of overall war grievances, which in many ways might influence individual behavior. Thus, we disentangle the *enemy-specific* component of war grievances from the more general effects of war experiences on an individual's response to assimilation policy.

Causal identification of the effect of enemy-specific war grievances rests on the assumption that holding an Italy-specific war grievance (e.g., one's father was killed by an Italian in WWI) is not systematically related to unobserved factors affecting individuals' response to assimilation policy two decades later. Thus, the main concern with this strategy would be that soldiers were non-randomly selected to fight on specific fronts. For example, our strategy would be invalid, if pre-existing anti-Italian attitudes made individuals self-select into fighting on the Italian front and if, simultaneously, these attitudes affected individuals' refusal to assimilate. However, such selection is unlikely because fighting on the Italian front was determined by an exogenous factor: the sudden redeployment of South Tyrolean soldiers after Italy' declared war in the summer of 1915. Nevertheless, we test for (self-)selection into front exposure in a series of tests. We show that South Tyrolean soldiers who were killed on the Italian front are not systematically different in their pre-war characteristics from soldiers who were killed on another front. Furthermore, in our analysis, we hold constant potential channels of selection based on an individual's place of origin or army unit by including fixed effects for municipalities and army units.

In the first set of findings, we investigate the effects of individuals' enemy-specific grievances on behavior that avoids assimilation. The first outcome we study is whether individuals emigrated to Germany. We find that South Tyroleans whose grievances were directed at Italy were not more likely to emigrate than those whose grievances were directed at another enemy. Across specifications and subsamples, the estimated effect of Italy-specific war grievances on emigration behavior remains near zero and statistically insignificant.

Since moving to another country is a high cost to pay to avoid assimilation, focusing solely on emigration might miss other effects of enemy-specific grievances on individuals' behavior. Thus, we investigate the effects of Italy-specific grievances on a relatively less costly display of identity: how people name their children. We study whether individuals with Italy-specific grievances were more likely to give their children a Germanic name (e.g., Adolf, Helga, Hermann). We again find that war grievances directed at Italy did not make individuals switch towards more Germanic naming patterns. Overall, we find no evidence for our hypothesis that enemy-specific grievances at the individual level increased assimilation avoidance.

In the second set of findings, we study whether grievances at the *community level* drive individuals' behavior, rather than grievances at the *individual level.* This is a similar, albeit theoretically distinct, hypothesis: since cultural identity is a community-based concept, it might be that factors at the community level are more salient and, thus, more relevant for identity-revealing behavior. We estimate individual-level regressions of our two outcomes—emigration and naming patterns—on the share of the male population in an individual's municipality who experienced a casualty in WWI on the Italian front. While we find evidence that such community-level Italy-specific grievances predict emigration behavior, we do not find that they predict individuals' choice of their children's names.

Our paper makes contributions to four strands of research. First, our findings relate to the literature on the effects of violence and war grievances. A consistent finding in this literature is that experiences of war can make individuals more pro-social and politically engaged (e.g., Bellows and Miguel, 2009; Gilligan et al., 2014).[1] At the same time, war experience and victimization can affect attitudes toward out-groups (Grossman et al., 2015; Dinas et al., 2021; Fouka and Voth, 2023) and mobilize violent movements (Dell and Querubin, 2018; Marchais et al., 2022). Many papers compare individuals or communities with varying degrees of war exposure to identify the effects of war grievances. We contribute to this literature by disentangling the effects of enemy-specific war grievances from overall war grievances.

Second, we contribute to a literature on the effects of post-conflict policy. One set of papers focuses on reconciliation policy and when it might be effective in healing societal divisions and grievances (e.g., Bauer et al., 2018; Blouin and Mukand, 2019, 2022). Another set of papers studies the effects of occupation and annexation on the conquered population (e.g., Dehdari and Gehring, 2022; Martinez et al., 2023). Our study complements this literature by studying individuals' reactions to oppressive post-conflict policy.

Third, we build on literature on the economics of identity.[2] One set of papers in this literature studies how social identity influences behavior (e.g., Akerlof and Kranton, 2000; Oh, 2023), while another set of papers investigates how individuals come to adopt a social identity (e.g., Shayo, 2009; Bisin et al., 2011). Since identity is not directly observable, the empirical literature on identity choices focuses on revealed preference measures (e.g., Atkin et al., 2021; Jia and Persson, 2021). Identity-revealing behavior has been used to investigate under which circumstances minorities assimilate into the majority culture (Fouka, 2019, 2020; Fouka et al., 2022). Similarly, recent research has studied the effects of nation-building policies (Kersting and Wolf, 2024), education (Cantoni et al., 2017; Alesina et al., 2021), and shared collective experiences such as sporting victories (Depetris-Chauvin et al., 2020) on individuals' sense of national identity. We add to this literature by identifying the effects of enemy-specific grievances on identity-revealing behavior and, in particular, on individuals' refusal to assimilate.

Last, this paper makes thematic contributions to the literature on forced relocation and selective emigration.[3] Many papers in this field focus on the long-run consequences of forced relocation on socio-economic outcomes (e.g., Becker et al., 2020; Sarvimäki et al., 2022). Recently, more attention has been paid to the factors influencing individual emigration behavior: for example, cultural traits such as individualism (Knudsen,

[1]For reviews of the literature on the behavioral and political effects of violence see Bauer et al. (2016) and Walden and Zhukov (2020).

[2]For reviews of the economics of identity see Charness and Chen (2020) and Shayo (2020).

[3]For reviews of the literature on forced relocation see Becker and Ferrara (2019) and Becker (2022).

2022), the role of networks (Becker et al., 2023), or the experience or threat of violence in the location of origin (Clemens, 2021; Buggle et al., 2023). We contribute by investigating whether war grievances can drive individuals into emigration in a setting lying on "the spectrum between perfectly voluntary migration and forced migration" (Becker and Ferrara, 2019, p. 14).

3.2 Historical Background

How *Südtirol* became *Alto Adige*

Until World War I, modern-day South Tyrol had for centuries been a German-speaking region in the Austro-Hungarian Empire's County of Tyrol.[4] Like millions of others across Europe, Tyrolean men were called to arms in the summer of 1914 after Austria-Hungary declared war against the Kingdom of Serbia. Initially, Austria-Hungary's war effort was focused on two fronts, against Serbia and Russia. The Kingdom of Italy, Austria-Hungary's neighbor to the South, remained neutral, yet formally was part of an alliance with Austria-Hungary. This changed when Italy unexpectedly declared war on Austria-Hungary on 23 May 1915. This "breach of faith, the like of which history has never seen," as proclaimed by Emperor Franz Joseph (Europeana, 2019), was met with surprise and outrage by many citizens of Austria-Hungary (Di Michele, 2020). Anti-Italian sentiment and propaganda were widespread (see Figure 3.1).

The Austro-Hungarian military command redeployed many army units from the Eastern front to the newly emerging front on the border with Italy. Among these army units were all Tyrolean regiments (Glaise-Horstenau, 1932). Three years of fighting followed, often at altitudes well above 2,000 meters (Thompson, 2008).

After an armistice was signed on 3 November 1918, some regions of Austria were occupied by Italy, including the Southern part of Tyrol (Thompson, 2008). The annexation of South Tyrol was codified in the Treaty of Saint-Germain-en-Laye in 1919 (see panel (b) of Figure 3.2 for post-WWI borders). As a result, the German-speaking population of South Tyrol suddenly found itself a minority under the rule of their former enemy Italy.

After the Italian Fascists under Benito Mussolini came to power in 1922, the German-speaking population in South Tyrol was systematically oppressed and con-

[4] In 1910, the population of South Tyrol included 89.0% German speakers and 2.9% Italian speakers (Landesinstitut für Statistik, 2020, p. 19). See panel (a) of Figure 3.2 for pre-WWI borders, and Appendix Figure 3.B.1 for a map of the historical borders of the County of Tyrol.

Figure 3.1: Anti-Italian WWI Postcard

Notes: The figure shows an Austro-Hungarian postcard from World War I with a German, a Bosnian, an Austrian, and a Hungarian soldier (from left to right). The text reads "May God punish treacherous Italy!" Source: Europeana (2023).

fronted with assimilation policies. These so-called Italianization policies were implemented in South Tyrol to weaken German culture among the local population (Steininger, 1997b, 2003; Di Michele, 2008). Specific measures included the introduction of Italian as the only official language, the establishment of an exclusively Italian-speaking school system, and the dismissal of German-speaking officials from public service. Villages, streets, and mountains were given newly created Italian names. The name of the region itself was changed to *Alto Adige*.[5] In addition to these assimilation policies, the settlement of Italians in South Tyrol further aggravated tensions (Steininger, 1997b).

These efforts to marginalize the German-speaking population in South Tyrol were met with backlash and resistance (Di Michele, 2008). A well-known example was the establishment of clandestine *Katakombenschulen* (catacomb schools), where dismissed school teachers secretly provided education to children in German. Other developments included the establishment of the *Völkischer Kampfring Südtirols*, a political group that embraced National Socialist ideology and emphasized South Tyrol's German identity (Steininger, 1997b).

[5]The name *Alto Adige*, "Upper Adige," refers to Italy's second-longest river and thereby emphasizes the region's geographic connection to Italy, whereas *Südtirol*, "South Tyrol," highlights the cultural connection to the Austrian region of Tyrol (Grote, 2012, p. 3).

Figure 3.2: Political Maps of South Tyrol 1914-1939

(a) 1914: Before World War I

(b) 1920: After World War I

(c) 1939: South Tyrol Option Agreement

Notes: The maps show political borders in 1914, 1920, and 1939. The area of South Tyrol is hatched. Shapefiles are provided by Census Mosaic Project (2022).

The South Tyrol Option

After the Nazi Party under Adolf Hitler came to power in Germany in 1933, its racist and nationalist ideology sought to unify all German speakers in an ethnically homogeneous nation-state (Hobsbawm, 1992, p. 133). Many South Tyroleans saw Germany as a protector of their culture against the oppression faced in Italy (Steininger, 1997b; Grote, 2012).[6] These expectations were fueled by Germany's annexations of Austria and the Sudetenland in 1938. However, the strategic importance of a German-Italian alliance made a German annexation of South Tyrol politically infeasible.

On 23 June 1939, Hitler and Mussolini reached the South Tyrol Option Agreement, concluding that "if the South Tyrolean issue was not going to go away, so the people must" (Grote, 2012, p. 67). The *Option* presented South Tyrolean German speakers with a difficult choice: they could either emigrate to Germany and retain their cultural identity, or alternatively remain in South Tyrol and be subjected to assimilation into Italian culture.

South Tyrolean heads of household had to declare their intent to opt for Germany until the end of 1939 at their local Italian municipality. The remainder of the process was handled by the *Amtliche Deutsche Ein- und Rückwanderungsstelle* (ADERSt, "Official German Immigration and Repatriation Office"), an administrative authority established for facilitating the mass emigration of South Tyroleans. The *Optanten* ("opters") had to report to the local ADERSt office to formally initiate the emigration procedure by renouncing Italian citizenship and requesting German citizenship. Each opter was assigned a unique identification number, and a file was prepared for further documentation and correspondence. After the emigration request was processed and a value assessment of property for compensation was completed, households received details on their departure. Emigrants were first brought to Innsbruck in Austria, then part of Germany, where they were centrally registered and temporarily housed, before traveling on to their final destination on their own (Alexander et al., 1993).

Many South Tyroleans considered emigration to Germany as an expression of their German identity. Nazi propaganda framed emigration as a part of the *Heim ins Reich* ideology (literally: "Back to the Empire"), which sought to unify all ethnic Germans in a "Greater Germany" (see Figure 3.3 for a propaganda poster). For example, one pamphlet in favor of emigration proclaimed that those who emigrate "sacrifice the

[6] This was not necessarily because they were sympathetic to Nazi ideology. Hannah Arendt pointed out that it would be a mistake to see the behavior of South Tyroleans as an example of mere "fanatic nationalist sentiment;" rather "these people no longer felt sure of their elementary rights if these were not protected by a government to which they belonged by birth" (Arendt, 1973, pp. 254–255).

land for the great goal, the great, holy German Empire" (Steininger, 1997a, 402–404, own translation). Those who stayed in South Tyrol, the *Dableiber* ("stayers"), were ostracized as unpatriotic (see Grote, 2012, pp. 67–71).

Figure 3.3: Nazi Propaganda Poster

Notes: The figure shows a Nazi propaganda poster promoting the mass relocation of South Tyroleans to Nazi Germany. The German texts read "Greater Germany is calling!" and "Back to the Empire!" Source: Obermair (2021, p. 55).

An overwhelming majority of South Tyroleans, around 85% of eligible households, opted in favor of emigrating to Germany (Steininger, 1997b; Wedekind, 2003; Grote, 2012). Ultimately, slightly fewer than half of those who opted for emigration actually left for Germany in the belief of never returning (Wedekind, 2003, p. 15, see also Table 3.1). The mass emigration came to a premature end in September 1943, when Nazi Germany occupied Northern Italy after Italy's armistice with the Allies (Wedekind, 2003, pp. 15–16). After the end of World War II, South Tyrol remained a part of Italy and those who had opted for Germany were allowed to regain Italian citizenship. After three further decades of political conflict and terrorist attacks, South Tyrol was granted extensive autonomy rights.[7]

[7]An overview of the history of post-WWII South Tyrol is found in Steininger (1997a).

3.3 Data

3.3.1 Data Sources

World War I Casualty Lists

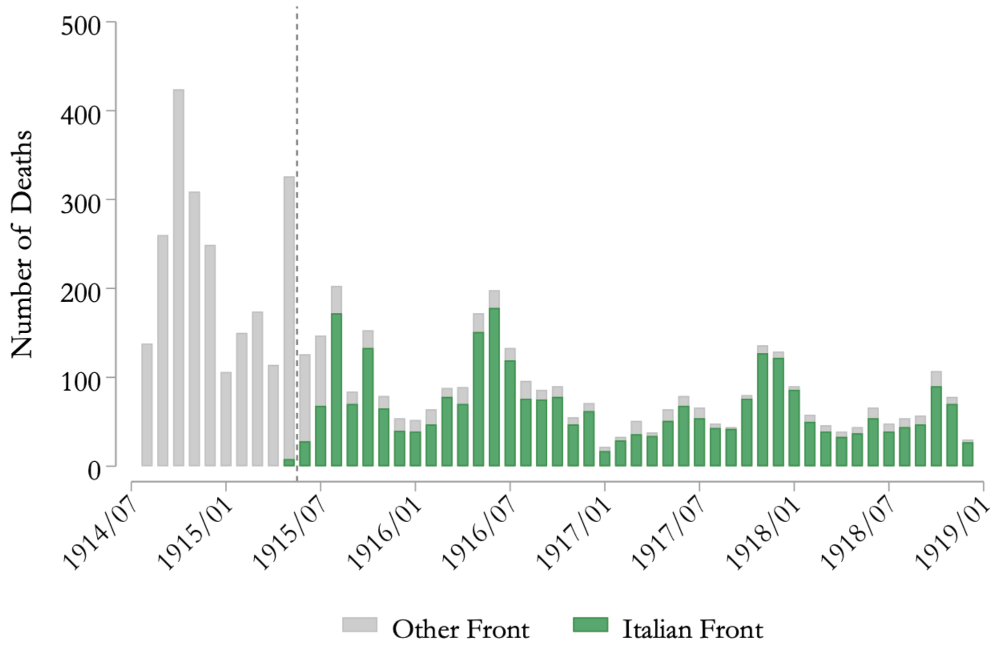
We collect WWI casualty data on South Tyrolean soldiers from two sources. We obtain records of all 23,756 Tyrolean soldiers who died in WWI from the *Tiroler Ehrenbücher* ("Tyrolean Honor Books," henceforth *Ehrenbücher*; Tiroler Landesmuseen, 2014). They were compiled after the end of WWI and were published in 1930. Based on soldiers' municipality of residence, we extract the set of 8,620 South Tyrolean dead soldiers. Data on these soldiers contain detailed individual-level information: date and place of birth, municipality of residence, occupation, family status, the soldiers' military unit (regiment and company), as well as the date, place, and cause of death (see Appendix Figure 3.A.1 for an example).

We complement these records with the *Verlustlisten Österreich-Ungarns* ("Casualty Lists of Austria-Hungary," henceforth *Verlustlisten*; Verein für Computergenealogie, 2023). These lists were published almost daily throughout WWI by the Austro-Hungarian Ministry of War. They recorded all recent casualties of the armed forces, i.e., soldiers who had died, were wounded, or were captured by the enemy. All entries contain information on the date and place of birth, military unit (regiment and company) as well as the date and place of the casualty (see Appendix Figure 3.A.2 for an example). Based on soldiers' municipalities we extract the set of 9,673 South Tyrolean soldiers in the *Verlustlisten*. While these contemporaneous casualty lists underreport the number of dead soldiers relative to the more comprehensive *Ehrenbücher*, they include information on soldiers who were wounded or captured, i.e., on soldiers who survived the war.

Measuring Enemy-Specific War Experience

We locate each entry in these casualty lists to a specific battlefront. Using data on the recorded place of the casualty, we construct a binary variable that captures whether a soldier was killed, wounded, or captured on the Italian front or on one of the other fronts of WWI. For entries lacking geographic information, we infer the front to which a soldier's military unit was deployed at the time of the entry based on historical accounts by Glaise-Horstenau (1932). The distribution of front-specific casualties over time is in line with the historical evidence that South Tyrolean soldiers were abruptly moved to the Italian Front after May 1915 (see Figure 3.4).[8]

Figure 3.4: WWI Deaths of South Tyroleans by Front Over Time



Notes: The figure shows the number of South Tyrolean deaths per month in the *Ehrenbücher* by the front on which they were recorded. The dashed vertical line indicates the Italian war entry in May 1915.

Emigration Requests

We digitize a random sample of 5,757 individual-level ADERSt emigration requests at the State Archive Bolzano.[9] This means we observe a random sample of those 85% of South Tyroleans who "opted," i.e., declared their intent to emigrate. All option files include the *Abwanderungsantrag* ("emigration request"), a three-page form that lists detailed personal information on the head of household and other family members (for an example, see Appendix Figures 3.A.3-3.A.6).[10]

[8]The analogous graph using data from the *Verlustlisten* is shown in Appendix Figure 3.B.2.

[9]In previous work (see Esguerra, 2023), we analyzed a smaller sample of 2,388 emigration requests, which was generously shared by Alexia Lochmann (see Lochmann, 2020). This sample is based on a random draw of 25 boxes of emigration requests, which on average contain 93.5 files. We then collected another representative sample of 3,369 emigration requests. After this further round of data collection, the results in the main analysis have changed. We will continue data collection and investigate these results further.

[10]While most of the heads of household were men, some women (e.g., widows or unmarried women of full age) were entitled to declare their decision during the Option. In our sample of option files, we observe 33.3% (1,915) female opters.

These emigration requests contain information on the head of household's place and date of birth, residential address, family status, religion, citizenship, ethnicity, all former places of residence, occupation,[11] property ownership, as well as military, criminal, and health records. Furthermore, the form lists information on the head of household's wife (first name, maiden name, date and place of birth), all children (first names, date and place of birth, occupation), and parents (full name and place of residence). Crucially, the emigration requests allow us to infer whether the household eventually emigrated: a stamp on the first page of the request with the letter A for *abgewandert* ("emigrated") indicates that the file has been closed and the individuals have emigrated to Germany (Lutt, 2016, p. 81).

Naming Patterns

We hand-code whether the names of South Tyrolean children listed in the emigration requests are of Germanic origin (e.g., Adolf, Helga, Hermann) based on data from Kohlheim and Kohlheim (2021) and an extensive web search.[12] In our dataset, 41.6% of children born between 1919 and 1942 were given a Germanic name, and 69.6% of parents who had any children between 1919 and 1942 gave at least one child a Germanic name.

We plot the share of newborn children in South Tyrol with a Germanic name over time in panel (a) of Figure 3.5. Four facts emerge from this plot: first, Germanic names became more popular throughout the 1920s and 1930s. Second, there was a strong increase after 1922, the year the Fascists came to power in Italy, marking the beginning of Italianization. Third, there was a further spike in the popularity of Germanic names in 1933, the year the Nazis came to power in Germany. And fourth, there was a final increase around the time of the Option (1939-1942). Especially, the first name Adolf became popular around the time of the Option (see the spike around 1939-1940 in panel (b) of Figure 3.5).[13]

[11] We manually classify all entries into four categories: skilled, semi-skilled, and unskilled workers, and farmers.

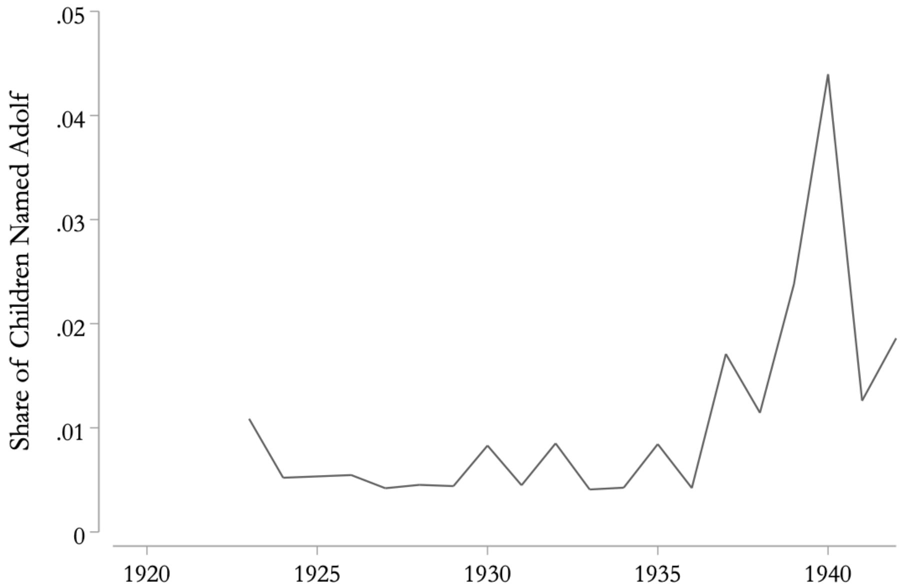
[12] This approach is similar to Lochmann (2020) and Kersting and Wolf (2024).

[13] While the general increase in Germanic names in South Tyrol in the 1920s and 1930s is similar to the trend in Germany, the peak in the use of the name Adolf around 1939 and 1940 is unique to South Tyrol (see Wolffsohn and Brechenmacher, 2001).

Figure 3.5: Naming Patterns of Children in South Tyrol

(a) Share of Children with a Germanic First Name

(b) Share of Children with the First Name Adolf



Notes: Panel (a) plots the share of newborn children with a Germanic name for each year between 1919 and 1942. Panel (b) plots the share of new-born children with the name Adolf for each year between 1923 and 1942. The graph starts only in 1923, because no children in our sample were given the name Adolf until 1922.

Historical Census Data

We complement our individual-level data with information on South Tyrolean municipalities. We digitize the pre-WWI census for Tyrol (k. k. Statistische Zentralkommission, 1907), which contains data on municipalities' population by gender and ethnicity, and on their local economic structure (e.g., number of factories, share of taxable land). As our data sources report municipality information in different years, we harmonize these records to their corresponding municipality in 1940.[14] This allows us to link casualty records, emigration requests, and census data.

3.3.2 Final Dataset

In our main analysis, we relate individual-level behavior to individual-level war grievances and, in particular, to the enemy associated with these grievances. We develop a cascading algorithm to link opters, i.e., individuals in the emigration requests, with entries in the WWI casualty lists. We match the father of an opter, first, to the *Ehrenbücher*, which exclusively contain dead soldiers, and, second, to the *Verlustlisten*, which contain data on killed, wounded, and captured soldiers. In a further step, we link male opters themselves to the *Verlustlisten*, where they might be recorded as wounded or captured. We match records only if an individual's full name, hometown, and, if available, year of birth align in both data sources (see Appendix Section 3.A.2 for more details).

Our final dataset consists of 5,757 opters, to whom we matched individual-level data from the casualty lists and municipal-level data from the census. Of these individuals, 490 (8.5 percent) hold a war grievance, i.e., they or their father were matched to the casualty lists.[15][16] 252 of these opters' war grievances are associated with the Italian front (e.g., the father was killed on the Italian front). Summary statistics on the final dataset are shown in Table 3.1. Overall, 44% of opters in our dataset eventually emigrated to Germany.[17]

[14]To track changes in administrative borders over time, we rely on historical information compiled by *Storia dei Comuni* (2022). Historical shapefiles are drawn from *Geoportal Südtirol* (Autonome Provinz Bozen, 2020).

[15]384 of these matches are based on fathers and 112 on the opting individuals themselves. These numbers do not add up to 490 because there are six cases where both the opter himself and his father were matched. See Appendix Table 3.A.1 for a summary of cases.

[16]Our matching rate is in line with historical evidence. Among the 5,049 emigration requests that include full information on fathers, we link 246 (4.9%) dead fathers to the casualty lists. This rate is similar to the death rate of 2.7% among the entire Tyrolean population (i.e., including women) reported in Winkler (1919, p. 23).

[17]While emigrating to Germany was for many South Tyroleans a question of identity, it was also driven by socio-economic factors. To explore these factors descriptively, we estimate regressions of ob-

Table 3.1: Summary Statistics

	Mean	Std. Dev.	Median	Min.	Max.	Obs.
Panel A: Individual-Level Variables						
Emigrated to Germany	0.44	0.50	0	0	1	5,757
War Grievance	0.09	0.28	0	0	1	5,757
Female	0.33	0.47	0	0	1	5,753
Military Service	0.47	0.50	0	0	1	5,737
Illness	0.16	0.37	0	0	1	5,734
Police Record	0.05	0.22	0	0	1	5,734
Previously Germany	0.09	0.29	0	0	1	5,757
Children	0.29	0.45	0	0	1	5,756
Married	0.30	0.46	0	0	1	5,751
Owns Property	0.31	0.46	0	0	1	5,590
Out of Labor Market	0.08	0.28	0	0	1	5,665
Farmer	0.45	0.50	0	0	1	5,757
Skilled Occupation	0.25	0.43	0	0	1	5,757
Birthyear	1898.28	17.06	1903	1845	1937	5,712
Panel B: Municipal-Level Variables						
Total Population	4,718.02	5,680.54	2,410	152	23,513	5,681
Population Density (per ha)	8.10	45.33	0.96	0.03	408.88	5,681
Share Male (%)	49.52	2.35	49.80	45.58	57.11	5,681
Share Italian (%)	1.83	2.89	0.79	0	24.59	5,681
Share German (%)	92.74	13.98	97.81	0.12	100	5,681

Notes: This table reports summary statistics of the variables used in our analysis. Panel A reports summary statistics on the individual-level variables drawn from the option files. Panel B reports summary statistics on the municipalities of the individuals in the option files, where the municipality-level data is drawn from the 1900 census.

3.4 Effects of Enemy-Specific War Grievances

3.4.1 Conceptual Framework

To study the behavior of individuals who are faced with pressure to assimilate, we rely on theories of social identity from both psychology (e.g., Tajfel and Turner, 2004) and economics (e.g., Shayo, 2009; Bénabou and Tirole, 2011; Charness and Chen, 2020; Shayo, 2020). Social identity theory holds that individuals want to belong to an identity group (Tajfel and Turner, 2004), which makes them behave in ways that signal their belonging (Shayo, 2009; Bénabou and Tirole, 2011). Identifying with a social group depends on the costs and benefits associated with such identity-revealing behaviors. These costs can be shaped my the broader context of society (e.g., in opposition to a majority group (Bisin et al., 2011)), but also by individuals' negative experiences with an out-group. Experiences of war can shift these psychological costs and thereby affect

served emigration behavior on a set of 11 individual-level characteristics obtained from the emigration requests. We visualize these results in Appendix Figure 3.B.5.

identity and behavior (e.g., Bauer et al., 2016; Henrich, 2020, pp. 328–340; Walden and Zhukov, 2020).

We hypothesize that enemy-specific war grievances can intensify one's own identity and increase identity-revealing behaviors. In the specific context of post-WWI South Tyrol, where ethnic identity is salient and under attack, this hypothesis implies that individuals with war grievances directed at Italy would more strongly resist Italian assimilation policy. Thus, we expect these individuals to behave in ways reflecting their non-Italian, i.e., German, identity. We study two identity-revealing behaviors that differ in their costs: first, the effects on whether an individual emigrated to avoid assimilation. In this sense, emigration to Germany was the cost one had to pay to retain one's German identity. Second, we study the effects on naming patterns of children—a less costly way of signaling identity.[18] Of course, other Italy-specific grievances were also present in post-WWI South Tyrol as a result of widespread oppression and discrimination. The effect we hypothesize is an additional effect of personal war grievances over and above any other grievances directed at Italy.

The formulated hypothesis is on the effect of *individual-level* war grievances on individuals' identity and behavior. Since national and cultural identities are inherently communal phenomena, their drivers might be more adequately captured at the *community level*. Studies on the effects of victimization (e.g., Gilligan et al., 2014; Bauer et al., 2016; Fouka and Voth, 2023) and on cultural transmission (e.g., Bisin and Verdier, 2011; Charnysh and Peisakhin, 2022) emphasize the importance of communities. That is, even if an individual was not directly affected by violence, their community's overall level of victimization can matter for their behavior after the war. We will, therefore, also test the alternative hypothesis that Italy-specific war grievances at the community level affect individuals' identity and behavior.

3.4.2 Empirical Strategy

We first test the hypothesis that individual-level Italy-specific war grievances made South Tyroleans more likely to refuse assimilation. Within the set of individuals who hold war grievances, we compare the behavior of those with Italy-specific grievances to those with grievances associated with another enemy. Italy's unexpected entry into World War I serves as a natural experiment that led to exogenous variation in the enemy that is associated with a specific war grievance. This variation allows us to identify the causal effect of Italy-specific war grievances on individual-level behavior.

[18]For other papers using naming patterns to study assimilation see, for example, Fouka (2019), Fouka (2020), and Fouka et al. (2022).

We estimate the regression:

$$\begin{aligned} Outcome_i = \gamma_1 \cdot WarGrievance_i + \gamma_2 \cdot WarGrievance_i \times ItalianFront_i \\ + X_i' \cdot \beta + BirthyearFE + MunicipalityFE + \epsilon_i \end{aligned} \tag{3.1}$$

where $Outcome_i$ measures individual i's identity-revealing behavior: in the first analysis, it is an indicator equal to one if household i accepted German citizenship and emigrated to Germany; in the second analysis, it is the share of i's children born between 1919 and 1942 who have a Germanic name (measured from 0 to 1). $WarGrievance_i$ is an indicator that equals one if opter i holds a war grievance. The interaction term $WarGrievance_i \times ItalianFront_i$ is equal to one if the grievance is associated with the Italian enemy. X_i is a vector of individual-level controls. Finally, we include fixed effects for the birth year of individual i and their municipality of residence.[19]

The estimate of γ_1 is interpreted as the percentage point change in emigration probability (or in the second analysis: the share of children with a Germanic name) associated with holding any war grievance. This reduced-form effect is not straightforward to interpret as it captures numerous financial, emotional, and other factors at once.[20] By controlling for $WarGrievance_i$, we can hold these effects fixed and isolate the effect of Italy-specific war grievances. Our main coefficient of interest γ_2 captures the differential effect of war grievances being associated with the Italian enemy. We hypothesize that this effect is positive, i.e., that Italy-specific war grievances make individuals avoid assimilation (see Section 3.4.1).

3.4.3 Validity of the Identifying Assumption

Causal identification of the Italy-specific component of war grievances relies on the assumption that a casualty happening on the Italian front is not systematically related to unobserved factors affecting individuals' emigration behavior two decades later (or, in the second analysis, their choice of names for their children). In the absence of randomized assignment of soldiers to a front, it is possible, for example, that soldiers selected themselves or were systematically selected into specific fronts or army units. This would invalidate our identification strategy. However, such selection is nearly impossible: before May 1915, all South Tyrolean casualties occurred on other fronts. With the sudden opening of the Italian front in 1915, the seven main regiments in which

[19] This specification corresponds to column (4) in Tables 3.4 and 3.5; in other columns we alter the set of controls.

[20] For example, Dupraz and Ferrara (2023) show that losing a father in war has long-lasting effects on individuals' socio-economic outcomes.

South Tyrolean soldiers were fighting were quickly redeployed to the border with Italy. Soon afterward, nearly all South Tyrolean casualties were recorded on the Italian front (see Figure 3.4). Thus, the sudden redeployment of soldiers to the Italian front led to exogenous variation in front exposure.

Nevertheless, we test whether South Tyrolean soldiers who died on different fronts were systematically different from each other. Since we have data on all dead South Tyrolean soldiers (and not just on those who we matched to the opters), we can test for balance in pre-WWI characteristics in the full set of fallen South Tyrolean soldiers (see Table 3.2). Soldiers who died on the Italian front did not differ from those who died on other fronts with respect to their socio-economic background and came from towns of similar population size and ethnic composition. Two variables (being part of the reserve force and being married) appear to predict dying on the Italian front. This is likely a mechanical result of the development of the war: with the outbreak of the war against Italy soldiers from reserve units were drafted to ensure the defense of Austrian territory in the South.[21] These reserve soldiers were, on average, older and more likely to be married. When omitting soldiers from reserve units from the balancing test (see Appendix Table 3.A.3), we find that individual characteristics are balanced; except for those soldiers who died on the Italian front being born slightly later (which is unsurprising since in the later years of the war younger cohorts were drafted). These results reaffirm the validity of our identifying assumption, that the front where a soldier experienced a casualty is exogenous to other individual-level characteristics.

In another validity check, we test whether Italy-specific war grievances predict other individual-level outcomes within the set of opters whom we matched to the casualty records. We report averages of our main control variables, conditional on treatment status, in Table 3.3. We find few systematic differences between those opters with Italy-specific war grievances and those whose grievances are associated with another enemy. This analysis does not directly test for selection into front exposure because the information on opters in the emigration requests are post-WWI characteristics. However, it does show that Italy-specific war grievances have no meaningful effect on other characteristics of opters.

Last, we show that there is no obvious regional pattern of casualties happening on the Italian front (see Appendix Figure 3.B.3).[22] Even though the Austro-Hungarian

[21] Reserve soldiers fought in separate units (Glaise-Horstenau, 1932). This allows us to account for them directly in our analysis by including army unit fixed effects.

[22] In Appendix Figure 3.B.4, we additionally show the geographic distribution of opters with war grievances by the enemy the war grievance is associated with.

Table 3.2: Balancing of Dead Soldiers by Front

	Other Front	Italian Front	Diff.	P-val.	Obs.
Panel A: Individual Characteristics					
Birthyear	1885.69	1886.05	-0.36*	(0.07)	6718
Farmer	0.46	0.46	-0.00	(0.86)	6489
Skilled Occupation	0.30	0.32	-0.01	(0.24)	6489
Married	0.24	0.28	-0.04***	(0.00)	6202
Reserve Force	0.06	0.26	-0.20***	(0.00)	6145
Panel B: Municipal Characteristics					
Total Population	4452.31	4662.04	-209.73	(0.14)	7210
Population Density (per ha)	8.37	7.99	0.39	(0.73)	7210
Share Male (%)	50.09	49.99	0.10**	(0.03)	7210
Share Italian (%)	3.51	3.52	-0.00	(0.98)	7210
Share German (%)	88.52	89.08	-0.56	(0.28)	7210

Notes: This table reports means of variables on dead South Tyrolean soldiers by treatment status, i.e., whether the death happened on the Italian or another front. The sample includes all dead South Tyrolean soldiers from the *Ehrenbücher* for whom we were able to locate their deaths on a specific front. The columns report group means, the difference between means, p-values for mean equality, and the number of observations. Significance levels: *** $p<0.01$, ** $p<0.05$, and * $p<0.1$.

Table 3.3: Descriptive Statistics of Opters With War Grievances by Front

	Other Front	Italian Front	Diff.	P-val.	Obs.
Birthyear	1905.22	1904.28	0.94	(0.34)	484
Female	0.21	0.26	-0.05	(0.16)	484
Military Service	0.62	0.60	0.02	(0.65)	484
Illness	0.10	0.16	-0.06*	(0.07)	484
Police Record	0.04	0.02	0.03*	(0.07)	484
Previously Germany	0.04	0.05	-0.02	(0.42)	484
Children	0.40	0.41	-0.01	(0.89)	484
Married	0.38	0.36	0.02	(0.60)	484
Owns Property	0.38	0.36	0.02	(0.67)	469
Out of Labor Market	0.02	0.04	-0.02	(0.23)	480
Farmer	0.64	0.54	0.10**	(0.02)	484
Skilled Occupation	0.24	0.23	0.00	(0.91)	484

Notes: This table reports averages of the control variables used in the main analysis for all opters with a war grievance, i.e., all opters whom we could link to the casualty lists (and locate the casualty to a front), conditional on the enemy associated with their grievance. While we matched 490 opters to WWI casualty records, for 6 individuals we could not assign their casualty to a battlefront. The columns report group means, the difference between means, p-values for mean equality, and the number of observations.

army drafted soldiers into army units based on their municipality of origin, soldiers were not systematically drafted to different fronts based on municipality.

3.5 Findings

3.5.1 Effects of Individual-Level Grievances

Emigration

We report estimates of Equation (3.1) in Table 3.4. Column (1) reports the estimates from a regression without any controls. The first estimate of -0.068 captures the association between holding any war grievance and emigrating. This is not a causal estimate (see Section 3.4.2). The second estimate captures the differential effect of Italy-specific war grievances. This effect is estimated from within those individuals who hold any war grievance. This enables us to identify the effects of the enemy associated with a war grievance. Under our identifying assumption—that a casualty happening on the Italian front is not systematically related to unobserved factors affecting emigration—we interpret this coefficient causally. While this specification indicates that individuals who hold Italy-specific war grievances were 1.9 percentage points more likely to emigrate to Germany, this effect is not statistically significant.

This null result remains stable with the inclusion of control variables and fixed effects. In column (2), we control for individual-level characteristics and birth year fixed effects. In column (3) we report estimates from a regression where we include municipality-level controls (e.g., population size and share of Italian speakers). In column (4), we instead include fixed effects for i's municipality of residence to control for any unobservable local aspects that might correlate with individual-level war grievances and emigration behavior. The result remains stable across specifications.

Last, in column (5), we report estimates from a regression that includes army unit fixed effects, thereby controlling for potential selection into units (see also Section 3.4.3) and any army unit-specific experiences, e.g., participating in particularly intense battles or experiencing specific leaders. In this specification, and in any specification that controls for army unit fixed effects, the baseline effect of holding any war grievance cannot be estimated. This is because individuals with war grievances are associated with one specific army unit and, hence, the sum of the army unit fixed effects is collinear with the indicator variable $WarGrievance_i$. However, this does not affect the identification of the coefficient of interest γ_2: within all army units, there is variation in front exposure, i.e., whether the grievance is associated with the Italian enemy. Again, the estimated coefficient is near zero and statistically insignificant.[23]

Across specifications, we find that the differential effect of Italy-specific war grievances is near zero and statistically insignificant. Relative to other individuals who hold war grievances, those individuals whose war grievances are directed at Italy are not more likely to emigrate to Germany.

Table 3.4: Effect of Enemy-Specific Grievances on Emigrating

	Dependent Variable: Emigrated to Germany				
	(1)	(2)	(3)	(4)	(5)
War Grievance	-0.068**	-0.035	-0.031	-0.029	
	(0.032)	(0.030)	(0.030)	(0.029)	
War Grievance × Italian Front	0.019	0.017	0.024	0.007	-0.009
	(0.044)	(0.042)	(0.042)	(0.041)	(0.041)
Birthyear Fixed Effects		Yes	Yes	Yes	Yes
Individual Controls		Yes	Yes	Yes	Yes
Municipality Controls			Yes		
Municipality Fixed Effects				Yes	Yes
Army Unit Fixed Effects					Yes
Observations	5,757	5,452	5,412	5,412	5,412
Adjusted R^2	0.001	0.200	0.222	0.250	0.251
Dependent Variable Mean	0.437	0.430	0.430	0.430	0.430

Notes: This table reports estimates of Equation (3.1). The dependent variable is an indicator equal to one if individual i emigrated. The explanatory variable *War Grievance* is an indicator equal to one if individual i holds any war grievance, i.e., whether i or i's father experienced a casualty in WWI. The explanatory variable *War Grievance × Italian Front* is an indicator equal to one if individual i's war grievance is associated with the Italian front. Column (1) reports estimates from a regression only on the two grievance indicator variables. Column (2) reports estimates from a regression which additionally controls for birth year dummies and individual-level characteristics (i.e., sex, military experience, having illnesses, having a police record, having previously lived in Germany, having children, being married, owning property, being out of the labor market, being a farmer, and working in a skilled or semi-skilled occupation). Column (3) additionally controls for characteristics of i's municipality of residence (i.e., population size, squared population size, and Italian population share). Column (4) instead controls for indicator variables for the municipality individual i lives in; consequently the municipal-level controls from column (3) are omitted. Column (5) additionally controls for indicator variables for the regiment to which the soldier, i.e., opting individual i or i's father, was assigned. Heteroskedasticity-robust standard errors are reported. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

[23] We further show that these findings are stable when using alternative fixed effects, imposing additional sample restrictions, and controlling for fine-grained information on the specific war grievance (see Appendix Section 3.C.1).

Naming Patterns

Moving to another country is a high cost to pay to avoid assimilation whereas other, more private, behaviors can be less costly. Hence, focusing only on emigration might miss other effects of enemy-specific grievances on individuals' behavior. We now investigate whether Italy-specific war grievances made individuals behave in less costly ways to signal their German identity. In particular, we study whether they gave their children more Germanic names, which was a common trend in German nationalism in the 19$^{\text{th}}$ and early 20$^{\text{th}}$ centuries (Kersting and Wolf, 2024).[24] This was amplified in the 1930s, when the Nazi government encouraged citizens to give their children Germanic names (e.g., Wolffsohn and Brechenmacher, 1999; Casquete, 2016). Also in South Tyrol, Germanic names increased in popularity during the interwar period (see Figure 3.5).

We estimate variants of Equation (3.1), in which we replace the dependent variable with the share of i's children born between 1919 and 1942 who have a Germanic first name (measured from 0 to 1). The remaining variable definitions are identical. These regressions are estimated from the sample of individuals who had any children between 1919 and 1942. We report the results of these regressions in Table 3.5. Across specifications, we find that the estimated coefficient on the Italy-specific grievance indicator is near zero and statistically insignificant. We conclude that grievances directed at Italy did not make South Tyroleans switch towards more Germanic names.[25]

While South Tyroleans overall have shited to more Germanic names under Fascist oppression (see Figure 3.5), we find that Italy-specific war grievances did not have a differential effect on naming patterns. Overall, we find no evidence for the hypothesis outlined in Section 3.4.1, that individual-level Italy-specific war grievances made South Tyroleans avoid assimilation.

[24] For other papers using naming patterns as a proxy for identity, see, for example, Fryer and Levitt (2004), Bazzi et al. (2020), Fouka (2020), Kersting and Wolf (2024), and Knudsen (2022). Another way of measuring identity-based behavior is interethnic marriage, i.e., in our setting, whether German-speaking South Tyroleans marry an Italian spouse. However, in our data there are too few spouses with Italian names to identify such behavior.

[25] We also investigate whether an effect might be present only during the Nazi period. In Appendix Table 3.C.3, we show that results are similar when restricting the analysis to individuals who had children after 1933, i.e., after the Nazis came to power in Germany.

Table 3.5: Effect of Enemy-Specific Grievances on Naming Patterns

	Dep. Var.: Share Children Germanic Name				
	(1)	(2)	(3)	(4)	(5)
War Grievance	-0.006	-0.023	-0.028	-0.023	
	(0.035)	(0.037)	(0.037)	(0.038)	
War Grievance × Italian Front	-0.036	-0.015	-0.004	-0.009	-0.033
	(0.048)	(0.051)	(0.051)	(0.052)	(0.056)
Birthyear Fixed Effects		Yes	Yes	Yes	Yes
Individual Controls		Yes	Yes	Yes	Yes
Municipality Controls			Yes		
Municipality Fixed Effects				Yes	Yes
Army Unit Fixed Effects					Yes
Observations	1,619	1,562	1,552	1,552	1,552
Adjusted R^2	0.000	0.058	0.067	0.088	0.087
Dependent Variable Mean	0.432	0.427	0.427	0.427	0.427

Notes: This table reports estimates of a variant of Equation (3.1). The dependent variable is the share of i's children who have a Germanic name (measured between 0 and 1). These regressions use the sample of individuals who had any children between 1919 and 1942. The explanatory variable *War Grievance* is an indicator equal to one if individual i holds any war grievance, i.e., whether i or i's father experienced a casualty in WWI. The explanatory variable *War Grievance* × *Italian Front* is an indicator equal to one if individual i's war grievance is associated with the Italian front. Column (1) reports estimates from a regression only on the two grievance indicator variables. Column (2) reports estimates from a regression which additionally controls for birth year dummies and individual-level characteristics (i.e., sex, military experience, having illnesses, having a police record, having previously lived in Germany, having children, being married, owning property, being out of the labor market, being a farmer, and working in a skilled or semi-skilled occupation). Column (3) additionally controls for characteristics of i's municipality of residence (i.e., population size, squared population size, and Italian population share). Column (4) instead controls for indicator variables for the municipality individual i lives in; consequently, the municipal-level controls from column (3) are omitted. Column (5) additionally controls for indicator variables for the regiment to which the soldier, i.e., opting individual i or i's father, was assigned. Heteroskedasticity-robust standard errors are reported. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

3.5.2 Effects of Community-Level Grievances

National and cultural identities are inherently communal phenomena. Hence, their drivers might be more adequately captured at the level of the community. In this second set of findings, we study a similar, but theoretically distinct, hypothesis: whether grievances at the level of the community make individuals more likely to avoid assimilation. We analyze whether the share of people in a municipality who experienced a casualty on the Italian front affects individual behaviors of assimilation avoidance.

We test this hypothesis by estimating the regression:

$$\begin{aligned} Outcome_i = \kappa_1 \cdot CasualtyShare_{m(i)} + \kappa_2 \cdot CasualtyShareItalianFront_{m(i)} \\ + X_i' \cdot \beta + M_{m(i)}' \cdot \theta + BirthyearFE + \epsilon_i \end{aligned} \tag{3.2}$$

where $Outcome_i$ is the specific behavior measuring assimilation avoidance; i.e., in the first analysis, an indicator that equals one if household i emigrated to Germany; in the second analysis, the share of i's children born between 1919 and 1942 who have a Germanic name. $CasualtyShare_{m(i)}$ is the percentage of the male population in municipality m of individual i who experienced a casualty in WWI. $CasualtyShareItalianFront_{m(i)}$ is the percentage of the male population in municipality m of individual i who experienced a casualty in WWI at the Italian front. X_i is a set of individual level controls and $M_{m(i)}$ is a set of municipal level controls. To account for potential correlations of regression residuals in a municipality, we cluster standard errors at the municipality level.

This approach has two advantages: first, it captures the wider effects of community victimization and not only those at the individual or family level. Second, the effect is now identified in the entire sample and not only in the subset of individuals for whom we measure individual-level grievances. The identifying assumption of this specification is that the municipal-level share of casualties at the Italian front is not systematically related to other factors affecting individuals' emigration behavior two decades later (or in the second analysis, the choice of names for their children).[26]

We report results on emigration in columns (1)-(3) of Table 3.6. Column (1) reports estimates from a regression only on the two casualty shares. While the overall casualty share negatively predicts emigration behavior, the estimate on the casualty share on the Italian front is positive and statistically significant at the 1%-level. In column (2), we control for individual-level characteristics and birth year fixed effects, and, in column (3), we additionally control for municipal-level characteristics. The estimates are slightly lower but remain qualitatively unchanged: an increase in the municipal-level share of the male population who experienced a casualty on the Italian front is associated with a 4.8 percentage point increase in the probability of emigrating.[27]

We repeat the analysis for the alternative outcome, the share of i's children with a Germanic name. We report the estimates of these regressions in columns (4)-(6)

[26]See Appendix Figure 3.B.3 for evidence that there is no regional pattern of casualties happening on the Italian front.

[27]In Appendix Table 3.C.4, we show that the results on emigration, i.e., columns (1)-(3), are stable to using only the subsample of individuals who had children between 1919 and 1942, i.e., the subsample used in columns (4)-(6).

Table 3.6: Effects of Community-Level Grievances

Dependent Variable:	*Emigrated to Germany*			*Children Germanic Name*		
	(1)	(2)	(3)	(4)	(5)	(6)
Casualty Share	-0.062***	-0.041***	-0.024*	-0.011	-0.001	0.008
	(0.016)	(0.013)	(0.013)	(0.010)	(0.009)	(0.010)
Casualty Share Italian Front	0.083***	0.059***	0.048**	-0.008	-0.017	-0.027*
	(0.023)	(0.019)	(0.020)	(0.016)	(0.015)	(0.016)
Birthyear Fixed Effects		Yes	Yes		Yes	Yes
Individual Controls		Yes	Yes		Yes	Yes
Municipal Controls			Yes			Yes
Observations	5,681	5,412	5,412	1,606	1,552	1,552
Adj. R^2	0.034	0.214	0.223	0.023	0.066	0.068
Dep. Var. Mean	0.437	0.430	0.430	0.432	0.427	0.427

Notes: This table reports estimates of a variant of Equation (3.2). In columns(1)-(3), the dependent variable is an indicator equal to one if individual i emigrated. In columns (4)-(6), the dependent variable is the share of i's children born between 1919 and 1942 who have a Germanic name (measured between 0 and 1). Regressions in columns (4)-(6) use the sample of individuals who had any children between 1919 and 1942. The explanatory variable $CasualtyShare$ is the municipal-level share of the male population who experienced a casualty in WWI. The explanatory variable $CasualtyShareItalianFront$ is the municipal-level share of the male population who experienced a casualty at the Italian front. Columns (1) and (4) report estimates from regressions only on the two casualty shares. Columns (2) and (5) report estimates from regressions which additionally control for birth year dummies and individual-level characteristics (i.e., sex, military experience, having illnesses, having a police record, having previously lived in Germany, having children, being married, owning property, being out of the labor market, being a farmer, and working in a skilled or semi-skilled occupation). Columns (3) and (6) additionally control for characteristics of i's municipality of residence (i.e., population size, squared population size, and Italian population share). Standard errors are clustered at the municipality level. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

of Table 3.6. Coefficients are consistently near zero or even slightly negative. In column (6), i.e., when including municipal- and individual-level characteristics, the estimate is -0.27 and significant at the 10%-level. As such, we find no evidence for the hypothesis that community-level Italy-specific grievances affected South Tyroleans' naming patterns for their children. Overall, we find mixed evidence for the hypothesis that community-level grievances affect assimilation avoidance.

3.6 Conclusion

In this paper, we test the hypothesis that enemy-specific war grievances cause individuals to resist assimilation after they were conquered. We focus on a unique historic setting: the German-speaking region of South Tyrol, whose population was targeted by intense assimilation policies after it was annexed by Italy. Using exogenous variation in soldiers' front experience during World War I, we isolate the effect of war grievances directed at Italy, thereby abstracting from other financial or emotional effects of war grievances. We then measure whether these soldiers or their children were more likely to refuse assimilation. While we find no effects of individual-level grievances on emigration or on naming patterns, we do find some evidence that community-level grievances matter for emigration.

Overall, we find little evidence that enemy-specific grievances make individuals behave in ways that avoid assimilation. This indicates that enemy-specific grievances have no differential effects beyond the effects of war grievances. An alternative interpretation is that the salience of such enemy-specific grievances in the specific context we study was relatively low. The political climate of interwar South Tyrol was marked by ethnic tensions. Other grievances directed at Italy—resulting, for example, from language prohibition in schools and discrimination in public life—might have been more salient than war grievances.

Appendix to Chapter 3

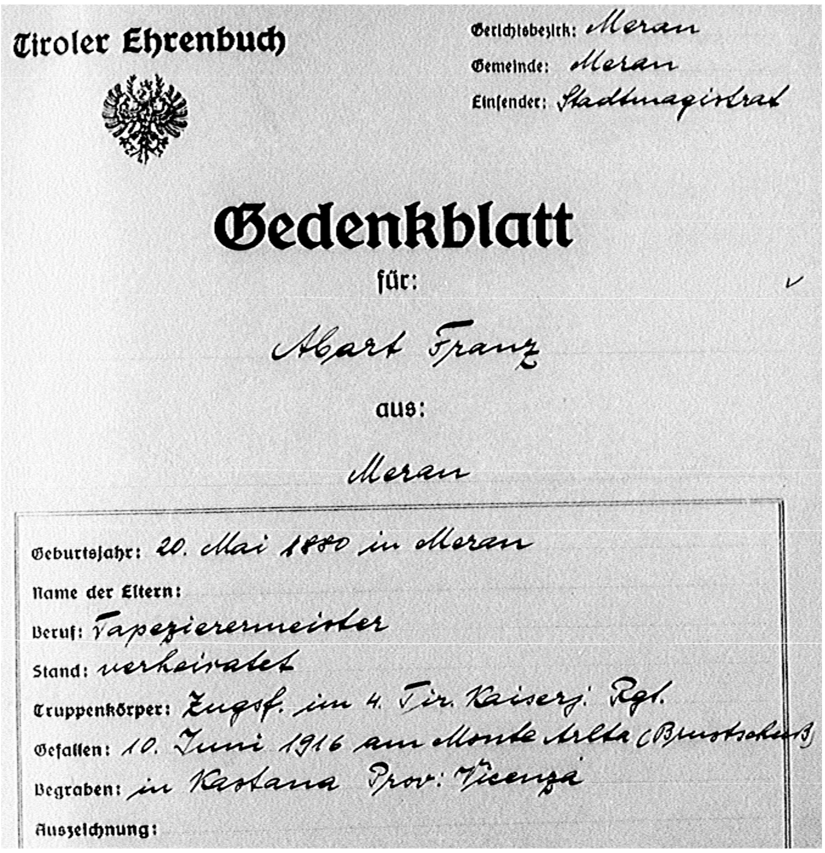
This appendix presents details on data collection and additional results:

- Section 3.A provides further details on data.
- Section 3.B provides additional figures.
- Section 3.C reports additional findings.

3.A Further Details on Data

3.A.1 Data Sources

Figure 3.A.1: Entry in *Ehrenbücher*



Tiroler Ehrenbuch

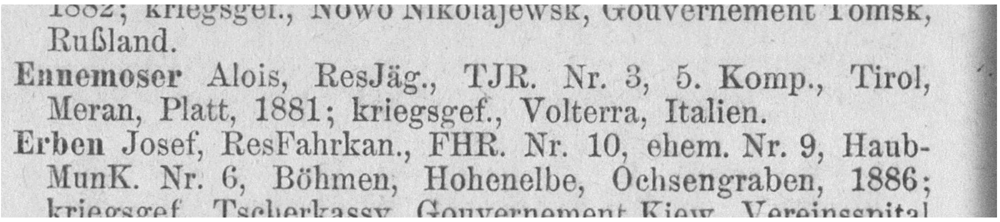
Gerichtsbezirk: Meran
Gemeinde: Meran
Einsender: Stadtmagistrat

Gedenkblatt
für:
Abart Franz
aus:
Meran

Geburtsjahr: 20. Mai 1880 in Meran
Name der Eltern:
Beruf: Tapezierermeister
Stand: verheiratet
Truppenkörper: Zugsf. im 4. Tir. Kaiserj. Rgt.
Gefallen: 10. Juni 1916 am Monte Arlta (Brustschuss)
Begraben: in Kastana Prov: Vicenza
Auszeichnung:

Notes: The picture shows the entry in *Ehrenbücher* for Franz Abart. The entry lists his date and place of birth (20 May 1880 in Meran); his occupation (upholsterer); marital status (married); rank and army unit (sergeant in the 4th *Tiroler Kaiserjäger* regiment); date, place and cause of death (10 June 1916 at the Monte Arlta (Italian front), shot in the chest); and burial place (Kastana, Province Vicenza). Source: Tiroler Landesmuseen (2014).

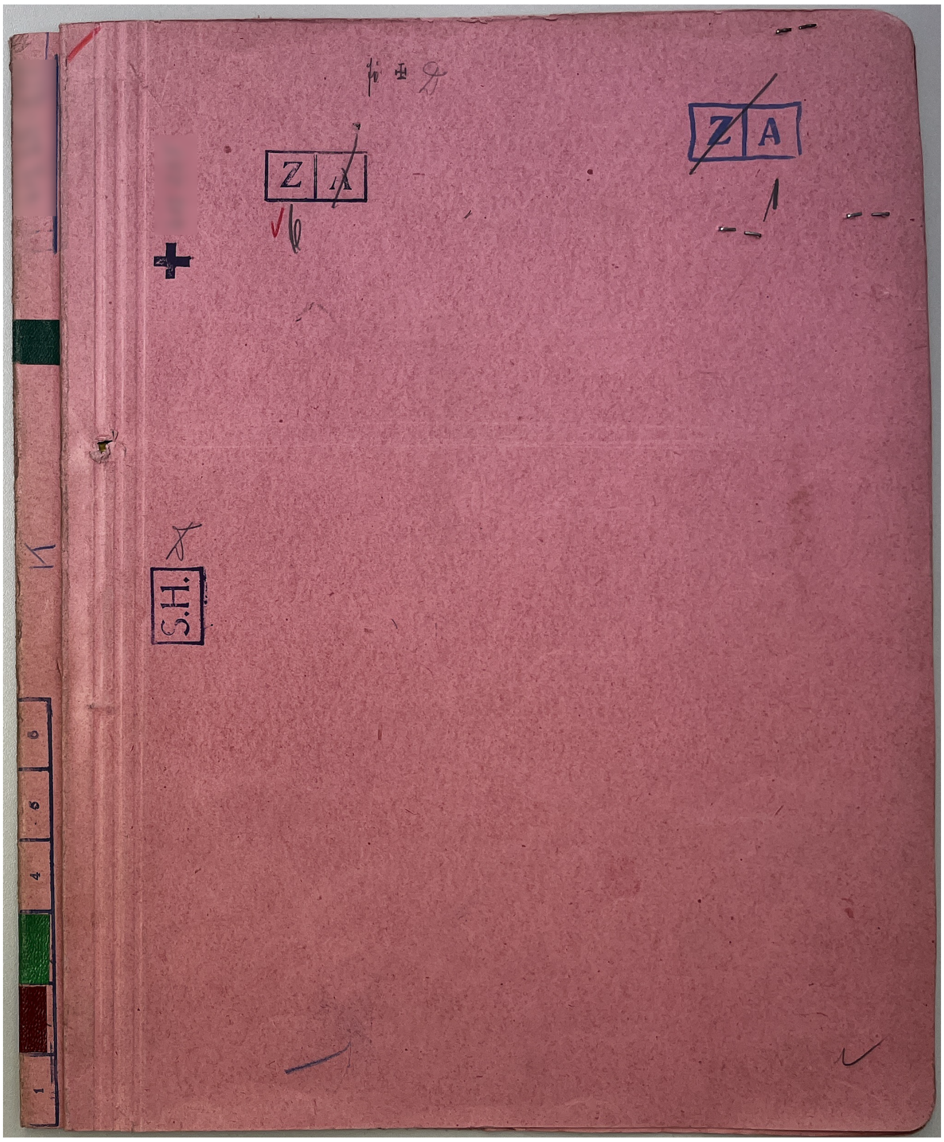
Figure 3.A.2: Entry in *Verlustlisten*



1882; kriegsgef., Nowo Nikolajewsk, Gouvernement Tomsk, Rußland.
Ennemoser Alois, ResJäg., TJR. Nr. 3, 5. Komp., Tirol, Meran, Platt, 1881; kriegsgef., Volterra, Italien.
Erben Josef, ResFahrkan., FHR. Nr. 10, ehem. Nr. 9, HaubMunK. Nr. 6, Böhmen, Hohenelbe, Ochsengraben, 1886; kriegsgef., Tscherkassy, Gouvernement Kiew, Vereinsspital

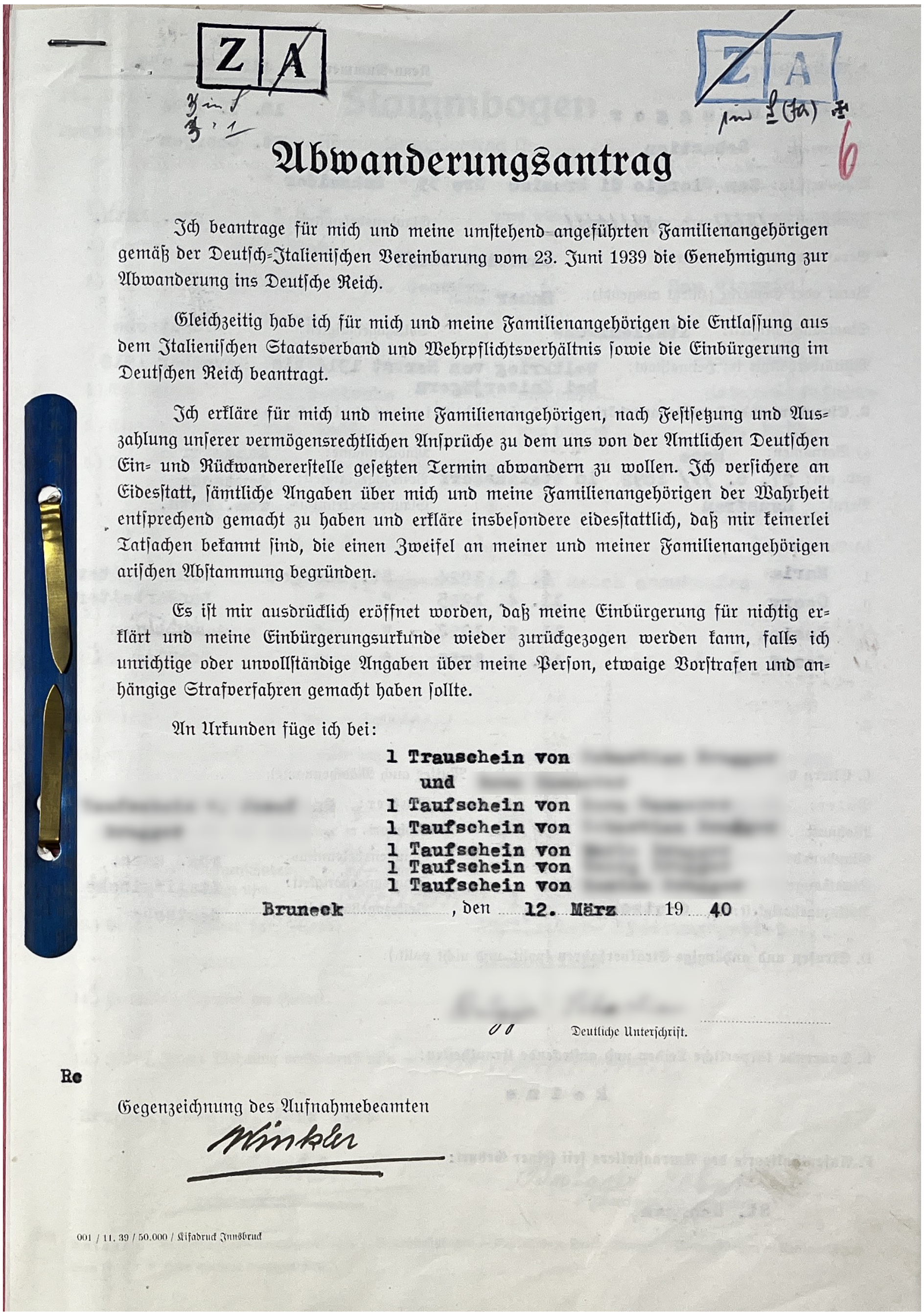
Notes: Entry for Alois Ennemoser, in the *Verlustliste* No. 397 from 22 May 1916. He was from the municipality of Platt in the district of Meran and was born in 1881. He served as *Reservejäger* (reserve rifleman) in the 5th company of the 3rd *Tiroler Kaiserjäger* regiment. He was held in captivity in Volterra, Italy. Source: Verein für Computergenealogie (2023).

Figure 3.A.3: Emigration Request: Cover Sheet



Notes: This scan shows the cover sheet of an emigration request folder with the opter's unique case number (blurred). It also includes a sign indicating their emigration status (here: in the upper-right corner the A for *abgewandert* (emigrated); this superseded the Z for *zurückgestellt* (shelved), which all files received while being processed).

Figure 3.A.4: Emigration Request: Page 1



Z A

Z A

6

Abwanderungsantrag

Ich beantrage für mich und meine umstehend angeführten Familienangehörigen gemäß der Deutsch-Italienischen Vereinbarung vom 23. Juni 1939 die Genehmigung zur Abwanderung ins Deutsche Reich.

Gleichzeitig habe ich für mich und meine Familienangehörigen die Entlassung aus dem Italienischen Staatsverband und Wehrpflichtsverhältnis sowie die Einbürgerung im Deutschen Reich beantragt.

Ich erkläre für mich und meine Familienangehörigen, nach Festsetzung und Auszahlung unserer vermögensrechtlichen Ansprüche zu dem uns von der Amtlichen Deutschen Ein- und Rückwandererstelle gesetzten Termin abwandern zu wollen. Ich versichere an Eidesstatt, sämtliche Angaben über mich und meine Familienangehörigen der Wahrheit entsprechend gemacht zu haben und erkläre insbesondere eidesstattlich, daß mir keinerlei Tatsachen bekannt sind, die einen Zweifel an meiner und meiner Familienangehörigen arischen Abstammung begründen.

Es ist mir ausdrücklich eröffnet worden, daß meine Einbürgerung für nichtig erklärt und meine Einbürgerungsurkunde wieder zurückgezogen werden kann, falls ich unrichtige oder unvollständige Angaben über meine Person, etwaige Vorstrafen und anhängige Strafverfahren gemacht haben sollte.

An Urkunden füge ich bei:

1 Trauschein von [redacted]
und [redacted]
1 Taufschein von [redacted]
1 Taufschein von [redacted]
1 Taufschein von [redacted]
1 Taufschein von [redacted]
1 Taufschein von [redacted]

Bruneck, den 12. März 1940

[redacted]

Deutliche Unterschrift.

Re

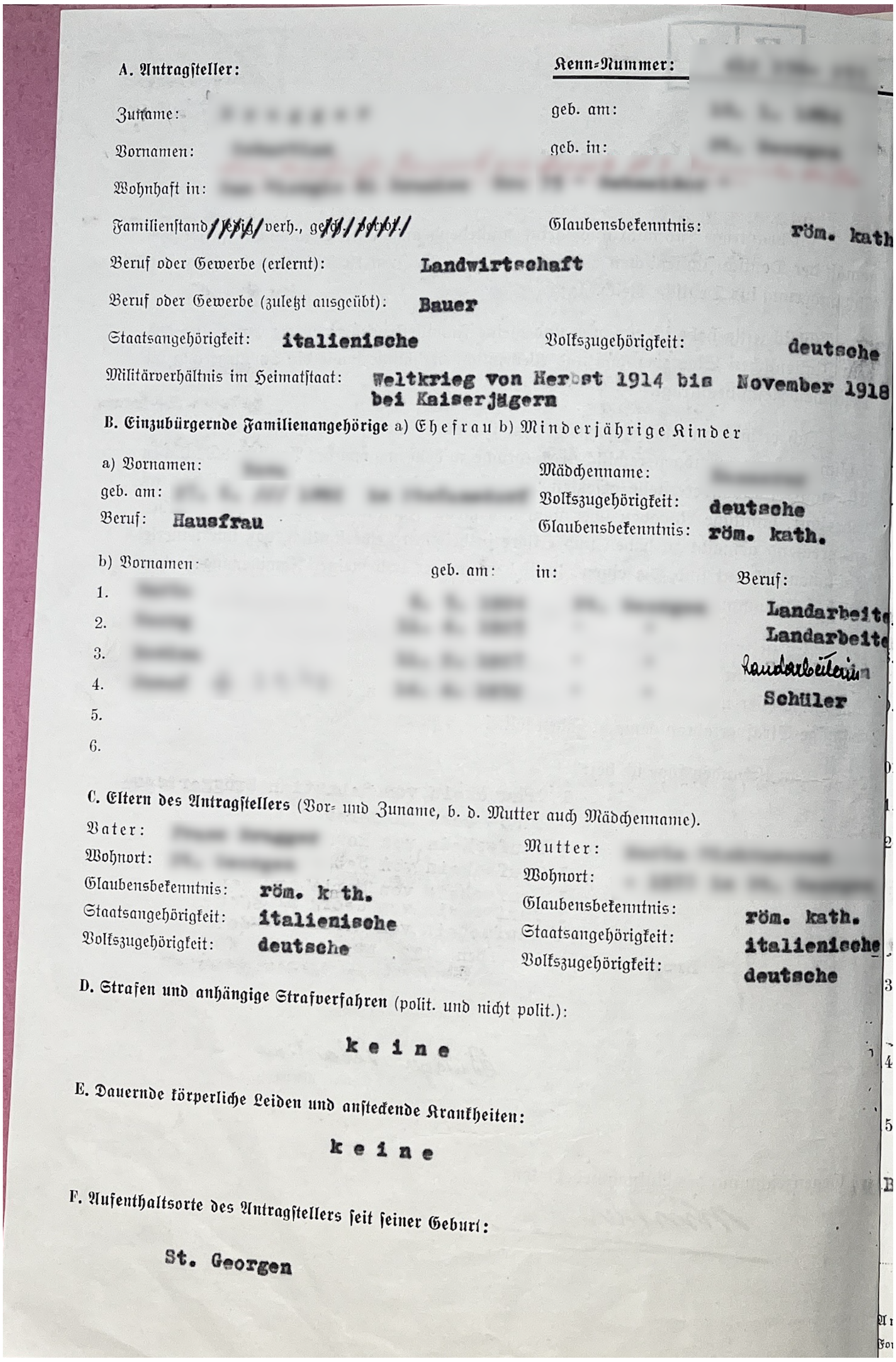
Gegenzeichnung des Aufnahmebeamten

Winkler

001 / 11. 39 / 50.000 / Kifadruck Innsbruck

Notes: This scan shows the first page of the *Abwanderungsantrag* (emigration request) of an individual who applied for German citizenship and emigration to Germany. The municipality of the office, the date, and the signatures of the officer and the opter are visible. The stamps indicating the emigration status (here: A for *abgewandert* (emigrated)) are included again on this page.

Figure 3.A.5: Emigration Request: Page 2


A. Antragsteller: Kenn-Nummer:

Zuname: geb. am:

Vornamen: geb. in:

Wohnhaft in:

Familienstand: ledig, verh., gesch., verw. Glaubensbekenntnis: röm. kath

Beruf oder Gewerbe (erlernt): Landwirtschaft

Beruf oder Gewerbe (zuletzt ausgeübt): Bauer

Staatsangehörigkeit: italienische Volkszugehörigkeit: deutsche

Militärverhältnis im Heimatstaat: Weltkrieg von Herbst 1914 bis November 1918 bei Kaiserjägern

B. Einzubürgernde Familienangehörige a) Ehefrau b) Minderjährige Kinder

a) Vornamen: Mädchenname:
geb. am: Volkszugehörigkeit: deutsche
Beruf: Hausfrau Glaubensbekenntnis: röm. kath.

b) Vornamen: geb. am: in: Beruf:
1. Landarbeite
2. Landarbeite
3. Landarbeiterin
4. Schüler
5.
6.

C. Eltern des Antragstellers (Vor- und Zuname, b. d. Mutter auch Mädchenname).
Vater: Mutter:
Wohnort: Wohnort:
Glaubensbekenntnis: röm. kath. Glaubensbekenntnis: röm. kath.
Staatsangehörigkeit: italienische Staatsangehörigkeit: italienische
Volkszugehörigkeit: deutsche Volkszugehörigkeit: deutsche

D. Strafen und anhängige Strafverfahren (polit. und nicht polit.):

keine

E. Dauernde körperliche Leiden und ansteckende Krankheiten:

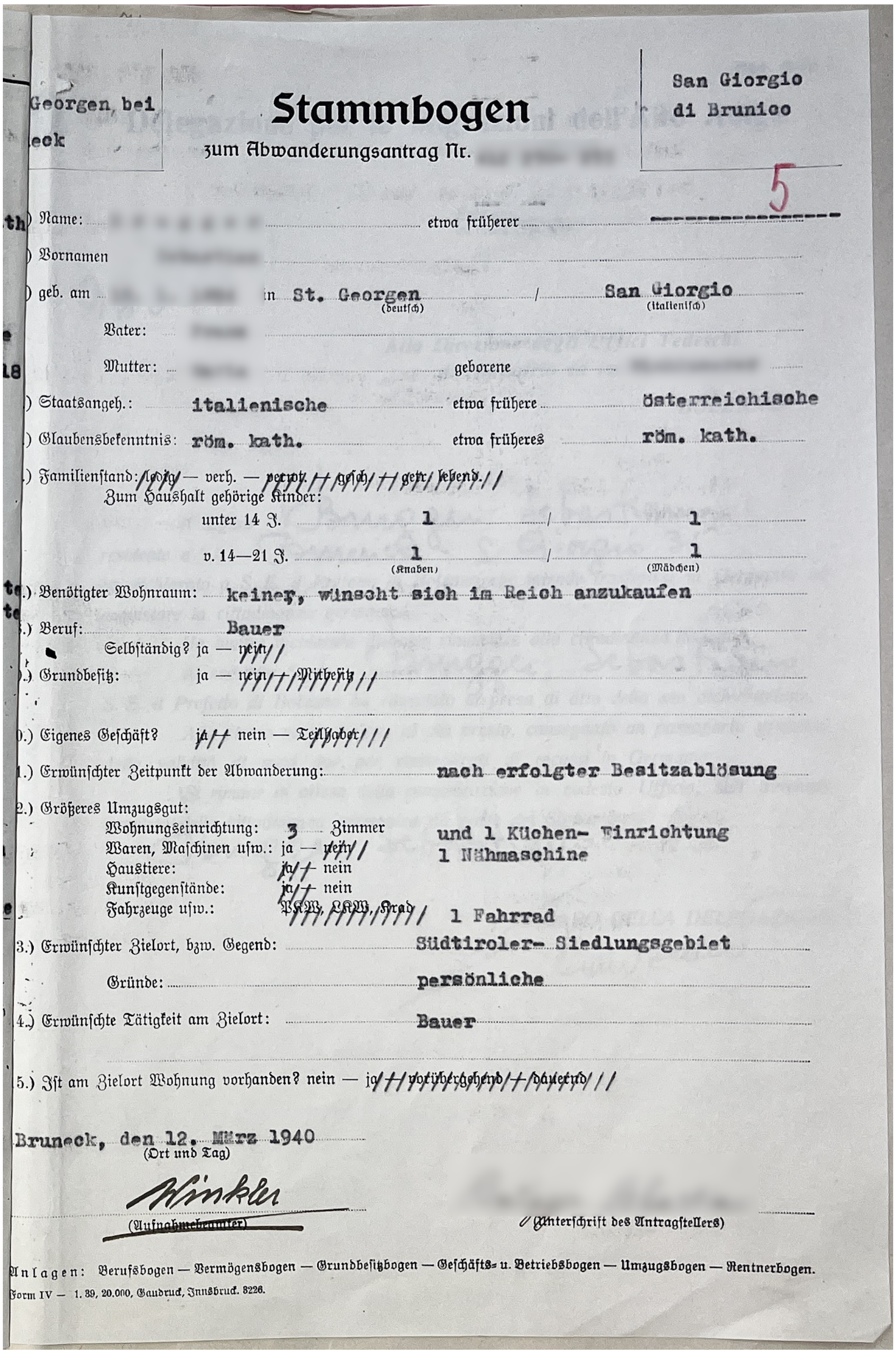
keine

F. Aufenthaltsorte des Antragstellers seit seiner Geburt:

St. Georgen

Notes: This scan shows the second page of the emigration request. Biographic details of the individual are recorded in six sections: (A) details on the applicant, (B) family members (spouse and children), (C) parents of the applicant, (D) criminal records, (E) health records, (F) all places of residence since birth.

Figure 3.A.6: Emigration Request: Page 3



Georgen, bei
eck

Stammbogen
zum Abwanderungsantrag Nr. [redacted]

San Giorgio
di Brunico

5

) Name: [redacted] etwa früherer ----------------
) Vornamen [redacted]
) geb. am [redacted] in St. Georgen (deutsch) / San Giorgio (italienisch)
Vater: [redacted]
Mutter: [redacted] geborene [redacted]
) Staatsangeh.: italienische etwa frühere österreichische
) Glaubensbekenntnis: röm. kath. etwa früheres röm. kath.
.) Familienstand: ~~ledig~~ — verh. — ~~verw.~~ — ~~gesch.~~ — ~~getr. lebend.~~
Zum Haushalt gehörige Kinder:
unter 14 J. 1 / 1
v. 14—21 J. 1 (Knaben) / 1 (Mädchen)
.) Benötigter Wohnraum: keiner, wünscht sich im Reich anzukaufen
.) Beruf: Bauer
Selbständig? ja — ~~nein~~
.) Grundbesitz: ja — ~~nein~~ — ~~Mitbesitz~~
.) Eigenes Geschäft? ~~ja~~ — nein — ~~Teilhaber~~
.) Erwünschter Zeitpunkt der Abwanderung: nach erfolgter Besitzablösung
.) Größeres Umzugsgut:
Wohnungseinrichtung: 3 Zimmer und 1 Küchen- Einrichtung
Waren, Maschinen usw.: ja — ~~nein~~ 1 Nähmaschine
Haustiere: ~~ja~~ — nein
Kunstgegenstände: ~~ja~~ — nein
Fahrzeuge usw.: ~~PKW, LKW, Rad~~ 1 Fahrrad
.) Erwünschter Zielort, bzw. Gegend: Südtiroler- Siedlungsgebiet
Gründe: persönliche
.) Erwünschte Tätigkeit am Zielort: Bauer
.) Ist am Zielort Wohnung vorhanden? nein — ~~ja — vorübergehend — dauernd~~

Bruneck, den 12. März 1940
(Ort und Tag)

Winkler
(Aufnahmebeamter)

[redacted]
(Unterschrift des Antragstellers)

Anlagen: Berufsbogen — Vermögensbogen — Grundbesitzbogen — Geschäfts- u. Betriebsbogen — Umzugsbogen — Rentnerbogen.
Form IV — 1. 39. 20.000, Gaudruck, Innsbruck. 8226.

Notes: This scan shows the third and final page of the emigration request. It includes information on the applicant's financial situation, family status, employment status, and further details.

3.A.2 Linking Option Files and Casualty Records

There are three types of potential match between individual opters in our emigration requests and soldiers in the WWI casualty lists:

1. *Option Files (Father)* ↔ *Ehrenbücher (Father)*: the father of an individual opter might be linked with his entry in the *Ehrenbücher* (all dead).
2. *Option Files (Father)* ↔ *Verlustlisten (Father)*: the father of an individual opter might be linked with his entry in the *Verlustlisten* (dead, wounded, or captured).
3. *Option Files (Opter)* ↔ *Verlustlisten (Opter)*: the opter himself might be linked with his entry in the *Verlustlisten* (wounded or captured).

For each of these potential links, we proceed in the following steps:

(1) We perform a fuzzy string match by first name and last name using the `reclink` command in Stata (Blasnik, 2010) to identify potential misspellings of names (using a minimum similarity score of 0.9). We manually correct these mistakes.

(2) We perform a perfect string match using the last name, first name, home town, and, in a type (3) match (i.e., *Option Files (Opter)* ↔ *Verlustlisten (Opter)*), the birth year of an individual. We exclude all observations from the option files that have at least one missing entry in one of these variables. The home town (*Heimatort*) in the casualty lists can be linked to either of three potential municipality entries listed in the option files: the birthplace of the opter, the place of residence of the opter, and the (last) place of residence of the father. We, thus, perform three rounds of perfect string matches and remove the already matched opters after each round. In the case of linking opters' fathers to the casualty lists (cases (1) and (2) above), the hierarchy is as follows: (i) place of residence of the father, (ii) birthplace of the opter, (iii) place of residence of the opter. In the case of linking opters themselves (case (3) above) to the casualty list, the hierarchy is: (i) birthplace of opter, (ii) place of residence of opter, (iii) place of residence of the father. To account for administrative changes in municipalities between World War I (when the casualty lists were compiled) and the Option (when the option files were compiled), we manually link all municipalities in our datasets to their corresponding municipality as of 1940 using information from *Storia dei Comuni* (2022; see Section 3.3.1).

(3) After each match, we perform several checks: in case of matches to fathers (cases (1) and (2)), where we do not have information on the birth year in the option file, we perform a sanity check in terms of the age difference and only keep matched pairs where the difference in age between the opter's father and the opter is at least 16 years. After each match, we hand-check duplicates of matching pairs. In the end, only unique combinations of opter and entry in the casualty list remain. We save these matching pairs in a separate file for each of the nine potential links (i.e., three types of municipality links for each of the three types of match listed above).

(4) We combine the resulting files into one final file containing all matched opters and their matched counterparts in the casualty lists (i.e., opters themselves or their fathers). Finally, we add the information for each of the 490 matched casualty list entries to the main database of opters from the emigration requests.

We report the numbers of matched fathers and matched opters in Table 3.A.1 and the numbers for each type of match in Table 3.A.2.

Table 3.A.1: Summary of Matches

Option Requests	**5,757**
father known	*5,049*
Matched Father	**384**
dead	246
wounded	78
captured	56
status unclear	4
Matched Opter	**112**
wounded	69
captured	43
Total Matches	**490**

Notes: This table reports the number of matches by source and type of casualty. The number of total matches is six fewer than the sum of matched fathers and matched opters because six individuals were matched both themselves and via their father.

Table 3.A.2: Source of Matches

Source	**Cases**
Opt (Father) ↔ VL (Father)	173
Opt (Father) ↔ EB (Father)	137
Opt (Opter) ↔ VL (Opter)	106
Opt (Father) ↔ EB (Father) + VL (Father)	68
Opt (Opter) ↔ EB (Father) + VL (Opter)	6
Total	**490**

Notes: This table reports the number of matches by data source of the match and by whether the opter himself or their father was matched. Abbreviations for data sources: Opt = Option files (emigration requests), EB = Ehrenbücher, VL = Verlustlisten.

3.A.3 Descriptive Statistics

Table 3.A.3: Balancing of Soldiers by Front (Excluding Reserve Force)

	Other Front	Italian Front	Diff.	P-val.	Obs.
Panel A: Individual Characteristics					
Birthyear	1886.55	1888.38	-1.83***	(0.00)	4819
Farmer	0.48	0.48	-0.00	(0.98)	4672
Skilled Occupation	0.28	0.28	-0.00	(0.72)	4672
Married	0.21	0.20	0.01	(0.38)	4484
Panel B: Municipal Characteristics					
Total Population	4135.48	4545.72	-410.24**	(0.01)	5148
Population Density (per ha)	8.93	7.83	1.09	(0.43)	5148
Share Male (%)	50.15	50.04	0.12**	(0.02)	5148
Share Italian (%)	3.40	3.78	-0.37*	(0.09)	5148
Share German (%)	88.52	89.55	-1.03	(0.10)	5148

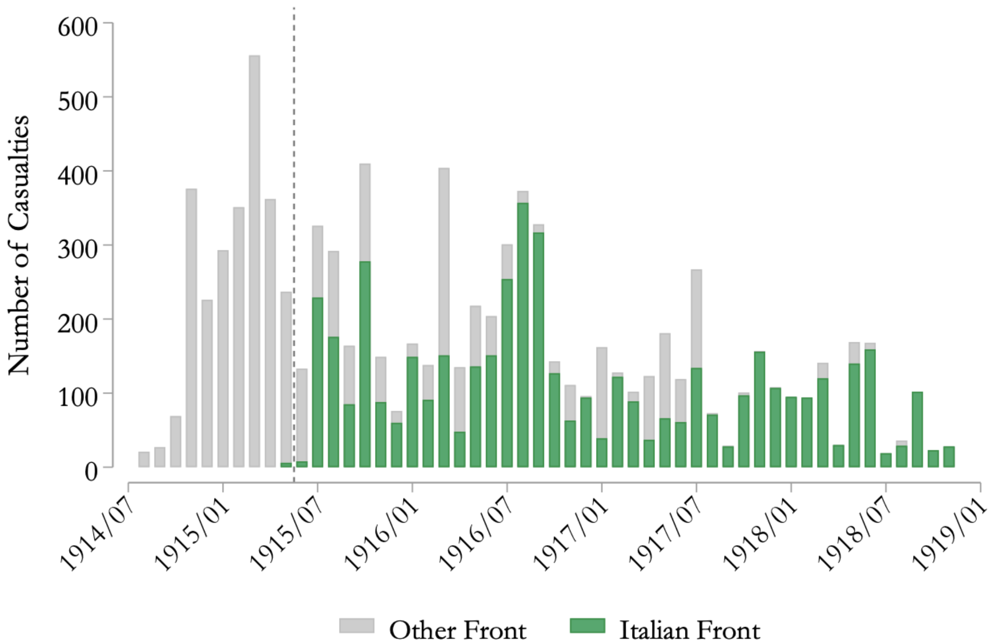
Notes: This table reports results from two-group mean-comparison tests by treatment status, i.e., whether the death happened on the Italian front. The sample includes all dead soldiers in *Ehrenbücher* from municipalities in South Tyrol for whom we were able to locate their death to a specific front, excluding soldiers who served in reserve force regiments (*Landsturm* and *Standschützen*). Columns report group averages, the difference between these averages, p-values from a test of equality of means, and the number of observations. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

3.B Additional Figures

Figure 3.B.1: Political Map of the County of Tyrol 1914

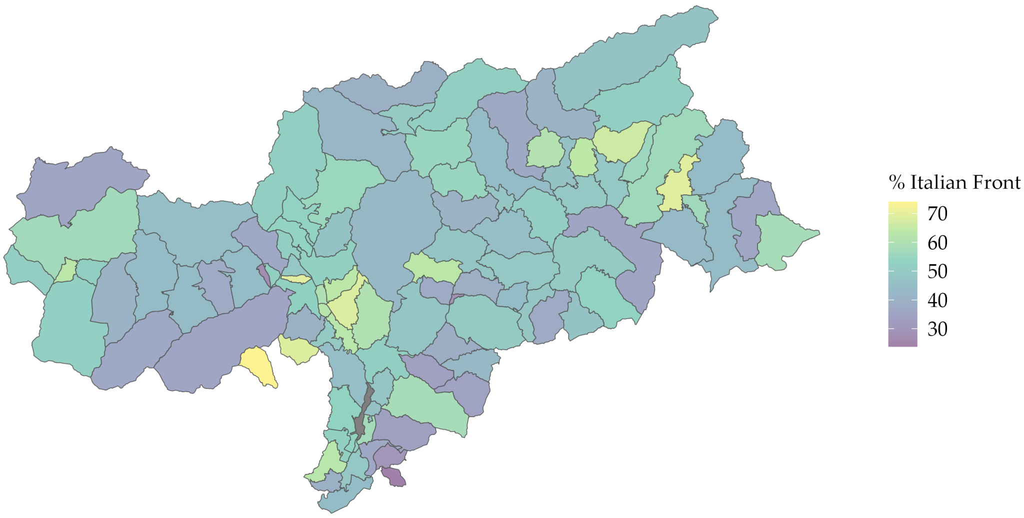
Notes: The map shows political borders in 1914 of the County of Tyrol, a part of Austria-Hungary, shaded in dark grey. The area of South Tyrol is hatched. Shapefiles are provided by Census Mosaic Project (2022).

Figure 3.B.2: Casualties by Front Over Time (*Verlustlisten*)



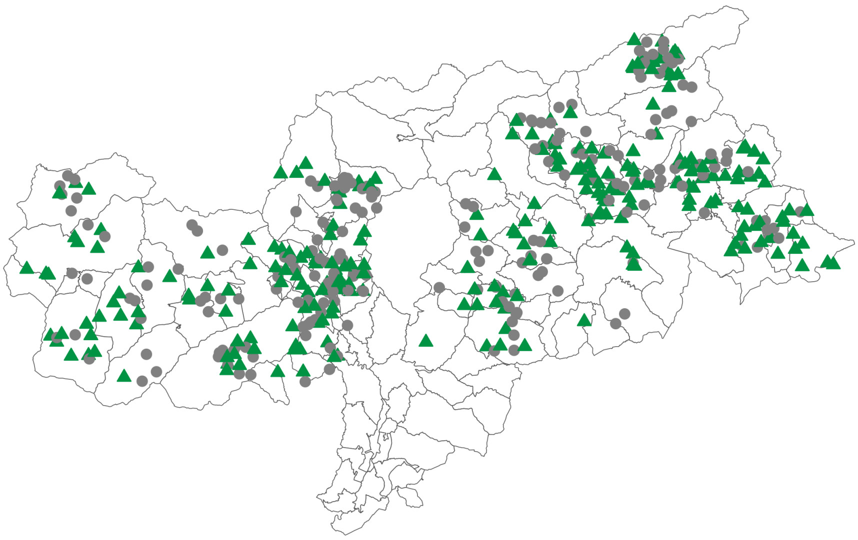
Notes: This figure shows the total number of South Tyrolean casualties per month in the *Verlustlisten* by the front on which they were recorded. The dashed vertical line indicates the Italian war entry in May 1915.

Figure 3.B.3: Share of Deaths on Italian Front by Municipality



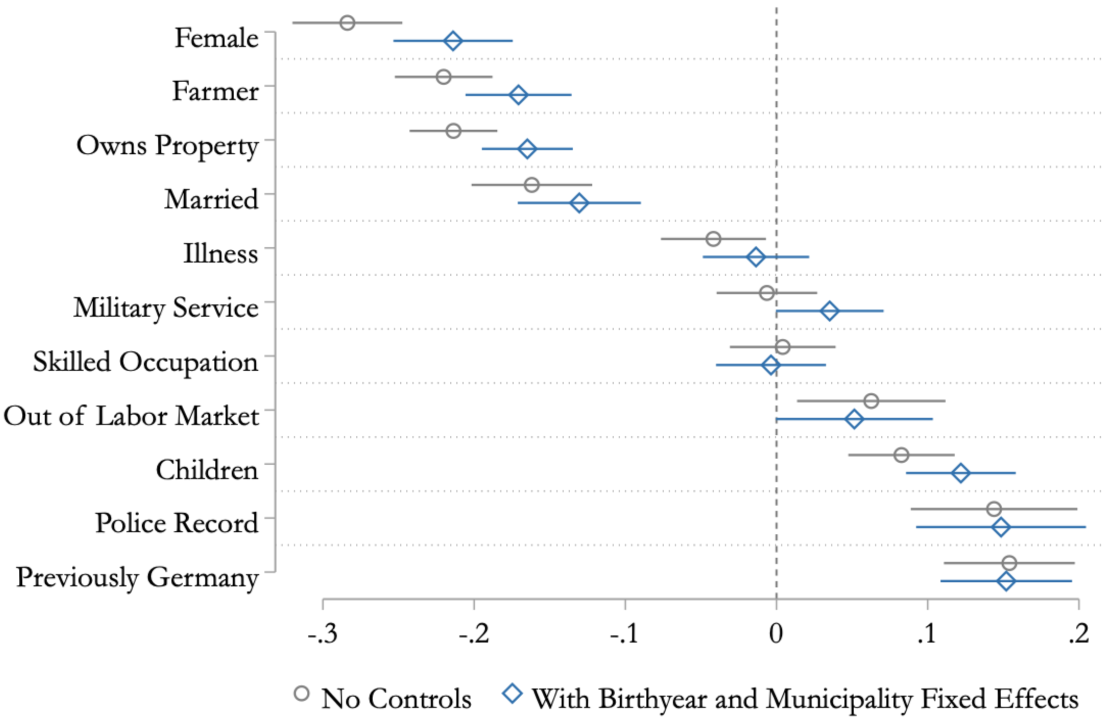
Notes: This figure shows a municipality-level map of South Tyrol as of 1940. Municipalities are colored according to the share of all dead soldiers in a municipality (as recorded in *Ehrenbücher*) who died on the Italian Front.

Figure 3.B.4: Geographic Distribution of Italy-Specific Grievances



Notes: This figure shows a municipality-level map of South Tyrol as of 1940. Every individual in our final dataset with war grievances (n=490), i.e., every opter who was matched to an entry in the casualty lists, is plotted within their municipality of residence. Green triangles indicate that the individual holds an Italy-specific war grievance; grey dots indicate that the individual's war grievance is directed at another enemy.

Figure 3.B.5: Determinants of Emigration to Germany



Notes: This figure plots coefficients from a regression of individual i's emigration behavior on the full set of individual-level controls. Two sets of coefficients are reported: the first set (blue markers) reports coefficients from a regression on these individual-level characteristics, and the second set (red markers) reports coefficients from a regression additionally controlling for birth year fixed effects and municipality fixed effects. 95% confidence intervals are reported.

3.C Additional Findings

3.C.1 Robustness: Effect on Emigration

Subsamples

In Table 3.C.1, we investigate whether the results on emigration hold across subgroups. One might be concerned that individuals with a higher exposure to Italians (or to Austrians) affect our results. In column (2), we drop all individuals from municipalities with an above-median pre-WWI share of Italian speakers and results remain stable. In column (3), we report estimates from a regression where we drop individuals from municipalities bordering Austria (then part of Germany). The results remain nearly unchanged. Analogously, dropping individuals from municipalities on the border to the rest of Italy, who might potentially have closer ties to Italy, does not affect our estimates (column (4)). In column (5), we drop all individuals from border regions, which again leaves our results unchanged. Finally, in column (6) we report estimates from a regression in which we drop all individuals linked to soldiers from the reserve force (see also Section 3.4.3). The estimated coefficient is again near zero.

Table 3.C.1: Robustness – Subsamples

	Dependent Variable: Emigrated to Germany					
	(1) Full Sample	(2) $\leq$ Mdn Italian	(3) No AT Border	(4) No IT Border	(5) No Border	(6) No Reserve
War Grievance × Italian Front	-0.009	-0.019	-0.024	-0.015	-0.006	0.010
	(0.041)	(0.049)	(0.050)	(0.045)	(0.053)	(0.043)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Army Unit FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,412	3,186	4,364	4,742	3,839	5,350
Adjusted R^2	0.251	0.218	0.264	0.258	0.272	0.253
Dependent Variable Mean	0.430	0.363	0.425	0.440	0.438	0.430

Notes: The table reports estimates of a variant of Equation (3.1) for various subsamples of the data. The dependent variable is an indicator equal to one if individual i emigrated. The explanatory variable *War Grievance × Italian Front* is an indicator equal to one if individual i's war grievance is associated with the Italian front. Column (1) reports estimated coefficients from our preferred specification, i.e., including individual-level controls, birth year fixed effects, municipality fixed effects, and army unit fixed effects (see column (5) in Table 3.4), and is included for reference. The further columns report estimates from the same specification but using different samples. In column (2), we drop individuals from municipalities with an above-median pre-WWI share of Italian speakers. In column (3), we drop individuals from municipalities with a border to the rest of Italy. In column (4), we drop individuals from municipalities with a border to Austria (then: Germany). In column (5) we drop individuals from all border municipalities. In column (6) we drop individuals whose casualty was reported as part of the reserve force. We report heteroskedasticity-robust standard errors. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

Specifics of War Grievance

We have argued that our results isolate the effect of the direction of a grievance while holding any other effects of war grievances fixed. However, war grievances might have different effects depending on the specific casualty, i.e., *who* was affected in WWI and *how*. We show in Table 3.C.2 that the results remain qualitatively unchanged when controlling for the specifics of the casualty. Column (2) controls for an indicator variable for the source of i's war grievance, i.e., whether the casualty occurred to i themselves or $i's$ father. Column (3) includes indicator variables for the type of i's war grievance, i.e., whether the soldier died, was wounded, or was captured. Column (4) includes indicator variables for the year from which i's war grievance stems. This allows us to compare individuals whose casualties occurred in the same year but on different fronts, thereby addressing the concern that Italy-specific war grievances might capture the effect of casualties happening later in the war. Column (5) includes all of these controls. Across specifications, the estimated effect remains near zero.

Table 3.C.2: Robustness – Specifics of War Grievance

	Dependent Variable: Emigrated to Germany				
	(1)	(2)	(3)	(4)	(5)
War Grievance × Italian Front	-0.009	-0.010	-0.012	-0.011	-0.005
	(0.041)	(0.042)	(0.043)	(0.052)	(0.057)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Birthyear FE	Yes	Yes	Yes	Yes	Yes
Army Unit FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Casualty Source Control		Yes			Yes
Casualty Type Controls			Yes		Yes
Casualty Year Controls				Yes	Yes
Observations	5,400	5,400	5,400	5,400	5,400
Adjusted R^2	0.250	0.250	0.250	0.250	0.250
Dependent Variable Mean	0.430	0.430	0.430	0.430	0.430

Notes: The table reports estimates of variants of Equation (3.1) using different controls for characteristics of i's war grievance. The dependent variable is an indicator equal to one if individual i emigrated. The explanatory variable *War Grievance* × *Italian Front* is an indicator equal to one if individual i's war grievance is associated with the Italian front. Column (1) reports estimated coefficients from our preferred specification, i.e., including individual-level controls, birth year fixed effects, municipality fixed effects, and army unit fixed effects (see column (5) in Table 3.4), and is included for reference. Column (2) controls for an indicator variable for the source of i's war grievance, i.e., whether the casualty occurred to i themselves or i's father. Column (3) includes indicator variables for the type of i's war grievance, i.e., whether the soldier died, was wounded, or was captured. Column (4) includes indicator variables for the year in which i's war grievance occurred. Column (5) includes all of these controls. We report heteroskedasticity-robust standard errors. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

3.C.2 Additional Findings: Naming Patterns

Table 3.C.3: Effect on Giving Children Germanic Name After 1933

	Dep. Var.: Share Children Germanic Name				
	(1)	(2)	(3)	(4)	(5)
War Grievance	-0.037	-0.044	-0.048	-0.044	
	(0.045)	(0.048)	(0.047)	(0.048)	
War Grievance × Italian Front	-0.041	-0.016	-0.001	-0.006	0.022
	(0.061)	(0.068)	(0.068)	(0.069)	(0.075)
Birthyear Fixed Effects		Yes	Yes	Yes	Yes
Individual Controls		Yes	Yes	Yes	Yes
Municipality Controls			Yes		
Municipality Fixed Effects				Yes	Yes
Army Unit Fixed Effects					Yes
Observations	1,072	1,025	1,020	1,020	1,020
Adjusted R^2	0.001	0.044	0.058	0.089	0.092
Dependent Variable Mean	0.465	0.461	0.460	0.460	0.460

Notes: This table reports estimates of a variant of Equation (3.1). The dependent variable is the share of i's children born between 1933 and 1942 who have a Germanic name (measured between 0 and 1). These regressions use the sample of individuals who had children between 1933 and 1942. The explanatory variable *War Grievance* is an indicator equal to one if individual i holds any war grievance, i.e., whether i or i's father experienced a casualty in WWI. The explanatory variable *War Grievance* × *Italian Front* is an indicator equal to one if individual i's war grievance is associated with the Italian front. Column (1) reports estimates from a regression only on the two grievance indicator variables. Column (2) reports estimates from a regression which additionally controls for birth year dummies and individual-level characteristics (i.e., sex, military experience, having illnesses, having a police record, having previously lived in Germany, having children, being married, owning property, being out of the labor market, being a farmer, and working in a skilled or semi-skilled occupation). Column (3) additionally controls for characteristics of i's municipality of residence (i.e., population size, squared population size, and Italian population share). Column (4) instead controls for indicator variables for the municipality individual i lives in; consequently, the municipal-level controls from column (3) are omitted. Column (5) additionally controls for indicator variables for the regiment to which the soldier, i.e., opting individual i or i's father, was assigned. Heteroskedasticity-robust standard errors are reported. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

3.C.3 Additional Findings: Community-Level Grievances

Table 3.C.4: Robustness: Community-Level Grievances

Dependent Variable:	*Emigrated to Germany*		
	(1)	(2)	(3)
Casualty Share	-0.074***	-0.051***	-0.033**
	(0.020)	(0.014)	(0.014)
Casualty Share Italian Front	0.103***	0.081***	0.065***
	(0.027)	(0.022)	(0.022)
Birthyear Fixed Effects		Yes	Yes
Individual Controls		Yes	Yes
Municipal Controls			Yes
Observations	1,606	1,552	1,552
Adj. R^2	0.046	0.152	0.159
Dep. Var. Mean	0.374	0.371	0.371

Notes: This table reports estimates of a variant of Equation (3.2). The dependent variable is an indicator equal to one if individual i emigrated. The regressions use the subsample of individuals who had any children between 1919 and 1942. The explanatory variable $CasualtyShare$ is the municipal-level share of the male population who experienced a casualty in WWI. The explanatory variable $CasualtyShareItalianFront$ is the municipal-level share of the male population who experienced a casualty at the Italian front. Column (1) reports estimates from regressions only on these two casualty shares. Column (2) reports estimates from a regression which additionally controls for birth year indicators and individual-level characteristics (i.e., sex, military experience, having illnesses, having a police record, having previously lived in Germany, having children, being married, owning property, being out of the labor market, being a farmer, and working in a skilled or semi-skilled occupation). Column (3) additionally controls for characteristics of i's municipality of residence (i.e., population size, squared population size, and Italian population share). Standard errors are clustered at the municipality level. Significance levels: $***$ p<0.01, $**$ p<0.05, and $*$ p<0.1.

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München, 12. März 2024

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