Discrimination between Religious and Non-Religious Groups: Evidence from Marking High-Stakes Exams*

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April 2021

Abstract

While religions frequently preach preferential treatment of fellow believers, the magnitude and economic implications of religiosity-based discrimination are largely unknown. Religiosity is often confounded with ethnicity, but it varies even within ethnicities and religious denominations. It is also seldom observed in administrative data. This paper exploits a setting that avoids these limitations. We analyse grading decisions in national matriculation exams in Israel, exploiting unique features that reveal student religiosity to the graders, and grader religiosity to the researcher. We find evidence of ingroup bias between religious and non-religious groups, but in our setting this effect is very small. There seem to be two main reasons. First, religious ingroup bias is limited to male graders only. Moreover, patterns of bunching in the grade distribution suggest the bias is primarily due to the religious---rather than secular---men. This is a small fraction of the grader population. A second potential reason is that many graders live in integrated

* We thank Josh Angrist, James Fenske, Jonathan Guryan, Imran Rasul, Asaf Zussman, and participants at seminars at Ben Gurion University, IFS London, Bank of Israel, IDC Herzliya, Northwestern University, University of Bonn, University of Zurich, University of Warwick, and CAGE Venice Applied Micro Economics Conference for useful comments and suggestions. We also thank the Israel's Ministry of Education and Dr. Haim Gat and Eliad Trefler for allowing restricted access to schooling data in the Ministry online protected research lab. Evgeni Rachkovski provided excellent research assistance. Lavy acknowledges financial support from the European Research Council through ERC Advanced Grant 323439. Shayo thanks the I-Core Program at the Israel Science Foundation (grant no. 1821/12). A previous version of this paper was circulated as "The Surprisingly Small Effects of Religion-Based Discrimination in Education".

communities. Our evidence suggests that living and working in close proximity to people with different levels of religiosity attenuates discrimination.

1. Introduction

Inter-group animosity across levels of religiosity is potentially widespread even in seemingly homogeneous societies, and within the same religious denomination.[1] While economists have long studied the causes and effects of religiosity and secularization (Barro and McCleary 2003; Gruber and Hungerman 2008; Iyer 2016; Becker, Rubin and Woessmann 2020), we know far less about the impact of the rift between secular and religious groups in modern economies. Many religions openly preach preferential treatment of fellow believers, and sometimes harsh treatment of non-believers. Secular people might similarly show bias against religious individuals. However, since one's level of religiosity is rarely observed in administrative data, and since groups that differ in their religiosity often vary in other dimensions (including race or ethnicity), religiosity-based discrimination is hard to isolate and quantify. This paper makes a first attempt.

Israel's high school matriculation system offers a unique opportunity to study religiosity-based discrimination. It is a centralized country-wide scheme of exams that affect both a student's prospects for continuing to higher education as well as their field of study. The exact same exams are taken by both religious and secular students.[2] Each exam booklet is randomly assigned to two independent graders (or "examiners"). Grading decisions are made under anonymous conditions, reducing the possibility of social pressure or reciprocity effects. Nonetheless, as explained in the next section, certain features of this setting allow the grader to infer student religiosity. Identifying the religiosity of both students and examiners is feasible in our setting because the Israeli public-school system is divided into religious and secular schools. We are thus able to infer student religiosity from the schools they go to, and examiner religiosity from the schools they send their children to. Finally, we have detailed data on the grades given to each exam booklet, where the grades range from 0 to 100. Observing the entire distribution of grades

[1] For example, while in the US non-Christian religious groups represent less than 7% of the population, 24% of Americans are estimated to be religiously unaffiliated (Cox and Jones 2017; see also Hout, Fischer, and Chaves 2013). Furthermore, the share of the religiously unaffiliated has been growing and they tend to be overrepresented among younger cohorts. About 85% of the unaffiliated identify as secular (the majority), agnostic, or atheist. And of course, even within the religiously affiliated Christian population, there is enormous diversity in denomination. Europe has undergone a lengthy process of secularization, but recent waves of immigration have re-ignited religious tensions.

[2] Ultra-orthodox students have a separate system and are not included in our data.

allows us to exploit bunching at certain points in the distribution to go beyond measuring relative ingroup bias, and identify the source of discrimination

We begin with a difference-in-differences model, exploiting the random assignment of exam booklets and allowing for systematic differences across levels of religiosity both in student ability and in examiner standards. Intuitively, we compare the mean difference in grades given to religious versus secular students by religious and secular examiners, controlling for student and questionnaire fixed effects ("questionnaire" refers to subject by level of proficiency, e.g. "math at level 4"). Using data from over 3.5 million grades given in 112 questionnaires in the years 2010–2015, we find evidence of a statistically significant but substantively small tendency toward religiosity-based ingroup bias. An exam grade is on average about 0.01 standard deviations higher when assigned to an examiner of the same (rather than different) level of religiosity as the student.

One clue into why the overall effect is so small comes from the fact that 83% of the examiners in our data are female. Research across cultures, time and samples, has demonstrated that, on average, men display more self-reported xenophobic and ethnocentric attitudes than do women. This has also been shown in lab experiments studying discrimination against outgroups and cooperation with the ingroup (see McDonald, Navarrete, and Van Vugt 2012 for a review). Our results confirm that the bias in grading is indeed driven almost entirely by male examiners. Female examiners exhibit little if any religiosity-based discrimination.

To further zoom in on the source of the bias, we examine the grade distributions. While the difference-in-differences analysis can detect ingroup bias, it does not identify the source of this discriminating behaviour. However, using the existence of bunching of test scores at important test score thresholds (the failing mark and the perfect 100 grade), we can test separately for religious and secular examiners, if the likelihood of just crossing the threshold is higher when the student is religious rather than secular. The results suggest that the main source of the

discriminating behaviour is the (male) religious examiners. Male religious examiners account for only 5% of the examiners in our sample.

Another possible reason for the small overall bias is that, for the most part, secular and (non ultra-orthodox) religious Jews live and work in the same places. In line with inter-group contact theory, we find that religiosity-based discrimination might indeed be affected by exposure to people from other groups: in our case, people with a different level of religiosity. We examine several measures of exposure both at the community level (the neighbourhoods where the examiners live) and at the workplace level (the schools where they teach). We also find that religiosity-based discrimination is much more pronounced among the 3% of examiners who teach in segregated religious communities.

The paper relates most directly to the literature on the economics of religion which has studied the effects of religiosity and secularization at both the national and individual levels (Iyer 2016 provides a review). At the individual level, the literature has focused on such outcomes as income, education, and health-related behaviour (Gruber and Hungerman 2008; Bryan, Choi, and Karlan 2018). Our analysis provides an important complement: while religiousness may have positive (or negative) effects relative to secularism, the cleavage itself might also have important implications as it can generate prejudice and discrimination, leading to bad allocations.

A second related literature studies discrimination and ingroup bias in settings such as the labour market and law enforcement (see Charles and Guryan 2013, Bertrand and Duflo 2017, and Shayo 2020 for reviews). We contribute to this literature in three important ways. First, we study discrimination along a very salient but little-studied dimension, namely, religiosity. Since we focus on a population with a similar ethnic and cultural background, we are able to isolate discrimination which is based on individuals' religious beliefs. Second, we study discrimination in the school system, which can have long-term implications for professional development and

lifetime earnings.[3] Third, we provide evidence on inter-group contact theory, which has received increasing attention from economists in recent years (see Bertrand and Duflo 2017).

Finally, there are both theoretical and empirical reasons to think that men are more prone than women to discriminate between ingroup and outgroup (see for example Balliet, Wu and De Dreu 2014). The argument is that male humans have evolved a specialized psychology that strengthens inter-group discrimination (e.g. Sidanius et al. 2000; Van Vugt, Cremer and Janssen 2007; Navarrete et al. 2010). However, most of the evidence in this literature is based on lab experiments and survey data, and the economics literature has not yet converged on a clear verdict. Our analysis suggests that the stronger male tendency for ingroup bias noted by psychologists might extend to professional, high-stakes decisions. At the same time, one cannot rule out the possibility that this gender difference is due to Israeli men being more firmly religious than Israeli women (Schnabel et al., 2018).

2. Institutional Background

We focus in this study on Israeli schools in the academic track where the language of instruction is Hebrew. The vast majority of students in these schools are Jewish. These public schools can belong to two distinct sectors, according to level of religiosity. "State schools" are secular and serve the secular Jewish population. "State-religious schools" serve mainly the religious Jewish population, observe religious practices, emphasize religious teachings and in some subjects follow a different curriculum. Students receive a matriculation certificate (a prerequisite for university admission) after passing a series of state exams administered mostly at end of twelfth grade. Some subjects are mandatory and at least one elective is required at an advanced level. Religious and secular schools share over half the matriculation test questionnaires.

[3] The literature on the economics of education often uses teachers' grading biases as a measure of discrimination. For example, Lavy (2008), Björn, Höglin, and Johannesson (2011), Burgess and Greaves (2013), Diamond and Persson (2016), Lavy and Sand (2015), and Terrier (2016) use the systematic difference between non-blind and blind assessment across groups as a measure of such discrimination.

The final matriculation score in a given questionnaire is the mean of the test scores in a school-level ("internal") exam and a national ("external") exam. The latter is graded independently by two examiners, randomly assigned by a computer algorithm.[4] Furthermore, the computerized process sends all exam booklets that were distributed in a specific classroom to the same two examiners. The final external score is the average of these two examiners' evaluations.[5]

The external exam booklets do not reveal a student's identity to the grader: they only include the student's ID number and school code. Nonetheless, most religious Jews write a special inscription—BS"D—at the top of every written document. Since the presence (or absence) of this inscription is very conspicuous, the level of religiosity of the student is effectively revealed to the examiners.[6]

3. Data and Descriptive Statistics

Our data include all matriculation questionnaires taken in the summer session in schoolyears 2010–2015 by Jewish students in the state education system (both religious and secular). We start with the matriculation test scores database. Each matriculation test score record contains student, school, and class identifiers, as well as the grade, questionnaire number, number of credit units, scores given by the first and second examiners, and the school-level ("internal") score. Importantly, we have identifiers for both examiners. We merge the matriculation exam record with the student database of the same year to obtain student characteristics (grades, class and school assignment and school zip code, gender, ethnicity, number of siblings, and parents'

[4] Examiners are teachers who had taught the subject of the exam for at least four years during the five years before the exam took place. They must hold an academic degree and a teaching diploma in the subject of the exam, and pass an interview with the subject's educational supervisor (see Ministry of Education Director's circular No. 4.3-34).

[5] If the difference in grades between the two examiners exceeds a certain threshold (either in the total score or in specific questions), the exam is sent to a senior examiner who makes a final decision. There are, however, no sanctions on the examiners in such cases.

[6] The inscription *BS"D* is an acronym for *Besiyata DiShmaya*, an Aramaic phrase meaning "with the help of heaven." Religious Jews write this inscription (or a variation thereof) at the top of the first page of every written document. See the online Appendix for more detailed discussion and examples of notebooks with the BS"D notation. Note that in addition to the inscription, examiners can, in principle, look up the school code and find out whether the school is religious or not.

education). Merging the data with the school file allows us to determine student religiosity according to their schools' religious orientation.

All examiners are teachers. We can thus obtain information on examiners from teachers' files for the years 2000–2015. This includes main field of instruction, main school assignment, gender, number of children, age, education and ethnicity. Examiners are defined as religious if at least one of their children attended a religious school. To determine examiner religiosity, we construct a new database that contains each parent who had a child enrolled in high school during 1998–2016. Using this database, we can determine the level of religiosity of about 85% of the examiners in our sample.[7] We also develop several measures of examiners' exposure to different environments, which we describe below when discussing the contact hypothesis. The online Appendix provides a detailed description of the construction of the database.

Tables A3 and A4 in the appendix present descriptive statistics at the student level and test level respectively. The total number of students who took at least one summer exam in Hebrew during 2010–2015 is 423,002 students. One-quarter of these students came from religious schools. The proportion of girls and the number of siblings are both higher among religious students (the proportion of girls is 62% versus 51% and the average number of siblings is 2.25 versus 0.9). Other characteristics are similar across sectors. With respect to test scores, secular and religious students have similar external test scores on average (70.5 versus 70), as well as similar probabilities of passing the exam.

Our sample includes 2508 examiners, 83% of whom are female and one-third are religious. Of the religious examiners, one third are Ultra-Orthodox and about 13% teach at schools located in segregated religious areas (religious settlements). Overall, secular and religious examiners have similar observed characteristics. See Appendix Tables A5. Appendix Tables A6 and A7 provide additional descriptive statistics on examinations. The dataset includes around 2 million exam booklets, from one thousand schools. The number of exam booklets graded by an examiner

[7] A series of balancing tests finds that students assigned to examiners with missing religiosity do not differ significantly from other students. See Appendix Table A2.

is on average 1650. The mean number of booklets per student is 4.88 (std.=2.77) and the total number of questionnaires is 112.

4. Identification and Estimation

To identify religiosity-based ingroup bias, we rely on the random assignment of students' exam booklets to examiners within a given questionnaire. To evaluate this identifying assumption, we test whether booklets assigned to religious examiners were systematically different from booklets assigned to secular examiners within a given questionnaire-year, in terms of a host of student characteristics. Appendix Table A8 presents the results for all examiners, and separately for male and female examiners. Except for one case, none of the estimated differences are significantly different from zero. These balancing tests confirm that the computer algorithm that assigns exam booklets is indeed random with respect to examiner religiosity.

Consider the following benchmark difference-in-differences specification:

(1) $$y_{bijqt} = \alpha_0 + \alpha_1 ReligStudent_i + \alpha_2 ReligExaminer_j + \alpha_3 ReligStudent * ReligExaminer_{ij} + \beta_i + \gamma_q + \delta_t + \varepsilon_{bijqt}$$

where y_{bijqt} is the outcome (e.g., test score) of exam booklet b, written by student i, assigned to examiner j, in questionnaire q, in year t. $ReligStudent_i$ and $ReligExaminer_j$ are indicator variables for religious student and religious examiner. The baseline specification includes questionnaire (γ_q) and year (δ_t) fixed effects. We further include student fixed effects (β_i). ε_{bijqt} is an error term clustered within examiner (the level of treatment).

Equation (1) allows for two possible differences across religiosity groups that do not necessarily indicate religious bias. First, it is possible that exams written by religious students have different unobserved characteristics (including, but not limited to, different quality) than those written by secular students. Thus, α_1 may be nonzero even in the absence of religious bias. Second, it is possible that religious and secular examiners have different grading standards (e.g., religious examiners may be more lenient). In other words, α_2 may be nonzero even in the absence of religious bias. Examiner religious bias is captured by α_3. This coefficient reflects a difference-

in-differences: by how much religious examiners are more generous than secular examiners when grading an exam written by a religious student rather than a secular one.

5. Results

Table 1 shows baseline results. The unit of observation is an exam booklet graded by a particular examiner. The dependent variable is the (normalized) score. The number of observations is twice the number of exam booklets, since each booklet is graded by two different examiners.

Before estimating equation (1), columns 1 and 2 estimate separately for religious and secular examiners, the difference in grades given to religious versus secular students, controlling for questionnaire and year fixed effects. Both religious and secular examiners give lower grades to religious students, but the difference is larger among secular examiners. Column 3 estimates equation (1) but for comparability only includes questionnaire and year fixed effects. Religious students' test scores are lower by 0.05 of a SD and religious examiners are marginally more generous. The ingroup bias estimate is reported in the third row and equals 0.011, which is the difference between the two religious student indicators' estimates in the first two columns. Thus, test scores are on average higher by 1% of a SD when the exam booklet is assigned to an examiner with similar religiosity to the student. This effect is quite small and statistically insignificant.

In column 4 we add student fixed effects (hence the religious student indicator drops out). The estimated ingroup bias remains unchanged though it is now statistically significant at p=0.077.[8] Column 5 further includes booklet fixed effects. The estimated ingroup bias is slightly smaller and much more precise. This last specification captures within-booklet differences in test scores given by examiners of a different religious orientation than both types of students. However, once we stratify the sample to different subgroups, we will not be able to perform this more demanding estimation strategy.

[8] Appendix Table A9 presents estimations of ingroup biases based on raw test scores, instead of standardized scores. The magnitude and significance of the estimated ingroup bias align with the results in Tables 1.

In Appendix Table A10 we report results of a similar exercise when restricting attention only to examiners who send *all* their children to one type of school (religious vs secular). This provides a sharper contrast between secular and religious examiners though based on a smaller sample. As one might expect, the results indicate larger ingroup bias for both male and female examiners compared to Table 1. However, the estimated overall ingroup bias in our preferred specification in column 4 remains small at 0.015 (SE=0.007).

Why is the effect so small? The psychology literature suggests that group biases are less common among females. As the last two columns of Table 1 show, the estimated ingroup bias of male examiners is 0.030 (se=0.015), three times larger than the average effect shown in column 3. Female ingroup bias is much smaller and not significantly different from zero. This is consistent with patterns seen in lab experiments studying ingroup favouritism (Balliet et al. 2014), but emerges here on a much larger scale and concerning decisions that have important lifetime implications.

One concern when interpreting α_3 is that it might capture differential treatment by religious and secular examiners of some other student characteristic, rather than their religiosity. In Table A11 we augment equation (1) with interaction terms of religious examiner and various student characteristics (gender, mother's and father's education, number of siblings, parental country of birth). The ingroup bias estimate is stable and virtually unaffected by the inclusion of the additional interaction terms. Furthermore, six of the eight additional interaction terms are not statistically significant. Thus, our estimated ingroup bias does not appear to capture omitted interaction bias due to examiners favouring other student characteristics.

Another concern is that religious and secular examiners may grade a given booklet differently because they differentially like a particular feature in it, for example, the student's way of reasoning. In other words, we might be capturing a coincidence of taste or style between the student and the examiner and not religiosity-based discrimination by examiners. Appendix Table A12 splits the sample to STEM and non-STEM subjects. The latter include social studies and humanities, where the examiner might be more prone to bias grades because of writing style or

expressed views. In STEM subjects the correct answer tends to be more definitive. However, the estimates of ingroup bias are very similar: 0.012 in STEM and 0.010 in non-STEM subjects.

5.1 Final Matriculation Outcomes and a Placebo Exercise

Table 2 (column 1) estimates the impact of ingroup bias on the *external score* (the average score of the two examiners). The treatment is the proportion of religious examiners for each exam booklet (zero, 0.5, or 1) times the indicator of religious student. The estimated ingroup bias is 0.02 standard deviations (statistically significant). When the treatment indicator is equal to 0.5 (one of the two examiners is of the same religiosity as the student), the ingroup bias effect is equal to 0.01, the respective estimate that we report in Table 1.

Recall that the *internal score* is provided by student's own teacher. Since it is filed prior to the external exam, it serves as a useful placebo outcome. As seen in column 2, the treatment effect on the internal grade is an order of magnitude smaller and not significantly different from zero. This supports the identification strategy in Table 1.

Column 3 reports the impact of ingroup bias on the final grade, which is an average of the external and internal scores. This estimate is 0.010 (se=0.004), close to the average of the estimates reported in columns 1 and 2.

Columns 4–6 in Table 2 examine the likelihood of passing the exam (the mean probability of passing in the sample is 89%). Overall, the probability of passing a matriculation exam increases by 0.005, or half a percentage point, when both examiners share the student's religiosity. Columns 5-6 break this effect by student background. Not surprisingly, students from high-education families are unaffected, since they have a much lower likelihood of being at the margin of failing or passing a matriculation exam. Among students from low-education families, however, the point estimate is 0.009 (relative to a mean probability of 0.83 of passing an exam in this group).

5.2 Sources of Bias: Evidence from Test Score Bunching

Figure 1 shows the distribution of test scores by examiner and student religiosity. In all distributions, we observe substantially larger mass at two points in the distribution: at 55, the passing score in a matriculation exam, and at 100, the highest score possible in these exams. This bunching can be viewed as evidence that examiners systematically adjust grades to be just enough to pass the exam or, for the best students, to get a perfect score. In this section we exploit these adjustments to explore the sources religiosity-based discrimination.

As in our baseline regressions, we continue to allow religious examiners to systematically display more (or less) of this bunching behaviour. We also allow religious students to systematically receive more (or less) of these upward adjustments.[9] In the regressions reported in columns 1–3 of Table 1, we cannot infer that religious students receive lower grades unjustifiably, as they may be systematically weaker. However, being more (or less) likely to receive an upward adjustment might indicate general discrimination against one group, beyond any preference for one's own group.

Table 3 focuses on the passing grade threshold, noting that examiners may push up a grade within a close range of the passing grade and not necessarily from 54 to 55. Panel A estimates a variant of equation (1) where the dependent variable is the probability of passing the exam (a grade of at least 55). We estimate these regressions using four different subsamples according to test scores, beginning with all exam booklets with test scores between 50 and 60 and then looking at narrower intervals: [54,60], [54,57], and [54,56]. Each column is a separate regression that includes questionnaire fixed effects.

Notice first that we find little consistent evidence of general discrimination in favour (or against) religious students (first row). The ingroup bias estimates are consistently positive but are largely restricted to male examiners. Ingroup bias among male examiners seems particularly large when focusing on the two ranges closest to the passing threshold: the likelihood of "bumping" a student from one's religious group from 54 to 55/56 or from 54 to 55-57 is 4.3 and

[9] This may be due to a general bias for or against one of the groups, but in the case of the bunching at 100, it might in principle also be due to one group having a higher proportion of students who write outstanding exams that get censored at 100. However, as we will see below, religious students have the same likelihood as secular students to score 100 rather than any score in the range 90-99.

3.2 percentage points higher (p= 0.101 and p=0.077), respectively. This effect is sizeable and is equivalent to about 5–6% of the mean passing rate in the whole sample. By contrast, the estimated ingroup bias of female examiners in these two ranges is zero.

Panel A of Table 4 repeats this exercise at the margin of scoring 100, restricting the sample to test scores within the following ranges: [90,100], [95,100], [98,100], and [99,100]. Importantly, there is no evidence that religious students are overall more likely to receive a grade of 100 rather than any grade in the 90-100 range (first row of first column). Furthermore, we again observe sharp differences in ingroup bias between male and female examiners. When looking only at exams graded by men, the likelihood of getting 100 versus 99 is almost 11 percentage points higher when the exam is assigned to an examiner of the same religiosity as the student. Strikingly, ingroup bias estimates among the female examiners in all four ranges are zero.

Before continuing, it is important to note that the overall ingroup bias we documented in the previous sections is not limited to these ranges. The ingroup bias estimate (in the preferred specification in column 4 of Table 1) remains 0.10 (se=0.06, p=0.096), even when we remove from the sample test scores in the ranges 55–60 and 95–100.

The difference-in-differences estimate we have been studying so far is a *relative* measure of ingroup bias. We cannot tell whether the source of discriminating behaviour is the secular or religious examiners. The difficulty is due to the lack of an objective test score for each exam. It may be the case that secular students perform better on exams and hence the extent to which secular examiners give them higher grades is not an indication of a bias. In this example the bias is entirely due to religious examiners. But, of course, the reverse is also possible and the bias might be entirely due to the secular examiners. This limitation is common in studies that attempt to identify ingroup bias in naturally occurring (non-experimental). For example, Shayo and Zussman (2011) detect ingroup bias among Arab and Jewish judges in Israel, but absent an objective measure of the "correct" outcome, cannot definitively determine whether the bias is driven by Jewish or Arab judges (or both). Similarly, Anwar Bayer and Hjalmarsson (2012) find that in Florida, the presence of a member of one's race in the jury pool entails a better outcome

for the defendant, but again cannot pin down the source of the bias. Here, we propose a simple way to help address this limitation.

Our approach is based on test score bunching. We examine whether the likelihood of increasing test scores above the failing grade or to the 100 score is higher among, say, religious examiners when they grade exam booklets of religious students versus secular students. Note that while secular and religious students may well write different quality exams on average, it is less likely that they systematically vary in the likelihood of writing an exam worth 99 versus 100 (or 54 versus 55). This allows us to test for discrimination separately for secular and religious examiners in these ranges.

Panels B-C of Table 3 focus on the probability of passing the exam. The dependent variable is an indicator for scoring 55 or higher and the main explanatory variable is a dummy for religious student. Consider first the male examiners. Among secular examiners (panel B), the coefficient on religious student is negative in all four columns, consistent with discrimination against religious students. However, all the estimates are imprecisely measured and, for the most part, are not statistically different from zero. At the same time, the estimated coefficients for *religious* examiners (panel C) are all positive, implying a pro-religious student bias, but again only one of the estimates is statistically different from zero (0.024, se=0.015, p=0.1). Note that the difference between the estimated pro-religious bias of the religious and secular examiners gives us the ingroup bias reported in panel A.

The patterns of bias towards the best students are sharper (Table 4, panels B-C). For male religious examiners the estimates in columns 5-8 are positive, high, and significant. For male secular examiners, they are negative as expected, but much smaller, and mostly insignificant. The bias toward religious students among male religious examiners is especially large in the 99–100 range. The probability of a score of 100 is higher by almost 10 percentage points when it is a religious student. The respective bias of the secular males is much lower at 0.017 (se=0.021). Clearly, the religious examiners drive most of the ingroup bias at this bunching of test scores.

Turning to female examiners, columns 9–12 of both Table 3 and 4 show little evidence of bias among either the secular or the religious examiners. In other words, the lack of overall bias

among women in Tables 1 is unlikely to be masking differences between religious and secular women (e.g., due to ingroup bias in one group and out-group bias in the other). This lends support to the argument that gender differences in intergroup relations is not limited to a particular culture.

An intriguing question about the nature of the discrimination of male religious examiners is whether they increase the grades of students from their own group ("ingroup love") or whether they lower the grades of students from the other group ("out-group hate"). The surplus mass at test scores 55 and 100 and the "hole" in the test score distribution at 54 and 99 suggest that male religious examiners inflate test scores of religious students and do not lower test scores of secular students. This is consistent with Feld, Salamanca, and Hamermesh (2016) who find that examiners' favouritism toward their own group, rather than discrimination against the outgroup, explains relative ingroup bias by nationality and by gender.

In Appendix Tables A13 and A14 we report results on the variation in bias by examiner characteristics. Most interesting are the results on differences across religious orientation *within* the religious group. The results in Appendix Table A14 (column 1) indicate that religious bias of Ultra-Orthodox examiners is small and not significantly different from zero. This result is consistent with the often-expressed opinion that Ultra-Orthodox Jews do not view the Religious-National Jews (who attend the state schools) as "truly" religious. Hence, they do not treat them as part of their ingroup.

5.3 Does Ingroup Bias Decline with Exposure to the Out-Group?

In this section, we examine whether religiosity-based discrimination declines with examiners' exposure to people of different levels of religiosity at home (the neighbourhood where they live) and at work (the school where they teach). Since many secular and (non-ultra-orthodox) religious Jews live and work in close proximity to each other, this might help further explain the

surprisingly small effects of religious ingroup bias documented above.[10] It is also an interesting question in its own right. A long line of literature dating back to the 1940s suggests that intergroup contact can reduce intergroup prejudice (Pettigrew and Tropp 2006). However, Paluck et al. (2019) argue that this literature tends to rely on young participants (less than 25 years old) and to focus on very short-term outcomes (typically measured on the day of the intervention). In this sense, our setting offers a valuable addition to the literature. We should stress, however, that we do not have random assignment of peers and hence the analysis in this section should be taken as suggestive.

We construct several measures of exposure to the outgroup at school. The teacher database contains information on all teachers in each school, including their demographic information and main fields of study. Since all examiners are teachers, merging it with parents' files enables us to compute for each examiner in a given year: (1) the proportion of peers at school from a religious background; (2) the proportion of peers at school from a religious background who teach the same subject as the examiner; and (3) the proportion of peers at school from a religious background who have the same gender.

Similarly, we compute a geographical measure of examiners' exposure to other religious outgroup each year in their neighbourhood, using the proportion of religious/secular students within the examiners' zip code. We use students' and teachers' neighbourhood zip codes received from the Ministry of Education which enable to characterize for each teacher's zip code in a given year the proportion of students who attended religious schools, and merge it with teachers' files for the relevant year. We provide the full regression results in the Appendix, and summarize the results here.

Start by looking at examiners who teach in segregated religious localities. Ninety percent of the Jewish settlements in the West Bank are such communities, and three percent of the examiners teach in one of them. In Appendix Table A14 (column 2) we augment equation (1) with

[10] Thus, on average 19% of the children in the same zip-code as the examiner have a different level of religiosity. In particular, for religious examiners, 30% of the children in their neighbourhood are secular. See Appendix Table A15

interactions with an indicator for religious examiners who teach in a religious settlement. The results suggest that ingroup bias of examiners from religious communities in the West Bank is about four times larger than the mean effect of 0.01.

Next, we use four different definitions of exposure, measured in two environments: the neighbourhood in which one lives and the school in which one works (Appendix Table A15 provides descriptive statistics and Tables A16 and A17 report the results.[11] Specifically, we look at:

a) neighbours within the examiner's home zip code;
b) peers (other teachers) at school;
c) peers (other teachers) at school who teach the same subject;
d) peers (other teachers) at school with the same gender.

We then augment equation (1) with the proportion of neighbours or peers in the environment with a different level of religiosity, fully interacted with the religiosity variables (and in particular with $ReligStudent * ReligExaminer_{ij}$). The regressions include year and student fixed effects as well as, importantly, examiner by environment (zip code or school) by questionnaire fixed effects. Thus for example, in case (a), the interaction picks up the variation in ingroup bias for a given examiner living in the same neighbourhood, whose neighbourhood's religious composition changed over time.

The estimates for male examiners suggest that ingroup bias declines sharply when examiners encounter a higher proportion of the outgroup in their neighbourhood. Ingroup bias is positive and quite large (estimated at 0.064, se=0.021) when the examiner is exposed to below-median proportion of neighbours with different religiosity, but drops to zero when the examiner is highly exposed to the other group in the neighbourhood. Male ingroup bias is also associated

[11] In Appendix Table A16 exposure is measured as a dummy variable indicating an above-median proportion of neighbors or peers in the environment with a different level of religiosity, while in Appendix Table A17 exposure is measured in continuous terms, as the proportion of neighbours or peers with a different religious orientation.

with changes in exposure to "others" at work, especially to teachers who teach the same subject or are of the same gender.

For female examiners, the estimates suggest an interesting pattern. The main ingroup bias in all four cases is small and not significantly different from zero. However, ingroup bias appears to emerge among female examiners when they are in the minority in terms of religiosity at school, and in particular among female teachers at school. This is inconsistent with a simple version of the contact hypothesis that ignores the importance of the conditions under which contact takes place.

6. Conclusions

While secularization—and its opposite, resacralization—have drawn enormous attention, the economic effects of religiosity-based discrimination have gone largely unnoticed. Using data from Israel's high-stakes matriculation exams we are able to identify the level of religiosity of both students and examiners, and thus study discrimination across religious and secular members of the same ethno-religious group. This allows us to disentangle religiosity-based from ethnic discrimination.

We have five main findings. First, we document the existence of ingroup bias in grading decisions. This bias is detectable among professional graders who are making highly consequential decisions. Second, the bias is, overall, very small, amounting to about 0.01 of a standard deviation in the grade. Third, looking at the sources of the bias, we find that it is almost entirely driven by male examiners: female examiners (who constitute over 80% of the examiners) show little if any bias. Fourth, using bunching in the grading distribution we find evidence that bias, at least at the top of the distribution, is largely driven by male religious examiners. Male religious examiners are six to ten percentage points more likely to bump a grade to 100 when the exam is written by a religious student, while male secular examiners are between one and three percentage points less likely to do so when grading a religious student. Finally, we find suggestive evidence that contact across religious and secular groups may attenuate these biases.

While our setting offers a rather unique opportunity to study religiosity-based discrimination using large administrative datasets, the basic idea could be replicated in an experimental setting by randomly revealing to examiners the religiosity of some of the students, using culturally-relevant cues. This could be done in different countries, at different phases of the secularization-resacralization process.

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Table 1: Religiosity-based Discrimination in Test Scores

	Religious Examiners	Secular Examiners	All Examiners			Male Examiners	Female Examiners
	Questionnaire and Year Fixed Effects	Questionnaire and Year Fixed Effects	Questionnaire and Year Fixed Effects	Questionnaire, Year and Student Fixed Effects	Booklet Fixed Effects	Questionnaire, Year and Student Fixed Effects	Questionnaire, Year and Student Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Religious Student	-0.041	-0.051	-0.051				
	(0.010)	(0.007)	(0.006)				
Religious Examiner			0.019	0.011	0.014	0.017	0.011
			(0.007)	(0.005)	(0.004)	(0.013)	(0.0060)
Religious Student x Religious Examiner			0.011	0.010	0.008	0.030	0.010
			(0.012)	(0.006)	(0.003)	(0.015)	(0.006)
Number of Observations	1,201,625	2,388,491	3,590,116	3,590,116	3,590,116	508,324	3,081,792
Number of Examiners	715	1,400	2508	2508	2508	431	2,064
Proportion of Examiners	0.338	0.662	1.000	1.000	1.000	0.173	0.827

Notes: The first two columns of the table present the difference in grades given to religious and secular students, separately by religious (column 1) and secular examiners (column 2). The estimates of the religious student indicator are from a specification that includes questionnaire and year fixed effects. The next four columns present the difference-in-differences ingroup bias estimates, from different specifications: in column 3 the specification includes only questionnaire and year fixed effects; in column 4 the specification includes also student fixed effects; and the last specification includes only booklet fixed effects. The next two columns present the difference-in-differences ingroup bias estimates from the preferred specification that includes questionnaire, year and student fixed effects, separately for male and female examiners. The number of observations is twice the number of booklets, since each booklet appears twice (once for each examiner). The numbers and proportions of examiners by gender and religiosity are presented in the last two columns. Dependent variables are standardized scores. Standard errors are corrected for examiners clustering and are presented in parentheses.

Table 2: Ingroup Bias in Related Exam Outcomes

	Average External Exam Grade	Internal Exam Grade: Placebo Test	Average Final Exam Grade	Probability of Passing the Exam		
				All Students	Students with Low Parental Education	Students with High Parental Education
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion of Religious Examiners	0.005	0.000	0.003	-0.002	-0.003	-0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Religious Student x Proportion of Religious Examiners	0.020	-0.002	0.010	0.005	0.009	0.001
	(0.005)	(0.005)	(0.004)	(0.002)	(0.004)	(0.002)
Number of Observations	1,565,252	1,535,550	1,535,550	1,535,556	627,818	883,892

Notes: The table presents the estimated effect of ingroup bias of examiners on additional outcomes: 1) the average external exam grade (the average of the two examiners' normalized scores); 2) the normalized internal exams, which are exams examined by students' school teachers; 3) the final exam score (the average of the external and internal exams' normalized scores); 4) probability of passing the exam (if final grade >=55); 5) probability of passing the exam from a subsample of students with low parental education (low parental education is equal to one if both parents have 12 or less years of schooling); 6) and the probability of passing the exam from a subsample of students with high parental education. The proportion of religious examiners is measured in each exam booklet. The number of observations is the number of booklets, since each booklet appears only once. All columns present the results from separated regressions based on the preferred specification (which includes year, questionnaire, and student fixed effects). Standard errors are corrected for clustering at the student level and are presented in parentheses.

Table 3: Ingroup Bias in the Probability of Passing the Exam, by Examiner Gender, Religiosity and Test Score Range

	All Examiners				Male Examiners				Female Examiners			
	[60,50]	[54,60]	[54,57]	[54,56]	[60,50]	[54,60]	[54,57]	[54,56]	[60,50]	[54,60]	[54,57]	[54,56]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All Examiners												
Religious Student	-0.009	-0.001	0.001	-0.001	-0.012	-0.006	-0.007	-0.016	-0.008	0.000	0.003	0.002
	(0.002)	(0.002)	(0.004)	(0.005)	(0.007)	(0.060)	(0.011)	(0.014)	(0.003)	(0.002)	(0.004)	(0.006)
Religious Examiner	-0.008	0.002	0.001	-0.004	-0.017	-0.010	-0.028	-0.054	-0.007	0.004	0.005	0.003
	(0.003)	(0.003)	(0.007)	(0.009)	(0.011)	(0.012)	(0.022)	(0.028)	(0.004)	(0.004)	(0.007)	(0.009)
Religious Student x Religious Examiner	0.006	0.007	0.010	0.009	0.012	0.017	0.032	0.043	0.005	0.005	0.006	0.002
	(0.004)	(0.004)	(0.007)	(0.009)	(0.012)	(0.010)	(0.018)	(0.026)	(0.004)	(0.004)	(0.007)	(0.010)
Number of Observations	371,094	255,779	127,998	84,110	51,394	42,279	18,070	11,722	319,700	220,236	109,028	72,388
B. Secular Examiners												
Religious Student	-0.009	-0.001	0.001	-0.001	-0.012	-0.006	-0.007	-0.016	-0.008	0.003	0.003	0.002
	(0.002)	(0.002)	(0.004)	(0.005)	(0.007)	(0.006)	(0.011)	(0.014)	(0.003)	(0.004)	(0.004)	(0.006)
Number of Observations	250,814	173,779	87,446	57,752	33,476	23,929	11,996	7,862	217,338	150,487	75,450	49,890
C. Religious Examiners												
Religious Student	-0.003	0.006	0.011	0.008	0.000	0.011	0.024	0.027	-0.003	0.005	0.009	0.004
	(0.003)	(0.003)	(0.006)	(0.008)	(0.010)	(0.008)	(0.015)	(0.022)	(0.004)	(0.003)	(0.006)	(0.008)
Number of Observations	120,280	82,000	40,552	26,358	17,918	12,251	6,074	3,860	102,362	64,740	34,478	22,498

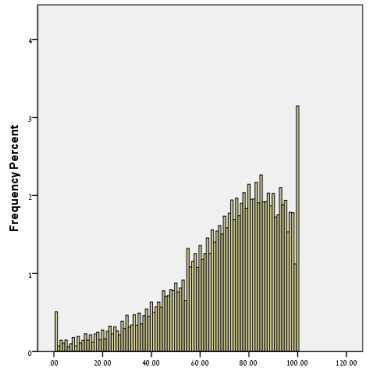
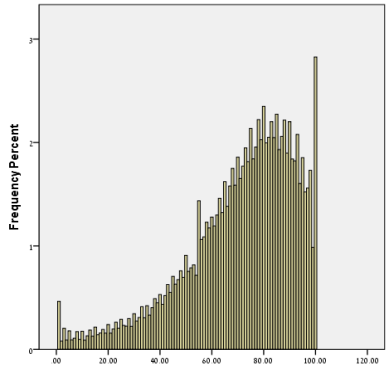
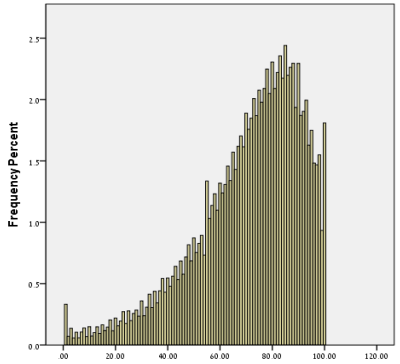
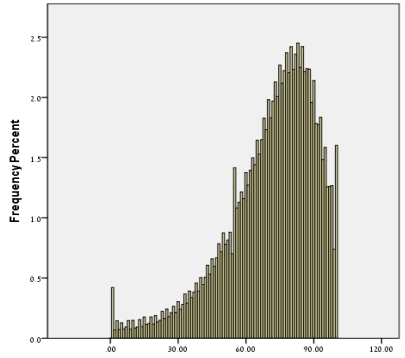
Notes: The dependent variable is the probability of passing the exam (if score>=55). The coefficients in each column are from separated regressions that include questionnaire fixed effects, for four different subsamples: in the first column the subsample includes all tests with scores between 50 and 60; in the second column the subsample includes all tests with scores between 54 and 60; in the third column the subsample includes all tests with scores between 54 and 57; and in the last column the subsample includes all tests with scores between 54 and 56. Panel A includes all examiners and Panel B (Panel C) includes only secular (religious) examiners. Standard errors are corrected for clustering at the examiner level and are presented in parentheses.

Table 4: Ingroup Bias in the Probability of Scoring 100, by Examiner Gender, Religiosity and Test Score Range

	All Examiners				Male Examiners				Female Examiners			
	[90,100]	[95,100]	[98,100]	[99,100]	[90,100]	[95,100]	[98,100]	[99,100]	[90,100]	[95,100]	[98,100]	[99,100]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All Examiners												
Religious Student	-0.001	-0.008	-0.023	-0.002	-0.006	-0.011	-0.035	-0.017	0.001	-0.006	-0.019	0.002
	(0.003)	(0.006)	(0.011)	(0.013)	(0.005)	(0.010)	(0.017)	(0.021)	(0.004)	(0.007)	(0.013)	(0.014)
Religious Examiner	0.000	-0.005	-0.022	-0.025	-0.001	-0.015	-0.050	-0.070	-0.001	-0.004	-0.016	-0.012
	(0.005)	(0.009)	(0.016)	(0.018)	(0.016)	(0.029)	(0.049)	(0.053)	(0.005)	(0.009)	(0.015)	(0.018)
Religious Student x Religious Examiner	0.005	0.009	0.026	0.025	0.029	0.046	0.098	0.109	0.001	0.000	0.006	0.000
	(0.005)	(0.009)	(0.016)	(0.018)	(0.011)	(0.019)	(0.032)	(0.036)	(0.005)	(0.009)	(0.018)	(0.019)
Number of Obs.	557,641	243,970	105,919	68,332	89,101	42,158	20,001	13,894	468,540	201,812	85,918	54,438
B. Secular Examiners												
Religious Student	-0.001	-0.008	-0.023	-0.002	-0.006	-0.011	-0.034	-0.017	0.001	-0.006	-0.019	0.002
	(0.003)	(0.006)	(0.011)	(0.013)	(0.005)	(0.010)	(0.017)	(0.021)	(0.004)	(0.007)	(0.013)	(0.014)
Number of Obs.	361,929	156,690	67,505	43,667	55,150	25,384	11,863	8,233	306,779	131,306	55,642	35,434
C. Religious Examiners												
Religious Student	0.004	0.000	0.003	0.023	0.023	0.035	0.065	0.096	0.001	-0.007	-0.013	0.000
	(0.003)	(0.006)	(0.012)	(0.013)	(0.009)	(0.016)	(0.027)	(0.031)	(0.003)	(0.007)	(0.012)	(0.013)
Number of Obs.	195,712	87,280	38,414	24,665	33,951	16,774	8,138	5,661	161,761	70,506	30,276	19,004

Notes: The dependent variable is the probability of scoring 100 on the exam. The coefficients in each column are from separated regressions that include questionnaire fixed effects, for four different subsamples: in the first column the subsample includes all tests with scores between 90 and 100; in the second column the subsample includes all tests with scores between 95 and 100; in the third column the subsample includes all tests with scores between 98 and 100; and in the last column the subsample includes all tests with scores between 99 and 100. Panel A includes all examiners and Panel B (Panel C) includes only secular (religious) examiners. Standard errors are corrected for clustering at the examiner level and are presented in parentheses.

Figures: The Distributions of Scores, by Examiners' Gender and Religiosity and Students' Religiosity

A. Religious Male Examiners		B. Religious Female Examiners	
Religious students	**Secular students**	**Religious students**	**Secular students**
			
C. Secular Male Examiners		**D. Secular Female Examiners**	
Religious students	**Secular students**	**Religious students**	**Secular students**