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Bad Luck, Worse Grades: Superstition and Academic Performance

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Bad Luck, Worse Grades: Superstition and Academic Performance*

Abstract

Can a culturally salient belief affect performance even when nothing about the underlying task has changed? Using two decades of administrative records from the University of Modena and Reggio Emilia, we exploit the Italian superstition surrounding Friday the 17th. We find a robust penalty, with grades declining by about 3.5 percent of a standard deviation. Differences in exam scheduling or difficulty do not account for the effect, which is significantly larger among students predicted to be more susceptible to superstition. We find little evidence of anticipation before the unlucky date and suggestive evidence of compensation afterward in this university setting.

JEL classification

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Keywords

superstition, academic performance, cognitive performance, cultural beliefs, education

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1 Introduction

Economists have long studied academic performance as the product of structured and measurable inputs, including ability, family background, parental investment, school quality, and incentives (Agostinelli et al., 2025; Attanasio et al., 2020; Boneva and Rauh, 2018; Chetty et al., 2014; Cunha and Heckman, 2007; Cunha et al., 2010; Del Boca et al., 2014; Dizon-Ross, 2019; Levitt et al., 2016; Todd and Wolpin, 2003). Yet performance on a given exam may also respond to beliefs that contain no information about ability, preparation, or the objective difficulty of the task. This paper asks whether one such belief – superstition – can affect performance in a consequential setting. Specifically, we examine whether university students in Italy perform worse when an exam falls on *Friday the 17th*, a date traditionally associated with bad luck. Combining two decades of administrative records on exam outcomes with survey measures of superstitious belief, we test whether a culturally transmitted superstition can measurably affect exam performance.

Superstitions are objectively false beliefs (Fudenberg and Levine, 2006), yet they can persist when feedback is limited, falsification is costly, or beliefs serve psychological or social functions. Fudenberg and Levine (2006) show that even rational and patient learners may sustain false beliefs when the events needed to disconfirm them are rarely observed, while Nunn and Sanchez de la Sierra (2017) document how beliefs can endure when they provide collective benefits despite imposing individual costs. More broadly, superstitions may help individuals cope with uncertainty and regain a sense of control when outcomes feel unpredictable or threatening (Vyse, 2014). These mechanisms make superstition potentially consequential even when the belief itself has no objective informational content.

Number-related superstitions provide salient examples. In China, eight is widely regarded as auspicious and four as unlucky, while in many Western countries thirteen is associated with misfortune. Italy has its own distinctive numerical superstition: *Friday the 17th (venerdì 17)* is traditionally considered a day of bad luck. The association of 17 with misfortune is often traced to its Roman numeral representation, XVII, which can be rearranged to form *VIXI*, Latin for "I have lived," a phrase associated with death. Combined with the negative connotations traditionally attached to Friday, this makes *venerdì 17* the Italian counterpart to *Friday the 13th* elsewhere.

In this paper, we test whether this culturally salient superstition affects test scores. Using administrative records from the University of Modena and Reggio Emilia (UNIMORE) between 2003 and 2022, we find that students perform significantly worse when an exam falls on Friday the 17th. In our preferred specification, exam scores decline by 0.13 points, or approximately 3.5 percent of a standard deviation. The estimate is robust to student, course, and detailed calendar fixed effects, and remains similar when we account for instructor-specific factors and when we use a battery of different specifications. For scale, the estimated penalty is about 40 percent of the effect associated with the average cumulative exposure to COVID-19 restrictions estimated by Bonacini et al. (2023) using the same university and outcome.

Our identification relies on the interaction between the university's exam schedule and the occurrence of Friday the 17th in the calendar. We therefore examine whether exams or students exposed to Friday the 17th differ systematically from those that are not. Potentially exposed students differ only modestly in observable characteristics, and our specifications condition on these differences. Moreover, course, faculty, and instructor fixed effects add little predictive power for whether an exam is scheduled on Friday the 17th once calendar variation is accounted for. Placebo tests using the 17th falling on other weekdays also reveal no comparable decline in performance.

To assess whether superstition is the mechanism, we combine the administrative records with the European Values Study and an original survey of UNIMORE students we collected to construct predicted measures of individual superstition through probabilistic imputation. The Friday 17 penalty is concentrated among students with higher predicted superstition and is absent among those with lower predicted superstition. It is also absent among students born abroad, for whom this specifically Italian superstition should be less salient. Among students with the highest predicted superstition, the decline in exam performance approaches 10 percent of a standard deviation.

Finally, we use the longitudinal structure of the data to study behavioral responses around the unlucky date. We find little evidence of anticipation before Friday the 17th and suggestive evidence of compensating improvements afterward. We do, however, detect a significant decline in the probability of passing an exam at a session held on Friday the 17th, particularly at the first session. We further show that the students whose grades are not observed on Friday the 17th are negatively selected on prior academic performance, while those whose grades

we do observe are comparatively stronger. This selection implies that estimates based only on observed grades are likely conservative. Consistent with this interpretation, a censored-grade model that incorporates sessions without a recorded passing grade yields a substantially larger Friday the 17th penalty.

We contribute to the literature on superstition and economic behavior by showing that a culturally specific, situational superstition can affect performance in a consequential setting. Existing work has examined whether persistent or identity-based superstitions, such as zodiac or birth-year beliefs, shape parental investment and educational outcomes (Wong and Yung, 2005; Mocan and Yu, 2020; Chou, 2022), while research on situational superstitions has documented responses in domains involving risk, housing, finance, accidents, life outcomes, and ritualistic behavior (Peltzer and Renner, 2003; Kramer and Block, 2008; Fortin et al., 2014; Zhang et al., 2014; Fidrmuc and Tena, 2015; Agarwal et al., 2016; Bhattacharya et al., 2018; Hirshleifer et al., 2018; He et al., 2020). We bring these strands together by studying a belief that is activated only by the coincidence of a consequential task with an unlucky date. This setting allows us to show that culturally transmitted beliefs can affect realized performance even when they contain no information about the task itself.

More broadly, we contribute to research on education and the determinants of academic performance. This literature has largely emphasized measurable inputs such as ability, family background, parental investment, school quality, and incentives, as well as subjective beliefs held by parents (Agostinelli et al., 2025; Attanasio et al., 2020; Boneva and Rauh, 2018; Chetty et al., 2014; Cunha and Heckman, 2007; Cunha et al., 2010; Del Boca et al., 2014; Dizon-Ross, 2019; Levitt et al., 2016; Todd and Wolpin, 2003). Our findings identify a different source of variation: culturally shared beliefs that are individually internalized and temporarily activated by the environment. They show that academic performance can respond to salient cultural cues that are unrelated to students' underlying ability or to the objective characteristics of the exam.

The rest of the paper unfolds as follows. Section 2 sets out a conceptual framework linking superstitious beliefs to cognitive performance. Section 3 documents the origins and the contemporary relevance of the fear of Friday the 17th in Italy. Section 4 describes the administrative data, the construction of the estimation sample and the way exams are scheduled. Section 5 presents the reduced-form effect of Friday 17 on exam scores, together with the robustness checks. Section 6 asks whether the penalty reflects exam difficulty or student superstition, us-

ing superstition scores imputed at the individual level. Section 7 examines behavior around the date, testing for anticipation before it and compensation after it. Section 8 concludes. Appendix A describes the two surveys and the imputation and validation of the superstition scores, Appendix B the construction of the estimation sample, Appendix C the selection into the exams we observe, and Appendix D collects additional figures and tables.

2 Conceptual Framework

Why should an unlucky date affect exam performance? There are two broad possibilities. The date may affect students' performance on the day of the exam, or it may be associated with differences in the difficulty of the exam they face. This section develops a simple framework that separates these two channels and derives the empirical implications that guide our analysis.

2.1 Superstition and exam performance

Consider student i taking exam e. Let Θ_{ie} denote the student's exam performance and D_e the difficulty of the exam. The observed grade is

$$G_{ie} = a + b\Theta_{ie} - cD_e, \qquad b, c > 0,$$

so that grades increase with student performance and decrease with exam difficulty.

Each exam takes place on calendar date $t(e)$. Let F_e be an indicator equal to one when a culturally salient superstition signal is present (e.g. an exam scheduled on Friday the 17th).

Student performance depends not only on ability and accumulated human capital, but also on the student's psychological state at the time of the exam. We summarize these short-run influences through a reduced-form mental-state variable M_{ie}:

$$\Theta_{ie} = A_i + \beta' X_{ie} + \phi M_{ie}, \qquad \phi > 0,$$

where A_i captures stable determinants of performance, such as ability, accumulated human capital, and study habits, while X_{ie} captures time-varying factors unrelated to superstition, such as health. The variable M_{ie} summarizes short-run psychological influences including

distraction, anxiety, confidence, attentional control, and intrusive thoughts.

Students differ in their susceptibility to superstition. Let $s_i \in [0, 1]$ denote student i's latent susceptibility, with larger values corresponding to a stronger tendency to attach meaning to superstition signals. We treat s_i as an exogenous and time-invariant characteristic. The purpose of the framework is not to explain why some students are more superstitious than others, but to characterize how a given level of susceptibility maps into performance when a salient signal is present.[1]

We assume that superstition affects performance only when activated by the signal:

$$M_{ie} = \bar{M}_i + u_{ie} - \lambda s_i F_e, \qquad \lambda > 0,$$

where $\bar{M}_i$ denotes the student's average mental state and u_{ie} captures transitory shocks. Substituting into the performance equation gives

$$\Theta_{ie} = \alpha_i + Z_{ie} - \kappa s_i F_e,$$

where $\alpha_i = A_i + \phi \bar{M}_i$, $Z_{ie} = \beta' X_{ie} + \phi u_{ie}$, and $\kappa = \phi\lambda > 0$. Exam performance therefore combines a time-invariant student component, time-varying factors unrelated to superstition, and a superstition channel that is activated when $F_e = 1$.

The second channel operates through exam difficulty. We allow

$$D_e = \bar{D}_e + \omega F_e,$$

where ω is unrestricted. If $\omega > 0$, exams associated with the superstition signal are more difficult; if $\omega < 0$, they are easier. Such differences could arise from instructor behavior or from institutional scheduling patterns.

Combining the two channels yields

$$G_{ie} = a + b\left(\alpha_i + Z_{ie} - \kappa s_i F_e\right) - c\left(\bar{D}_e + \omega F_e\right).$$

[1] In the empirical analysis, we show that susceptibility to superstition can be predicted using socio-demographic characteristics and exploit this variation to better understand the role of superstition.

2.2 Empirical implications

Because F_e is binary, the relevant object is the difference in grades between an exam with and without the superstition signal:

$$G_{ie}(1) - G_{ie}(0) = -b\kappa s_i - c\omega.$$

This expression highlights the two channels through which the signal may affect grades and yields three empirical implications.

First, the overall effect of the superstition signal is theoretically ambiguous. The student-side component, $-b\kappa s_i$, is weakly negative, while the exam-difficulty component, $-c\omega$, may be positive or negative depending on whether exams associated with the signal are easier or harder. The sign of the reduced-form effect is therefore an empirical question.

Second, among students with no susceptibility to superstition, $s_i = 0$, the student-side channel disappears:

$$G_{ie}(1) - G_{ie}(0) = -c\omega.$$

Any remaining difference in grades for such students must therefore operate through exam difficulty rather than through the superstition channel. This provides an empirical implication for distinguishing student-side responses from differences in the exams themselves.

Third, the effect of the superstition signal becomes more negative as susceptibility increases. From the expression above,

$$\frac{\partial[G_{ie}(1) - G_{ie}(0)]}{\partial s_i} = -b\kappa < 0.$$

The penalty associated with the signal should therefore be larger among students who are more susceptible to superstition. This heterogeneity provides an empirical implication of the student-side mechanism.

3 Heptadecaphobia and Superstition in Italy

We now turn from the general framework to the empirical setting of the paper: Friday the 17th in Italy.

3.1 The Origins of Friday the 17th

Unlike most Western countries, where Friday the 13th is traditionally regarded as unlucky, Italy associates bad luck with Friday the 17th. This belief, sometimes referred to as *heptadecaphobia*, is one of the most distinctive and persistent superstitions in Italian culture (Di Nola, 1993).

The origins of this belief are generally traced to the symbolism attached to the number seventeen. Written in Roman numerals as XVII, the number can be rearranged to spell *VIXI*, a Latin expression meaning "I have lived." Frequently found on funerary inscriptions, the phrase came to be associated with death and misfortune. Other historical interpretations emphasize religious and numerological traditions in which seventeen symbolized disorder or the disruption of harmony. While the precise origins of the superstition remain debated, historians generally agree that the association between the number 17 and bad luck has deep roots in Italian culture (Di Nola, 1993).

Friday itself also carries negative connotations. In Christian tradition, it is believed to be the day of Christ's crucifixion and has long been regarded as inauspicious for major undertakings. This belief survives in the well-known Italian proverb *"né di Venere né di Marte, non si sposa, non si parte, né si dà principio all'arte"* ("Neither on Friday nor on Tuesday should one marry, depart, or begin a new enterprise"), which advises against marrying, traveling, or beginning new projects on a Friday. The combination of Friday and the number 17 therefore merges two independent symbols of misfortune into a superstition that is both highly salient and distinctively Italian (Ruiu and Breschi, 2017).

3.2 Contemporary Relevance

For the purposes of this paper, the crucial question is not the historical origin of the superstition but whether it remains salient today. Contemporary evidence suggests that it does.

Italian media regularly highlight the recurrence of Friday the 17th, and the number 17 is often omitted from airline seats, hotel rooms, and other public settings, much as the number 13 is avoided in English-speaking countries. Additional evidence suggests that the superstition remains highly salient in contemporary Italy. Figure 1 uses Google Trends data to track searches for the terms "sfortuna" and "sfortunato" ("bad luck" and "unlucky") around occurrences of

Friday the 17th between 2012 and 2025. Search intensity exhibits a sharp spike on the day itself, reaching more than three times its average level within the surrounding time window. A strikingly similar pattern emerges in the United States around Friday the 13th, where searches for terms related to bad luck increase dramatically on the unlucky date (Appendix Figure D1). These patterns suggest that Friday the 17th occupies a place in the Italian cultural imagination comparable to that of Friday the 13th in English-speaking countries.

Importantly, the salience of Friday the 17th does not appear to be declining over time. Appendix Figure D2 plots the search intensity observed on each Friday the 17th in Italy and compares it to the corresponding pattern around Friday the 13th in the United States. In both countries, search activity remains persistently elevated throughout the sample period, with little evidence of a decline. If anything, the attention devoted to Friday the 17th in Italy appears remarkably stable over the last decade.

4 Data, Sample and Exam Timing

4.1 Data

The empirical analysis focuses on the University of Modena and Reggio Emilia (UNIMORE), a large Italian higher education institution comprising thirteen departments and covering a broad range of disciplinary fields. Within the Italian university system, UNIMORE primarily attracts students from the provinces of Modena and Reggio Emilia, while maintaining a national reach.

The study draws on *UNIMOREdata*, a comprehensive database developed within a dedicated institutional project. It integrates individual-level information on students derived from administrative records and several large-scale surveys, with coverage starting from 2001.

For the present analysis, we use a dataset obtained by merging four main administrative archives: i) the register reporting demographic characteristics of each enrolled student; ii) the archive containing yearly information on all UNIMORE courses attended; iii) the archive providing information on the instructor responsible for each exam (from the academic year 2015/16 onward); and iv) the archive listing all exams successfully completed by students. The latter component is particularly relevant, as it includes detailed information on each passed exam, such as the grade awarded and the exam date.

The analysis focuses on exams passed between January 2003 and December 2022.[2] The unit of observation is a passed exam, that is, a student sitting the exam of a course on a given date, and the outcome is the grade, which ranges from 18 to 30, with 30 cum laude coded as 31. Our records do not include unsuccessful sittings: students who fail an exam or decline the grade are therefore not observed at that sitting, and grades are observed conditional on passing. We return to this selection issue in Section 5.3.3 and in greater detail in Appendix C.

The instructor archive derives from the yearly course-offer files and reports, for each course, the instructor responsible for the exam, together with the instructor's gender, age, academic role, region of birth and whether the instructor belongs to the department that offers the course. When a course has more than one instructor, we retain the one who teaches the most hours.

From the student register and the enrollment archive, we use gender, age, region of birth, whether the student was born abroad, the type of secondary-school diploma, the degree level, the department, the year of enrollment and the years since the first exam session of the course. As a proxy for family income, we use the quartile of the university fee paid by the student within the year of enrollment, since fees at Italian public universities depend on the economic situation of the family.

Unsurprisingly, the administrative records contain no direct measure of superstitious beliefs. We therefore complement them with survey data and use a probabilistic imputation procedure to construct individual-level measures of susceptibility to superstition. Appendix A describes the survey data, the imputation procedure, and its validation in detail. We return to these data and measures in greater detail in Section 6, where we use them to investigate the role of superstition in driving the estimated effects.

4.2 Description of the Final Estimation Sample

We keep graded exams of single courses held between January 2003 and December 2022 by full-time students who enrolled by the age of 35, and exclude internship courses, programs taught at a distance and the weeks in which the university is largely closed. We then impose a small number of additional restrictions required for the empirical analysis, including removing singletons in the fixed effects used in the analysis. This leaves a final sample of 1,603,920

[2]Exams started to be reliably registered in the system in 2003. We exclude prior exams, for which the records are not yet reliable.

exams taken by 88,672 students (106,334 student careers) in 6,749 courses and 217 degree programs. Appendix B details each restriction and the number of exams it removes.

Table 1 describes the exam-level sample. The average grade is 25.8 (standard deviation 3.7), and 0.79 percent of the exams take place on a Friday the 17th. The average student is 22.9 years old at the exam; 54 percent of the exams are taken by women, 6 percent by students born abroad and, among Italian-born students, 71 percent by students born in the North-East, the macro-region of UNIMORE. Appendix Table B1 reports the same characteristics with one observation per student career: 11 percent of the 106,334 careers include at least one exam on a Friday the 17th.

As explained above, instructor information is available only from the academic year 2015/16 onward, so analyses that use these characteristics are conducted on a smaller subsample of 582,744 exams. Descriptive statistics at the exam and student level for this subsample are reported in Appendix Tables B2 and B3.

4.3 Exam Timing and Exposure to Friday the 17th

A natural question is whether exposure to Friday the 17th is driven primarily by the calendar or instead reflects some form of endogenous selection. Two possibilities are particularly relevant. First, exam dates themselves could be scheduled differently when Friday the 17th occurs, or particular courses or instructors could be systematically more or less likely to hold exams on that date. Second, the students who are potentially exposed to Friday the 17th could differ systematically from those who are not. Although it may seem unlikely that universities or instructors explicitly take superstition into account when scheduling exams, we consider that it is necessary to examine both sources of selection directly.

We begin with exam scheduling. Institutional qualitative information gathered from the University of Modena and Reggio Emilia indicates that exam dates are largely coordinated centrally, taking into account classroom availability, the broader exam calendar, and potential conflicts across exams, rather than being chosen unilaterally by individual instructors. Consistent with this institutional setting, we find no evidence that exams are unusually concentrated on, or avoided around, Friday the 17th. Figure 2 plots the average number of courses examined on each day of the month, separately for months with an exam on a Friday the 17th and

other months. Exam density on the 17th in months with a Friday the 17th closely tracks both neighboring days and the corresponding day-of-month profile in other months.

We further examine whether particular courses, faculties, or instructors are systematically more likely to hold an exam on Friday the 17th. We collapse the session calendar of compulsory courses (Appendix C) to one observation per course and exam date and regress an indicator for whether the exam falls on Friday the 17th on calendar fixed effects and, in turn, course, faculty, and instructor fixed effects. Once calendar variation is taken into account, these fixed effects add little incremental predictive power for whether an exam falls on Friday the 17th. Appendix Table D1 reports the corresponding estimates. Together, the institutional evidence and these scheduling checks provide little indication that Friday the 17th systematically affects which exams are held or who administers them.

We next examine whether students who are potentially exposed to Friday the 17th differ systematically from those who are not. We define a student as exposed if at least one session of a compulsory exam they had not yet passed was scheduled on a Friday the 17th. Appendix Table D2 shows that observable differences between exposed and unexposed students are generally modest, although some differences emerge across degree programs and demographic characteristics.[3] Given the size of the sample, even small differences are often statistically significant. Our empirical specifications therefore condition on these observable dimensions.

Overall, the evidence supports the interpretation of Friday 17 exposure as primarily a calendar-driven source of variation rather than the outcome of systematic scheduling or sorting. The empirical strategy below nevertheless conditions on the dimensions along which residual differences could arise.

5 The Net Effect of Friday the 17th on Test Scores

5.1 Estimating the Reduced-Form Effect of Friday the 17th

The conceptual framework of Section 2 implies that the effect of a superstition signal on grades reflects the combination of two potentially offsetting forces: a student-side effect on exam per-

[3]This exercise is based on the subset of compulsory courses for which we can reconstruct the full exam-session calendar; Appendix C describes the construction of this sample. Appendix Table D3 reports an alternative balance exercise based on realized exposure, where a student is classified as exposed if at least one of their passed exams took place on a Friday the 17th. The results are similar.

formance and possible differences in the difficulty of exams held on the salient date. At the same time, the evidence in the previous section suggests that exposure to Friday the 17th may be systematically, albeit modestly, related to student characteristics, courses, instructors, or the academic calendar. Our empirical specification is designed to accommodate both considerations.

As stated in the conceptual framework, the grade equation can be written as

$$G_{ie} = a + b\left(\alpha_i + Z_{ie} - \kappa s_i F_e\right) - c\left(\bar{D}_e + \omega F_e\right). \tag{1}$$

We therefore estimate the reduced-form effect of Friday the 17th while flexibly accounting for the student, course, and calendar determinants of grades highlighted by the framework and by the preceding analysis.

We estimate the following specification:

$$Y_{ipct} = \delta F_{ipct} + \beta X_{it} + \gamma_i + \psi_c + \zeta_w + \mu_d + \theta_m + \iota_y + \varepsilon_{ipct}, \tag{2}$$

where Y_{ipct} denotes the exam score of student i, enrolled in program p, in course c, taking an exam at date t. Standard errors are clustered at the individual level.[4] The variable of interest, F_{ipct}, is an indicator equal to one when the exam takes place on Friday the 17th (labeled FR17 in the tables) and zero otherwise. The coefficient δ therefore captures the average difference in performance between exams held on Friday the 17th and exams held on any other date, conditional on the included controls.

The specification in Equation (2) follows directly from Equation (1). Student fixed effects (γ_i) absorb all time-invariant determinants of performance, including ability, study habits, accumulated human capital, and any other stable student characteristics.[5] They therefore account for the student-specific component α_i. Identification comes from comparing the performance of the same student across exams taken on different dates.

[4] We cluster standard errors at the student level throughout, since our panel contains repeated exam outcomes for each student and arbitrary within-student correlation therefore represents a natural source of residual dependence. Individual-level clustering is consequently a natural baseline given the panel structure of the data.

[5] We define the student fixed effect at the level of the student "career", that is, the combination of student, year of first enrollment and degree program, which yields 106,334 units. A student who re-enrolls in a different program faces a different curriculum and grading environment, so the within-career comparison is the cleaner contrast. Standard errors are clustered on the physical student identifier, of which there are 88,672, the coarser and therefore more conservative level.

The vector X_{it} and the calendar fixed effects capture time-varying determinants of performance that are unrelated to superstition. Specifically, we control for the time elapsed since the student's enrollment and since the first exam session of the course for the student's cohort, thereby accounting for changes in academic maturity and experience over time. The vector X_{it} further includes an indicator for another exam passed by the student in the previous seven days, indicators for exams held in the seven days before and after a holiday, and an indicator for bridge days between a holiday and a weekend. We also include day-of-week (ζ_w), day-of-month (μ_d), month-of-year (θ_m), and year (ι_y) fixed effects to absorb systematic temporal factors that may affect students' effective performance independently of superstition. Examples include end-of-week fatigue, seasonal fluctuations in well-being, examination cycles, workload patterns, and broader academic trends. These controls proxy for observable and predictable components of Z_{ie}.

Equation (1) also highlights that observed differences in grades may arise because exams differ in their underlying difficulty. While we do not directly observe exam content, we absorb persistent differences in grading standards, subject difficulty, and teaching practices through course fixed effects (ψ_c). We additionally exploit the subsample for which information on instructor identity is available to assess whether the results are sensitive to instructor-specific factors.

Taken together, these controls absorb the main determinants of exam performance highlighted by the conceptual framework while addressing the dimensions along which exposure to Friday the 17th may be systematically correlated with student composition, course characteristics, and calendar timing. In particular, conditioning separately on day of the week, day of the month, month, and year ensures that identification comes from the coincidence of Friday with the 17th, rather than from either calendar dimension on its own or from broader temporal patterns. Under this specification, the coefficient on Friday the 17th, δ, arguably provides a credible empirical counterpart to the reduced-form effect of the superstition signal on grades derived in Section 2. In Section 6, we then investigate the channels underlying this reduced-form effect.

5.2 A Friday 17 Penalty

Table 2 reports estimates of the reduced-form effect of Friday the 17th on academic performance. Column (1) presents the raw association between exam timing and test scores. Students taking an exam on Friday 17 score, on average, 0.44 points less on the 18–31 grading scale.

Columns (2)–(4) separately account for calendar variation, persistent differences across courses, and within-student differences in performance. Each set of fixed effects attenuates the raw Friday 17 penalty, but none eliminates it. The estimated coefficient remains negative in all three specifications, ranging from -0.10 to -0.25 points.

Column (5) combines all controls and fixed effects and corresponds to our preferred specification. The estimated Friday 17 penalty is 0.13 points and is statistically significant at the one percent level. Relative to the standard deviation of exam scores, this corresponds to approximately 3.5 percent of a standard deviation. For context, Bonacini et al. (2023), studying the same university and using the same data, estimate the effect of cumulative exposure to COVID-19 restrictions on exam performance. The Friday 17 penalty is about 40 percent of the effect associated with the average cumulative exposure to these restrictions (105 days).

Columns (6) and (7) re-estimate the full specification on the subset of observations for which instructor information is available. In Column (6), we add instructor characteristics, while Column (7) includes instructor fixed effects. The estimated Friday 17 penalty remains remarkably stable, ranging between -0.17 and -0.18 points. The stability of the estimate after adding instructor fixed effects makes it unlikely that the result is driven by particular instructors being more likely to schedule or grade exams differently on Friday the 17th.

5.3 Robustness Checks

We subject the result to three sets of checks. We first assess its sensitivity to alternative specifications, then ask whether Friday the 17th stands out relative to other calendar dates, and finally examine selection into the set of passed exams observed in our data.

5.3.1 About the Specification

Table 3 examines the robustness of our baseline findings to a range of alternative specifications. Throughout, we start from the preferred specification reported in Column (5) of Table 2,

reported in the first column for convenience, and modify individual components of the model to assess whether the estimated Friday 17 effect is sensitive to particular modeling choices.

Columns (2) to (4) investigate the role of the three main fixed-effect dimensions included in the baseline specification. In Column (2), we remove all individual-level controls: the individual fixed effects and the student-specific time-varying controls. The estimated coefficient remains negative and statistically significant at -0.08 points. In Column (3), we exclude course fixed effects while retaining the remaining controls. The estimated Friday 17 penalty increases in magnitude to -0.26 points. In Column (4), we instead remove the calendar fixed effects, obtaining an estimate of -0.10 points. The persistence of the effect across these specifications suggests that the main result is not driven by any particular block of fixed effects.

Columns (5) and (6) examine alternative ways of modeling time. In Column (5), we additionally interact year and faculty fixed effects to allow for faculty-specific trends over time. In Column (6), we replace calendar-year fixed effects with academic-year fixed effects, the academic year running from October to September. The estimated Friday 17 coefficient remains virtually unchanged, at -0.13 and -0.12 points, respectively.

The final two columns ask whether the result is specific to the use of individual and course fixed effects. These fixed effects provide demanding controls for persistent differences across students and courses, but they also restrict identification to variation within students and courses. We therefore consider specifications that replace each fixed effect with a predetermined measure of the corresponding source of heterogeneity. In Column (7), we replace individual fixed effects with the student's average grade prior to the exam, while retaining course fixed effects. In Column (8), we replace course fixed effects with the course's average grade in prior exams, while retaining individual fixed effects.

The estimated Friday 17 penalty remains statistically significant in both cases, at -0.11 points in Column (7) and -0.24 points in Column (8). The result therefore does not depend on absorbing all time-invariant student or course heterogeneity through fixed effects, although the larger estimate in Column (8) indicates that controlling flexibly for persistent differences across courses matters for the magnitude of the coefficient.

5.3.2 Placebo Dates and Permutation Tests

Exams held on the 17th of the month on other weekdays are not penalized. Appendix Table D4 replaces the Friday 17 indicator with an indicator for the 17th falling on Monday, Tuesday, Wednesday, or Thursday. The four placebo coefficients range from -0.04 to 0.03 points and none is statistically significant, compared with -0.13 points for Friday the 17th.

We also compare Friday the 17th with the other 149 combinations of weekday (Monday to Friday) and day of the month (1 to 30). For each combination, we re-estimate Columns (1) to (5) of Table 2, which share the same estimation sample, and compare its t-statistic with that of Friday the 17th (Appendix Figure D3 and Appendix Table D5). Friday the 17th is negative in every specification, whereas placebo dates that appear significant at conventional levels in one specification are rarely as significant in others. Every placebo regression also controls for the true Friday 17 indicator, so the true effect does not contaminate the placebo distribution. Summarizing each date by its average t-statistic across Columns (1) to (5), no placebo date is as consistently negative and significant as Friday the 17th, yielding a two-sided p-value of 0.007. The result is similar after standardizing the t-statistics within specifications, with a two-sided p-value of 0.007.[6]

5.3.3 Selection into Passed Exams

One feature of the Italian university system that we previously mentioned deserves particular attention: Students are not required to complete an exam simply because they are registered for it. They may not attend the session, attend and fail, or pass and decline the grade. In all three cases no grade appears in our records and the exam can be retaken in a later session.

This raises a potential concern for our analysis. Because our administrative data only record passed exams, the observed Friday 17 penalty is estimated among students who sit the exam, pass it and accept the grade. If exposure to Friday the 17th affects whether students sit the exam or whether they pass it, the composition of the students who pass may differ from the composition of all students who could have taken the exam.

Appendix C examines this issue in detail. The analysis combines information on passing at each exam session with the measures of predicted superstition introduced later in the paper.

[6] Appendix Table D5 also reports the permutation test separately for each specification.

Because these measures are central to the heterogeneity analysis that follows, we defer the full selection analysis to Appendix C and report its main findings here.

Using information on the set of exam opportunities available to students within the same program and cohort, we reconstruct the sessions at which each student could have taken each compulsory exam and estimate the effect of a Friday 17 session on the probability of passing the exam at that session. We find that students are less likely to pass an exam at a session held on Friday the 17th. The decline in passing implies that Friday the 17th generates a missing mass of grades in our administrative data. What matters for the interpretation of the estimated grade penalty is therefore what these unobserved grades would have looked like. If the students who fail to generate an observed grade on Friday the 17th would otherwise have received relatively low grades, conditioning on passed exams selects a stronger group of students on Friday the 17th and attenuates the estimated grade penalty. The evidence presented in Appendix C favors this interpretation: students who pass on Friday the 17th are more positively selected than those who pass at other sessions.[7] Selection into observed grades therefore appears to work against finding a Friday 17 penalty. If anything, the estimates reported above may understate the broader effect of Friday the 17th on academic outcomes.

6 Exam Difficulty or Student Superstition?

The evidence presented so far establishes a robust negative association between Friday the 17th and academic performance. From the perspective of the conceptual framework, however, these reduced-form estimates remain silent about the underlying mechanism. A lower grade on Friday the 17th could arise because students perform worse when exposed to a culturally salient superstition signal ($-b\kappa s_i$), because exams are systematically more difficult on that date ($-c\omega$), or because both channels operate simultaneously.

Remember that the conceptual framework provides two predictions that allow us to assess the importance of these explanations. The second implication of Section 2 is that, among individuals with no susceptibility to the superstition signal ($s_i = 0$), any Friday 17 effect must

[7] The Appendix provides two additional pieces of evidence consistent with this interpretation. First, the decline in passing at the first available session is larger among students with higher predicted susceptibility to superstition, using the measures introduced in Section 6; this gradient fades at later sessions. Second, estimating a Tobit model on the session panel that treats sessions without a recorded grade as censored below the passing threshold yields a stronger negative Friday 17 effect. See Appendix C and Appendix Table C5.

operate through exam difficulty. The third is that the effect of Friday the 17th should become increasingly negative as susceptibility to superstition increases ($\frac{\partial[G_{ie}(1)-G_{ie}(0)]}{\partial s_i} < 0$).

A natural challenge is that susceptibility to superstition s_i is not observed in the administrative records. We therefore exploit two complementary strategies. The first relies on a group of students for whom susceptibility to the superstition signal is plausibly absent. The second uses external survey data to construct predicted measures of individual superstition that can be linked to the administrative records through probabilistic imputation.

6.1 Students Born Abroad as a Benchmark Group

The conceptual framework predicts that any Friday 17 effect observed among individuals with $s_i = 0$ must operate through exam difficulty. While we do not directly observe individual levels of superstition, students born abroad enrolled at UNIMORE provide a natural benchmark group.

Friday the 17th is a distinctly Italian superstition. Although students born abroad may hold superstitious beliefs of their own, there is little reason to expect them to attach the same cultural meaning to Friday the 17th as Italian students. In practice, many are likely to be unaware of the superstition altogether. To the extent that this is true, students born abroad approximate a group for which susceptibility to the Friday 17 signal is close to zero.

This observation provides a simple empirical test. If exams are systematically more difficult on Friday the 17th, students born abroad should experience a performance penalty. Conversely, if the effect solely reflects the reaction of students to a culturally salient superstition, the penalty should be entirely absent among them.[8]

Column (1) of Table 4 reports the results. Consistent with the latter interpretation, we find little evidence of a Friday 17 penalty among students born abroad.[9] While the estimates are less precise because of the smaller sample size, they are economically small and statistically indistinguishable from zero. These findings suggest that exam difficulty is unlikely to be the primary driver of the reduced-form effect documented above.

[8] This interpretation abstracts from possible peer spillovers. For example, anxiety among Italian students could in principle affect students born abroad, or more generally students with $s_i = 0$, through interactions before or during the exam. We do not model this channel, which we view as second order relative to the direct activation of individual superstition.

[9] In Appendix Table D6 we include students who are either born abroad or without Italian citizenship; the estimates are virtually identical.

6.2 The Moderating Effect of Predicted Individual Superstition

The benchmark-group test identifies the difficulty channel only at $s_i = 0$. To trace the whole gradient predicted by the framework we need a measure of susceptibility s_i for every student, and the administrative records contain none. We therefore turn to external survey evidence and rely on two complementary data sources.

The first is the European Values Study (EVS), a large nationally representative survey containing information on superstitious beliefs together with rich socio-demographic characteristics. The main advantage of the EVS is its scale and representativeness. It partially covers the period spanned by our administrative data and provides sufficient statistical power to estimate the relationship between superstition and observable characteristics. Its main limitation is that respondents are drawn from the general population rather than from the student population, and the survey does not specifically measure beliefs related to Friday the 17th.

The second source is an original survey we conducted among students at UNIMORE. Unlike the EVS, this survey directly targets the population under study and includes questions specifically designed to capture beliefs about Friday the 17th. It therefore provides a closer match to the mechanism investigated in this paper. The trade-off is that the sample is considerably smaller and was collected after the period covered by the administrative data.

Using these surveys, we estimate the relationship between superstition and observable characteristics such as gender, age, region of origin, and family background. The resulting coefficients are then used to impute predicted superstition scores for students in the administrative records. Appendix A describes the two surveys, the imputation model and the validation of the scores.

We then augment Equation (2) by interacting the Friday 17 indicator with the predicted superstition score, normalized so that zero corresponds to the lowest predicted level of superstition and one to the highest. This specification provides a direct test of the third prediction of the conceptual framework: if the observed effect is driven by superstition, the Friday 17 penalty should become increasingly negative as susceptibility to superstition rises. Table 4 reports the estimates, and Figure 3 plots the effect along the 0–1 support of the EVS index.

The results strongly support the prediction of the model. Across both survey sources and alternative measures of superstition, the estimated Friday 17 effect becomes increasingly neg-

ative as predicted susceptibility to superstition rises.[10] Figure 3 illustrates this pattern for the EVS index (column (3) of Table 4). At $s_i = 0$, the estimated effect is slightly positive and statistically indistinguishable from zero; it then declines steadily with predicted superstition, reaching about -0.35 grade points at $s_i = 1$. This corresponds to a decline of roughly 0.45 grade points in the Friday 17 effect over the full support of the index.[11] The same pattern appears across the alternative measures reported in Table 4: the estimated Friday 17 effect declines by between 0.22 and 0.54 grade points as predicted susceptibility moves from $s_i = 0$ to $s_i = 1$. For five of the six measures, the estimated effect at $s_i = 0$ is not statistically significant at the 5 percent level.[12]

Taken together, the evidence from students born abroad and probabilistic imputation points to the same conclusion. We find little evidence that Friday the 17th affects performance through systematic changes in exam difficulty, but strong evidence that the effect becomes more negative among students who are more likely to be susceptible to superstition. The results therefore suggest that student superstition, rather than instructor behavior or exam design, is the primary mechanism underlying the Friday 17 penalty.

7 Anticipation, Adaptation, or a Temporary Shock?

The analysis thus far has focused on the decline in test scores observed when an exam falls on Friday the 17th. We now examine whether this decline is confined to the exam itself or extends to the exams surrounding it. This exercise serves two purposes. First, performance before Friday the 17th provides a check on whether the estimated penalty is preceded by broader changes in students' academic performance. If students who eventually take an exam on Friday the 17th were already performing differently beforehand, the decline on the unlucky date could partly

[10] A potential concern is that predicted superstition may proxy for other correlated traits rather than superstition itself. To assess this, Appendix Table A6 augments the specification with interactions between Friday the 17th and twelve other predicted survey traits separately, one at a time. The interaction between Friday the 17th and predicted superstition remains negative and statistically significant.

[11] We also allow the relationship between the Friday 17 effect and predicted superstition to be nonlinear by including a quadratic term in the predicted superstition score. We find no evidence of a statistically significant nonlinear component, and the main conclusions are unchanged.

[12] Appendix Table D7 examines heterogeneity in the average Friday 17 effect by gender, degree level, and region of birth, while Appendix Table D8 examines whether the gradient with predicted superstition differs across the same groups. The average effect varies across some groups, whereas the superstition gradient differs significantly by degree level but not by gender or region of birth.

reflect these underlying trajectories. Second, the pattern before and after the event provides additional evidence on how the superstition operates. The conceptual framework treats Friday the 17th as a signal that becomes relevant when students are confronted with the unlucky date. A decline concentrated on the Friday 17 exam would be consistent with such a contemporaneous effect, whereas changes beforehand or afterward could indicate anticipation, persistence, adaptation, or compensating behavior.

A distinctive feature of our setting is that students take exams at different frequencies and at different points in their academic careers. Calendar time is therefore not a particularly informative metric for studying these patterns. Instead, we measure distance from Friday the 17th in terms of the sequence of exams taken by each student. Specifically, we treat each exam occurring on Friday the 17th as an event and follow students through the exams that precede and follow it.

Formally, let t_0 denote an exam taking place on Friday the 17th. For each student, we index surrounding exams according to their distance from that exam and estimate

$$Y_{it} = \sum_{k=-K}^{K} \delta_k \text{EventTime}_{it}^{k} + \beta X_{it} + \gamma_{is} + \lambda_n + \kappa_c + \zeta_w + \mu_d + \theta_m + \iota_y + \varepsilon_{it}, \tag{3}$$

where $\text{EventTime}_{it}^{k}$ is a set of indicators for the position of exam t relative to a Friday 17 exam, with $K = 11$. Exams are counted across all degree programs of a student, and each Friday 17 exam defines a stack that runs from the exam after the student's previous Friday 17 exam to the exam before the next one, so that γ_{is} is a student-by-stack fixed effect. λ_n are fixed effects for the position of the exam in the student's sequence and κ_c for the year of the degree program in which course c is normally taken. X_{it} includes the time-varying controls of the baseline specification (column (5) of Table 2) and, in place of course fixed effects, the average grade in the course on earlier dates. We estimate Equation (3) with the imputation estimator of Borusyak et al. (2024), which fits the fixed effects and controls on exams not yet affected by a Friday 17 exam and imputes the grade that each later exam would have received without it. The coefficients for $k \geq 0$ are the average differences between observed and imputed grades; those for $k < 0$ test for differences before the event, relative to exams more than eleven positions earlier and to students who never sit a Friday 17 exam.

Figure 4 reports the estimates. We find little evidence of changes in performance before

Friday the 17th. The coefficients for the preceding exams are small, between -0.07 and 0.07 points, and none is statistically different from zero at the 5 percent level.

The decline is instead concentrated at the Friday 17 exam itself, where grades fall by 0.21 points on the 18–31 scale (standard error 0.03). This estimate is larger than the -0.13 points of the baseline specification; the two are not directly comparable, since the event study relies on the imputation estimator and on its own estimation sample.

The effect also appears short-lived. At the next exam grades are slightly above their imputed level (0.06 points), and among the following exams only the fourth after the event shows a sizeable coefficient (0.13 points). One possible interpretation is that compensating behavior does not appear immediately, since students may not yet know how they performed on the Friday 17 exam when they take their next exam. The positive coefficient at the fourth subsequent exam is therefore consistent with some delayed compensation. This evidence is only suggestive, however. Unlike the effect at the Friday 17 exam, which is significant at the one percent level in every specification of Appendix Table D9, this coefficient is sensitive to the conditioning set: without the control for course difficulty it falls to 0.08 points, and to about 0.06 points, no longer statistically significant, when the course-year fixed effects are dropped as well.

We next examine whether these patterns differ according to students' susceptibility to superstition. Figure 5 and Appendix Table D10 repeat the analysis separately for students in the bottom and top halves of the predicted-superstition distribution derived from the EVS-based measure.

Among students with low predicted levels of superstition, the coefficient at t_0 is -0.09 points and not statistically different from zero at the 5 percent level ($p = 0.06$). Among students with high predicted levels of superstition, by contrast, a pronounced decline occurs precisely when the exam falls on Friday the 17th: grades fall by 0.30 points, against 0.21 in the full sample, and the gap with the less superstitious students is 0.22 points (standard error 0.07). The pattern mirrors the heterogeneity documented in the previous section.

Taken together, these results show that the Friday 17 penalty is concentrated on the unlucky date, particularly among students predicted to be more susceptible to superstition, while performance changes little before and after the event. The pattern is therefore consistent with a temporary disruption when the superstition signal becomes salient.

8 Conclusion

This paper shows that a culturally salient but objectively uninformative belief can affect performance in a consequential setting. Using administrative records from the University of Modena and Reggio Emilia, we find that students receive lower grades when an exam falls on *Friday the 17th*, a date traditionally associated with bad luck in Italy. In our preferred specification, the decline is about 0.13 grade points, or 3.5 percent of a standard deviation. The estimate is stable across specifications with student, course, and detailed calendar fixed effects, and is not accompanied by comparable declines when the 17th falls on other weekdays.

Several pieces of evidence support an interpretation based on superstition rather than systematic differences in the exams or students observed on the unlucky date. Scheduling patterns provide little indication that Friday 17 exams are drawn from systematically different courses or instructors, while potentially exposed students differ only modestly from other students in observable characteristics. More importantly, the estimated penalty is concentrated among students with higher predicted superstition and is absent among those with low predicted superstition. It is also absent among students born abroad, for whom this specifically Italian belief should be less salient. These patterns are difficult to reconcile with explanations based only on the calendar or on persistent differences in exam difficulty.

The longitudinal structure of the data further shows that the effect is localized around the unlucky date. We find little evidence of weaker performance before Friday the 17th and suggestive evidence of compensating improvements afterward. At the same time, the probability of passing declines at sessions held on Friday the 17th, particularly at the first session among students with higher predicted superstition. Because the administrative records contain only successful exams, we cannot distinguish whether this pattern reflects avoidance of the session, a higher probability of failure conditional on attending, or both. This limitation also means that the effect estimated from observed grades may not capture the full behavioral response to the unlucky date.

The broader implication is that cultural beliefs need not operate only through persistent preferences, long-run investments, or stable differences across individuals. They can also become temporarily salient and alter realized performance at a specific moment. In our setting, the student's underlying ability is unchanged, and we find little evidence that exams scheduled

on Friday the 17th are systematically different from those held on other dates. Yet performance declines when the task coincides with a culturally meaningful cue. The evidence does not allow us to distinguish whether this response operates through anxiety, attention, effort, avoidance, or some combination of these channels. It does show, however, that culturally transmitted beliefs can affect performance even in a highly structured environment with fixed evaluation criteria and strong incentives to perform well.

Friday the 17th provides a setting in which a culturally salient belief can be separated from persistent differences across students. The result therefore speaks to a broader mechanism through which culture may affect behavior: beliefs that are irrelevant to the objective returns to an action can nevertheless become salient at particular moments and influence realized outcomes.

Whether repeated short-run responses of this kind accumulate into differences in educational attainment, human-capital investment, or subsequent labor-market outcomes remains an open question. Establishing such cumulative effects requires settings in which culturally salient cues can be followed over longer horizons and across repeated consequential decisions.

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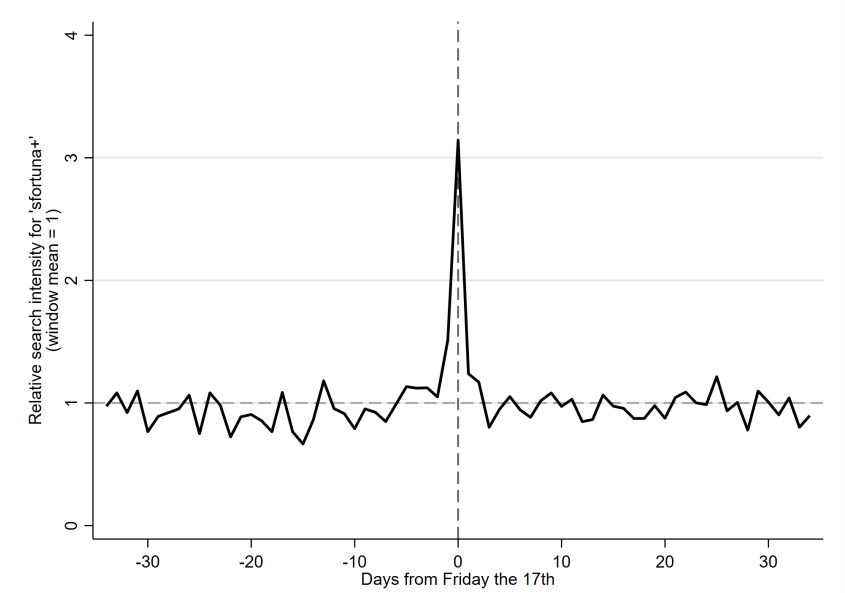
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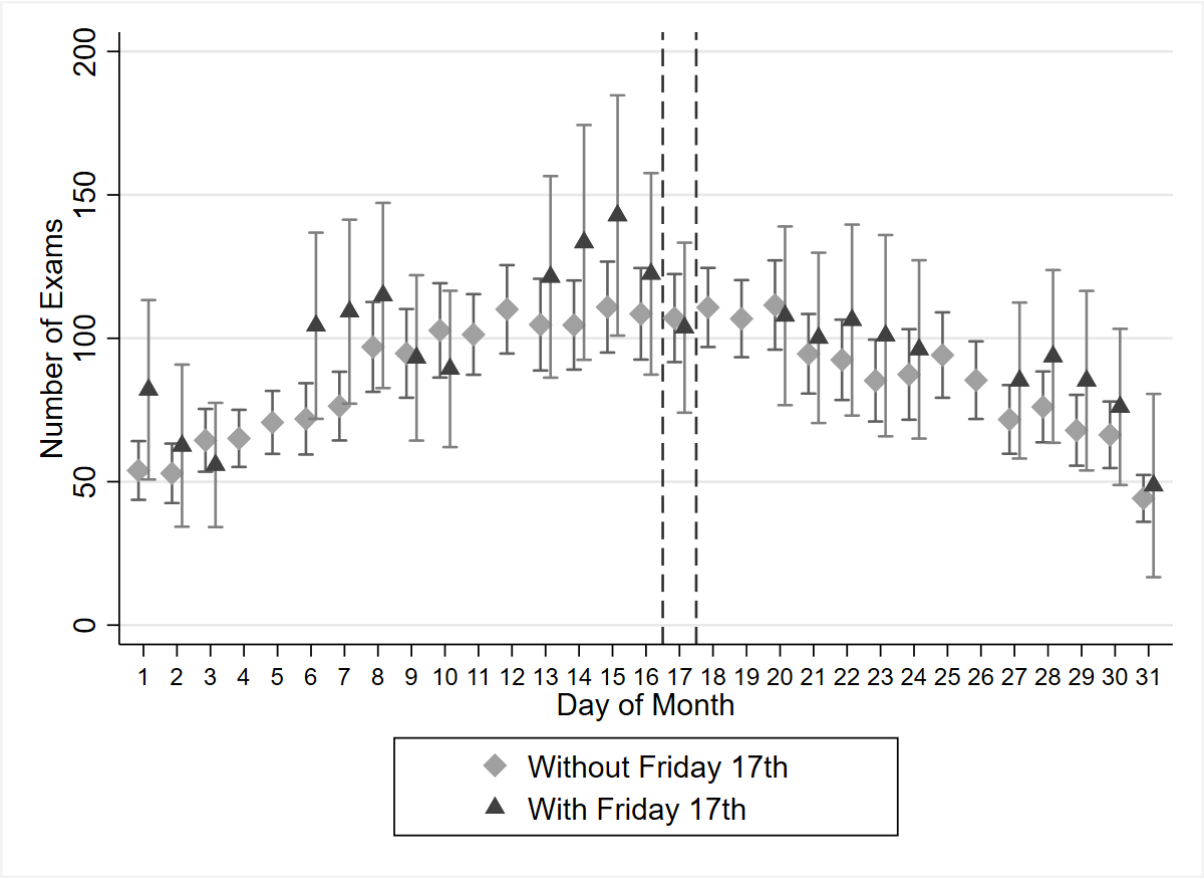
Figures and Tables

Figure 1: Google Searches for Bad Luck around Friday the 17th in Italy



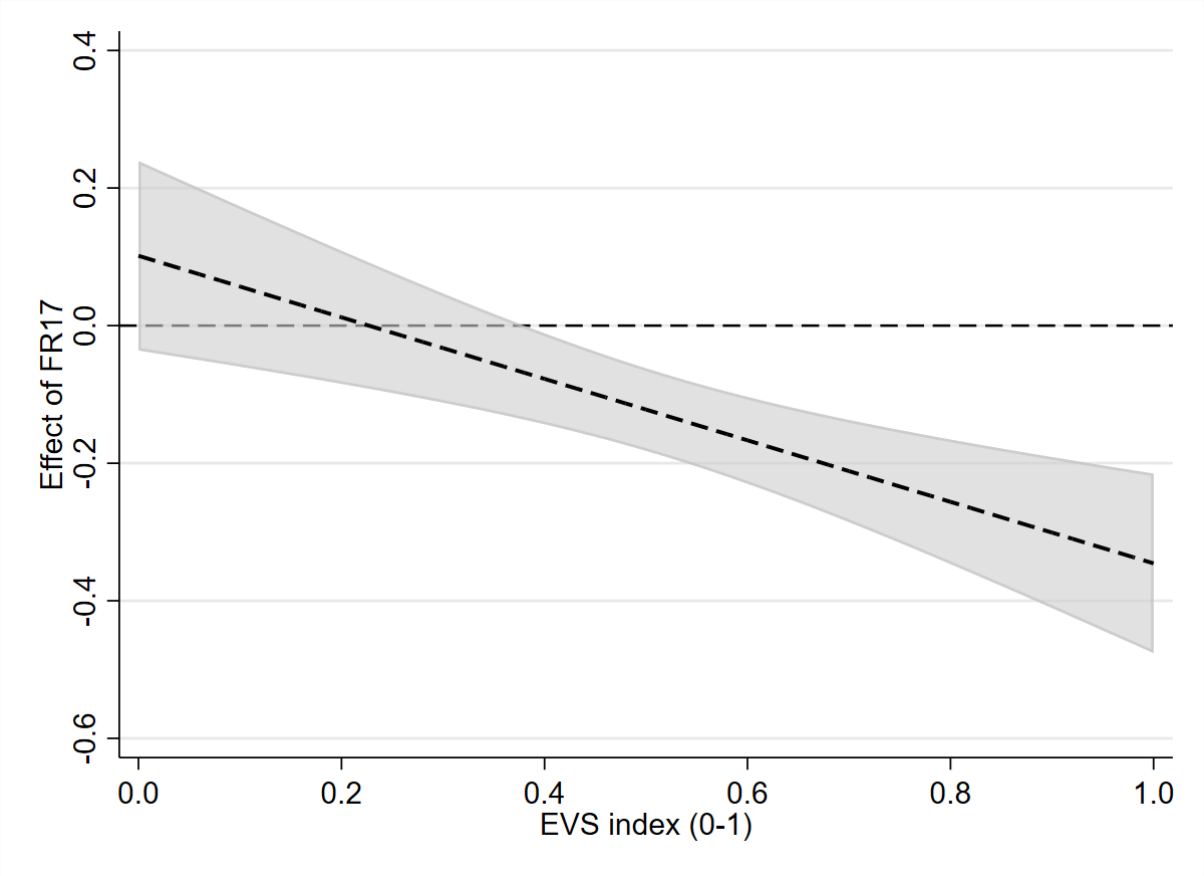
Notes: The figure reports Google Trends search intensity for the terms "sfortuna" and "sfortunato" around Friday the 17th in Italy. Each window covers 34 days before and after Friday the 17th and is rescaled so that the window mean equals one. The window is constructed for each of the 25 Fridays the 17th between 2012 and 2025; earlier occurrences are not usable because daily search volume for these terms is close to zero before 2012. Any other Friday the 17th falling inside a window is dropped from that window, and January 1–5 is excluded from every window because of the new-year spike in these searches. The plotted line is the cross-event mean of the rescaled series.

Figure 2: Exam Scheduling by Presence of a Friday the 17th



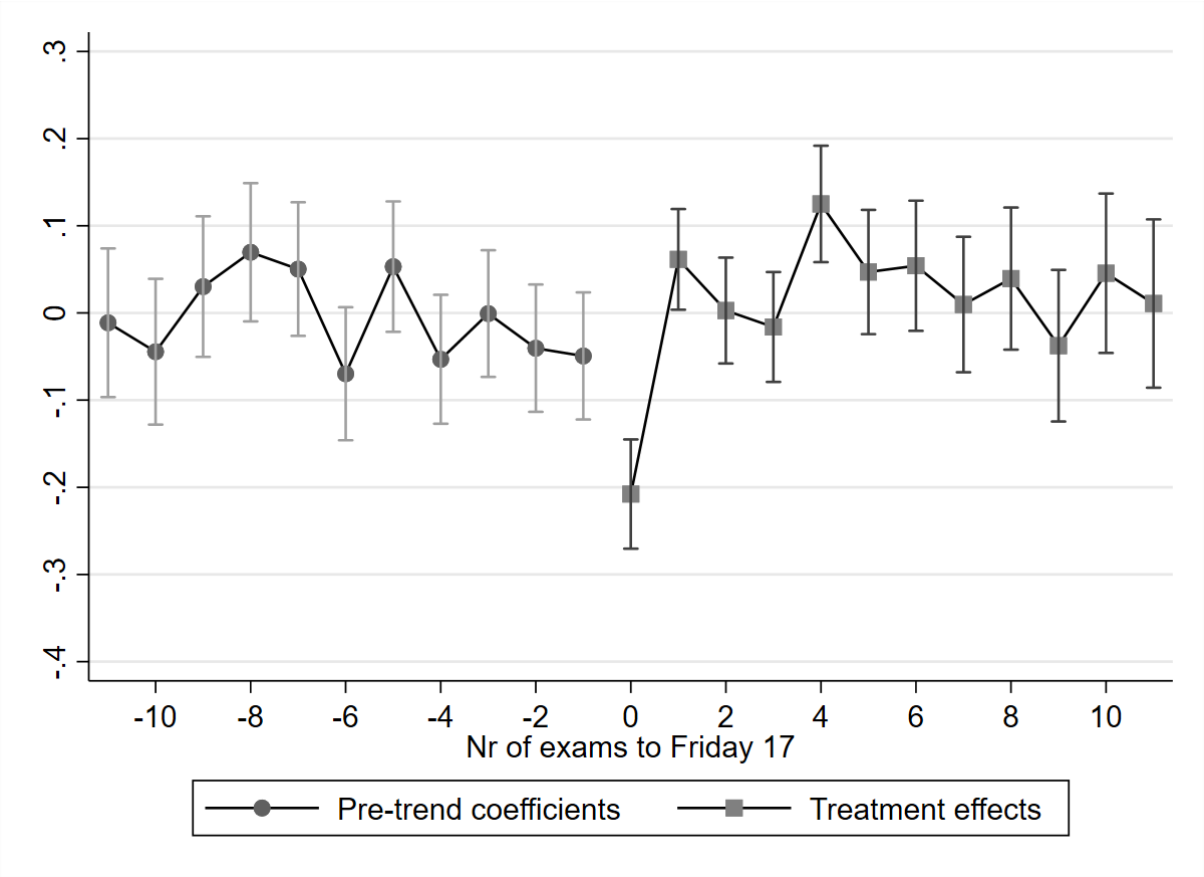
Notes: The figure plots the average number of courses examined on each day of the month, comparing months with an exam on a *Friday the 17th* (black triangles) with all other months (gray diamonds). Vertical dashed lines mark the 17th day of the month. Error bars represent 95 percent confidence intervals around the mean number of courses examined per day. Data are drawn from administrative records of the University of Modena and Reggio Emilia.

Figure 3: Friday 17 Effect on Test Scores by Predicted Superstition Score



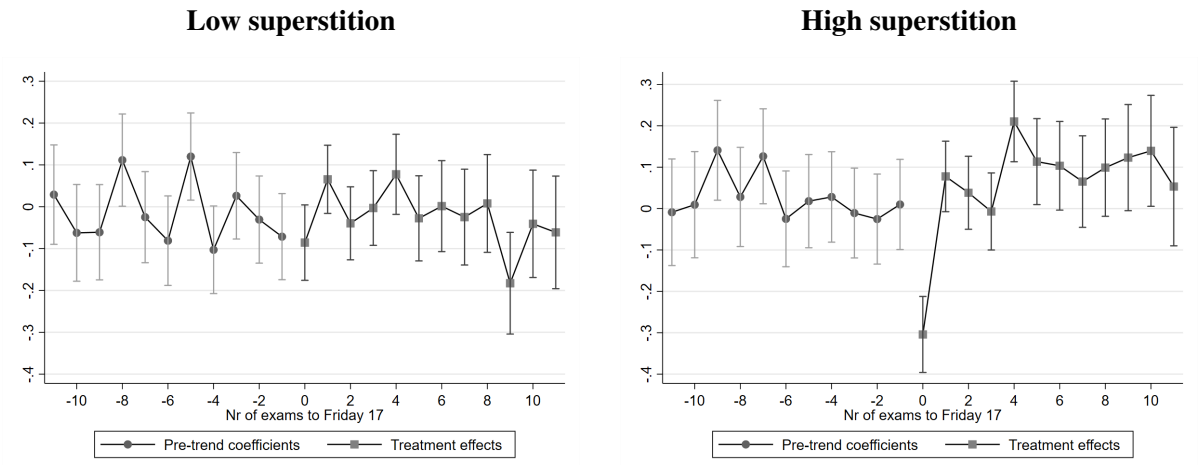
Notes: This figure plots the estimated effect of taking an exam on Friday the 17th as a function of students' predicted superstition score derived from the European Values Study. The dashed line represents point estimates and the shaded area the 95 percent confidence interval. The score is the EVS index of column (3) of Table 4, rescaled to the 0–1 interval within the estimation sample of that table. Data are drawn from administrative records of the University of Modena and Reggio Emilia.

Figure 4: Event-Study Estimates of the Friday 17 Effect on Test Scores



Notes: This figure plots the coefficients from the event-study specification described in Equation (3). Bars denote 95 percent confidence intervals. Data are drawn from administrative records of the University of Modena and Reggio Emilia.

Figure 5: Event-Study Estimates of the Friday 17 Effect on Test Scores by Predicted Superstition Score



Notes: This figure plots the coefficients from the event-study specification described in Equation (3), estimated separately for students with low and high predicted superstition scores derived from the European Values Study. Students are split at the median of the EVS-based index; bars denote 95 percent confidence intervals. Data are drawn from administrative records of the University of Modena and Reggio Emilia.

Table 1: Descriptive Statistics: Exam-Level Sample

	Mean	SD	Min	Max	N
Exam outcome and exposure					
Test score (18–31)	25.79	3.71	18.0	31.0	1,603,920
Exam on Friday the 17th	0.0079	0.0887	0.0	1.0	1,603,920
Student characteristics					
Age at exam	22.9	2.8	18.0	50.0	1,603,920
Women	0.538	0.499	0.0	1.0	1,603,920
Born abroad	0.058	0.235	0.0	1.0	1,603,920
Educational background					
No classical or scientific lyceum diploma	0.511	0.500	0.0	1.0	1,560,895
Region of birth					
North-West	0.064	0.244	0.0	1.0	1,510,208
North-East	0.707	0.455	0.0	1.0	1,510,208
Center	0.038	0.192	0.0	1.0	1,510,208
South and Islands	0.191	0.393	0.0	1.0	1,510,208
Degree characteristics					
Master's degree	0.346	0.476	0.0	1.0	1,603,920
Social Sciences & Humanities	0.247	0.431	0.0	1.0	1,603,920
Business & Law	0.278	0.448	0.0	1.0	1,603,920
Engineering, Physical & Natural Sciences	0.350	0.477	0.0	1.0	1,603,920
Biomedical & Clinical Sciences	0.124	0.330	0.0	1.0	1,603,920
Socioeconomic background					
Income quartile 1	0.260	0.439	0.0	1.0	1,598,661
Income quartile 2	0.334	0.472	0.0	1.0	1,598,661
Income quartile 3	0.212	0.409	0.0	1.0	1,598,661
Income quartile 4	0.195	0.396	0.0	1.0	1,598,661
Academic timing					
Year of study	2.155	1.237	1.0	18.0	1,603,920
Years since first session	0.580	0.914	0.0	4.6	1,603,920

Notes: One observation per passed exam. Test scores range from 18 to 30, with 30 cum laude coded as 31. All statistics are computed on the harmonized exam-level estimation sample. Region-of-birth shares are computed among observations with non-missing region of birth. Diploma type, region of birth and the fee quartile are time-invariant student characteristics: they are absorbed by the individual fixed effects and never enter the regressions as covariates, which is why the estimation sample stays at 1,603,920 exams in Table 2 despite the item non-response reported here. Region of birth is undefined for students born abroad, which accounts for all of its missing values.

Table 2: Effect of Friday the 17th on Test Scores

	Test score (18–31)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exam on FR17	-0.44***	-0.24***	-0.10***	-0.25***	-0.13***	-0.18***	-0.17***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)
Observations	1,603,920	1,603,920	1,603,920	1,603,920	1,603,920	582,744	582,744
Students	88,672	88,672	88,672	88,672	88,672	49,603	49,603
Day-of-week FE	.	✓	.	.	✓	✓	✓
Day-of-month FE	.	✓	.	.	✓	✓	✓
Month-of-year FE	.	✓	.	.	✓	✓	✓
Year FE	.	✓	.	.	✓	✓	✓
Course FE	.	.	✓	.	✓	✓	✓
Individual FE	.	.	.	✓	✓	✓	✓
Time-varying X	.	.	.	✓	✓	✓	✓
Instructor Characteristics	.	.	.	.	.	✓	.
Instructor FE	.	.	.	.	.	.	✓
Nr X	1	1	1	7	7	36	7
Nr FEs	0	65	6,749	106,334	113,146	58,272	59,139
R^2	0.000	0.008	0.248	0.406	0.523	0.544	0.556

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. Columns (1) to (5) use the harmonized exam-level sample; columns (6) and (7) use the harmonized subsample with instructor identifiers. Standard errors clustered at the student level in parentheses. The individual fixed effects are defined at the level of the student career, that is student $\times$ year of first enrollment $\times$ degree program (106,334 units), while standard errors are clustered on the physical student identifier (88,672 units), which is the coarser and therefore more conservative level. The "students" row reports the number of physical students. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Effect of Friday the 17th on Test Scores: Robustness Checks

	Test score (18–31)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam on FR17	-0.13***	-0.08**	-0.26***	-0.10***	-0.13***	-0.12***	-0.11***	-0.24***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Drop one FE block from the baseline:								
Time FE	✓	✓	✓	.	✓	✓	✓	✓
Course FE	✓	✓	.	✓	✓	✓	✓	.
Individual FE	✓	.	✓	✓	✓	✓	.	✓
Alternative time / saturated FE:								
Year FE definition	calendar	calendar	calendar	—	calendar	academic	calendar	calendar
Year × Faculty FE	.	.	.	.	✓	.	.	.
Replace individual / course FE with prior means:								
Prior indiv. avg. grade	.	.	.	.	.	.	✓	.
Prior course avg. grade	.	.	.	.	.	.	.	✓
Time-varying X	✓	partial	✓	✓	✓	✓	✓	✓
Observations	1,603,920	1,603,920	1,603,920	1,603,920	1,603,920	1,603,920	1,603,920	1,603,920
R^2	0.523	0.255	0.409	0.521	0.524	0.523	0.396	0.492

Notes: All columns use the harmonized exam-level sample. Column (1) is the baseline estimate. Columns (3) to (8) include the time-varying controls of column (5) of Table 2; column (2) includes only the pre- and post-holiday and bridge-day indicators. The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Heterogeneity of the Friday 17 Effect by Predicted Superstition Score

	Test score (18–31)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exam taking place on a Friday the 17th:							
Independent effect	-0.055	0.108	0.102	0.160**	-0.013	0.122*	0.125
	(0.133)	(0.072)	(0.070)	(0.073)	(0.064)	(0.067)	(0.097)
Interacted with superstition score (0–1)		-0.449***	-0.447***	-0.544***	-0.223**	-0.515***	-0.449***
		(0.124)	(0.122)	(0.126)	(0.106)	(0.121)	(0.161)
N	92,799	1,503,996	1,503,996	1,503,996	1,503,996	1,503,996	1,503,996
Sample:							
Born in Italy	.	✓	✓	✓	✓	✓	✓
Born abroad	✓	.	.	.	.	.	.
Version of superstition score:							
Aggregate index	.	✓	.	.	.	.	.
EVS index	.	.	✓	.	.	.	.
Lucky charm	.	.	.	✓	.	.	.
Consult horoscope	.	.	.	.	✓	.	.
Account horoscope	.	.	.	.	.	✓	.
Friday 17 belief	.	.	.	.	.	.	✓

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. All columns use the harmonized exam-level sample and include day-of-week, day-of-month, month, year, course and individual fixed effects and the time-varying controls of column (5) of Table 2. Column (1) restricts the sample to students born abroad. Columns (2) to (7) restrict it to students born in Italy with all six superstition scores non-missing, since the EVS-based scores are defined for students born in Italy only (Appendix A); the six columns therefore share the same N. Each score is min-max rescaled to $[0, 1]$ within this sample, so the independent effect is the Friday 17 effect for the least superstitious student and the interaction is the change in the effect from the least to the most superstitious student. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Supplementary Appendix

A Imputing Superstition Scores: From Survey to Administrative Data

The administrative records contain no direct measure of superstition. We therefore use two external surveys to estimate how superstitious beliefs vary with characteristics that are also observed in the administrative data, and use these relationships to construct predicted superstition scores for each student. This appendix describes the two surveys, the imputation procedure, and a series of validation exercises.

A.1 The UNIMORE Student Survey

We conducted an original survey on the UNIMORE online platform (computer-assisted web interviewing) between February 20 and 23, 2025 among bachelor's students attending one UNIMORE course. Of the 227 respondents, 216 can be matched by student number to the administrative records of the four degree programs represented in the course, and 196 have complete information on the characteristics used in the imputation.

Respondents first report how strongly they believe that a lucky charm can protect or help them, how often they consult their horoscope, and how much they take it into account in everyday life. They then report their agreement, on a scale from 1 to 7, with four statements concerning common superstitions: breaking a mirror brings seven years of bad luck; when a black cat crosses the road, I prefer to change direction or be careful; "I avoid making important decisions or doing risky things on Friday the 17th"; and I avoid walking under a ladder. The Friday 17 item is therefore embedded among several measures of superstition rather than presented as the focus of the survey.

Since the respondents are university students, a stated belief about Friday the 17th could partly reflect a bad experience with an exam on that date rather than a prior belief. This concern is limited, since the imputed score depends only on predetermined characteristics of the student and almost none of the respondents took exams in the period covered by the administrative data, which ends in December 2022.

A.2 The European Values Study Items

The second source consists of the Italian samples of the European Values Study (EVS). The item on lucky charms is available in the waves fielded in Italy in 1999 and 2009, which we pool (2,430 respondents with complete information). The two items on how often respondents consult their horoscope and how much they take it into account are available in the 1999 wave only (1,508 and 1,504 respondents). All estimates use the EVS survey weights. The EVS records gender, year of birth, region of residence, education and household income, which we map into the administrative variables as shown in Table A1.

A.3 Probabilistic Imputation

For each of the four items (the three EVS items and the Friday 17 statement), we estimate an ordered logit model of the item on characteristics that the survey shares with the administrative data. In the EVS, the regressors are gender, year of birth, indicators for North-West, North-East and Center (with South and Islands as reference), an indicator for no university degree, household income quartiles and the survey year, which enters linearly. In the UNIMORE survey, whose respondents are matched to the administrative records, they are gender, year of birth, birth in Italy, five types of secondary-school diploma, university-fee quartiles and the same macro-region indicators. We then compute the linear index predicted by each model for every student in the administrative data, replacing each survey variable with its administrative counterpart and the survey year with the year of the exam. We treat the EVS split between respondents with and without a university degree as equivalent to the split in our data between students with a classical or scientific lyceum diploma and those with any other diploma, on the assumption that both separate the more from the less educated. Since the survey year varies only for the lucky-charm item, the scores based on it change with the year of the exam, while the Friday 17 score is fixed for each student. Because the survey year enters linearly, for exam years after 2009 the lucky-charm score extrapolates the change observed between the two waves.

We winsorize each predicted index at the 0.1st and 99.9th percentiles and standardize it to mean zero and unit variance. The EVS-based scores are assigned to students born in Italy only, since the Friday 17 superstition is specific to Italian culture, and are missing for students born

abroad. We combine the scores into two composite indices: the EVS index, the standardized sum of the three EVS scores, and the aggregate index, the mean of the three EVS scores and the Friday 17 score, winsorized and standardized in the same way. The six scores used in Table 4 are the aggregate index, the EVS index, the three EVS scores and the Friday 17 score.

In Table 4 and Figure 3, each score is rescaled to the unit interval by a min-max transformation within the common estimation sample, $\tilde{s}_i = (s_i - \min s)/(\max s - \min s)$, in line with $s_i \in [0, 1]$ in the conceptual framework.

A.4 Validation

Table A2 reports the determinants of the four items, estimated by OLS on the regressors of the imputation models. Women report stronger beliefs in all four items. In the EVS, younger cohorts and respondents without a university degree hold stronger beliefs, while the lucky-charm and horoscope-attention items decline with income, most clearly in the top quartile. In the UNIMORE survey, only the gender gap is precisely estimated, which is expected given the smaller sample of 196 respondents.

Table A3 reports the pairwise correlations of the six scores across students. The EVS-based scores are highly correlated with one another. These high correlations reflect similar estimated associations with the shared predictors: the three items display similar gradients in gender, year of birth, and education (Table A2), so students predicted to hold one belief also tend to be predicted to hold the others. The Friday 17 score, estimated on a different sample and with a different set of regressors, correlates between 0.67 and 0.76 with the three EVS scores and 0.84 with the aggregate index, of which it is a component. The socio-demographic profile associated with belief in Friday the 17th, which the EVS does not ask about, is therefore similar to that associated with the other superstition measures. Figure A1 shows that the EVS index spans about four standard deviations across students, with no mass point.

Reported beliefs are also positively correlated within respondents (Table A4). In the 1999 EVS wave, the only one containing all three items, their pairwise correlations range from 0.23 to 0.52. Among the UNIMORE respondents, the same three items correlate between 0.34 and 0.70, while agreement with the Friday 17 statement correlates between 0.21 and 0.56 with each of the other six superstition items. This provides separate support for using the EVS items,

which do not directly cover Friday the 17th, as proxies for susceptibility to it. In both sources, women report stronger beliefs (Table A2). In levels, the students consult the horoscope less often than the EVS respondents (1.6 against 2.2 on a scale from 1 to 5), take it into account about as much (1.4 against 1.5), and believe more in lucky charms (3.1 against 2.2, and 2.8 among EVS respondents aged 18 to 29; Panel A).

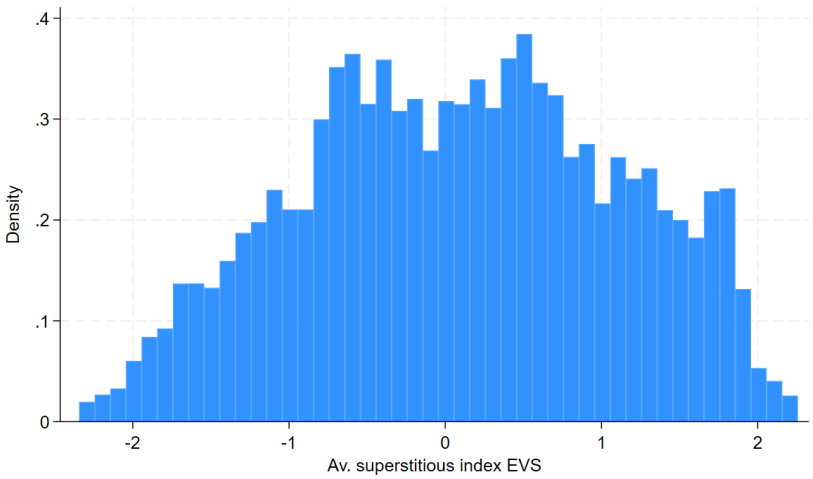
Table A5 and Figure A2 compare the survey respondents with the students in the estimation sample on the characteristics that enter the imputation. The respondents were born eleven years later on average, are more often women and from the North-East, and more often hold a commercial or vocational diploma. The survey score is therefore extrapolated to cohorts born well before the respondents, whereas the EVS scores are estimated using broader samples of the adult population.

A.5 Predicted Superstition and Other Predicted Traits

We finally ask whether the superstition gradient documented in Table 4 can be accounted for by other traits correlated with the same student characteristics. We consider twelve additional traits measured in the UNIMORE survey: the Big Five personality traits from the Ten-Item Personality Inventory, Machiavellianism, psychopathy, narcissism, trust, sustainability orientation, financial literacy, and impatience. Each trait is measured in the UNIMORE survey and imputed to the administrative sample using the same set of predictors as for the Friday 17 statement, before being rescaled to the unit interval. Because these imputed traits are constructed from overlapping predictors, they may capture some of the same observable heterogeneity as the predicted-superstition measure.

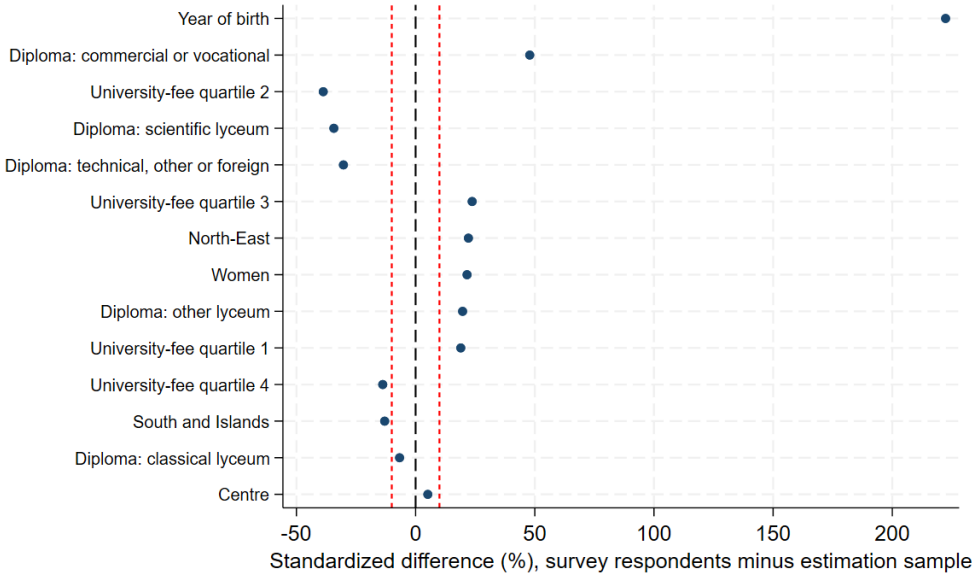
Table A6 then adds the interaction between Friday the 17th and each predicted trait separately to the interaction with the EVS index, using a common estimation sample. The interaction with predicted superstition remains negative and statistically significant at the 5 percent level in every specification. Its coefficient ranges from -0.33 to -0.56 points, compared with -0.45 points in the specification without any additional trait. No single predicted trait therefore accounts for the superstition gradient. Among the additional traits, only the interactions with conscientiousness (0.79), financial literacy (-0.71), openness (0.44), and extraversion (0.37) are statistically significant at the 5 percent level.

Figure A1: Distribution of the imputed EVS superstition index



Notes: Histogram of the EVS index, the standardized sum of the three imputed EVS scores, one observation per Italian-born student in the exam data.

Figure A2: Standardized differences between survey respondents and the estimation sample



Notes: Standardized differences of Table A5, in percent, sorted by absolute size; suppressed cells are omitted. The dotted red lines mark ± 10 percent.

Table A1: Equivalence between survey and administrative variables

	European Values Study	Administrative data
Gender	Gender of the respondent	Gender of the student
Birth year	Year of birth	Year of birth
Region	Region of residence	Region of birth
Education	No university degree	No classical or scientific lyceum diploma
Income	Household income quartile, by survey year	University-fee quartile, within the academic year of the exam
Time	Survey year	Exam year

Notes: Regions are aggregated into North-West, North-East, Center, and South and Islands in both sources. The survey year enters only the lucky-charm model, the only EVS item observed in two waves. The UNIMORE survey needs no mapping, since its respondents are matched to the administrative records and their characteristics are taken from them.

Table A2: Determinants of the superstition items: EVS and UNIMORE survey

	Lucky charm (1)	Consult horoscope (2)	Account horoscope (3)	Friday 17 belief (4)
Women	0.119***	0.389***	0.231***	0.562***
	(0.039)	(0.050)	(0.051)	(0.196)
Year of birth	0.007***	0.012***	0.010***	0.044
	(0.001)	(0.002)	(0.002)	(0.042)
No university degree	0.168***	0.185***	0.224***	—
	(0.047)	(0.066)	(0.060)	—
Diploma type (Ref.: Classical lyceum)				
Scientific lyceum	—	—	—	0.182
				(0.447)
Other lyceum	—	—	—	0.379
				(0.455)
Commercial or vocational	—	—	—	0.305
				(0.454)
Technical, other or foreign	—	—	—	0.571
				(0.519)
Income				
Q2	-0.064	-0.023	-0.125*	0.385
	(0.057)	(0.070)	(0.076)	(0.354)
Q3	-0.030	-0.093	-0.173**	0.137
	(0.059)	(0.076)	(0.081)	(0.227)
Q4	-0.177***	-0.047	-0.246***	-0.202
	(0.053)	(0.076)	(0.075)	(0.263)
Macro-region (Ref.: South and Islands)				
North-East	-0.086	-0.027	-0.055	0.191
	(0.055)	(0.073)	(0.074)	(0.294)
Center	-0.062	0.025	0.033	0.510
	(0.055)	(0.072)	(0.076)	(0.520)
North-West	-0.103**	-0.082	-0.067	0.125
	(0.052)	(0.065)	(0.065)	(0.535)
Born in Italy	—	—	—	0.504
				(0.621)
Observations	2,430	1,508	1,504	196

Notes: OLS estimates. Columns (1) to (3): EVS items standardized to mean zero and unit variance, Italian samples (lucky charm: 1999 and 2009 waves pooled, with the survey year among the regressors; horoscope items: 1999 wave), weighted with the EVS survey weights, robust standard errors in parentheses. Column (4): agreement with "I avoid making important decisions or doing risky things on Friday the 17th" (1 to 7) among the UNIMORE survey respondents with complete information, unweighted, conventional standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A3: Pairwise correlations of the imputed superstition scores

	(1)	(2)	(3)	(4)	(5)	(6)
(1) Aggregate index	1.00					
(2) EVS index	0.98	1.00				
(3) Lucky charm	0.94	0.96	1.00			
(4) Consult horoscope	0.94	0.94	0.81	1.00		
(5) Account horoscope	0.96	0.99	0.96	0.89	1.00	
(6) Friday 17 belief	0.84	0.73	0.68	0.76	0.67	1.00
Student careers			104,941			

Notes: Pearson correlations of the imputed scores (before the 0–1 rescaling), one observation per student career at its first exam, in the exam records before the removal of singletons; correlations are computed pairwise.

Table A4: Superstition items in the EVS and in the UNIMORE survey

	EVS			UNIMORE survey		
	Lucky charm (1–10)	Consult horoscope (1–5)	Account horoscope (1–5)	Lucky charm (1–10)	Consult horoscope (1–5)	Account horoscope (1–5)
Panel A. Mean (standard deviation)						
All ages	2.245	2.166	1.466	3.127	1.629	1.446
	(2.251)	(1.363)	(0.841)	(2.314)	(0.921)	(0.729)
Aged 18–29	2.752	2.543	1.675	—	—	—
	(2.426)	(1.350)	(0.899)			

Panel B. Correlations within respondents	EVS 1999	UNIMORE survey
Lucky charm and consult horoscope	0.231	0.337
Lucky charm and account horoscope	0.332	0.369
Consult and account horoscope	0.519	0.704
Friday 17 statement and:		
lucky charm	—	0.291
consult horoscope	—	0.214
account horoscope	—	0.334
broken mirror	—	0.512
black cat	—	0.561
walking under a ladder	—	0.387

Notes: EVS: Italy, 1999 and 2009 pooled for lucky charm (N = 3,412) and 1999 only for the two horoscope items (N = 1,986/1,981 all ages; N = 724/442/440 aged 18–29), survey-weighted; UNIMORE survey: unweighted (N = 213). The survey respondents are bachelor's students, so the age split applies to the EVS only. All three items use the same response categories in both sources: lucky charm 1–10, both horoscope items five categories, from every day to never for consulting it and from always to never for taking it into account, recoded so higher values are more frequent. Don't-know answers are set to missing in both sources. Panel B Pearson correlations use N = 1,975–1,986 (EVS 1999) and 210–216 (survey).

Table A5: Imputation covariates: survey respondents and estimation sample

	Respondents	Estimation sample	Diff.	Std. diff. (%)
Women	0.639	0.533	0.106	21.6
Year of birth	2002.7	1991.6	11.1	222.3
Born in Italy	–	0.937	–	–
North-West	–	0.061	–	–
North-East	0.745	0.644	0.101	22.1
Center	0.051	0.040	0.011	5.1
South and Islands	0.144	0.192	-0.048	-13.0
Secondary-school diploma				
Classical lyceum	0.065	0.082	-0.018	-6.7
Scientific lyceum	0.227	0.383	-0.156	-34.3
Other lyceum	0.259	0.178	0.081	19.7
Commercial or vocational	0.347	0.146	0.201	47.9
Technical, other or foreign	0.102	0.211	-0.109	-30.3
University-fee quartile				
Quartile 1	0.306	0.223	0.083	18.9
Quartile 2	0.092	0.232	-0.140	-38.8
Quartile 3	0.418	0.305	0.113	23.7
Quartile 4	0.184	0.240	-0.056	-13.8
Number of students	216	106,328		

Notes: Survey respondents are the students matched to the February 2025 UNIMORE student survey; the estimation sample is the students of the harmonized exam-level sample, one row per student and degree program. Both groups come from the first-enrollment register, and the characteristics are those that enter the imputation of the survey superstition score (Appendix A). The standardized difference is the difference in means divided by the square root of the mean of the two group variances, in percent. Shares resting on fewer than 10 respondents with or without the characteristic are suppressed (–).

Table A6: Friday the 17th, superstition and other imputed traits: horse race

Trait	(1) FR17 × EVS index (0–1)	(2) FR17 × trait (0–1)	(3) Correlation
EVS index only (no trait)	-0.447***		
	(0.122)		
Extraversion	-0.385***	0.371**	-0.190
	(0.126)	(0.173)	
Agreeableness	-0.414***	0.328	-0.191
	(0.124)	(0.202)	
Conscientiousness	-0.558***	0.788***	0.207
	(0.124)	(0.159)	
Emotional stability	-0.442***	0.035	-0.209
	(0.127)	(0.204)	
Openness	-0.332**	0.437**	-0.383
	(0.132)	(0.190)	
Machiavellianism	-0.488***	-0.203	-0.243
	(0.129)	(0.182)	
Psychopathy	-0.430***	0.049	-0.479
	(0.142)	(0.194)	
Narcissism	-0.457***	0.313*	0.074
	(0.122)	(0.189)	
Trust	-0.491***	-0.207	-0.321
	(0.131)	(0.208)	
Sustainability	-0.472***	0.086	0.452
	(0.137)	(0.203)	
Financial literacy	-0.555***	-0.713***	-0.158
	(0.125)	(0.171)	
Impatience	-0.456***	0.054	0.282
	(0.126)	(0.197)	

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. All rows use one common exam-level sample of N = 1,503,996 observations: the harmonized exam-level sample restricted to students with all twelve imputed traits non-missing, further restricted to students with the EVS index non-missing, using the specification of Table 4: a regression of the exam grade on Friday the 17th interacted with the EVS index and (except in the first row) with the trait, day-of-week, day-of-month, month, year, course and individual fixed effects and the time-varying controls of column (5) of Table 2; standard errors clustered at the student level. The first row, *EVS index only (no trait)*, re-estimates the Friday the 17th × EVS interaction of Table 4 on this same sample, without any trait in the model. Each subsequent row adds one trait at a time: column (1) is the Friday the 17th × EVS index interaction and column (2) the Friday the 17th × trait interaction from the same regression, so the two columns test whether the EVS gradient survives once the trait may also moderate the Friday 17 effect. Column (3) is the Pearson correlation between the EVS index and the trait, one observation per student. Both scores are min-max rescaled to $[0, 1]$ within this common sample. Every trait, like the Friday 17 score, is imputed from the same characteristics measured in the February 2025 UNIMORE student survey (gender, birth year, born in Italy, secondary-school diploma type, university-fee quartile and macro-region; Appendix A) and winsorized at the 0.1st and 99.9th percentiles before rescaling, so the columns compare different weightings of the same characteristics rather than independent information. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B Construction of the Estimation Sample

Starting from the 1,940,817 passed exams held on weekdays other than public holidays between 2003 and 2024, we restrict the sample in five steps. First, we keep the exams held between January 2003 and December 2022 (1,737,081 exams) and drop records with a missing program, student or course identifier or with a placeholder course code (1,736,466). Second, we keep single course exams worth at most 18 credits, which excludes aggregated records of multi-module courses (1,736,466). Third, we drop programs whose graduates typically complete fewer than 100 credits, programs taught in blended or distance mode and exams held in calendar weeks 32, 34 and 52, when the university is largely closed (1,695,976). Fourth, we keep full-time students (1,686,172) and drop internship courses (1,638,641). Finally, we drop students who enrolled after the age of 35, which leaves 1,610,675 exams taken by 93,626 students in 7,018 courses and 218 degree programs. Removing the 6,755 exams that are singletons in the fixed effects of the regressions yields the estimation sample of 1,603,920 exams taken by 88,672 students in 6,749 courses and 217 degree programs.

Table B1 describes the harmonized estimation sample with one observation per student career.

Table B1: Descriptive statistics of covariates (enrollment-level)

	Mean	SD	Min	Max	N
Average test score (18–31)	25.64	2.50	18.0	31.0	106,334
Ever passed an exam on Friday the 17th	0.110	0.313	0.0	1.0	106,334
Age at enrollment	21.0	2.8	17.0	35.0	106,334
No classical or scientific lyceum diploma	0.521	0.500	0.0	1.0	103,328
Women	0.533	0.499	0.0	1.0	106,334
Born abroad	0.063	0.243	0.0	1.0	106,334
North-West	0.065	0.247	0.0	1.0	99,659
North-East	0.687	0.464	0.0	1.0	99,659
Center	0.043	0.203	0.0	1.0	99,659
South and Islands	0.205	0.404	0.0	1.0	99,659
Master's degree	0.372	0.483	0.0	1.0	106,334
Social Sciences & Humanities	0.263	0.440	0.0	1.0	106,334
Business & Law	0.247	0.431	0.0	1.0	106,334
Engineering, Physical & Natural Sciences	0.360	0.480	0.0	1.0	106,334
Biomedical & Clinical Sciences	0.130	0.336	0.0	1.0	106,334
Income quartile 1	0.276	0.447	0.0	1.0	105,960
Income quartile 2	0.305	0.461	0.0	1.0	105,960
Income quartile 3	0.206	0.405	0.0	1.0	105,960
Income quartile 4	0.212	0.409	0.0	1.0	105,960

Notes: One observation per student career within the harmonized sample. Test scores range from 18 to 30, with 30 cum laude coded as 31. Average test score and Friday 17 exposure are computed over the student's exams in the harmonized sample.

Table B2: Descriptive statistics of covariates (exam-level, instructor subsample)

	Mean	SD	Min	Max	N
Exam outcome and exposure					
Test score (18–31)	25.96	3.71	18.0	31.0	582,744
Exam on Friday the 17th	0.0093	0.0958	0.0	1.0	582,744
Student characteristics					
Age at exam	22.8	2.7	18.0	46.0	582,744
Women	0.549	0.498	0.0	1.0	582,744
Born abroad	0.057	0.232	0.0	1.0	582,744
Educational background					
No classical or scientific lyceum diploma	0.532	0.499	0.0	1.0	572,352
Region of birth					
North-West	0.068	0.251	0.0	1.0	549,581
North-East	0.690	0.463	0.0	1.0	549,581
Center	0.048	0.213	0.0	1.0	549,581
South and Islands	0.194	0.396	0.0	1.0	549,581
Degree characteristics					
Master's degree	0.366	0.482	0.0	1.0	582,744
Social Sciences & Humanities	0.294	0.456	0.0	1.0	582,744
Business & Law	0.226	0.418	0.0	1.0	582,744
Engineering, Physical & Natural Sciences	0.362	0.481	0.0	1.0	582,744
Biomedical & Clinical Sciences	0.118	0.323	0.0	1.0	582,744
Socioeconomic background					
Income quartile 1	0.259	0.438	0.0	1.0	581,568
Income quartile 2	0.315	0.464	0.0	1.0	581,568
Income quartile 3	0.206	0.405	0.0	1.0	581,568
Income quartile 4	0.220	0.414	0.0	1.0	581,568
Academic timing					
Year of study	2.103	1.173	1.0	14.0	582,744
Years since first session	0.531	0.861	0.0	4.6	582,744

Notes: One observation per passed exam. Test scores range from 18 to 30, with 30 cum laude coded as 31. All statistics are computed on the instructor subsample, the exams with non-missing instructor information used in the instructor specifications (N = 582,744). Region-of-birth shares are computed on non-missing region of birth.

Table B3: Descriptive statistics of covariates (enrollment-level, instructor subsample)

	Mean	SD	Min	Max	N
Exam outcomes and exposure					
Average test score (18–31)	25.79	2.56	18.0	31.0	55,786
Ever passed an exam on Friday the 17th	0.093	0.290	0.0	1.0	55,786
Student characteristics					
Age at enrollment	20.9	2.7	17.0	35.0	55,786
Women	0.535	0.499	0.0	1.0	55,786
Born abroad	0.063	0.242	0.0	1.0	55,786
Educational background					
No classical or scientific lyceum diploma	0.529	0.499	0.0	1.0	54,575
Region of birth					
North-West	0.068	0.251	0.0	1.0	52,293
North-East	0.676	0.468	0.0	1.0	52,293
Center	0.050	0.217	0.0	1.0	52,293
South and Islands	0.207	0.405	0.0	1.0	52,293
Degree characteristics					
Master's degree	0.393	0.488	0.0	1.0	55,786
Social Sciences & Humanities	0.289	0.454	0.0	1.0	55,786
Business & Law	0.207	0.405	0.0	1.0	55,786
Engineering, Physical & Natural Sciences	0.376	0.484	0.0	1.0	55,786
Biomedical & Clinical Sciences	0.128	0.334	0.0	1.0	55,786
Socioeconomic background					
Income quartile 1	0.277	0.448	0.0	1.0	55,634
Income quartile 2	0.329	0.470	0.0	1.0	55,634
Income quartile 3	0.166	0.372	0.0	1.0	55,634
Income quartile 4	0.227	0.419	0.0	1.0	55,634

Notes: One observation per student career within the instructor subsample. Test scores range from 18 to 30, with 30 cum laude coded as 31. Average test score and Friday 17 exposure are computed over the student's exams in that subsample. Region-of-birth shares are computed on non-missing region of birth.

C Selection into Passed Exams

This appendix examines selection into the set of passed exams observed in the administrative data. We first construct the set of exam opportunities available to each student, then estimate whether Friday the 17th affects the probability of passing at a given session. We next examine how this selection changes the composition of students whose grades are observed, and finally estimate a censored-grade model that incorporates sessions without a recorded passing grade.

C.1 Constructing the Exam-Opportunity Sample

Our administrative data record passed exams only, so the estimates in the main text condition on passing. To assess selection, we reconstruct the exam sessions at which each student could have taken each compulsory course of the program. We define a course as compulsory for a program and cohort when at least 80 percent of the graduates of that cohort passed it; its sessions are the dates on which at least one student of the program and cohort passed it, with dates at most seven days apart merged into one session, dated at the day on which most students passed. A session is therefore observed only if at least one student passes it.

A student is at risk at every session of a compulsory course from the first one until the session at which the course is passed. We stop following a course after more than six months without an observed session, since such a long gap is likely to hide sessions at which nobody passed, and we exclude program cohorts with fewer than 25 students, in which a session is more likely to go unobserved because no student passes it. The outcome is an indicator for passing the exam at a session, which cannot distinguish a student who did not attend from one who failed or declined the grade. The unit of observation is therefore a student, course and session, a sample distinct from the exam-level sample of the main analysis. The same session calendar defines the scheduled exposure to Friday the 17th in Appendix Table D2.

C.2 Friday the 17th and the Probability of Passing

Appendix Table C1 reports the effect of a Friday 17 session on the probability of passing the exam at the first session of the course. With the full set of fixed effects, the probability falls by 6.0 percentage points (column (5)). The effect shrinks when students are followed over more

sessions, to 3.8, 2.6 and 1.8 percentage points for three, nine and all sessions.

Part of this decline reflects the lower passing rate at later sessions (Appendix Figure C1), which averages 16.4, 16.5, 13.1 and 11.4 percent over one, three, nine and all sessions. Relative to these rates the effect still shrinks, from 37 percent of the passing rate at the first session to 23, 20 and 16 percent, so fewer students passing at later sessions explains the decline at long horizons only in part. The 17th of the month on other weekdays does not affect passing: the four placebo coefficients in Appendix Table C2 range from -0.006 to 0.003.

Appendix Table C3 interacts the Friday 17 indicator with the standardized superstition scores at the first session. A one-standard-deviation increase in the EVS-based scores lowers the probability of passing on a Friday the 17th by a further 0.9 to 1.1 percentage points, against an effect of 6.1 points at the mean score, while the survey measure of the Friday 17 belief shows no gradient. When students are followed for three sessions, the EVS-based gradients shrink to 0.3 to 0.7 percentage points, significant at the 5 percent level for one score only, while the survey measure of the Friday 17 belief shows a gradient of the opposite sign, 0.9 points. No gradient remains when students are followed for nine or more sessions.

C.3 Selection into Observed Grades

Appendix Table C4 compares the students who pass at the first session with those who do not, separately for Friday the 17th and other sessions. The pass rate is 11.3 percent on a Friday the 17th against 16.4 percent otherwise (538 of 4,749 and 156,924 of 955,412 sessions).

Those who pass on a Friday the 17th are more positively selected than those who pass on other days: the standardized difference between passers and non-passers in the average grade obtained so far is 56 percent, against 26 percent on other days, and passers on a Friday the 17th are less often women and less superstitious according to the EVS score. The grades observed on Friday the 17th thus come from stronger students, which implies that conditioning on passed exams attenuates the estimated Friday 17 penalty.

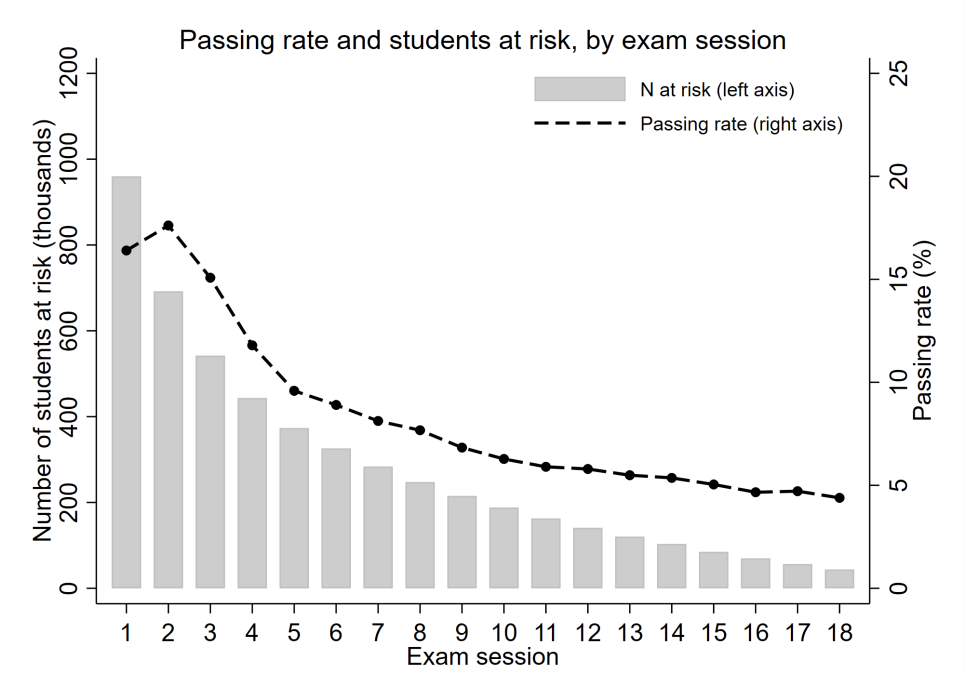
C.4 Censored Grades

The grade itself is observed only above the passing threshold. Appendix Table C5 therefore estimates a Tobit model of the grade on the same sessions, coding sessions without a recorded

grade at 17 and treating them as censored below the passing grade of 18. With calendar and course fixed effects, the student's average grade and the time-varying controls, a Friday 17 session lowers the expected censored grade by 0.41 points (column (5)), and by 0.34 points when students are followed for three sessions.

The effect is about three times that estimated on passed exams in Table 2, since the censored grade combines lower grades with a lower probability of passing. We estimate the censored-grade model for the first session and for the first three sessions only.[13]

Figure C1: Passing rate and students at risk, by exam session



Notes: At-risk panel of Appendix C, from the first session of each compulsory course until the session at which the student passes it. Bars: number of student-course pairs at risk at each session, in thousands (left axis). Line: share of them passing the exam at that session (right axis). Sessions 1 to 18.

[13] Estimating the corresponding specifications over longer horizons is computationally prohibitive with the full set of course fixed effects.

Table C1: Friday the 17th and passing the exam at the first session

	Passed				
	(1)	(2)	(3)	(4)	(5)
Exam on FR17	-0.051***	-0.058***	-0.050***	-0.051***	-0.060***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	960,161	960,161	960,161	960,042	960,042
Clusters	53,640	53,640	53,640	53,544	53,544
Day-of-week FE	.	✓	.	.	✓
Day-of-month FE	.	✓	.	.	✓
Month-of-year FE	.	✓	.	.	✓
Year FE	.	✓	.	.	✓
Course FE	.	.	✓	.	✓
Individual FE	.	.	.	✓	✓
Time-varying X	.	.	.	✓	✓
Nr X	1	1	1	6	6
Nr FEs	0	64	2,147	61,209	63,417
R^2	0.000	0.028	0.148	0.208	0.315

Notes: One observation per student, compulsory course and exam session at which the student is at risk of taking the exam, restricted to the first session of each course (Appendix C). The dependent variable is an indicator for passing the exam at the session. Columns (4) and (5) control for the time since enrollment, pre- and post-holiday indicators over seven days, bridge days and the number of the student's compulsory-course sessions from seven days before to one day after the session. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C2: Placebo: passing at sessions on the 17th of other weekdays

	Passing exam (0–1)				
	Monday	Tuesday	Wednesday	Thursday	Friday
Exam on the 17th	0.002	0.003	-0.000	-0.006	-0.060***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Observations	955,214	955,214	955,214	955,214	960,042
Clusters	53,478	53,478	53,478	53,478	53,544
Day-of-week FE	✓	✓	✓	✓	✓
Day-of-month FE	✓	✓	✓	✓	✓
Month-of-year FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Program FE	.	.	.	.	.
Course FE	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓
Time-varying X	✓	✓	✓	✓	✓
Nr X	6	6	6	6	6
Nr FEs	63,337	63,337	63,337	63,337	63,417
R^2	0.315	0.315	0.315	0.315	0.315

Notes: Sample and specification of column (5) of Table C1, replacing the Friday 17 indicator with an indicator for sessions held on the 17th of the month when it falls on the weekday in the column header. The Monday to Thursday columns exclude the 4,749 sessions held on a Friday the 17th, so that they do not enter the comparison group, hence their smaller number of observations; the Friday column is the Friday 17 effect of column (5) of Table C1, on the full sample. Observations are net of the singleton groups dropped by the fixed-effects estimator. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C3: Friday the 17th, superstition and passing at the first session

	(1) Aggregate index	(2) EVS index	(3) Lucky charm	(4) Consult horoscope	(5) Account horoscope	(6) Friday 17 belief
Exam on FR17	-0.061***	-0.061***	-0.061***	-0.062***	-0.062***	-0.061***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
FR17 × score	-0.009*	-0.011**	-0.009**	-0.011**	-0.011**	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	892,290	892,290	892,290	892,290	892,290	892,290
R^2	0.317	0.317	0.317	0.317	0.317	0.317

Notes: The dependent variable is a binary indicator equal to one if the student passed the exam, estimated on the at-risk panel (student times compulsory course times session), following the specification and sample of column (5) of Table C1: full fixed effects (exam year, exam month, day, day of week, course and individual) and the time-varying controls of that column, with standard errors clustered at the student level. The sample is restricted to students born in Italy with all six superstition scores non-missing, as in Table 4, so that all six columns share the same number of observations. The scores are standardized to mean zero and unit variance, not rescaled to $[0, 1]$ as in Table 4. The first row reports the Friday 17 effect at the mean score; the second reports the change in that effect for a one-standard-deviation increase in the score. Column (1) uses the aggregate index, columns (2) to (5) use the EVS-based scores; column (6) uses the student-survey measure of belief in Friday the 17th. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C4: Who passes at the first session: Friday the 17th and other sessions

	Exam on FR17		Exam not on FR17		Std. diff. (%)	
	Passed (1)	Not passed (2)	Passed (3)	Not passed (4)	FR17 (5)	Other (6)
Student's passing rate at previous sessions	0.304	0.196	0.316	0.182	51.7	62.0
Course's passing rate at previous sessions	0.227	0.145	0.204	0.138	64.6	56.7
Student's average grade in previous exams	26.5	24.8	25.9	25.1	55.9	26.0
Course's average grade in previous exams	26.2	26.5	26.0	26.1	-14.8	-6.5
Course's passing rate in previous exams	0.338	0.153	0.313	0.148	105.2	93.0
Superstition score (EVS)	-0.236	-0.100	-0.089	-0.008	-14.4	-8.4
High-school diploma grade	84.4	79.5	83.7	79.8	42.0	32.6
Women	0.383	0.485	0.513	0.531	-20.6	-3.6
Born abroad	0.058	0.090	0.052	0.073	-12.2	-8.6
Year of birth	1989.2	1990.8	1989.2	1990.5	-29.9	-23.1
No classical or scientific lyceum diploma	0.533	0.541	0.522	0.565	-1.5	-8.7
North-West	0.065	0.055	0.058	0.062	4.1	-1.8
North-East	0.765	0.740	0.739	0.730	5.8	2.2
Center	0.032	0.021	0.027	0.028	6.5	-0.3
South and Islands	0.138	0.183	0.176	0.181	-12.3	-1.3
Fee-exempt	0.132	0.054	0.033	0.019	27.0	9.3
Income quartile 1	0.197	0.240	0.237	0.257	-10.3	-4.6
Income quartile 2	0.169	0.321	0.260	0.283	-35.9	-5.2
Income quartile 3	0.242	0.140	0.191	0.176	26.0	4.0
Income quartile 4	0.260	0.245	0.279	0.266	3.6	2.9
Years since enrollment	1.307	1.503	1.545	1.491	-23.9	6.4
Master's degree	0.193	0.214	0.258	0.275	-5.2	-3.9
Course credits	6.6	7.2	6.9	7.6	-22.6	-24.8
N (sessions)	538	4,211	156,924	798,488		

Notes: Sample of Table C1 (first session of each compulsory course). Rows on the student describe the student's earlier sessions and passed exams; rows on the course describe the earlier sessions and passed exams of all students of the course, and measure how hard the course is. Columns (1) to (4) report the means of students who pass and do not pass, for sessions on a Friday the 17th and on other days; columns (5) and (6) report the standardized difference between passers and non-passers within each type of day (passers minus non-passers, divided by the square root of the mean of the two group variances, in percent). If selection into passing were the same on Friday the 17th and on other days, the two columns would be close. Region shares refer to students born in Italy.

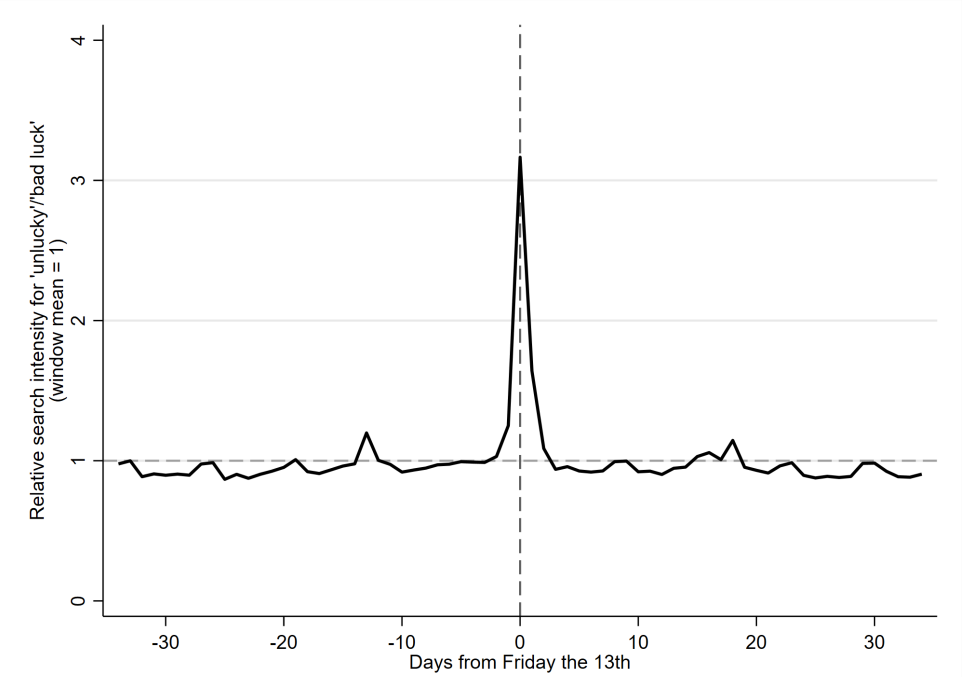
Table C5: Friday the 17th and the grade at the first session, Tobit model

	Test score (Tobit, 17–31)				
	(1)	(2)	(3)	(4)	(5)
Exam on FR17	-0.487***	-0.489***	-0.442***	-0.280***	-0.414***
	(0.041)	(0.047)	(0.044)	(0.041)	(0.047)
Observations	960,161	960,161	960,161	960,161	960,161
Clusters	53,640	53,640	53,640	53,640	53,640
Day-of-week FE	.	✓	.	.	✓
Day-of-month FE	.	✓	.	.	✓
Month-of-year FE	.	✓	.	.	✓
Year FE	.	✓	.	.	✓
Course FE	.	.	✓	✓	✓
Individual avg. grade	.	.	.	✓	✓
Time-varying X	.	.	.	✓	✓
Nr X	0	0	0	7	7
Nr FEs	0	63	2,146	2,146	2,209
Pseudo R^2	0.000	0.016	0.070	0.112	0.117

Notes: Sample of Table C1. Tobit model of the exam grade in which sessions without a recorded grade are coded 17 and treated as censored below the passing grade of 18. The coefficients are average marginal effects of a Friday the 17th session on the expected censored grade. Columns (4) and (5) control for the student's average grade instead of student fixed effects, and for the time-varying controls of Table C1. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

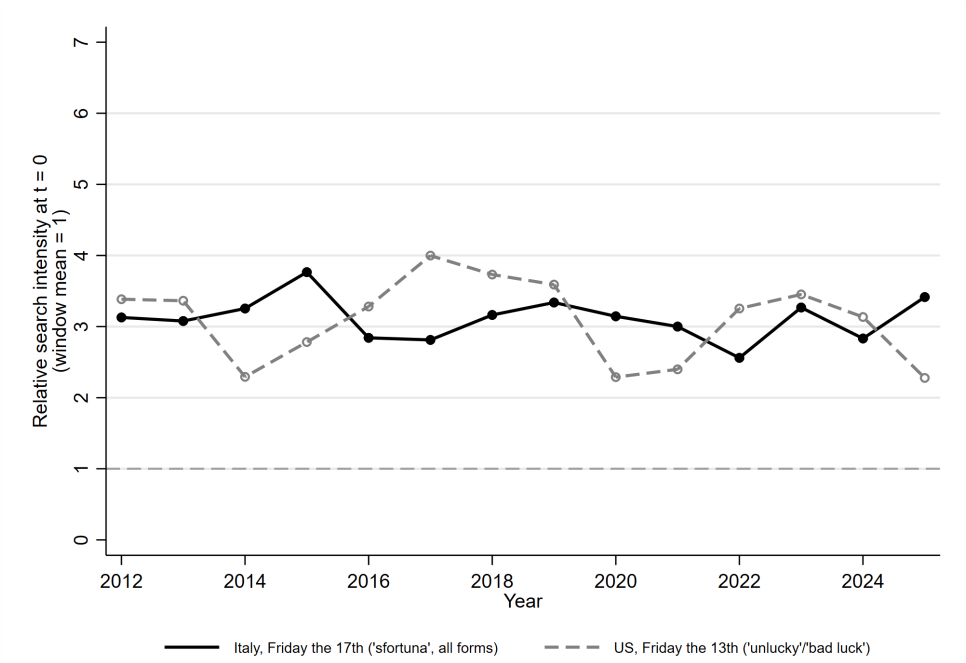
D Additional Figures and Tables

Figure D1: Google searches for bad luck around Friday the 13th in the United States



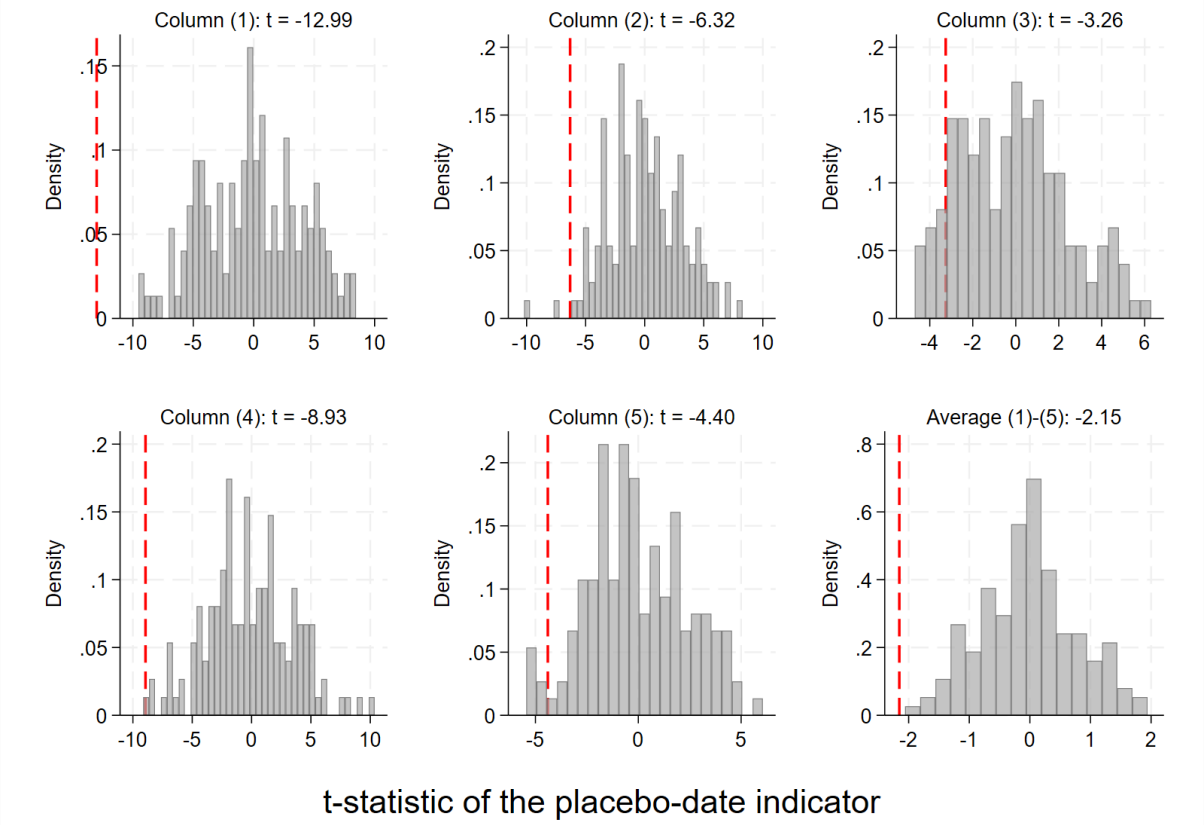
Notes: The figure reports Google Trends search intensity for the terms "unlucky" and "bad luck" around Friday the 13th in the United States. Each window covers 34 days before and after Friday the 13th and is rescaled so that the window mean equals one.

Figure D2: Salience of unlucky Fridays over time



Notes: The figure reports relative Google Trends search intensity on unlucky Fridays over time. The solid line shows searches for *"sfortuna"* and *"sfortunato"* around Friday the 17th in Italy. The dashed line shows searches for "unlucky" and "bad luck" around Friday the 13th in the United States.

Figure D3: Permutation test: Friday the 17th against 149 placebo dates, by specification



Notes: Each histogram shows the t-statistics of 149 placebo indicators, one for each combination of weekday (Monday to Friday) and day of the month (1 to 30) other than Friday the 17th. Each placebo indicator is added, one at a time, to the column of Table 2 named in the panel heading, which keeps the Friday 17 indicator; columns (1) to (5) use the harmonized exam-level sample. The red line is the t-statistic of Friday the 17th in that column. The last panel summarizes each date by its average t-statistic over columns (1) to (5), each divided by the standard deviation of that column's placebo t-statistics. Standard errors clustered at the student level. Table D5 reports the corresponding permutation p-values.

Table D1: Probability of scheduling an exam on Friday the 17th, all weekdays

	Pr(exam on Friday 17)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full weekday sample						
Year FE	.	✓	✓	✓	✓	
Month FE	.	.	✓	✓	✓	
Day-of-week FE	.	.	✓	✓	✓	
Day-of-month FE	.	.	✓	✓	✓	
Course FE	.	.	.	✓	✓	
Faculty FE	.	.	.	.	✓	
Observations	83,571	83,571	83,571	83,571	83,571	
Nr FEs	0	21	66	2,194	2,203	
R^2	0.0000	0.0046	0.1996	0.2213	0.2213	
Adj. R^2	0.0000	0.0044	0.1990	0.2003	0.2003	
Panel B: Instructor subsample						
Year FE	.	✓	✓	✓	✓	✓
Month FE	.	.	✓	✓	✓	✓
Day-of-week FE	.	.	✓	✓	✓	✓
Day-of-month FE	.	.	✓	✓	✓	✓
Course FE	.	.	.	✓	✓	✓
Faculty FE	.	.	.	.	✓	.
Instructor FE	.	.	.	.	.	✓
Observations	25,776	25,776	25,776	25,773	25,773	25,762
Nr FEs	0	9	54	956	957	1,266
R^2	0.0000	0.0058	0.2416	0.2653	0.2653	0.2738
Adj. R^2	0.0000	0.0055	0.2401	0.2370	0.2370	0.2363

Notes: Sample is one observation per (course, exam date) on weekdays (Mon–Fri), from the session calendar of the compulsory courses of Appendix C. Outcome is 100 if the exam falls on Friday the 17th, 0 otherwise. Panel A: full sample. Panel B: subsample with non-missing instructor ID. Adding course, faculty, or instructor FE barely changes the adjusted R^2 obtained with the calendar fixed effects of column (3): scheduling on Friday the 17th is essentially unpredictable from these dimensions.

Table D2: Balance of student characteristics: ever scheduled a Friday 17 exam (compulsory-exam sample)

	Ever scheduled on a Friday the 17th		Mean differences	Std. diff. (%)
	Yes	No		
Age at enrollment	20.6	20.9	-0.3	-11.9
No classical or scientific lyceum diploma	0.574	0.534	0.040	8.0
Women	0.499	0.566	-0.068	-13.7
Born abroad	0.085	0.057	0.028	10.8
North-West	0.063	0.062	0.001	0.3
North-East	0.717	0.706	0.011	2.5
Center	0.029	0.037	-0.008	-4.3
South and Islands	0.191	0.195	-0.004	-1.1
Master's degree	0.288	0.379	-0.091	-19.4
Social Sciences & Humanities	0.271	0.317	-0.046	-10.1
Business & Law	0.252	0.257	-0.005	-1.1
Engineering, Physical & Natural Sciences	0.405	0.339	0.066	13.7
Biomedical & Clinical Sciences	0.072	0.087	-0.015	-5.5
Income quartile 1	0.264	0.258	0.006	1.5
Income quartile 2	0.340	0.321	0.019	4.0
Income quartile 3	0.201	0.191	0.010	2.4
Income quartile 4	0.196	0.230	-0.035	-8.5
Number of student careers	19,943	37,642		

Notes: One observation per student career of the harmonized exam-level sample; restricted to careers matched to the reconstructed session calendar of their compulsory exams (Appendix C), Yes indicating that at least one session of a compulsory exam the student had not yet passed was scheduled on a Friday the 17th. The columns report the group means, their difference (Yes minus No) and the standardized difference, that is, the difference in means divided by the square root of the mean of the two group variances, in percent.

Table D3: Balance of student characteristics: exposure to a Friday 17 exam

	Experienced a Friday the 17th		Mean differences	Std. diff. (%)
	Yes	No		
Age at enrollment	20.6	21.0	-0.4	-15.5
No classical or scientific lyceum diploma	0.525	0.521	0.004	0.9
Women	0.505	0.537	-0.031	-6.3
Born abroad	0.058	0.063	-0.005	-2.0
North-West	0.065	0.065	0.000	0.0
North-East	0.696	0.686	0.010	2.1
Center	0.047	0.042	0.004	2.0
South and Islands	0.192	0.206	-0.014	-3.5
Master's degree	0.315	0.379	-0.065	-13.6
Social Sciences & Humanities	0.297	0.259	0.038	8.5
Business & Law	0.263	0.245	0.018	4.2
Engineering, Physical & Natural Sciences	0.351	0.361	-0.010	-2.2
Biomedical & Clinical Sciences	0.089	0.135	-0.046	-14.7
Income quartile 1	0.277	0.276	0.001	0.3
Income quartile 2	0.327	0.303	0.024	5.2
Income quartile 3	0.196	0.208	-0.011	-2.9
Income quartile 4	0.200	0.214	-0.014	-3.5
Number of student careers	11,744	94,590		

Notes: One observation per student career of the harmonized exam-level sample; Yes indicates at least one exam passed on a Friday the 17th. The columns report the group means, their difference (Yes minus No) and the standardized difference, that is, the difference in means divided by the square root of the mean of the two group variances, in percent.

Table D4: Placebo: exams on the 17th of other weekdays

	Test score (18–31)				
	Monday	Tuesday	Wednesday	Thursday	Friday
Exam on the 17th	-0.00	-0.04	0.01	0.03	-0.13***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	1,591,116	1,591,116	1,591,116	1,591,116	1,603,920
Clusters	88,602	88,602	88,602	88,602	88,672

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. Each column estimates the specification of column (5) of Table 2 on the harmonized exam-level sample, replacing the Friday 17 indicator with an indicator for exams held on the 17th of the month when it falls on the weekday in the column header. The Monday to Thursday columns exclude the Friday 17 exams; the Friday column is the Friday 17 effect. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D5: Permutation test by specification

	Friday 17	Friday 17	SD of placebo	Placebo dates	Permutation p-value	
	coefficient	t	t-statistics	at or below	two-sided	one-sided
Column (1)	-0.441	-12.99	4.09	0	0.007	0.007
Column (2)	-0.241	-6.32	3.14	2	0.040	0.020
Column (3)	-0.100	-3.26	2.48	14	0.207	0.100
Column (4)	-0.252	-8.93	3.67	1	0.027	0.013
Column (5)	-0.131	-4.40	2.40	6	0.087	0.047
Average t over columns (1)–(5)		-7.18		0	0.007	0.007
standardized within column		-2.15		0	0.007	0.007
average percentile rank		0.037		0		0.007

Notes: Columns (1) to (5) of Table 2, which share the harmonized exam-level sample, are re-estimated adding, one at a time, an indicator for each of the 149 other combinations of weekday (Monday to Friday) and day of the month (1 to 30); every placebo regression keeps the true Friday 17 indicator, and standard errors are clustered at the student level. The column of placebo dates at or below counts the placebo dates whose t-statistic is at least as negative as that of Friday the 17th. The permutation p-value is that count plus one, divided by 150: with 149 placebo dates the resolution of the test is $1/150$, so a count of zero does not mean a probability of zero, and the $(r+1)/(n+1)$ convention never returns zero. Under correctly sized standard errors the placebo t-statistics would have a standard deviation of one. The last block summarizes each date with one number computed identically for Friday the 17th and for every placebo date: the average t over the columns, the same average after dividing each column's t by the standard deviation of that column's placebo t-statistics, and the average of the within-column percentile ranks. The two-sided p-value counts the placebo dates at least as far from zero in either direction, and is the one reported in the text; it has no counterpart for the percentile rank, which is directional by construction. Friday the 17th is consistently negative in every column, whereas a placebo date extreme in one column is rarely extreme in the others.

Table D6: Friday 17 penalty in the benchmark group: students born abroad versus students born abroad or without Italian citizenship

	Test score (18–31)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Students born abroad (benchmark definition, as in Table 4)												
Exam on Friday the 17th	-0.38***	-0.22	-0.11	-0.31**	-0.05	-0.11	-0.32**	-0.08	-0.03	-0.05	-0.04	-0.31**
	(0.15)	(0.17)	(0.14)	(0.12)	(0.13)	(0.15)	(0.14)	(0.12)	(0.13)	(0.13)	(0.14)	(0.13)
Observations	93,712	93,712	92,809	93,712	92,799	92,809	93,712	92,799	92,799	92,799	92,809	93,712
Clusters	5,759	5,759	5,746	5,759	5,742	5,746	5,759	5,742	5,742	5,742	5,746	5,759
Panel B. Students born abroad or without Italian citizenship												
Exam on Friday the 17th	-0.41***	-0.25	-0.13	-0.33***	-0.06	-0.12	-0.33**	-0.09	-0.04	-0.06	-0.05	-0.32**
	(0.15)	(0.17)	(0.14)	(0.12)	(0.13)	(0.15)	(0.14)	(0.12)	(0.13)	(0.13)	(0.14)	(0.13)
Observations	94,762	94,762	93,869	94,762	93,859	93,869	94,762	93,859	93,859	93,859	93,869	94,762
Clusters	5,832	5,832	5,820	5,832	5,816	5,820	5,832	5,816	5,816	5,816	5,820	5,832
Time FE (year, month, day, day of week)	.	✓	.	.	✓	✓	✓	.	✓	✓	✓	✓
Course FE	.	.	✓	.	✓	✓	.	✓	✓	✓	✓	.
Individual FE	.	.	.	✓	✓	.	✓	✓	✓	✓	.	✓
Year × Faculty FE	.	.	.	.	.	.	.	.	✓	.	.	.
Prior individual avg. grade	.	.	.	.	.	.	.	.	.	.	✓	.
Prior course avg. grade	.	.	.	.	.	.	.	.	.	.	.	✓
Time-varying X	.	.	.	✓	✓	partial	✓	✓	✓	✓	✓	✓

Notes: Each cell reports the coefficient on the Friday 17 indicator from a separate regression on the benchmark group, with standard errors clustered at the student level in parentheses. Panel A uses the definition of the main text (students born abroad); Panel B widens the group to students who are either born abroad or without Italian citizenship. Column (10) replaces calendar-year with academic-year fixed effects. Specifications follow the columns of the main results tables. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D7: Effect of Friday the 17th by student characteristics

	FR17 effect	Observations
Panel A: gender		
Men	-0.140***	741,073
	(0.040)	
Women	-0.122***	862,847
	(0.039)	
p-value, equal effects	0.727	
Panel B: degree level		
Bachelor's degree	-0.195***	1,048,328
	(0.036)	
Master's degree	0.014	555,592
	(0.044)	
p-value, equal effects	0.000	
Panel C: macro-region of birth		
North-West	-0.120	96,187
	(0.103)	
North-East	-0.067*	1,067,195
	(0.035)	
Center	-0.300***	57,793
	(0.111)	
South and Islands	-0.321***	289,033
	(0.062)	
Born abroad	-0.141	93,712
	(0.116)	
p-value, equal effects	0.003	

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. Panels A to C use the harmonized exam-level sample and the specification of column (5) of Table 2, with the Friday 17 indicator interacted with each group; the group main effects are absorbed by the student fixed effects. The p-value tests that the Friday 17 effect is the same in all groups of the panel. Observations are the exams in each group. Standard errors clustered at the student level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D8: Superstition gradient of the Friday 17 effect within student groups

Panel A. Gender and degree level

	Gender		Degree level	
	Men	Women	Bachelor's	Master's
FR17 × EVS index (0–1)	-0.995***	-0.790***	-0.589***	0.001
	(0.255)	(0.228)	(0.157)	(0.197)
FR17 at index 0	0.235**	0.403***	0.131	-0.005
	(0.101)	(0.156)	(0.093)	(0.104)
Exams	697,972	806,024	977,834	526,162
p-value, equal gradients	0.548		0.019	

Panel B. Macro-region of birth

	North-West	North-East	Center	South and Islands
FR17 × EVS index (0–1)	-0.600	-0.468***	0.252	-0.407
	(0.484)	(0.147)	(0.504)	(0.282)
FR17 at index 0	0.173	0.172**	-0.408	-0.096
	(0.259)	(0.083)	(0.277)	(0.164)
Exams	95,629	1,065,236	56,871	286,260
p-value, equal gradients	0.565			

Notes: The dependent variable is the exam grade, from 18 to 30, with 30 cum laude coded as 31. Sample and specification are those of Table 4: the harmonized exam-level sample of students born in Italy with all six superstition scores non-missing, controls for the time since enrollment, the time since the first exam session of the course, an indicator for another exam in the previous seven days, indicators for the seven days before and after a holiday and a bridge-day indicator, and day-of-week, day-of-month, month, year, course and individual fixed effects; standard errors clustered at the student level. The EVS index is min-max rescaled to $[0, 1]$ within this sample, as in column (3) of Table 4. Each group variable enters one pooled regression in which Friday the 17th and its interaction with the index are further interacted with the group named in the column heading (gender and degree level in Panel A, macro-region of birth in Panel B); group main effects, and any other student characteristic that does not vary within a career, are absorbed by the individual fixed effects and dropped automatically. *FR17 × EVS index (0–1)* is the Friday 17 superstition gradient in each group: the coefficient on the interaction in the base group (men, Bachelor's students, North-East) plus the group-specific increment. *FR17 at index 0* is the corresponding Friday 17 effect at the lowest value of the index in the common sample. *p-value, equal gradients* is the p-value of the joint test that the group-specific increments to the gradient are zero, i.e. that the gradient is equal across the columns of the panel. The macro-region model is estimated on students with a known region of birth (North-West, North-East, Center or South and Islands); the base group is North-East. Within each group, the EVS index varies only through student characteristics other than the one defining the group, since gender, degree level and region of birth do not vary within a student's career. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D9: Event study: robustness of the estimates at event times 0, 1 and 4

	Event time 0 (Friday 17)	Event time 1 (next exam)	Event time 4	Observations
(1) Baseline	-0.208***	0.061**	0.125***	1,538,941
	(0.032)	(0.029)	(0.034)	
(2) Baseline without the course mean grade	-0.276***	0.085***	0.080**	1,538,941
	(0.034)	(0.031)	(0.037)	
(3) Without course-year fixed effects	-0.211***	0.056*	0.118***	1,538,941
	(0.032)	(0.029)	(0.034)	
(4) Without course-year fixed effects and without the course mean grade	-0.289***	0.069**	0.056	1,538,941
	(0.035)	(0.032)	(0.037)	
(5) Fixed effects only, no controls	-0.325***	0.093***	0.054	1,538,941
	(0.035)	(0.032)	(0.037)	
(6) Baseline controls and nine additional characteristics	-0.185***	0.052*	0.124***	1,538,941
	(0.032)	(0.029)	(0.034)	
(7) Nine additional characteristics, without the course mean grade	-0.205***	0.045	0.117***	1,538,941
	(0.032)	(0.029)	(0.034)	
(8) Course mean grade within degree program and cohort	-0.249***	0.086***	0.103***	1,538,941
	(0.033)	(0.030)	(0.035)	
(9) No stacking	-0.225***	0.058*	0.117***	1,526,024
	(0.033)	(0.031)	(0.035)	
(10) Exams between two events split at the median	-0.207***	0.059**	0.118***	1,532,940
	(0.032)	(0.030)	(0.035)	

Notes: Estimates from the imputation estimator of Borusyak, Jaravel and Spiess on all students in the Friday 17 event-study sample, with 11 exams before and after the event; standard errors clustered by student in parentheses. Row (1) is the specification of Figure 4. Row (2) drops the average grade in the course on earlier dates. Row (3) drops the course-year fixed effects; rows (4) to (10) start from row (3). Row (4) also drops the average grade in the course on earlier dates. Row (5) keeps only the fixed effects, without controls and month fixed effects. Row (6) adds to row (3) nine characteristics of the exam: credits, log sitting size, course hours, deciles of the course mean grade, week of year, activity type and language, course year, the instructor's mean grade and rank; row (7) drops the course mean grade from row (6), keeping its deciles and the instructor's mean grade. Row (8) computes the course mean grade within the student's degree program and cohort. Row (9) estimates the event study without stacking, with student fixed effects and the rows after a student's second Friday 17 exam dropped; row (10) assigns the exams between two Friday 17 exams of a student to the first event up to the median exam and to the second event after it. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D10: Event-study estimates of the Friday 17 effect by predicted superstition

	Test score (18–31)			
	All students	Low superstition	High superstition	High − Low
	(1)	(2)	(3)	(4)
Friday 17 exam (event time 0)	-0.208***	-0.086*	-0.304***	-0.218***
	(0.032)	(0.046)	(0.047)	(0.066)
Observations	1,538,941	719,346	724,512	

Notes: Estimates from the imputation estimator of Borusyak, Jaravel and Spiess, with the specification of Figure 4, estimated on all students (column (1)) and separately on Italian-born students below and at or above the median of the EVS-based superstition index (columns (2) and (3)), each event classified by the index at its Friday 17 exam and students who never sit a Friday 17 exam by the index at one of their exams drawn at random. Column (4) is the difference between columns (3) and (2), with standard error $\sqrt{se_2^2 + se_3^2}$. Standard errors clustered by student in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.