Reply to Kelly

This short note keeps track of Morgan Kelly’s (2021) critique of my paper “Devotion and Development: Religiosity, Education, and Economic Progress in 19th-Century France” (AER 2020). Since Kelly’s critique keeps changing, I will continue to update this note, summarizing the main points of each version as well as my responses.

While I thank Prof. Morgan Kelly for the reading of my article and for his interest in my results – and I truly believe in the importance of constructive criticism – I would have appreciated a more transparent and data-driven critique of my findings.

Background and answers to Kelly’s (2021) main points

I first received an email from Kelly on May 22nd, 2021: he attached a paper (that I denote as K1 – click here to download it) criticizing the findings of my article, and he asked for my comments. Kelly’s main points were that:

a) my cross-sectional department-level relationship between religiosity and economic development is confounded by living standards;

b) my main measure of religiosity, the share of refractory clergy, is not appropriate.

In my reply to Kelly (May 24th, 2021) I made him aware that his concerns were already addressed in my paper, thus disproving his main points. I specified that:

a) besides running several robustness checks (e.g., accounting for regional differences), I used the more granular variation at the canton level. This includes department FE and ensures that all department-level unobserved factors are controlled for;

b) I perform a careful internal validation of my religiosity measure.1

About 10 days later (June 2nd, 2021), I received a second email. Kelly writes: “I have extensively revised that draft I sent you” (click here to download Kelly’s second draft, K2). Kelly’s main changes/additions, based on my replies, are:

a) a completely new section (K2-Section 4) dealing with the canton-level analysis and suggesting that my results are driven by the most populous cantons;

b) further emphasis on the regional differences (compare, K1-Table 6 and K2-Table 7), claiming that my panel results do not hold when excluding half of the country, i.e., 40 out of 80 departments located in the North-East, East, and South-East of France.

These additional critiques are not solid:

c) the sign and significance of the coefficient of interest hold when using Kelly’s sample, and the slight decrease in magnitude (which he is concerned about) does not occur when using more systematic methods to control for population (see point 1 in the Appendix below for more details).

d) interacting the share of Catholic schools/students with Kelly’s “North and East” dummy (which, however, also includes several Southern departments) indicates that

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1 In addition, the historical and economics literatures provide additional external evidence in favor of my choice (e.g., Tackett, 1986; Murphy, 2015; Franck and Johnson, 2016; Blanc, 2021).
differences across the two parts of France are quantitatively small and statistically insignificant. This suggests that Kelly’s findings are not driven by systematic regional differences – as he alleges – but rather by cutting my sample in an arbitrary fashion.

More broadly, besides these selected sample cuts, Kelly’s critiques (in both K1 and K2) are the result of “cherry-picking” variables/specifications and of the addition of ad hoc department dummies (I provide more details in points 2 and 3 of the Appendix).

Therefore, I am confident that, despite its attempts, Kelly’s work still proves that my results hold throughout.

Sincerely,
Mara P. Squicciarini

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Appendix

Let me now give further details on Kelly’s sample cut in my canton-level analysis and provide a couple of examples of his “cherry picking” approach.

1. Sample cut in the canton-level analysis (K2 – Table 3)
Kelly’s main concern is that my canton-level findings are driven by the most populous cantons (point c) raised in the note); his approach of dealing with this potential issue is to exclude the 30 most populous cantons. Despite the sample cut, the coefficient on religiosity stays negative and significant, but Kelly is concerned that the magnitude of the coefficient decreases (from -0.047 to -0.037). First, cutting arbitrarily the sample is not the right way of proceeding. In my article, I control for population in all specifications. In addition, if one wants to further explore the role of population using more systematic methods than a sample cut (e.g., including polynomials of this variable, dummies for those cantons in the 95 percentile of the population distribution, or even dummies for his 30 most populous cantons), all coefficients are extremely stable (about -0.046) and strongly significant. Once again, this proves the robustness of my results.

2. Addition of ad hoc department dummies (K2 – Table 5)
In my paper, religiosity is not significantly associated with the share of Catholic schools in 1851 and 1866, but explains their growth between 1866 and 1901. Kelly (2021) argues that the inclusion of proxies for demand and supply factors for primary education makes the latter relationship insignificant (K2-Table 5). This is wrong, as their addition leaves the coefficient on religiosity still positive and strongly significant. What makes the relation insignificant is the ad hoc inclusion of dummies for selected departments which are not consistent across

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2 In addition, running Kelly’s specification (as explained in K2) gives a standard error of 0.021. In the Table, he reports a standard error of 0.023.
specifications and not always correctly mentioned. Kelly (2021) justifies their introduction in the notes to K2-Table 5 by writing: “The growth regressions for 1866–1901 have dummies for Aveyron and Lozère (heavy donations) and Hautes-Pyrénées and Pyrénées-Orientales (low growth of Catholic schools).” However, these are not the departments with the heaviest donations or the lowest growth of Catholic schools, but just some cherry-picked departments with heavy donations or low growth.

3. Cherry-picking of variables and addition of ad hoc department dummies (K2 – Table A.1)
In my cross-sectional analysis, I used two main outcome variables to study the link between religiosity and development: the share of industrial employment and the number of industrial machineries per capita. Kelly’s (2021) critique is only based on the first outcome (see, K2-Table 1). Running his specifications using machineries per capita as outcome variable, all my results hold. In the attempt to “make them insignificant,” Kelly (2021) further adds dummies for very selected departments, and controls for distance from Paris in days (see K2-Table A.1). First, once again, the department dummies are not consistent across specifications and their inclusion is not justified throughout. In addition, Squicciarini (2020) already included the physical distance from Paris in km (which is also extensively used in the previous literature).

References


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3 Despite not being specified in the notes, Kelly’s regressions also include a dummy for the Loire department.