Letter to the Editor and Referee

Dear Professor Carlton,

We are very grateful for the opportunity to revise our article for the Journal of Law and Economics.

It took us more time to revise our paper than we expected: this is partly due to one of the authors (RF) whose wife had a complicated pregnancy and who could not commit fully to research during the last few months.

We are also grateful to the Referee for the detailed and insightful comments. Below we intersperse our responses to the comments in italics and also describe the changes we made to the manuscript in response to the comments.

The main changes which we made to the draft are as follows:

1. We incorporated more qualitative evidence on the feudal laws which prevented transaction of all properties before the French Revolution. We also added qualitative evidence on the confiscation and redistribution of Church properties during the revolution.

2. We added several robustness checks in support of our main results:

    - We show that the potential revenues generated by the auctions, notably with respect to municipal revenues, do not drive our main results
    - We provide additional spatial regressions, where we follow the approach of Kelly (2019) and compute Moran's I statistics for the residuals of the relevant regressions.
    - We use micro-data on individual properties to measure variation in the average estimated auction price for each district and the average auctioned plot size to analyze whether including additional measures of the quality of confiscated properties as controls detracts from our main explanation.

In addition, we followed the Referee's suggestions regarding some of our minor statements and made stylistic corrections where need be.

As you can see, the revision has been substantial since we tried to address the many points which the Referee raised. More importantly, after the revision process, we feel that we ended up with a much better paper. We hope that you will agree with us.

Yours sincerely,

Theresa Finley, Raphaël Franck and Noel D. Johnson

Answer to Referee 1's Comments

1. **Comment**: *As I understand the evidence on the distribution of farm sizes, the test that the manuscript presents does not do what the authors claim. There is apparently a database of average farm sizes by area. The quantile regression would therefore explain differences in average farm size by area, not in the distribution of farm sizes within an area. The manuscript interprets the regression coefficients as, for example, changing the size of farms at the 90th percentile within a place, whereas what it's really doing is changing the (conditional) 90th percentile of area-level averages. The latter is not quite so informative for the question of interest. It may be that there's a mistake in the table (which reports the same number of observation as the rest of the study) and what we have here is actually micro-data, in which case the manuscript's interpretation would be correct. This needs to be either fixed or clarified.*

   Answer: We thank the Referee for closely reading the article and pointing that our statement "(...) there were significantly more large farms in the right hand tail of the distribution"is not really supported by the results in Table 6. As the Referee correctly inferred, the dependent variable, Average Farm Size in 1862, is a department-level average.

   Nonetheless, we think that this Table is informative as it shows that, as we write in this revised draft "(the) results suggest that districts with more confiscations had on average larger farms.". In particular, we think that this Table supports our interpretation of the consequences of Church land redistribution in France which we view as contributing to an increase in land concentration. In any case, should the Editor think that this Table is really not informative, we can remove it from the article.

2. **Comment**: *Relatedly, I find the qualitative evidence on pp. 36–37 quite important to the argument. I would suggest amplifying this with more of the case studies cited in passing. Can these be all quantified?*

   Answer: We incorporated more qualitative evidence (now on page 33) from Bodinier and Teyssier (2000), including two districts, Limoux and Coiron, to supplement the example for Bernay as well as additional evidence on the timing of the resale of confiscated properties for Tours and Coiron.

3. **Comment**: *Table 9 displays results for sharecropping. I find these results a bit puzzling as an indicator of less concentrated ownership. Sharecropping (as well as renting) is associated*

with small-scale operation, but often with concentrated ownership.

Answer: In line with the referee's remark, we modify our discussion of sharecropping. While we acknowledge the referee's comment that sharecropping was often associated with concentrated ownership, it was also (at least in France) associated with many tenants operating on a small scale. During the long 19th century, contemporaries such as Sismondi (1827) and Guillaumin (1904) denounced sharecropping as a pernicious system of cultivation which impoverished small farmers. For instance, in the Allier and Landes departments, sharecropping in the 19th century still implied that the tenant would work on the landlord's property in addition to providing a share of harvest of the piece of land he rented (Agulhon et al., 2003). While Barzel (1997) takes a more positive view of the practice since share contracts can, for example, provide a balance between incentivizing work and sharing risk, sharecropping remained in 19th century France a feudal legacy and indicated inefficient agricultural practices.

4. **Comment**: *The manuscript spends an inordinate amount of time talking about potatoes, but I do not understand or agree with the logic of the test. The most basic problem is that there's only so much land, so a result for one crop cannot be totally independent of the results for another. But, in any case, I do not think that the manuscript has presented enough evidence to justify the claim that potatoes would not be affected by improvements and property rights. This analysis needs to either be greatly improved or deleted.*

   Answer: We accept this criticism and eliminated the placebo analysis from the paper. We deleted references to the potato where appropriate and removed sections 3.2.5 and 5.4.1 from the previous draft. We also deleted Table 6, the three rows concerning potatoes in the table of descriptive statistics as well as Figures 8, 9, and 10.

5. **Comment**: *There's some analysis in the section called "mechanisms." First, I believe that "mechanism" is such a poorly defined term as to be avoided. In most papers, it refers to lower–quality statistical evidence that is vaguely consistent with a preferred hypothesis. In any case, the evidence presented on the attenuation of the confiscation coefficient is not, by any reckoning, a mechanism, but rather a central result. The evidence on inequality may be compromised, as I discussed above, and should be clarified.*

   Answer: We agree with the referee that the sub-title "Mechanisms" did not reflect the content of the section. We changed the section title to "The effect of revolutionary confiscations in the long-run".

6. **Comment**: *The result for attenuation (Table 10) is confounded with a switch in the unit of analysis (district to department). I would find this result more believable at comparable units of aggregation. Why not aggregate to department for 1841 and 1852 as a check?*

Answer: We appreciate this comment as we struggled to present the results in the Convergence Table in Section 6.2 of the paper effectively. Originally, we actually included the results that we think the Referee wants to see. We eventually excluded these because we thought they were cluttering up the explanation.

Percent Land Confiscated in District and Wheat Yields, 1841-1929

	Dep. Variable: Log Wheat Yields			
	Baseline	Robust Est.	Mean Agg.	Median Agg.
	(1)	(2)	(3)	(4)
Percent Confiscated X 1841	0.0094**	0.0050*	0.026**	0.020**
	(0.0046)	(0.0027)	(0.011)	(0.011)
Percent Confiscated X 1852	0.0078*	0.0048*	0.020**	0.015*
	(0.0043)	(0.0025)	(0.009)	(0.009)
Percent Confiscated X 1875	-0.0002	-0.0015	0.006	-0.002
	(0.0024)	(0.0028)	(0.515)	(0.791)
Percent Confiscated X 1892	0.0021	0.0009	0.007	-0.000
	(0.0020)	(0.0027)	(0.417)	(0.006)
Percent Confiscated X 1912	0.0045*	0.0034	0.016*	0.010
	(0.0024)	(0.0027)	(0.008)	(0.008)
Percent Confiscated X 1929	0.0036*	0.0030	0.010	0.005
	(0.0021)	(0.0027)	(0.329)	(0.009)
Wheat Suitability X Year	Yes	Yes	Yes	Yes
Market Access 1789 X Year	Yes	Yes	Yes	Yes
Region FE's X Year	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Robust Estimator	No	Yes	No	No
Vars Aggregated to Dept	No	No	Yes	Yes
N	1,152	1,152	363	363
adj. R-sq	0.71	0.77	0.69	0.66

Table 1: Percent Land Confiscated in District (cols. 1-2) or Department (cols. 3-4) and Wheat Yields, 1841-1929

In Table 1 we report these excluded regressions. In column 3 we run our convergence regressions using Department level data (that is, we aggregate everything up to the highest

level). We aggregate up the variables on the RHS of the regression using the mean. In column 4 we do the exact same thing, but we aggregate to the Department level using the median.

The regressions in Columns 3 and 4 are highly consistent with those in Columns 1 and 2 that are reported in the paper. Specifically, just as the significant coefficients in 1912 and 1929 become insignificant when controlling for the Arles outlier, the coefficient in 1912 becomes insignificant in Column 4 when we aggregate using the median.

We feel the overall message of the analysis continues to be that there is no robust relationship between confiscations during the revolution and wheat yields after 1852. In other words, the results are consistent with there being convergence in land reallocation between regions over time.

We leave it up to the Editor to decide if we should include a new version of the Convergence Table in the revised paper including all four regression models. We prefer using the existing version of the table as we feel the paper already has enough results for the reader to interpret and the additional regressions don't add much information.

7. **Comment**: *The auctions generated revenue. How much, therefore, might municipal revenue be part of the story?*

   Answer: In line with the referee's remark, we discuss the potential revenues generated by the auctions, notably with respect to municipal revenues, in Section 2.2 (Ecclesiastic Wealth and Revolutionary Confiscations) . From the Revolutionaries' viewpoint, the purpose of selling Church land was to raise revenue in order to solve the financial crisis of the French state. When they realized that the auctions may take several years to complete, they issued bonds known as Assignats that could be redeemed for confiscated property at a later date. However, they began to print more Assignats than there was property to back them and they became, in effect, a fiat currency. There is therefore no evidence that the central government recovered much for the sales of Church land, or that local governments where more Church land was sold up became richer in the long-run. In regressions shown in Appendix Table 11 (and reported in this letter below), we indeed find that towns with more church land did not have more revenues in 1880 and 1900.

8. **Comment**: *The controls for "market access" seems appropriate. The question of whether market access responds endogenously to consolidation (which it mostly does not) seems*

Percent Land Confiscated in District and District Revnues in 1880, 1900

	Dependent Variable: District Revenues					
	1880			1900		
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Confiscated	53403.7	241266.8	226198.0	53346.2	379662.6	350462.3
	(241820.4)	(256056.5)	(254284.8)	(460242.2)	(489421.4)	(492317.6)
Wheat Suitability	No	Yes	Yes	No	Yes	Yes
Market Access 1789	No	No	Yes	No	No	Yes
Region FE's	Yes	Yes	Yes	Yes	Yes	Yes
N	194	194	194	194	194	194
adj. R-sq	0.108	0.258	0.380	0.205	0.311	0.419

Table 2: Percent Land Confiscated and District Revenues in 1880, 1900.

entirely tangential. I don't understand why this is in the paper at all, even as just an appendix.

Answer: We accept this criticism and removed the portions of the analysis in which market access is treated as a dependent variable. This involved removing references to the 1841 market access measure in section 3.2.4 as well eliminating section 5.3 of the original submission.

9. **Comment**: *I applaud the use of clustering at a higher level as a check for the inference. It's not clear to me whether the department is sufficient for this, however, so I would suggest the use of Conley's estimator, with various ranges, as well. Particularly this is an issue for the market-access control, which pulls in data from essentially everywhere, and the pipe variable, which could cross district boundaries.*

   Answer: This is a great point. To address the comment, we follow Kelly (2019) and calculate Moran's I statistics for the residuals of the relevant regressions. For this purpose, we assume distance cut-offs for the spatial weighting matrix of 100 km's, 250 km's and no cut-off (infinite km's). We report the Z-statistic associated with the calculated Moran's I where the null hypothesis is "no spatial auto-correlation". These statistics are in brackets in Table 3 below for each of the main regressions in the paper. The structure of the Table is based on Table 5 (Robustness) in the revised draft. For example, the Z-statistic for Moran's

	(1)	(2)	(3)
Dependent Variable	*100 kms*	*250 kms*	*inf kms*
Wheat Yields	0.0094**	0.0094***	0.0094***
	(0.0042)	(0.0032)	(0.0033)
	[5.82]	[4.30]	[4.38]
Pipe Mnfg.	0.1642**	0.1642***	0.1642**
	(0.0737)	(0.0627)	(0.0647)
	[6.49]	[3.92]	[5.21]
Fallow	-0.0038***	-0.0038***	-0.0038***
	(0.0013)	(0.0012)	(0.0013)
	[7.23]	[6.18]	[5.77]
Prairies	0.0016**	0.0016***	0.0016**
	(0.0006)	(0.0058)	(0.0007)
	[7.41]	[6.42]	[7.00]
Region FE's	Yes	Yes	Yes
Include All Covariates	Yes	Yes	Yes
N	194	194	194

Table 3: Conley Standard Errors and Moran's I Statistics at Different Spatial Ranges

I for regression (6) in Table 2 of the main paper, where the outcome is Log Wheat Yields, is 5.82 with a cut-off range of 100 km. Likewise, Moran's I with the same cut-off range for our baseline regression looking at the effect of confiscations on Pipe Manufacturers (specification (6) of Table 4 in the revised draft) is 6.49. Both of these Z-statistics reject the null hypothesis of zero spatial auto-correlation, as do the Z-statistics for all the other regressions reported on in Table 3 below. To put these results in perspective, Kelly (2019) calculates Moran's I's for 23 papers using historical spatial data published in top-5 journals in the last ten years and finds Z-statistics ranging from 0.1 to 32.8 (median 6.5) (Kelly, 2019, Figure 6). Overall, the implications of the Moran's I computed in Table 3 are two-fold. First, our results should be shown to be robust to spatial auto-correlation. Second, the appropriate cut-off range for the spatial weighting matrix should be around 100 km since auto-correlation decreases with greater distances according to the Moran's I's.

In addition, in Table 3, we report in parentheses the standard errors for our main regressions

adjusted according to the method described by Conley (1999). We do this using the code provided by Hsiang (2010). As expected several of the coefficients are less precisely estimated after this adjustment, but in no case do the coefficients drop below the 5% significance level.

In this revised draft, we made several adjustments to the paper in light of this specific remark of the referee. In column (2) of Table 5 (Robustness) we report the Conley standard errors from Column (1) of Table 3. We dropped the robustness regressions which included the potato suitability control in order to make room for these regressions. We retain the regressions in column (3) which cluster at the Department level.

10. **Comment**: *Distance to a bishop must be confounded with distance to populated areas. I suppose that the market-access control would clean this up, but why not include controls for distances to nearest city of various population categories, for example? The meaningful comparison is to a place of comparable distance to a similarly sized cities, albeit with no bishop.*

Answer: This is a good question. The Referee is correct, however, that this is precisely the sort of endogeneity problem that led us to create market access measures and include them in virtually every regression in the paper. Based on his/her comments, we suspect the Referee is already familiar with the construction and interpretation of market access measures. Nonetheless, as a reminder, it is constructed as:

$$MA_i = \sum_{j}^{d} N_j \tau_{ij}^{-\sigma} \tag{1}$$

where, MA_i is market access for district i, the total number of districts (and non-French cities) is d, N_j is the population of district or foreign city j, and τ_{ij} is the cost of traveling between districts and cities i and j using the shortest path. The term σ in equation 1 is a trade elasticity which measures the responsiveness of trade to transport costs between locations.

Market access is thus defined as a weighted sum of the populations surrounding a region, where the weights are inversely related to distance. This is precisely the control that should be chosen to prevent distance to populated areas confounding the instrument (or anything else on the RHS of a regression equation). Furthermore, market access is not an arbitrary choice as a control variable. It can be derived as a sufficient statistic for trade potential

and (with adjustments) welfare under a gravity model of firm level trade in the style of Eaton and Kortum (2002). In economic history, the most well known use of this measure to our knowledge is Donaldson and Hornbeck (2016), but there are several other prevalent studies which use market access, including Dell (2015). As such, we think our market access control is the theoretically appropriate control for the sort of potential endogeneity mentioned by the Referee.

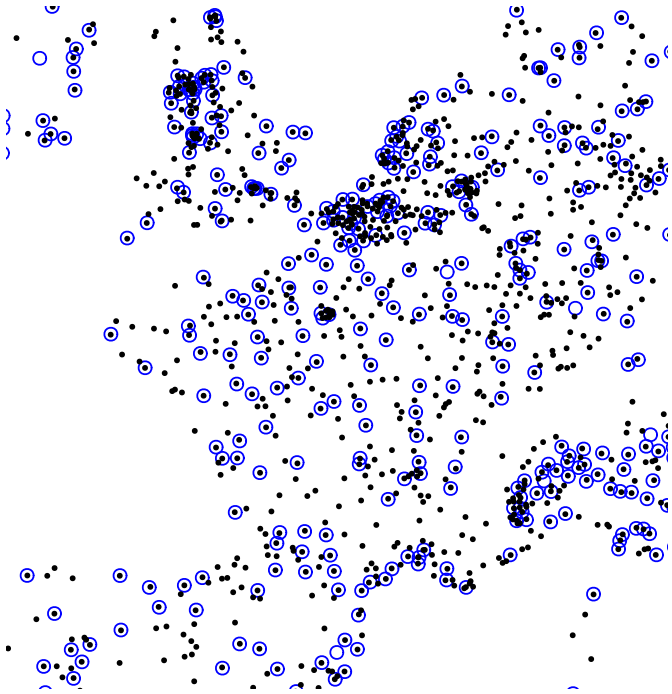

Figure 1: Bairoch Cities and Bishoprics, centered on France. Bairoch cities are solid black dots. Bishoprics are blue circles.

In order to directly address the referee's comment, however, we undertook to create both the measures he/she requested as controls as well as creating new versions of the IV that are less correlated with proximity to large cities. We construct the IVs under the assumption that a primary concern of the Referee is that bishoprics were disproportionally located in larger cities and, as such, the instrument is simply picking this up.

First, we placed all the Bairoch cities into population quartiles based on their populations in 1800. Second, we calculated the distance to the nearest city in each quartile for every district. Third, we overlaid a shapefile of all the bishoprics on the Bairoch cities shapefile as shown in Figure 1. In order to find the population of each bishopric, we successfully linked each of them to the nearest Bairoch city using a tolerance of 3 km. We then created two variables to be used as new IVs that will be less correlated with proximity to larger cities.

The first is distance to bishopric, excluding the top quartile bishoprics by population. The second is distance to bishopric excluding all but the first quartile bishoprics by population (the least populous bishoprics).

Dependent Variable	(1) *Baseline OLS (w/Region FE's)*	(2) *IV=Dist. Nearest Bishopric*	(3) *IV=Dist. Nearest Bishopric*	(4) *IV=Dist. Nearest Bishopric (1st-3rd Quartiles)*	(5) *IV=Dist. Nearest Bishopric (1st Quartile)*
Wheat Yields	0.00941***	0.0513***	-0.00799	0.0331*	0.0430***
	(0.00341)	(0.0153)	(0.0658)	(0.0193)	(0.00709)
		[10.6]	[0.3]	[5.0]	[59.6]
Pipe Mnfg.	0.164**	0.1642**	1.003	0.525	0.532***
	(0.0674)	(0.0737)	(1.222)	(0.337)	(0.146)
		[11.1]	[0.5]	[5.8]	[63.9]
Fallow	-0.00384***	-0.0106*	0.0670	0.0125	-0.00967***
	(0.00134)	(0.00570)	(0.0780)	(0.0124)	(0.00266)
		[10.6]	[0.3]	[5.0]	[59.6]
Prairies	0.0016**	0.00616**	-0.0148	0.00670*	0.00596***
	(0.00072)	(0.00295)	(0.0248)	(0.00399)	(0.00145)
		[10.6]	[0.3]	[5.0]	[59.6]
Baseline Controls	Yes	Yes	Yes	Yes	Yes
City Pop. Bin Controls	No	No	Yes	Yes	Yes
N	194	194	194	194	194

Table 4: IV Regressions Controlling for City Population Bins and Using Low Population Bishoprics as IV's

Table 4 shows the results of our analysis using these new variables. The structure of the Table roughly follows that of Table 5 (Robustness) in the main paper, with each cell representing a separate regression, each row representing a different dependent variable, and each column representing a different estimation strategy. There are thus 20 regressions in Table 4.

In the first column we replicate the baseline results from the paper using OLS (along with controls and region fixed effects) as a reference point. In column 2 we replicate the IV results from the original version of the paper (Table 7, Column 1) using distance to nearest bishopric as the IV (controls included, but no fixed effects). In Column 3 we run the same regression as in Column 2, but we add the four distance to city population bin controls. As suspected by the referee this does cause problems for our IV strategy. The first stage F-stats plummet across regressions and the point estimates become statistically equivalent

to zero. In Table 5 below, we illustrate one possible reason the district to bishopric variable loses explanatory power when the population bins are included as controls. The correlation between distance to a city in the top population bin (City Pop 4) and the distance to nearest bishopric IV (Bishop Dist) is 0.70.

	Bishop_Dist	Bishop_1_2_3	Bishop_1	City_Pop_4
Bishop_Dist	1			
Bishop_1_2_3	0.6379	1		
Bishop_1	0.1664	0.2384	1	
City_Pop_4	0.7031	0.3752	0.2588	1

Table 5: Correlations Between Different IV's and Distance to Large Cities

To address this potential source of endogeneity, we construct two additional IVs in which we exclude bishoprics from larger cities. In Column (4) we use as our IV the distance to the nearest bishopric, excluding bishoprics in the highest population quartile. As can be seen by the Correlation Table, the correlation of this IV (Bishop 1 2 3) is only half of the correlation of the original IV. In the Regression Table, the results improve markedly using this IV, with first stage F-stats hovering around 5.0 and coefficient estimates on land confiscated becoming statistically and economically significant when using Wheat Yields or Prairies as the dependent variables. In Column 5 we use as our IV the distance to nearest bishopric, excluding all but the bishoprics in the lowest quartile of the population. This should be the most valid instrument as it is, by construction, not correlated with proximity to larger cities. This is supported by Table 5 which shows a correlation coefficient between City Pop 4 and the new instrument of 0.26. In the Regression Table, new IV appears to perform quite well. The first stage F-stat is about 60 and all coefficients are both economically and statistically significant.

Depending on the preferences of the Editor and Referee, we are willing to go different ways with revising the paper based on this analysis. For now, we have not changed the IV analysis. This is because we find the inclusion of the city bin population controls for the IVs to be somewhat inconsistent with what we do in the other regressions. Combined with the fact that the IV strategy is robust to their inclusion using the "small bishoprics" instrument, we lean towards not changing the IV analysis in the paper. However, if the Editor and Referee prefer us to update the analysis based on what has just been explained, we will be happy to include Table 4 from above in the paper (replacing the IV's in the current robustness table, Table 7, Column 1). We will also be happy to update the appendix with

a figure showing the non-parametric relationship between the "small bishoprics" IV and land confiscations variable (analogous to the current Figure 13).

11. **Comment**: *In Table 2, panel B, there is a regression of log wheat hectares on percentage of land confiscated from the Church. Is there a reason why these units do not match? Why not percentage of land on both sides?*

    Answer: We thank the referee for the suggestion to consider the percentage of land devoted to wheat production as a potential alternative outcome variable. We returned to the 1842 agricultural survey and collected the data on the size of each district. After dividing the variable for wheat hectares by the total size, we duplicated the results from Panel A of Table 2 in the paper and estimated the effect of the percent confiscated on the percent of wheat production. The results of these regressions are presented in Table 6.

Percent Land Confiscated in District and Wheat Production

	Dependent Variable: Percent Wheat Production					
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Confiscated	0.399***	0.262***	0.183***	0.207**	0.160*	0.157*
	(0.0677)	(0.0644)	(0.0638)	(0.0973)	(0.0878)	(0.0861)
Potato Suitability	No	Yes	Yes	No	Yes	Yes
Market Access 1789	No	No	Yes	No	No	Yes
Region FE's	No	No	No	Yes	Yes	Yes
N	194	194	194	194	194	194
adj. R-sq	0.124	0.306	0.375	0.289	0.398	0.430

Table 6: Fraction of Land Used For Wheat Production

12. **Comment**: *Please define better the outcomes for Table 2. Is fallow land normalized by all land or by all land in farms? What is "artificial prairie?"*

    Answer: The outcome variables used in Table 2 are fallow land and artificial prairie. They both come from the 1852 Enquête Agricole. Fallow Land is variable 273 in the Detailed Description file which is titled "jachères", which translates as fallow land. The description is given as "étendue totale" which we interpret as "total extent". So it the total amount in the district, not the average amount across farms. Artificial Prairies is variable 260 "Prairies

Artificielles" which is described as "étendue totale (luzerne [alfalfa], sainfoin, trèfle [clover], mélanges divers)". So we interpret this variable as the total area in a district planted with nitrogen fixing plants. Both variables are given in hectares.

We added this information in footnote 14 in Section 3.2.1 where the variables are described.

13. **Comment**: *For whatever land concentration there was after confiscation, probably it was higher when the Church was a large landowner. Please provide some discussion of what prevented the Church itself from serving a coordinator for change in agricultural practices.*

    Answer: Following the Referee's remark, we emphasize in Section 2.2 (Ecclesiastic Wealth and Revolutionary Confiscations) that feudal ownership rules did not only prevent individual farmers and landlords from selling plots or land, but also constrained the Church from serving a coordinator for change in agricultural practices. In particular, we discuss the practice of mortmain whereby Church property could not be alienated before a lengthy legal procedure requiring the King's eventual agreement.

14. **Comment**: *I do not understand the test in Table 7, columns 9-10. Aren't these new variables just alternative measures of the importance of the land being confiscated?*

    Answer: As the Referee observed, the variables from Columns 9-10 of (now) Table 5 are alternative measures of the importance of the land being confiscated. Specifically, we use micro-data on individual properties to measure variation in the average estimated auction price for each district and the average auctioned plot size. We feel this robustness check allows us to analyze whether including additional measures of the quality of confiscated properties as controls detracts from our explanation. Although the coefficient for wheat yields shrinks by about one-third, it retains its statistical significance. We believe that this addresses the concern that districts with more confiscations merely had better quality confiscated properties auctioned off which led to higher wheat yields.

15. **Comment**: *There should be somewhere a consistent reporting of the units of measurement. Ideally, the tables notes (which are missing) would have this info as well. I see from page 14 that wheat is measured in hectoliters (with yields in hl per hectare). Are potatoes measured similarly?*

    Answer: We proofread the paper in an attempt to be more consistent in the reporting of units of measurement, including Tables 2, 5 and 8. In Table 2, for example, the table note reads "This table presents OLS regressions relating the percentage of confiscated Church

land during the Revolution in each district to the log of the wheat yield (hectoliters per hectare) in 1841 in Panel A, and the log of wheat hectares in 1841 in Panel B.".

16. **Comment**: *Does Galan (2018) study Peru or Colombia? Footnote 2 says one thing and the citation says another.*

    Answer: Galan (2018) studies Colombia. We corrected the reference in footnote 2 accordingly.

17. **Comment**: *Be mindful of the that/which distinction. See the first sentence of Section 2.1, for example. On page 7, third line from the bottom, sentence starting "First" is not a complete sentence, but rather a list. Was there supposed to be a colon at the end of the previous sentence? On page 8, is the name "McPhee" or "Mcphee"? (Please take the manuscript to a proofreader.). On the top of page 26, should it be "percent" or "percentage point"? Tables 2 and 3 could be compressed into one. There are currently a lot of needlessly repeated rows.*

    Answer: We proofread the manuscript and paid attention to the distinction between "that" and "which". We corrected typos in the names of authors where need be. At the end of section 5.1.1 (at the top of page 26 in the previous draft), the correct term is "percent".

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