The Historical Origins of Territorial Disputes: Online Supplementary Appendix

The appendix to "The Historical Origins of Territorial Disputes" includes: a number of results noted in the paper, robustness checks noted in the paper, and additional results not mentioned in the main manuscript. Below is a table of contents for this document that summarizes the additional results included. The numbering of the table of contents is identical to the numbering of the corresponding sections below.

1. We report results that use the post-1815 dyad as the unit of analysis. We find results very similar to those reported using the grid-square.

2. We calculate four alternative measures of ethnicity at the dyadic level: The sum of shared ethnic groups; the presence of a shared ethnic group that takes power at some point in both countries; the presence of a shared ethnic group that takes power in one country and not in the other; and the presence of a shared ethnic group with a plurality of the population in one state but not the other. We find that ethnicity neither affects the relationship between *Historical Border Variability*$_i$ and claims nor has significant positive effect on claims, regardless of how we measure it.

3. We provide a figure that graphs trends in the timing of precedent-based territorial claims and territorial claims without precedent. The figure demonstrates that historical precedents play an important role in the emergence of claims until at least the end of the Cold War.

4. We provide the results of a robustness check where we remove all territorial claims that significantly overlap with claims that were made earlier in the post-1815 time period.

5. We report the results of tests for interactive effects between *Historical Border Variability*$_i$ and our measures of territorial value on claim emergence.

6. We test for the possibility that more claims emerge more quickly in areas dense with historical precedents because these areas are likely to see militarized conflict. Using causal mediation

analysis, we show that the effect of *Historical Border Variability*$_i$ is *not* mediated by the propensity for militarized interstate disputes.

7. The results are robust if we exclude the borders of contemporary states, i.e., states that exist post-1815, from our *Historical Border Variability*$_i$ measure. This provides an additional test of the persistent coordination effects explanation.

8. We assess whether "empty" grid-squares in the interior of large states, e.g. Russia, affect our findings. We do two things which show that these grid-squares do not affect our findings: 1.) we reestimate our models removing these units from the sample, and 2.) we estimate models including the number of post-1816 states in a grid-square as a control variable.

9. The results are qualitatively similar if we allow the effect of border variability and density to vary across Western and Eastern Europe.

10. We provide additional evidence that the effect of more recent historical borders is larger than that associated with more temporally distant boundaries. These results complement those in Table 4 of the main manuscript.

11. We conduct a placebo test to see whether pre-Westphalian borders affect post-1815 claims, finding no statistically significant relationship as we expected.

12. Treating ethnicity either as a pre-treatment confounder or as a post-treatment mediating variable does not substantively alter our results.

13. We investigate whether historical border precedents are associated with claim duration by exploring the total number of months which a given unit had an unresolved claim. We find that historical border variability is also strongly associated with this dependent variable.

14. Following Oster (2013) we conduct sensitivity analysis and show that the estimated effect of *Historical Border Variability*$_i$ is robust to substantively large violations of our assumption of exogeneity conditional on observable covariates.

15. We provide results for the timing of claims that distinguish among the timing of the first claim that follows historical precedent and the timing of the first claim that does not follow

precedent. These results are analogous to the results for the number of claims presented in Table 3 in the main text.

16. We reproduce the results from Table 3 in the main text via seemingly unrelated regression, allowing the error terms to be correlated across our models of claims based on historical precedent and claims without historical precedent. Our results are essentially identical to those reported in the main text.

17. We report the full table of results from the regression of the number of claims where we estimate the effect of $\textit{Historical Border Variability}_i$ with non-parametric regression splines, i.e., Figure 5 in the main text.

18. We outline the process of using smoothers to approximate latitude/longitude fixed effects and report the results for the number of claims.

19. We reproduce the results from Table 4 in the main text using negative binomial regression instead of OLS. Recall that Table 4 shows that leaders make more claims over precedents that are more recent and were in place for longer periods as international boundaries before being changed. The results are substantively the same.

20. We report results analogous to those in Table 4 in the main text analyzing the timing of claims, whereas Table 4 only examines the number of claims. We report both OLS and Cox models, which as noted in the text, produce results that mirror analysis of the number of territorial claims, i.e., Table 4.

21. We produce results with three alternative measures of the degree of variation in historical borders, all of which substantively match our main results.

22. We show a map that depicts the spatial distribution our timing of territorial claims variable.

23. We report a survival curve based on a Cox model from table 2 in the main text.

1 Results with the Dyad as Unit of Analysis

In this section, we replicate our main result using the post-1815 country-dyad as unit of analysis. To ensure that we identify historical precedents that are tied to claims over the current border, we follow the approach in Huth (1996, 256–263), constructing a 50 kilometer buffer area surrounding the borders of each pair of post-1815 (contiguous) states.[1] Then, we measure *Historical Border Variability* and *Historical Border Density* exclusively within these 50 kilometer buffer zones. This ensures that any connection between claims and precedents is not the spurious result of historical lines far from contemporary borders.[2] The units range from significantly smaller than our grids to much larger than our grids.

Table 1 contains the results of models that are analogous to those in table 1 of the manuscript, with the post-1815 dyad as the unit of analysis. The results regress the number of claims taking place between a dyadic pair after 1815 on our measures of historical border density and variability in these dyadic buffer units.[3] Since each unit is of a different size we provide results that weight each observation by the inverse of the total area within each border buffer area. The results in each specification do not differ substantively from those we present in the main text.

[Table 1 about here.]

Given that our concern with the use of the post-1815 dyad as the unit of analysis was that these units are endogenous to political and military processes that are measured by some of our regressors, we do sensitivity analysis to understand whether our concerns are warranted. Specifically, we implement the test developed by Oster (2013), which provides an assessment of how robust the relationship between one's key independent variable and a dependent variable is to selection on factors unobserved and thus not included in the model specification. In other words, this is a test that provides insight into how robust a result is to concerns about endogeneity, in our case being the idea that unmeasured factors bias the relationship between claims and historical boundary variability. Implementation of this test provides good comparability to the results that rely on the

[1] Note that while we chose a 50 km buffer area, Huth's is 50 miles, which actually corresponds to about 80 kilometers. Thus, ours is slightly more conservative than his.

[2] We see this as an additional test to complement our disaggregation of precedent-based claims and claims without precedent in the manuscript, i.e., table 2.

[3] The models of claim timing and all of the other key results are similarly robust to the dyadic approach.

grid-square unit, as we implement this test on our main results in the manuscript, and find that the relationship between *Historical Boundary Variability* and is very robust, see section 14 of this appendix.

In contrast to the results using the grid-square unit, we find that the dyad unit results are not robust to selection on unobservables at any specification of the test. Oster (2013) notes that if a value of δ is greater than one, a result is considered robust. The parameter δ represents the degree of selection on unobservables that would have to hold to overturn a main result. Thus, we have to believe that selection on unobservables is δ times as great as the influence of all regressors included in the specification, which becomes implausible at higher levels of δ unless the specification in question does not explain much of any variance in the dependent variable (which is obviously not the case here). Unfortunately, the value of δ is always less than one when we implement the test using the dyadic unit of analysis. In fact, even if we only assume that inclusion of all relevant observables would increase R^2 from 0.1882 to 0.20, δ is about 0.24, which is far below 1. In contrast, with the grid-square unit the δ from a similar specification of the test is greater than 40.

2 Dyadic Ethnicity Tests

Using the same dyadic unit of analysis defined above we are able to characterize several features of (country-pair) ethnic relations using the geoEPR data (Wucherpfennig et al., 2011). We show in Table 2 that controlling for these do not substantively alter our results. In the first two columns we simply take as our measure of ethnic power the number of shared ethnic groups by both countries in a dyadic pair. Then in columns 3-4 we construct a dummy variable taking on a value of one if there is a common ethnic group in both counties in a dyad that is in power in both states. In columns 5-6 we construct a similar indicator which takes on a value of one if a shared ethnic group is in power in one state within a dyad and is not in power in the other. Last, in columns 7-9 we construct a dummy variable which takes on a value of one if there is a shared ethnic group that in one state within a dyad has a plurality by size and in the other does not. Across all specifications our main results hold. Moreover, none of these ethnicity measures are statistically significant and in the expected positive direction.

[Table 2 about here.]

3 Timing of Precedent Claims vs. No Precedent Claims

In this section we provide more information over the temporal distribution of claims that follow precedent and claims without precedent. One specific worry that we respond to here is the idea that precedent-based territorial claims might be very important in the early to mid-19th century, but relatively unimportant in the 20th century. Figure 1 provides a graph that summarizes the frequency with which these two types of claims are made after 1815. The graph demonstrates that precedent-based claims are important, and tend to be made more often than claims without precedent, until at least 1975 or so. Moreover, the vast majority of claims in Europe in our data are made prior to 1950. This indicates that precedents from the pre-1790 era remain important across time periods in which we observe most of the claims in the data.

[Figure 1 about here.]

4 Robustness Test Removing all Overlapping Claims

Next to be assured that our results are not biased by the continued effects of post-1815 claims that overlap with prior claims, we eliminate from our analysis claims which are related to each other by being centered around common territory. For example, multiple claims surrounding the Czech town of Teschen are thus consolidated into a single claim. We replicate our main results with these restricted data in Table 4. None of our results change substantively. However, to be still further assured that repeated claims are not driving our findings we completely discretize our outcome and construct a binary indicator for whether a claim took place on each of our units or not, completely subsuming all subsequent claims following the first. We reestimate our main results with this binary outcome in Table 5. Again, none of our substantive findings are altered, though because we have discretized our outcome (and in the process lost information over multiple but distinct claims in an area) the magnitude of the relationship between historical border variability and the presence of a claim is attenuated.

[Table 4 about here.]

[Table 5 about here.]

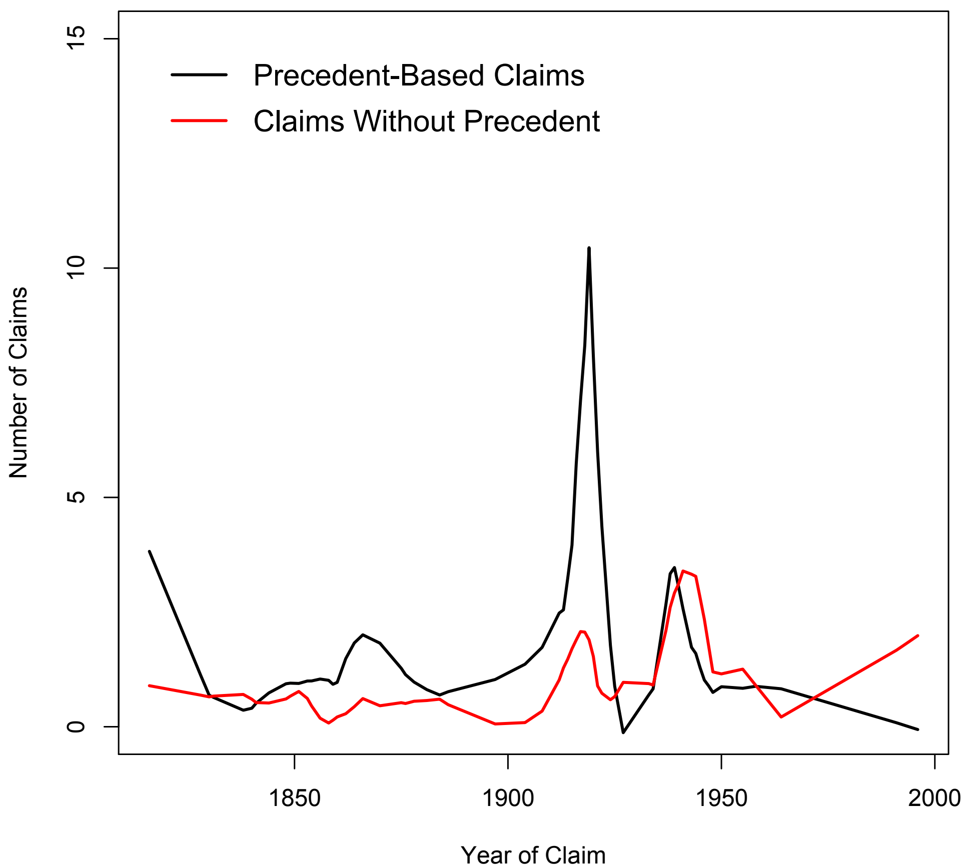


Figure 1. The Timing of Precedent-Based Claims and Claims without Precedent after 1815

5 Interaction of Historical Border Variability and Territorial Value

In this section we provide results that test for interactive effects between *Historical Border Variability*$_i$ and our measures of territorial value. As we note in the main text, it is possible that historical precedents remain important to leaders in that they provide greater opportunity to make claims, but that incentive derives from the fact that some precedents also bound territory of great value. To see if there an interactive effect of border variability that changes with the value of territory, we interact our measure of historical variability with the following pre-treatment measures of strategic and economic worth: agricultural suitability, urban population, river length, terrain ruggedness, and iron production. The results of these specifications are presented in Table 6.

In the first six columns we treat the number of claims as the outcome and find no evidence in support of an interactive relationship between variability, territorial value, and the number of claims on a given grid-square. Specifically, while our estimates of *Historical Border Variability*$_i$ remain very similar to those reported in the main text, the interactive effects are always small and statistically insignificant. In columns 7–12, we treat the number of months until the first claim as the outcome. When we include these interactions separately we find some support for the idea that the density of rivers (column 7) and urban population centers (column 8) in interaction with historical variability hasten the onset of territorial claims. In contrast, the interaction of agricultural suitability and historical border variability (column 11) increases the time until the first claim. However, when we include all of the interactions in the same model (column 12) we find that only the interaction of river density and border variability remains statistically significant.

[Table 6 about here.]

6 Implications of the Perpetual Conflict Explanation: Do Militarized Disputes Matter?

As we note in the manuscript, a plausible explanation for why leaders have incentive to dispute along precedents derives from the idea that perpetual conflict over the most valuable territories explains both the presence of competing historical precedents and subsequent claims. If this explanation is true, we would expect our estimated effect for *Historical Border Variability*$_i$ to be a proxy for something else, such as the presence of valuable resources or militarily strategic territory. While we find no evidence that this is the case in either any of the results reported in the main manuscript or in this appendix, we test an additional observable implication of this idea here. Namely, we assess the idea that locations with greater historical border variability have a greater underlying propensity for military conflict and that this propensity for militarized conflict drives the effect of historical border precedents on subsequent claims. The test presented here allows for the possibility that our measures of territorial value are not the proper ones by directly tapping into the idea that the effect of *Historical Border Variability*$_i$ on claims might be mediated by militarized conflict propensity.

To test this alternative we adopt the method of causal mediation analysis for identifying causal mechanisms proposed by Imai, Keele and Tingley (2010) and Imai et al. (2011). This approach allows us to statistically identify the effect of historical borders on the number and timing of territorial claims as it operates through an intervening post-treatment variable, in this instance post-1815 militarized conflict. We operationalize the observed propensity for military conflict as the the count of militarized interstate disputes taking place within each of our grid-square units after 1815.[4]

The conjectured statistical relationship is captured diagrammatically in Figure 2. If the effect of historical boundary variability operates through conflict, then we should expect a large mediating effect of the MIDs measure and a diminution of the direct effect of historical borders to insignificance. If the perpetual conflict explanation is wrong then we expect the direct effect to remain robust and the mediating effect of conflict to be small or negligible. In other words, if the persistent coordination effects explanation is correct historical boundary variability should not affect our outcomes through its influence on militarized conflict.[5]

[Table 7 about here.]

Table 7 presents results where we estimate the linear direct effect of historical boundaries and mediating effect of conflict (measured by the number of militarized interstate disputes) on our two

[4]We rely upon the geo-referencing of MIDs by Braithwaite (2010).

[5]We implement the procedure as detailed in Imai, Keele and Tingley (2010). Formally, the mediation effect is defined as $\zeta = \mathbb{E}\left[Y(t, M(m+1)) - Y(t, M(m))\right]$, the direct effect, $\delta = \mathbb{E}\left[Y(t+1, M(m)) - Y(t, M(m))\right]$ and the total effect $\tau = \mathbb{E}\left[Y(t+1, M(m+1)) - Y(t, M(m))\right] = \zeta + \delta$. If our "treatment" historical boundary density does not affect our outcomes through the mediating conflict measure, then ζ will be estimated to be zero and $\tau = \delta$.

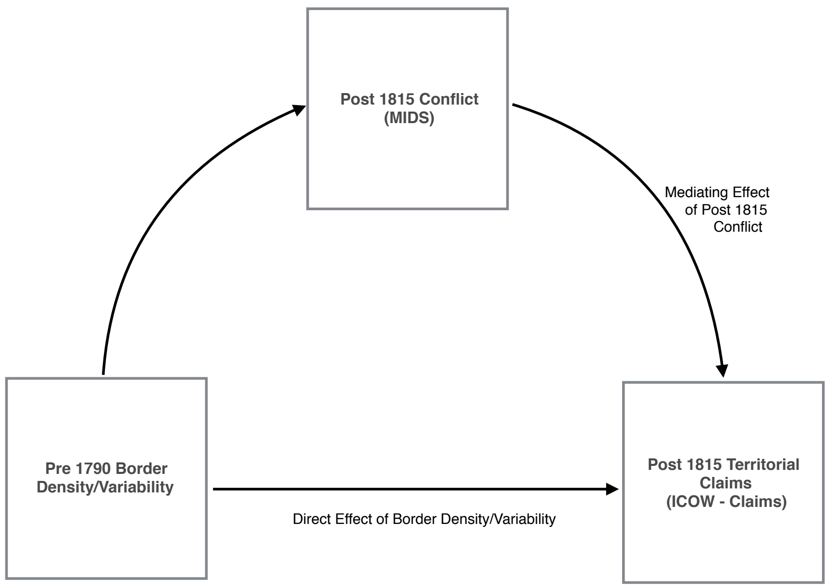


Figure 2. This diagram captures the possible mediating relationship we estimate. We estimate the direct effect of historical boundary density/variable on territorial claims as well as the indirect effect as it operates through nineteenth and twentieth century militarized disputes. We find little evidence of a mediating effect of militarized conflict on territorial claims.

outcomes of interest: the number of claims on a given unit and the number of months until the first claim was made on a given unit. We can also use non-linear (e.g. count and survival models) to estimate these effects when the outcome variable is either the count or timing of claims and our results remain qualitatively the same. In each model we control for the full set of pre-treatment confounders described in the previous section.[6] Again, the inclusion or exclusion of these covariates does not affect the estimates of our variables of interest.

Across all specifications the mediating effect of militarized interstate disputes is statistically indistinguishable from zero. We find scant evidence that our measures of historical boundary density or variability affect our measures of territorial claims through an increased likelihood of militarized conflict. Moreover, our estimates of the direct effect of our historical boundary measures are nearly identical to the estimates reported in table 1 in the main text.

7 Measuring Historical Precedents with Only Dead States

In this section, we analyze whether our findings survive an exclusive focus on precedents from dead states. In other words, does it matter whether the historical precedents in a given grid-square are the former boundaries of the potential challengers or if these precedents bounded states that do not exist post-1815? This analysis helps us probe two issues. First, a focus on precedents from dead states is a good test of the importance of persistent coordination effects, as whether the precedent is from a contemporary or dead state does not matter for the idea that the persistent coordination effects of historical precedents provide incentive. Rather, whether or not historical precedents are well-established and recent is of central importance to this explanation. Second, these results also speak to the idea that the connection between precedents and claims might reflect states continuing to dispute the same territory over time. Thus, we can address the idea that our results might be driven by the same states, e.g., France, disputing and changing their borders pre-1789 and post-1815. This critique is related to the militarized conflict critique discussed above in that it essentially posits that history matters because it "repeats itself." To address these issues, we recreate our historical density and variability measures only using historical borders that bound states that *do not exist* after 1816, i.e., dead states. Thus, the historical precedents relevant to the

[6] Recall that in our main specifications we address with conflict propensity by controlling for factors that are theorized in the extant literature to be associated with conflict.

borders of perpetual challengers such as France are not included in our historical border density or variability measures.

The results in table 8 provide more evidence in favor of the idea that persistent coordination effects are important to our main results. The models in the first six columns of the table assess the influence of $Historical\ Border\ Variability_i$ and $Historical\ Border\ Density_i$ on the number of claims when we only measure historical precedents from states that are dead after 1815. The results for $Historical\ Border\ Variability_i$ across these six models are strikingly similar to the main results that leverage information on all border precedents. Most importantly, note that the models that include both variability and mean density, i.e., models 5 and 6, show that historical border variability retains its positive and significant effect while historical border density becomes negative.

We learn two main things from these findings. First, the finding that our main results are robust to a focus on dead state precedents bolsters the idea that persistent coordination effects are central to the connection between historical precedents and territorial claims. Second, the results in table 8 provide further weight against the idea that our findings might be the result of states disputing and trading back and forth the same territories historically and currently. We also constructed an alternative version of our $Border\ Changes_i$ variable that we use in the main text to measure the number of historical precedents. This alternative version only counts an historical precedent if the border change that produces it led to state failure. The results using this variable are similar to those reported in table 8.

[Table 8 about here.]

8 Do "Empty" Grid-Squares Matter?

As figure 1 in the paper (or figures 3 or 6) shows, there are a number of grid-squares that are contained in the interior of one state over 1650–1790 and thus do not exhibit any variability in historical borders. While we include $Historical\ Border\ Density_i$ in all of our main specifications as a control for the number of states/borders in each grid-square, we do two additional things to ensure that no bias results from the inclusion of these units in our estimation here. First we exclude completely interior states from the data and reestimate our models. Second, we add as a regressor the count of the average number of states that existed within a given grid-square unit post-1815.

These results are presented in Table 9. In both situations - only including non-interior states or including the count of states as a regressor - our estimates remain qualitatively similar to those in the main text.

[Table 9 about here.]

9 Varying Effects: Western and Eastern Europe

A related worry is that the results are dependent on particular countries such as France, Germany, or Russia. France and Germany have a number of disputes post-1815, as the map in Figure 1 of the main text shows, which might lead to worries about the results depending on the border area between the two states. In contrast, much of the vast expanse of the Russian state is both without precedents and subsequent claims, which may lead to worries that the results depend on comparison of grid-squares near boundaries to these empty Russian grid-squares. Given that our focus is not on particular states, but on grid-square areas, it is not straightforward to remove particular countries, especially since their borders change after 1815. However, we assuage these concerns by showing that the effect of multiple historical boundaries are very similar across both Western and Eastern Europe. Thus, estimates are very similar regardless of whether they reflect all of Europe, are only relevant to the subsample of Eastern European grid-squares, which excludes the French-German claims, or are only relevant to Western European grid-squares, which excludes the Russian and Ottoman/Turkish grid-squares. These results are presented in Table 10. Again, we find that the effect of historical border variability is across model specification consistently statistically significant, in the predicted direction, and consistent across both regions and for all outcomes of interest.

[Table 10 about here.]

10 Do More Recent Precedents Matter More?

As we note in the main text, a key implication of the the persistent coordination effects explanation is that leaders' incentive to claim a precedent will wane as it becomes older. Accordingly, we report results in Table 4 of the main text that explore how the average age of precedents in a grid-square affect claim emergence. Here we report the results of an alternative strategy for measuring the age of precedents. Specifically, we produce distinct measures of both *Historical Border Variability*$_i$ and *Historical Border Density*$_i$ across three periods: 1650–1699, 1700–1749, and 1750–1789. The persistent coordination effects theory implies that the estimated effect of border variability in the latest period should dominate those of the prior two periods. We explore whether this is true for both of our key dependent variables, i.e., the number of territorial claims and the timing of the first claim.

The results across both dependent variables demonstrate that in each period historical border density and variability are separately correlated with the number and timing of contemporary territorial claims. However, when we include the measures from each period simultaneously we find that the most recent measures of border variability, those measured between 1750 and 1789, are the only statistically significant predictors. We include all of the control variables discussed above in all of the model specifications. The results are given in Tables 11 and 12.

[Table 11 about here.]

[Table 12 about here.]

11 Placebo Test: Do Pre-1650 Precedents Also Matter?

We also check to see if very old historical precedents exert systematic influence on post-1815 territorial claims. Again, our arguments imply that more recent precedents will be more salient and more closely associated with territorial claims. We examine our density and variability measure pre-1650 to ensure that these very old pre-Westphalia precedents do not exert similar effect to 18th century precedents, which would raise worries about spuriousness. We find that for each of our dependent variables *Historical Border Variability*$_i$, our main measure of interest, measured from 1500–1649 has no effect on territorial claims.

[Table 13 about here.]

12 Are Multiple Historical Precedents a Proxy for Multiple Ethnic Groups?

As we point out in the main text, there is little historical evidence to support the idea that pre-1789 European political boundaries had much to due with ethnic or national identity, the latter being an idea that had not even taken hold yet. However, we address this claim here in more detail to demonstrate that our historical boundary density and variability are not simply picking up the number or distribution of ethnic groups. Spatial data on ethnicity has historically been elusive as it is difficult to code. However, recently Wucherpfennig et al. (2011) have produced data on the spatial distribution of ethnic groups with creation of the geo-coded ethnic power relations data (EPR). This data purports to measure the distribution of all ethnic groups post-1945. Given that the data only covers post-1945, we do not produce measures using the exact spatial distribution data, but rather use the data to count the number of distinct ethnic groups in each of our grid-square units.[7]

We account for the possible confounding effects of ethnicity in two ways. First, we treat ethnicity as a pre-treatment confounder. This is appropriate if our view is that ethnicity is not something that is shaped by historical borders, but is either an innate feature of individuals or something that predates post-Westphalia political boundaries. These results are summarized in table 14. We see that the inclusion of the count of ethnicities as a pre-treatment confounder does not substantively alter our results.

[Table 14 about here.]

Second, in analysis analogous to what we did with MIDs in table 7 we assume that the distribution of ethnic groups post-WWII is post-treatment, where the treatment is our data on historical precedents. Thus, the idea here is that historical borders affect ethnic identity, an idea that is put forth by scholars such as Sahlins (1989) and Goemans (2006). Thus, we allow ethnicity to mediate the effect of historical border density and variability on post-WWII territorial claims. Specifically, we consider the effect of ethnicity as if it were a post-treatment outcome of historical border precedents that may also effect the onset, duration, and timing of territorial claims and estimate the effect of border precedents as they might operate through the distribution of ethnic groups across

[7]Results are similar if we create and use a Herfendahl Index of ethnic territory for each of our units.

territory. Treating the count of ethnicities on a given unit as a mediating variable, we follow Imai, Keele and Tingley (2010) and Imai et al. (2011) and estimate the direct and mediating effect of historical border precedents. Across our outcomes the direct effect of both the historical variability and density of borders is statistically significant and in the predicted direction. However, the mediating effect of both outcomes is both small relative to the direct effect and statistically insignificant. Only when we treat the outcome as the number of years until the first claim is made as the outcome and consider past border variability as the "treatment" variable does the mediating effect of ethnicity become statistically significant. Even here, however, the mediating effect is only 6.26 % of the direct effect.

[Table 15 about here.]

In sum, when ethnicity is thought of as a pre-treatment confounder the effect of historical border precedent is not confounded. Similarly, when we treat ethnicity as a post-treatment mediator, the direct effect of historical borders remains strong and statistically significant but there very little evidence that it operates through the mediator of ethnicity.

13 The Duration of Unresolved Disputes

Given that the main theoretical and empirical contribution of the paper is to explore the emergence of territorial claims, we focus our attention on the number of claims that emerge and how quickly they emerge in the main text. However, we have also explored the duration of claims that emerge as a test of whether claims that follow precedent are systematically more easy or quick to resolve than those that do not follow precedent. Although we acknowledge that claim duration is not a perfect measure of claim salience, we see it as way to show that the results in the manuscript do not seem to be sensitive to the fact that we only look at emergence and ignore how long claims take to resolve.

The results in table 16 demonstrate that areas with high variability in historical borders experience significantly longer disputes in addition to having more disputes that arise more quickly. These results provide further evidence that multiple historical precedents are associated with subsequent territorial dispute, as variability in border density is the key factor behind the emergence

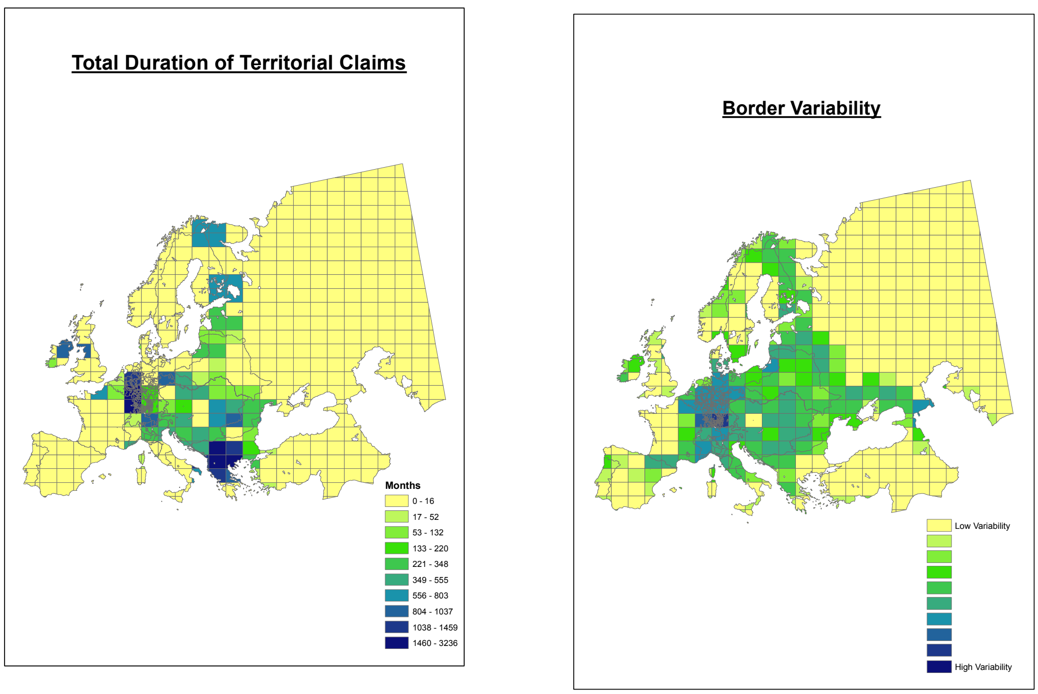


Figure 3. The duration of territorial claims across territory. Europe (1650)

and duration of territorial claims. We also note that these results confirm an interesting additional observable implication of our arguments. The idea that claims tied to historical precedents have greater legitimacy and credibility with domestic and international audiences implies that settlements will be more costly and perhaps more difficult to subsequently reverse. Thus, claims in areas with multiple historical precedents should be more difficult to resolve and involve harder bargaining, as the shadow of the future implied by settlement looms larger for disputants (Fearon, 1998). However, we hasten to note that a full assessment of claim duration and whether duration is a reasonable measure claim intensity necessitates exploration of both claim militarization and claim settlement, which is beyond the scope of this study. However, the results in table 16 do provide an interesting starting point for such a study.

[Table 16 about here.]

14 Sensitivity to Assumptions about Omitted Variables

All of our results rely upon the standard (but arguably strong) assumption that conditional upon observable variables that we control for, our treatment measures of historical border variability are exogenous. As noted in the main text, we assess the validity of this assumption with the test developed by Oster (2013). In order to place bounds on the bias of a treatment effect estimate caused by the presence of unobservables, i.e., omitted variables, this method uses information from changes in both point estimates and R^2 values derived from comparing the unconditional estimated causal impact of $\textit{Historical Border Variability}_i$ to the this variable's estimated effect after conditioning on all other observable covariates, i.e., our measures of territorial value. The procedure allows researchers to evaluate the degree to which unobservable factors are likely to bias their estimates of the causal quantity of interest and builds upon the econometric theory developed by Altonji, Elder and Taber (2005).

[Figure 4 about here.]

[Figure 5 about here.]

As noted in the manuscript, our key results are highly robust to a variety of specifications of this test. We expand upon our discussion of these results here by showing graphical depictions of

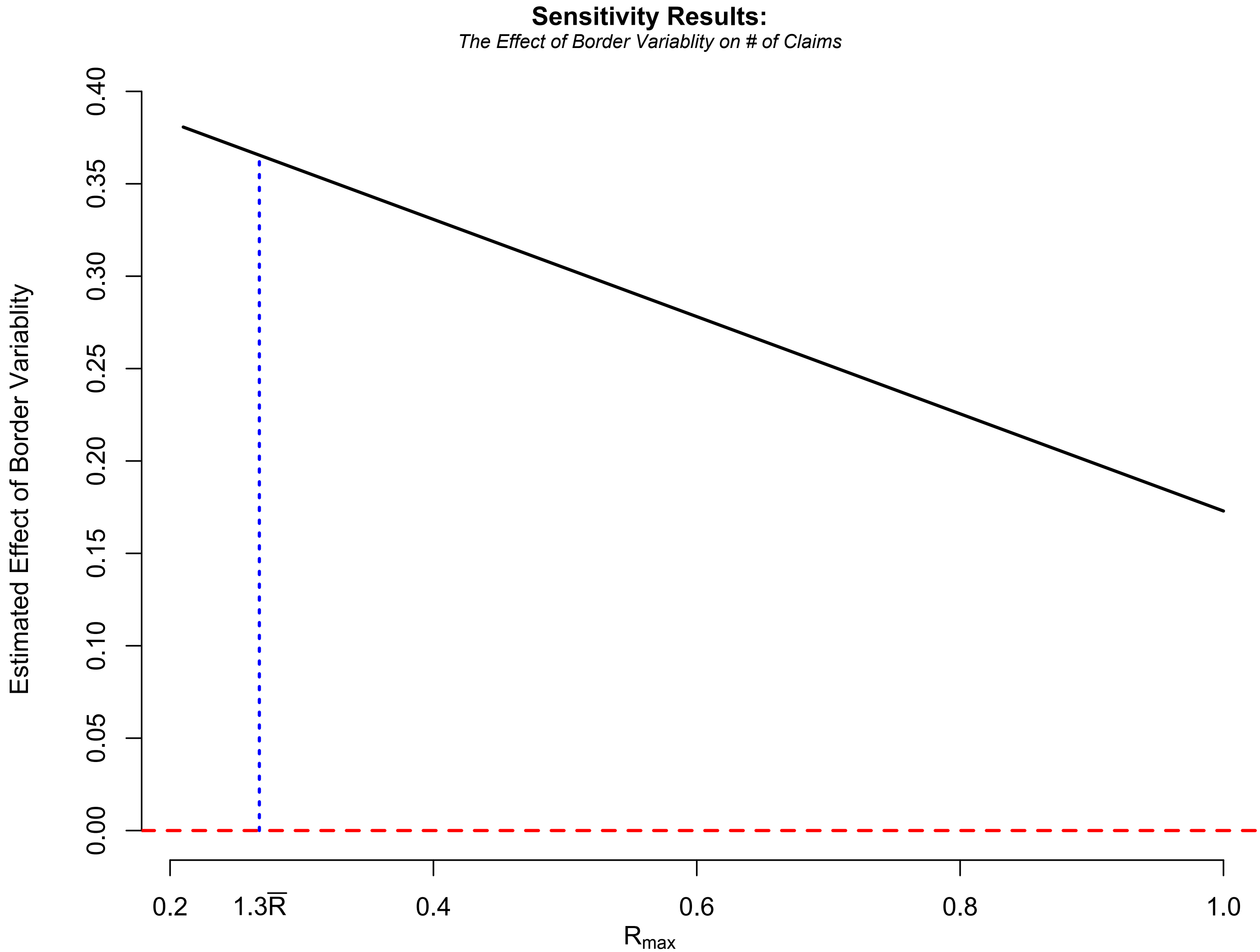


Figure 4. Sensitivity of the effect of historical border variability derived from the procedure proposed by Oster (2013). The outcome is the number of claims made on a given grid-square. The vertical blue line indicates the predicted effect of our historical variability measure for a hypothetical regression including both observable and unobservable factors that explains 1.3 times the R^2 of the model that contains just observables.

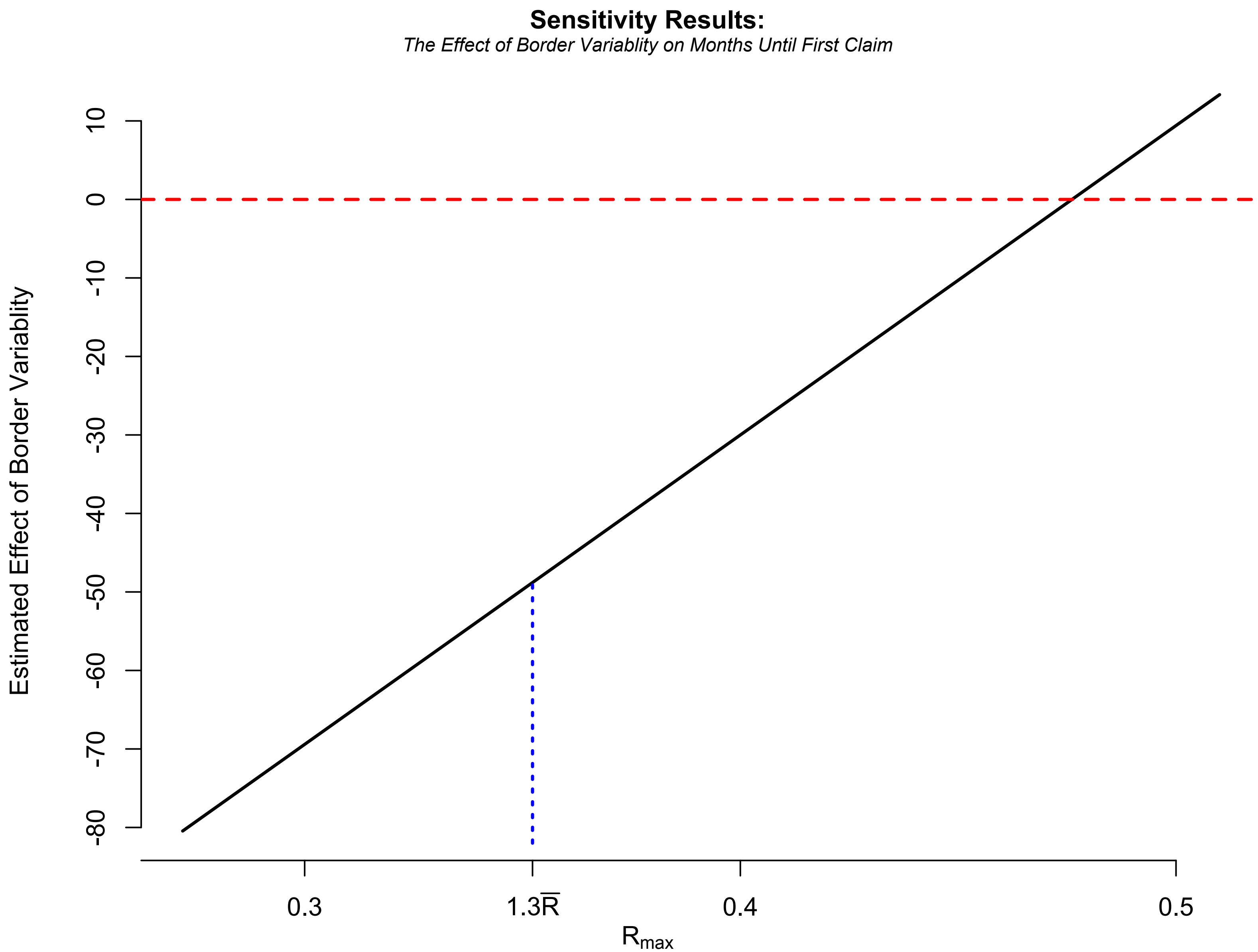


Figure 5. Sensitivity of the effect of historical border variability derived from the procedure proposed by Oster (2013). The outcome is the number of months until the first claim was made on a given grid-square. The vertical blue line indicates the predicted effect of our historical variability measure for a hypothetical regression including both observable and unobservable factors that explains 1.3 times the R^2 of the model that contains just observables.

the test results. Figures 4 & 5 show how the estimated impact of historical border variability would change if we allow for possible violations of our identifying assumption of exogeniety conditional on observables. On the x-axis of both figures we consider the maximum R^2 from a hypothetical regression of our outcomes on both our full set of observables as well as unobservables. On the y-axis we give the estimated causal effect associated with the regression associated with the maximum R^2 given the x-axis. As a "rule of thumb," Oster (2013) considers those results that survive 1.3 times the R^2 associated with the regression containing only the full set of observed controls to be robust.[8] The estimated effect of $\textit{Historical Border Variability}_i$ on both the number of claims on a given unit and the number of months until the first claim was made greatly exceed this rule of thumb. When we treat the number of claims made on a unit as our outcome our result survives up to the maximum possible violation up to 4.85 times the R^2 associated with the model containing the full set of controls. That is, this result survives even when we allow for a hypothetical model with unobservables to be perfectly predictive of our outcome ($R_{max} = 1$). When we treat the number of months until the first claim was made as the outcome, our results remain similarly robust, surviving up an R^2 from the hypothetical regression containing unobservables up to a 1.75 times the value of the R^2 associated with the regression containing just observables. In all, the main statistical findings of this paper survive when we allow for the existence of possible unobservable confounders, providing more confidence in a causal interpretation of our results.

15 Claims with Precedent and Claims Without Precedent: Analysis of Claim Timing

Table 3 in the main text presents results for the number of territorial claims where we distinguish among claims that follow precedent and claims that do not follow precedent. We find that our key result that $\textit{Historical Border Variability}_i$ leads to subsequent claims is driven by claims along historical precedents. Table 17 replicates this analysis for the timing of a unit's first territorial claims. The results are analogous to those reported in the main text in that the timing of claims that follow precedent are driven by $\textit{Historical Border Variability}_i$. Models 1–6 in Table 17 show

[8] Oster (2013) finds that 90% of a random sample of randomized control trials (N=65) published in the *American Economic Review, Journal of Political Economy, Quarterly Journal of Economics, Econometrica* and *American Economic Journal: Applied Economics* would survive this threshold.

that variability has a very similar effect regardless of whether we control for mean density. In contrast, mean density becomes insignificant when included in the same model as historical border variability. In contrast, the timing of claims without precedent do not show a consistent pattern when both density and variability are included in the same model. In the OLS specification, i.e., model 11, density remains significant while variability loses significance, while the opposite is true in the Cox model presented in column 12.

[Table 17 about here.]

16 Claims with Precedent and Claims Without Precedent: Seemingly Unrelated Regression

In Table 3 of the main text we treat the two dependent variables, claims with precedent and claims without precedent, as independent. In other words, we assume that there is no relation between the error terms of these two equations. Here, we investigate whether this assumption is consequential or not by allowing for possible correlation across these two distinct types of territorial claims. Specifically, we use seemingly unrelated regression (SUR) where the error terms in the equations for claims with precedent and claims without precedent are allowed to be correlated. These results are presented in Table 18 and provide qualitatively similar estimates. Most importantly, the estimates of $\textit{Historical Border Variability}_i$ are essentially no different than reported in table 3 of the manuscript.

[Table 18 about here.]

17 The Effect of Historical Border Variability: Non-parametric Estimation

Table 19 contains the full results for the generalized additive model (GAM) that we use to estimate the effect of $\textit{Historical Border Variability}_i$ reported in Figure 5 in the manuscript. The model is a negative binomial model of the number of territorial claims identical to the full specification in column 12 of table 1, except that we estimate $\textit{Historical Border Variability}_i$ using penalized regression splines. We use the mgcv package in R, and implement the smoother as described in Wood (2011). Specifically, we fit 10 knots along the range of *Historical Border Variability*$_i$, and estimate the smoothness of this variable's effect on claims with the data. We tried a number of alternative specifications, changing the number of knots, and specifying the smoothness parameters rather than estimating them, and found qualitatively similar effects to that reported in Figure 5 of the main text.

[Table 19 about here.]

18 Using Penalized Regression Splines to Approximate Latitude and Longitude Fixed Effects

Fixed effects for either latitude or longitude are not possible in our framework, as there would be at least as many fixed effects as observations. However, we can approximate a fixed effects approach by estimating the effect of latitude and longitude with penalized regression splines. The penalized regression splines can approximate fixed effects in the sense that, if we fit enough knots, the effect of both latitude and longitude on the number of claims is allowed to change quite flexibly across the map of Europe. This seems a reasonable approach here as we simply want to pick up any unmeasured spatial variables that might affect the value of territory to leaders (and thus affect claim emergence).

Table 20 contains the results of the GAM model. Similar to in the model reported in table 19, we estimate a negative binomial model that is identical to the specification in column 12 of table 1 in the manuscript, except for the fact that both latitude and longitude are estimated non-parametrically. To ensure that both latitude and longitude can have virtually any (smoothed) effect on claims, we fit 200 knots to each variable, allowing the smoothness parameters to be estimated from the data. The results are very similar if we fit many fewer knots, e.g., 10 knots. As in our estimation in table 19, use the mgcv package in R, as described by Wood (2011).

The result of interest in table 20 is that the estimated effect of *Historical Border Variability*$_i$ is very similar to that reported in table 1 in the main text. The estimated coefficient remains large and statistically significant, despite the fact that we control for a wide range of potential confounders by allowing the effects of latitude and longitude to take a wide-variety of shapes. This result further suggests that the effect of $\textit{Historical Border Variability}_i$ is not attributable to some omitted spatial variable that it serves as a proxy for or is highly correlated with.

[Table 20 about here.]

19 Reproducing Table 4 with a Negative Binomial Model

In Table 4 of the main text we present results of the interactive effects of the average time since the last border change and the average border duration with the number of border changes. These results provide critical tests of the persistent coordination effects explanation, as they demonstrate that leaders make more territorial claims over precedents that were better established as international boundaries and were “removed” from the map more recently. In the main text we estimate these relationship via OLS as this makes interpretation of the interaction effects much more straightforward. In Table 21 we reproduce these results using negative binomial regression to demonstrate that they remain substantively similar.

[Table 21 about here.]

20 Reproducing Table 4 Exploring the Timing of Claims

Table 4 of the main text reports results over how the interactive effects of the average time since the last border change and the average border duration with the number of precedents affects the volume of claims. These results provide critical tests of the persistent coordination effects explanation, as they demonstrate that leaders make more territorial claims over precedents that were better established as international boundaries and were “removed” from the map more recently. Here we report the analogous results for our second dependent variable, the timing of the first territorial claim. Table 22 contains the results of OLS models, while Table 23 contains the results of Cox models. Both modeling approaches yield results that are similar to each other and mirror the results in the main text that examine the number of claims. Thus, leaders not only make more claims over better-established and more recent precedents, but also make claims more quickly over these precedents. These results demonstrate further support for the persistent coordination effects explanation.

[Table 22 about here.]

[Table 23 about here.]

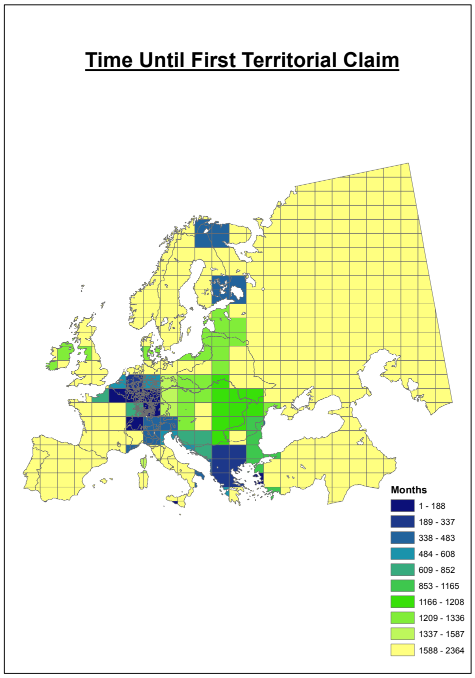
21 Alternative Measures of Border Stability

In this section we report additional results from three alternative measures. The first two measures are alternatives to our main measure *Historical Border Variability*$_i$, while the third is an alternative measure of how old historical precedents are. The measures are summarized below and the results using these measures are presented in Table 24. The results using these measures are all consistent with those reported in the main text and elsewhere in this appendix.

- **Annualized percent border change:** For each grid-square we take the border density in each 5 year time period. We then measure the percent annual change. That is, for each period t we calculate $p_t = 1 - |\frac{Density_{t+1} - Density_t}{Density_t}|$. Then over the entire period from 1650-1790, we estimate the annualized change, equal to $\mathbf{p} = \prod_{1650}^{1789} p_t$. If there have been no changes, this measure takes a value of 1, but when there are large changes across time p will become increasingly small. Thus, our expectation is that this measure is negatively associated with the emergence of claims.

- **Cumulative Border Stability** For each grid square in each time period we observe the average precedent change and then sum this period-specific average change across all time periods. That is, for every border in each grid-square we take average number of border changes in each period. We then take the sum of this across time. This results in a measure that, if there have been no border changes $\mathbf{c}$ = T, while if every border changed every year $\mathbf{c}$ = 0.

- **Time Since Last Change** Rather than taking the average for each precedent (as we do in the main text) we record for each grid square the number of years that have passed since the **last** pre-1790 border occurred on grid-square unit i. Accordingly, we expect this variable to have a negative effect on claim emergence, as higher values indicate older precedents.



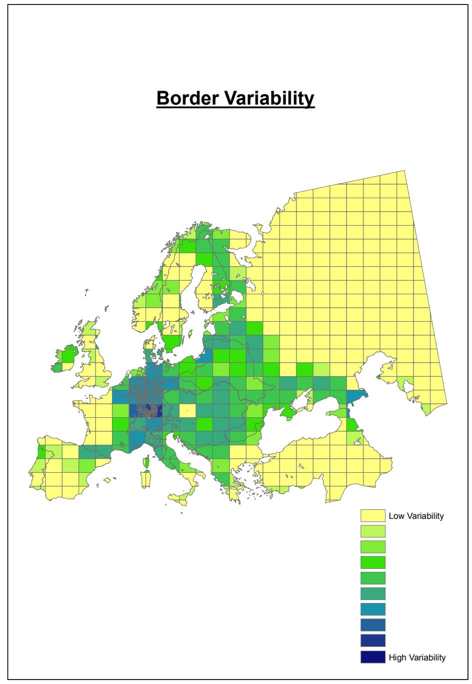


Figure 6. The timing of territorial claims across territory. Europe (1650)

[Table 24 about here.]

22 Mapping the Timing of Territorial Claims Across Europe

Figure 6 shows the distribution of claim timing and *Historical Border Variability*$_i$ across Europe. Europe's 1650 borders are mapped in the background. This Figure is analogous to Figure 1 in the main text.

23 A Survival Curve for the Timing of Territorial Claims Across Europe

The substantive effect of border variability on claim timing based on the Cox regression models is large, as seen in the predicted survival plots in Figure 7. The predicted decline in time until the first failure is calculated by increasing each variable from its observed 5th percentile to its

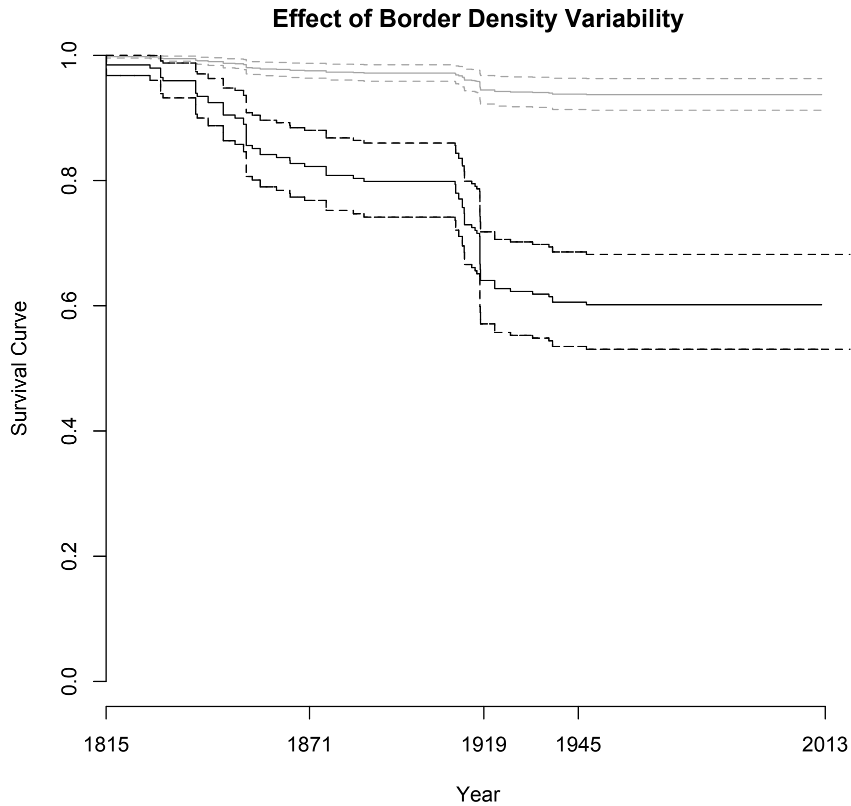


Figure 7. The parameter estimates used to derive the predictions are from column 12.

95th percentile. The difference in predictions across these values are stark and show that greater variability in the density of historical borders leads to much earlier territorial claims. By 1871 the survival probability for the unit at the ninety-fifth percentile of border variability is just above 55% and declines by 1945 to just under 20%. For the unit at the fifth percentile the predictions at the same points in time are just above 80% and just above 60%, respectively, with 95% confidence intervals that do not overlap with the predictions for units with high border variability.

The Effect of Border Variability: Dyadic Unit of Analysis

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Historical Border Variability	1.158^{**}	0.917^{***}	1.760^{***}	1.531^{***}					1.916^{***}	1.243^{***}	2.437^{***}	2.011^{***}
	(0.453)	(0.213)	(0.558)	(0.260)					(0.491)	(0.222)	(0.594)	(0.266)
Historical Border Density					-0.145	0.041	-0.075	0.111	-0.641^{**}	-0.232^{***}	-0.625^{*}	-0.349^{***}
					(0.288)	(0.121)	(0.313)	(0.162)	(0.289)	(0.089)	(0.318)	(0.109)
Latitude	0.074^{*}	0.031^{*}	0.072^{*}	0.049^{**}	0.064	0.032^{*}	0.060^{*}	0.039^{*}	0.047	0.022	0.041	0.030
	(0.042)	(0.017)	(0.040)	(0.021)	(0.041)	(0.019)	(0.034)	(0.020)	(0.039)	(0.017)	(0.032)	(0.019)
Longitude	-0.226^{**}	-0.113^{***}	-0.248^{**}	-0.130^{***}	-0.164^{**}	-0.090^{**}	-0.191^{**}	-0.119^{**}	-0.155^{*}	-0.074^{**}	-0.171^{**}	-0.068
	(0.096)	(0.037)	(0.107)	(0.046)	(0.082)	(0.036)	(0.090)	(0.047)	(0.080)	(0.036)	(0.080)	(0.042)
Urban Population	-0.009^{***}	-0.007^{***}	-0.009^{**}	-0.006^{*}	-0.007^{***}	-0.006^{***}	-0.008^{**}	-0.006^{**}	-0.007^{**}	-0.006^{***}	-0.007^{**}	-0.004
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Iron Production	0.003	-0.024	-0.027	-0.030	0.127	0.062	0.152	0.087	0.002	-0.022	-0.023	-0.027
	(0.106)	(0.043)	(0.116)	(0.055)	(0.107)	(0.047)	(0.116)	(0.059)	(0.105)	(0.045)	(0.119)	(0.059)
Terrain Ruggedness	-0.007	-0.003	-0.008^{*}	-0.004^{*}	-0.006	-0.003	-0.007^{*}	-0.005^{*}	-0.004	-0.002	-0.005	-0.002
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Agricultural Suitability	0.000^{*}	0.000^{*}	0.000^{*}	0.000^{*}	0.000^{*}	0.000^{**}	0.000^{**}	0.000^{**}	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Intercept	-1.185	-5.773^{*}	-8.069	-13.402^{***}	13.431^{**}	5.492^{**}	14.401^{**}	6.625^{**}	-9.028	-10.273^{***}	-15.235^{**}	-20.204^{***}
	(7.529)	(3.190)	(8.848)	(3.980)	(6.076)	(2.290)	(7.194)	(3.022)	(6.134)	(3.016)	(6.738)	(3.700)
N	104	104	104	104	104	104	104	104	104	104	104	104
Model	OLS	NegBin	OLS	NegBin	OLS	NegBin	OLS	NegBin	OLS	NegBin	OLS	NegBin
Weight By Area	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 1. This table produces our main results now treating the unit of observation as the 50 kilometer buffer area surrounding the dyadic border of post 1815 states.

The Effect of Border Variability: Dyadic Unit of Analysis Controlling for Ethnic Power Relations

	1.	2.	3.	4.	5.	6.	7.	8.
Historical Border Variability	1.87^{***}	2.42^{***}	1.90^{***}	2.43^{***}	1.79^{***}	2.35^{***}	2.23^{***}	2.86^{***}
	(0.50)	(0.61)	(0.49)	(0.59)	(0.44)	(0.56)	(0.62)	(0.74)
Historical Border Density	-0.66^{**}	-0.64^{**}	-0.62^{**}	-0.61^{*}	-0.61^{**}	-0.60^{**}	-0.70^{**}	-0.61^{*}
	(0.29)	(0.31)	(0.29)	(0.31)	(0.26)	(0.30)	(0.28)	(0.31)
Latitude	0.03	0.02	0.05	0.04	0.08^{*}	0.08^{*}	0.07	0.06
	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)	(0.06)	(0.06)
Longitude	-0.15^{*}	-0.17^{**}	-0.17^{*}	-0.18^{**}	-0.19^{**}	-0.21^{**}	-0.19^{*}	-0.25^{*}
	(0.08)	(0.08)	(0.09)	(0.08)	(0.09)	(0.09)	(0.11)	(0.13)
Urban Population	-0.01^{**}	-0.01^{*}	-0.01^{**}	-0.01^{**}	-0.01^{***}	-0.01^{**}	−0.01	−0.02
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Iron Production	0.02	−0.02	0.00	−0.03	0.00	−0.01	0.07	0.05
	(0.10)	(0.12)	(0.11)	(0.12)	(0.10)	(0.12)	(0.13)	(0.15)
Terrain Ruggedness	0.00	0.00	0.00	0.00	0.00	−0.01	−0.01	−0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Agricultural Suitability	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Shared Ethnic Groups	0.08	0.06						
	(0.07)	(0.08)						
Shared Ethnic Group Both in Power			−0.97	−0.58				
			(0.98)	(0.92)				
Shared Ethnic Group 1 in Power					-1.33^{*}	-1.38^{*}		
					(0.70)	(0.82)		
Shared Ethnic Group 1 w/ Plurality							−1.00	−1.14
							(0.68)	(0.78)
Intercept	−8.25	-14.78^{**}	−8.39	-14.90^{**}	−5.89	-12.47^{*}	−10.35	-15.86^{*}
	(5.98)	(6.58)	(6.30)	(6.82)	(6.34)	(7.26)	(7.70)	(8.31)
Weighted By Area	N	Y	N	Y	N	Y	N	Y
N	104	104	104	104	104	104	104	104

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 2. This table gives results which control for features of dyadic ethnic differences and similarities.

The Effect of Border Variability: Grid as The Unit of Analysis Controlling for Ethnic Power Relations

	1.	2.	3.	4.	5.	6.	7.	8.
Historical Border Variability	0.359***	0.418***	0.353***	0.381***	0.327***	0.478***	0.415***	0.425***
	(0.10)	(0.10)	(0.09)	(0.12)	(0.09)	(0.12)	(0.10)	(0.12)
Historical Border Density	-0.259	0.080	-0.198	0.049	-0.132	0.261	-0.294	-0.007
	(0.23)	(0.26)	(0.21)	(0.30)	(0.22)	(0.26)	(0.24)	(0.30)
Latitude	0.008	-0.034**	0.007	-0.042*	0.007	-0.029**	0.008	-0.045**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Longitude	0.023*	0.039*	0.027*	0.022	0.017	0.018	0.028*	0.018
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Iron Production	-0.169	-0.374***	-0.043	-0.158**	-0.149	-0.124	-0.122	-0.188*
	(0.13)	(0.07)	(0.14)	(0.06)	(0.17)	(0.07)	(0.15)	(0.08)
Terrain Ruggedness	0.004**	0.005***	0.003*	0.002	0.003*	0.003	0.004**	0.003
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Urban Population	0.025*	0.012*	0.024**	0.009	0.020*	0.002	0.025*	0.009
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Agricultural Suitability	0.006	-0.028	-0.006	-0.023	-0.029	-0.133	-0.005	-0.011
	(0.05)	(0.08)	(0.04)	(0.08)	(0.04)	(0.10)	(0.05)	(0.08)
River Length	0.002*	0.002***	0.002*	0.003**	0.002	0.002*	0.003*	0.003**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Shared Ethnic Groups	1.701	4.142**						
	(1.01)	(1.37)						
Shared Ethnic Group Both in Power			1094.795	258.167**				
			(578.74)	(93.46)				
Shared Ethnic Group 1 in Power					62.428*	80.511***		
					(30.07)	(22.90)		
Shared Ethnic Group 1 w/ Plurality							-0.925	1.461
							(1.15)	(5.20)
N	418	418	418	418	418	418	418	418
Model	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 3. This table gives results which control for features of gridded ethnic differences and similarities.

The Effect of Border Variability Eliminating Overlapping Claims

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Historical Border Density	0.133**	0.358***			−0.251***	−0.231**	0.215*	0.384*			−0.261	−0.167
	(0.063)	(0.084)			(0.094)	(0.099)	(0.117)	(0.202)			(0.177)	(0.242)
Historical Border Variability			0.360***	0.666***	0.436***	0.710***			0.291***	0.638***	0.352***	0.659***
			(0.055)	(0.106)	(0.066)	(0.113)			(0.055)	(0.081)	(0.078)	(0.115)
Urban Population							0.019***	0.014**	0.018**	0.016**	0.017**	0.016**
							(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
Latitude							0.010**	−0.035***	0.012***	−0.012**	0.008*	−0.016**
							(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	(0.008)
Longitude							0.009	0.046***	0.013*	0.016	0.013*	0.011
							(0.007)	(0.016)	(0.007)	(0.017)	(0.007)	(0.016)
Agricultural Suitability							−0.065*	−0.135	−0.056*	−0.056	−0.032	−0.032
							(0.037)	(0.084)	(0.033)	(0.070)	(0.036)	(0.080)
Terrain Ruggedness							0.002**	0.003**	0.002**	0.002**	0.002**	0.002*
							(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Iron Production							0.006	−0.127	−0.002	−0.119	0.008	−0.105
							(0.141)	(0.104)	(0.141)	(0.104)	(0.142)	(0.103)
River Length							0.002***	0.003***	0.002**	0.002***	0.002**	0.002***
							(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Intercept	0.584***	−0.788***	1.714***	0.887***	2.248***	1.300***	−0.725	−2.768***	0.094	−0.452	0.689	0.158
	(0.116)	(0.203)	(0.218)	(0.150)	(0.331)	(0.261)	(0.472)	(0.957)	(0.504)	(1.091)	(0.557)	(0.915)
N	466	466	466	466	466	466	466	466	466	466	466	466
Model:	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 4. This table reproduces our main results after having eliminated contemporary territorial claims that are plausibly related to each other

The Effect of Border Variability Binary Outcome

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Historical Border Density	0.056^{***}	0.268^{***}			-0.024	-0.085	0.089^{***}	0.381^{***}			0.005	0.047
	(0.014)	(0.066)			(0.016)	(0.071)	(0.023)	(0.096)			(0.029)	(0.132)
Historical Border Variability			0.084^{***}	0.343^{***}	0.091^{***}	0.368^{***}			0.064^{***}	0.302^{***}	0.062^{***}	0.292^{***}
			(0.011)	(0.045)	(0.012)	(0.051)			(0.010)	(0.045)	(0.013)	(0.058)
Urban Population							0.002^{**}	0.007^{**}	0.002^{*}	0.006^{*}	0.002^{*}	0.006^{*}
							(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)
River Length							0.001^{***}	0.002^{***}	0.000^{***}	0.001^{**}	0.000^{***}	0.001^{**}
							(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latitude							0.001	-0.004	0.000	-0.010^{**}	0.000	-0.009^{**}
							(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)
Longitude							0.001	0.013	0.002	0.011	0.002	0.012
							(0.001)	(0.009)	(0.001)	(0.010)	(0.001)	(0.010)
Agricultural Suitability							-0.033^{***}	-0.198^{***}	-0.027^{***}	-0.157^{***}	-0.027^{***}	-0.163^{***}
							(0.008)	(0.055)	(0.007)	(0.047)	(0.008)	(0.052)
Terrain Ruggedness							0.000^{**}	0.002^{***}	0.000^{**}	0.002^{***}	0.000^{**}	0.002^{***}
							(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Iron Production							0.008	0.005	0.008	0.004	0.008	0.003
							(0.014)	(0.041)	(0.014)	(0.040)	(0.014)	(0.041)
Intercept	0.108^{***}	-1.298^{***}	0.405^{***}	-0.188	0.456^{***}	-0.011	0.005	-1.726^{***}	0.267^{***}	-0.329	0.256^{**}	-0.451
	(0.020)	(0.112)	(0.041)	(0.127)	(0.055)	(0.191)	(0.102)	(0.578)	(0.101)	(0.594)	(0.108)	(0.551)
N	466	466	466	466	466	466	466	466	466	466	466	466
Model:	OLS	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS	Probit

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 5. This table reproduces our main results treating a binary indicator for the presence of any number of territorial claims as the outcome of interest

The Effect of Border Variability in Interaction with Territorial Value

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
	Number of Claims						*Time Until First Claim*					
Historical Border Density	−0.312	−0.248	−0.310	−0.290	−0.250	−0.286	−35.853	−62.448	−41.033	−50.089	−67.932	−49.252
	(0.215)	(0.205)	(0.211)	(0.211)	(0.225)	(0.216)	(49.659)	(52.120)	(51.430)	(53.091)	(56.948)	(55.749)
Historical Border Variability	0.327***	0.295***	0.354***	0.421***	0.516***	0.348**	−52.553**	−52.379**	−70.504***	−85.991***	−150.403***	−81.169**
	(0.088)	(0.084)	(0.097)	(0.094)	(0.137)	(0.153)	(20.356)	(21.040)	(20.752)	(24.884)	(32.353)	(38.002)
Urban Population	0.022**	0.036***	0.022***	0.022**	0.021**	0.032**	−3.301*	−7.723***	−3.434*	−3.108*	−3.026*	−5.071*
	(0.009)	(0.013)	(0.009)	(0.009)	(0.009)	(0.015)	(1.754)	(2.521)	(1.756)	(1.765)	(1.756)	(2.805)
River Length	0.003***	0.002**	0.002**	0.003**	0.002**	0.003	−1.012***	−0.580**	−0.590***	−0.608***	−0.579**	−0.832***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.262)	(0.229)	(0.226)	(0.228)	(0.225)	(0.308)
Latitude	0.008	0.009	0.010	0.013**	0.012**	0.009	0.554	−0.364	−0.579	−1.448	−1.676	0.365
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(1.464)	(1.550)	(1.479)	(1.540)	(1.540)	(1.668)
Longitude	0.004	0.003	0.004	0.007	0.007	0.003	−1.540	−1.797	−2.218	−3.023	−3.103	−1.484
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(2.568)	(2.617)	(2.605)	(2.741)	(2.588)	(2.680)
Agricultural Suitability	−0.027	−0.031	−0.038	−0.045	−0.125	−0.036	19.466	24.174**	25.980**	28.522**	70.119**	39.579
	(0.040)	(0.041)	(0.040)	(0.040)	(0.101)	(0.114)	(11.893)	(11.858)	(11.977)	(12.044)	(29.432)	(32.628)
Terrain Ruggedness	0.003**	0.003**	0.003**	0.002	0.003**	0.002	−0.452*	−0.471*	−0.485**	−0.473	−0.614**	−0.436
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.249)	(0.251)	(0.245)	(0.475)	(0.258)	(0.443)
Iron Production	−0.099	−0.094	−0.011	−0.068	−0.072	−0.043	−18.975	−24.861	−55.154**	−31.786	−32.949	−32.300
	(0.154)	(0.157)	(0.191)	(0.162)	(0.157)	(0.250)	(25.210)	(25.843)	(25.270)	(27.929)	(27.043)	(30.773)
Historical Border Variability × River Length	0.001					0.000	−0.281***					−0.179**
	(0.000)					(0.001)	(0.070)					(0.086)
Historical Border Variability × Urban Population		0.007				0.005		−2.131**				−0.798
		(0.004)				(0.005)		(0.838)				(1.013)
Historical Border Variability × Iron Production			0.078			0.049			−28.913			−11.859
			(0.092)			(0.118)			(19.417)			(12.687)
Historical Border Variability × Terrain Ruggedness				0.000		0.000				0.046		0.008
				(0.000)		(0.001)				(0.149)		(0.145)
Historical Border Variability × Agricultural Suitability					−0.029	−0.003					14.917**	6.950
					(0.024)	(0.027)					(6.777)	(7.581)
Intercept	1.129	0.987	1.143	1.048	1.317*	1.131	2126.169***	2191.326***	2135.154***	2174.332***	2028.778***	2063.850***
	(0.699)	(0.666)	(0.694)	(0.675)	(0.750)	(0.759)	(156.777)	(159.720)	(161.518)	(165.401)	(191.887)	(190.888)
N	466	466	466	466	466	466	466	466	466	466	466	466

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 6. This table produces our main results now interacting the measure of historical variability with measures of territorial value.

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The Effect of Historical Borders: MIDS as a Mediator

Outcome:	Number of Claims		Duration Until Claim	
Average Causal Mediation Effect (MIDS)	0.025	0.01	-3.512	-1.46
	(0.179)	(0.139)	(0.272)	(0.290)
Total Effect *Historical Border Density*$_i$	0.236	.	-159.851	.
	(0.00)	.	(0.00)	.
Average Direct Effect *Historical Border Density*$_i$	0.211	.	-156.339	.
	(0.00)	.	(0.00)	.
Total Effect *Historical Border Variability*$_i$		0.316	.	-92.496
	.	(0.00)	.	(0.00)
Average Direct Effect *Historical Border Variability*$_i$		0.305	.	-91.036
	.	(0.00)	.	(0.00)

Table 7. This table presents the estimated mediation effect of MIDS on the frequency and timing of territorial claims as well as the direct and total effects of border density and variability. All models include controls for Urban Population, River Length, Iron Production, Latitude, Longitude, and Terrain Ruggedness. p-value derived from quasi-bayesian simulation in parentheses.

The Effect of Historical Borders on Territorial Claims - No Post 1816 States

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
	Number of Claims						*Time Until First Claim*					
Historical Border Density	0.01	0.10			−0.18***	−0.20**	−23.70	0.07			32.19	−0.19**
	(0.05)	(0.08)			(0.07)	(0.10)	(18.68)	(0.08)			(20.28)	(0.08)
Historical Border Variability			0.23***	0.36***	0.30***	0.47***			−76.82***	0.28***	−89.87***	0.37***
			(0.06)	(0.08)	(0.07)	(0.11)			(15.49)	(0.05)	(17.23)	(0.07)
Urban Population	0.02**	0.01	0.02**	0.01	0.02**	0.01	−2.54	0.01	−2.46	0.00	−2.40	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1.85)	(0.00)	(1.82)	(0.00)	(1.83)	(0.00)
River Length	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	−0.67***	0.00***	−0.56**	0.00***	−0.53**	0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.23)	(0.00)	(0.22)	(0.00)	(0.22)	(0.00)
Latitude	0.01*	−0.05***	0.01**	−0.03***	0.01***	−0.03***	1.46	−0.02**	0.33	−0.02**	0.17	−0.02**
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(1.37)	(0.01)	(1.30)	(0.01)	(1.31)	(0.01)
Longitude	0.00	0.02	0.01	0.00	0.00	−0.01	−1.43	0.02	−2.94	0.02	−2.83	0.02
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(2.52)	(0.02)	(2.61)	(0.02)	(2.58)	(0.02)
Agricultural Suitability	−0.06	−0.07	−0.08**	−0.08	−0.05	0.00	23.86**	−0.32***	26.28**	−0.27***	21.57**	−0.22***
	(0.04)	(0.07)	(0.04)	(0.07)	(0.04)	(0.07)	(11.09)	(0.09)	(10.35)	(0.09)	(10.56)	(0.09)
Terrain Ruggedness	0.00**	0.00	0.00**	0.00	0.00**	0.00	−0.60**	0.00***	−0.53**	0.00***	−0.55**	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.26)	(0.00)	(0.26)	(0.00)	(0.26)	(0.00)
Iron Production	−0.07	−0.15	−0.08	−0.19	−0.07	−0.15	−37.34	0.05	−33.65	0.06	−35.41	0.06
	(0.16)	(0.11)	(0.16)	(0.15)	(0.16)	(0.11)	(29.60)	(0.04)	(27.80)	(0.04)	(27.90)	(0.04)
Intercept	−0.05	−0.57	0.35	0.64	0.61	1.40	2205.04***		2053.16***		2006.04***	
	(0.60)	(1.17)	(0.60)	(1.26)	(0.59)	(1.23)	(173.97)		(172.22)		(169.15)	
N	466	466	466	466	466	466	466	466	466	466	466	466
Model: OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Cox	OLS	Cox	OLS	Cox	

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 8. This table gives results removing all borders of states that failed after 1815. Odd numbered columns are OLS. Columns 2,4, and 6 are negative binomial regressions. Columns 8, 10, 12 are Cox proportional hazards models. Heteroskedacticity robust standard errors in parentheses.

The Effect of Historical Borders Accounting for Units With Only One State

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
	Number of Claims						Time Until First Claim					
	Exclude Interior States			*Full Sample*			*Exclude Interior States*			*Full Sample*		
Historical Border Density	0.42		0.12	0.12		-0.39^{*}	-485.25^{***}		-424.51^{***}	-106.90^{***}		0.65
	(0.47)		(0.48)	(0.15)		(0.22)	(115.47)		(119.42)	(37.83)		(49.62)
Historical Border Variability		0.73^{**}	0.71^{**}		0.29^{***}	0.38^{***}		-220.06^{**}	-142.54^{**}		-79.80^{***}	-79.94^{***}
		(0.32)	(0.32)		(0.07)	(0.09)		(89.20)	(65.43)		(15.01)	(20.00)
# of States				0.06	0.05	0.06				-28.68^{***}	-28.38^{***}	-28.40^{***}
				(0.06)	(0.06)	(0.06)				(9.27)	(8.53)	(8.74)
Urban Population	0.03^{***}	0.03^{**}	0.03^{**}	0.02^{***}	0.02^{***}	0.02^{***}	-4.45^{*}	-1.94	-3.75	-4.09^{**}	-3.76^{**}	-3.76^{**}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(2.44)	(2.67)	(2.48)	(1.78)	(1.75)	(1.76)
River Length	0.00^{**}	0.00	0.00^{*}	0.00^{**}	0.00^{**}	0.00^{*}	-0.80^{***}	-0.30	-0.63^{**}	-0.58^{***}	-0.39^{*}	-0.39^{*}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.27)	(0.30)	(0.28)	(0.22)	(0.21)	(0.22)
Latitude	0.09^{***}	0.06^{***}	0.06^{***}	0.01^{**}	0.02^{***}	0.01^{*}	-16.45^{**}	-2.17	-11.07	-1.12	-0.53	-0.52
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(6.33)	(6.48)	(6.87)	(1.42)	(1.29)	(1.42)
Longitude	-0.03	-0.04	-0.04	0.00	0.01	0.01	1.44	6.15	2.75	-1.52	-2.53	-2.52
	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)	(10.46)	(12.58)	(12.51)	(2.48)	(2.58)	(2.58)
Iron Production	-0.05	-0.09	-0.10	-0.14	-0.14	-0.14	-16.74	-28.24	-6.18	-0.69	-1.46	-1.46
	(0.18)	(0.20)	(0.19)	(0.14)	(0.14)	(0.14)	(27.33)	(32.99)	(28.92)	(26.85)	(26.03)	(26.09)
Agricultural Suitability	0.22	0.30	0.29	-0.07	-0.07^{*}	-0.03	-14.19	-56.27	-27.04	29.84^{**}	22.32^{**}	22.26^{*}
	(0.20)	(0.26)	(0.26)	(0.04)	(0.04)	(0.04)	(57.79)	(68.10)	(72.59)	(11.97)	(10.19)	(11.88)
Terrain Ruggedness	0.00	0.00	0.00	0.00^{***}	0.00^{***}	0.00^{***}	-0.19	-0.03	-0.13	-0.66^{**}	-0.64^{**}	-0.64^{**}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.84)	(0.91)	(0.97)	(0.26)	(0.25)	(0.25)
Intercept	-1.11	1.39	1.08	-0.39	0.27	1.13^{*}	3068.36^{***}	1552.43^{**}	2626.11^{***}	2449.36^{***}	2130.21^{***}	2128.78^{***}
	(1.70)	(2.10)	(2.12)	(0.60)	(0.60)	(0.68)	(470.23)	(626.50)	(568.67)	(160.84)	(170.64)	(157.66)
N	174	174	174	466	466	466	174	174	174	466	466	466

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 9. This table gives results excluding from the analysis all grid-units with only one state and then controlling for the number of states within a grid. The first six columns treat as the dependent variable the number of claims on a unit. Columns seven through twelve treat the time until the first claim as the dependent variable. Heteroskedacticity robust standard errors in parentheses.

The Effect of Historical Borders: Eastern Vs. Western Europe

	1.	2.	3.	4.	5.	6.	7.	8.
	Number of Claims				*Time Until First Claim*			
Historical Border Density	0.03	0.49**			−95.20**	0.32		
	(0.11)	(0.21)			(45.75)	(0.19)		
Historical Border Density × West Europe	0.57***	0.05			−170.55**	0.55**		
	(0.21)	(0.31)			(67.68)	(0.24)		
Historical Border Variability			0.15***	0.35***			−52.58***	0.30***
			(0.04)	(0.11)			(17.17)	(0.10)
Historical Border Variability× West Europe			0.22**	0.44***			−123.01***	0.34**
			(0.09)	(0.17)			(32.68)	(0.15)
West Europe	−2.58***	−1.98**	−1.06**	−0.99	776.42***	−2.91***	195.86*	−1.63***
	(0.55)	(1.00)	(0.42)	(0.62)	(152.58)	(0.60)	(110.10)	(0.41)
Urban Population	0.01***	0.01**	0.01***	0.01**	−3.92***	0.01	−3.26***	0.01
	(0.00)	(0.01)	(0.00)	(0.01)	(1.26)	(0.00)	(1.21)	(0.00)
River Length	0.00***	0.00***	0.00***	0.00***	−0.84***	0.00***	−0.54***	0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.15)	(0.00)	(0.15)	(0.00)
Latitude	−0.02***	−0.06***	−0.02***	−0.06***	6.17***	−0.05***	8.56***	−0.06***
	(0.01)	(0.01)	(0.01)	(0.02)	(2.27)	(0.01)	(2.22)	(0.01)
Longitude	0.02***	0.07***	0.02***	0.04**	−5.22*	0.00	−5.37*	0.01
	(0.01)	(0.02)	(0.01)	(0.02)	(2.88)	(0.01)	(2.83)	(0.02)
Agricultural Suitability	0.09***	0.13*	0.08***	0.18**	−0.90	−0.16*	−7.79	−0.04
	(0.03)	(0.07)	(0.03)	(0.07)	(15.57)	(0.09)	(14.54)	(0.09)
Terrain Ruggedness	0.00	0.00	0.00	0.00	−0.28	0.00	−0.17	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.25)	(0.00)	(0.25)	(0.00)
Iron Production	0.08	−0.02	0.06	−0.04	−43.95***	0.11**	−35.83**	0.09*
	(0.10)	(0.06)	(0.10)	(0.06)	(16.64)	(0.05)	(16.50)	(0.05)
Model	OLS	Neg Bin	OLS	Neg Bin	OLS	Cox	OLS	Cox
N	466	466	466	466	466	466	466	466

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Table 10. This Table gives results interacting each of our measures of border density and variability with a dummy for Western Europe. Odd numbered columns give OLS estimates. Columns 2 and 4 are negative binomial estimates and 6 and 8 cox proportional hazards estimates. Heteroskedacticity robust standard errors in parentheses.

The Period by Period Effect of Historical Borders on the Number of Territorial Claims

	1.	2.	3.	4.	5.	6.	7.	8.
Historical Border Density$_{1650-1700}$	0.18***			0.00				
	(0.06)			(0.06)				
Historical Border Density$_{1700-1750}$		0.25**		0.15				
		(0.12)		(0.11)				
Historical Border Density$_{1750-1790}$			0.19***	0.15**				
			(0.05)	(0.06)				
Historical Border Variability$_{1650-1700}$					0.32***			0.18
					(0.08)			(0.16)
Historical Border Variability$_{1700-1750}$						0.20***		−0.05
						(0.07)		(0.12)
Historical Border Variability$_{1750-1790}$							0.38***	0.29*
							(0.09)	(0.16)
Intercept	−0.26	−0.52	−0.13	−0.40	0.88	0.31	1.29*	1.38*
	(0.59)	(0.59)	(0.60)	(0.61)	(0.67)	(0.59)	(0.72)	(0.77)
N	466	466	466	466	466	466	466	466

Table 11. OLS estimates of the changing relationship between border density, μ, and variability, σ and the total number of territorial claims made on a given piece of geography. μ_{t-t+1} indicates the mean for the period between Year t and $t+1$. σ_{t-t+1} is similarly defined. Robust standard errors in parentheses. In each model the full set of controls are included but not shown.

The Period by Period Effect of Historical Borders on the Time Until First Territorial Claim

	1.	2.	3.	4.	5.	6.	7.	8.
Historical Border Density$_{1650-1700}$	−87.60***			−26.96				
	(21.68)			(30.85)				
Historical Border Density$_{1700-1750}$		−149.14***		−111.38***				
		(39.51)		(37.41)				
Historical Border Density$_{1750-1790}$			−80.03***	−32.90				
			(18.22)	(25.26)				
Historical Border Variability$_{1650-1700}$					−92.27***			−31.81
					(0.08)			(29.34)
Historical Border Variability$_{1700-1750}$						−78.98***		−20.66
						(0.07)		(26.67)
Historical Border Variability$_{1750-1790}$							−114.45***	−76.52**
							(21.17)	(33.08)
Intercept	2296.32***	2469.89***	2223.82***	2447.00***	1923.88***	2044.94***	1781.75***	1788.15***
	(167.26)	(168.39)	(172.61)	(171.38)	(0.67)	(0.59)	(199.41)	(207.91)
N.	466	466	466	466	466	466	466	466

Table 12. OLS estimates of the changing relationship between border density, μ, and variability, σ and months until the first territorial claim on a given piece of geography. μ_{t-t+1} indicates the mean for the period between Year t and $t+1$. σ_{t-t+1} is similarly defined. Robust standard errors in parentheses.

Placebo Test: The (Non-)Effect of Pre-Westphalian Historical Border Precedents (1500-1650)

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
	Number of Claims						*Time Until First Claim*					
Historical Border Density	0.09	−0.02			0.15	0.12	−119.09***	0.42***			−148.86***	0.45***
	(0.13)	(0.20)			(0.15)	(0.22)	(37.14)	(0.13)			(46.66)	(0.14)
Historical Border Variability			−0.02	−0.09	−0.05	−0.10			−6.69	0.05	22.29	−0.03
			(0.05)	(0.07)	(0.06)	(0.08)			(10.16)	(0.07)	(13.88)	(0.07)
Urban Population	0.02**	0.01	0.02**	0.01	0.02**	0.01	−3.24*	0.01*	−2.58	0.01	−3.21*	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1.79)	(0.00)	(1.84)	(0.00)	(1.78)	(0.00)
River Length	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	−0.78***	0.00***	−0.66***	0.00***	−0.83***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.23)	(0.00)	(0.24)	(0.00)	(0.23)	(0.00)
Latitude	0.01**	−0.05***	0.01*	−0.05***	0.01**	−0.05***	−1.07	−0.01	1.22	−0.02**	−1.37	−0.01
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(1.45)	(0.01)	(1.36)	(0.01)	(1.51)	(0.01)
Longitude	0.00	0.01	0.00	0.02	0.00	0.02	−1.69	0.01	−1.03	0.02	−1.54	0.01
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(2.55)	(0.02)	(2.57)	(0.02)	(2.59)	(0.02)
Agricultural Suitability	−0.07*	−0.03	−0.06	−0.05	−0.07*	−0.07	32.53***	−0.34***	19.71*	−0.30***	35.85***	−0.34***
	(0.04)	(0.08)	(0.04)	(0.07)	(0.04)	(0.08)	(12.23)	(0.09)	(10.68)	(0.09)	(12.72)	(0.09)
Terrain Ruggedness	0.00**	0.00	0.00**	0.00	0.00***	0.00	−0.65**	0.00***	−0.64**	0.00***	−0.67**	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.27)	(0.00)	(0.27)	(0.00)	(0.28)	(0.00)
Iron Production	−0.07	−0.13	−0.06	−0.12	−0.07	−0.13	−30.89	0.05	−38.04	0.05	−31.98	0.05
	(0.16)	(0.10)	(0.16)	(0.10)	(0.16)	(0.10)	(28.17)	(0.05)	(29.79)	(0.04)	(27.77)	(0.05)
Intercept	−0.20	−0.25	−0.03	−0.45	−0.37	−0.92	2431.24***		2177.91***		2514.76***	
	(0.60)	(1.06)	(0.60)	(1.23)	(0.60)	(1.02)	(165.28)		(175.18)		(169.35)	
Model	OLS	Neg Bin	OLS	Neg Bin	OLS	Neg Bin	OLS	Cox	OLS	Cox	OLS	Cox
N	463	463	463	463	463	463	463	463	463	463	463	463

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 13. This table gives results using historical borders for the 150 years prior to our original sample. The First two columns are a negative binomial regression of the number of claims on our variability and density measures. The next two regress the total number of years on these measures. The last two are a cox-proportional hazards model of time duration on these measures.

The Effect of Historical Borders Conditional on Ethnicity

Outcome:	Number of Claims		Duration Until Claim	
Historical Border Density	0.29^{*}		-172.75^{***}	
	(0.17)		(47.83)	
Historical Border Variability		0.31^{***}		-88.55^{***}
		(0.07)		(15.95)
Ethnic Number	0.00^{***}	0.00^{***}	-0.51^{***}	-0.35^{**}
	(0.00)	(0.00)	(0.15)	(0.14)
Urban Population	0.03^{***}	0.03^{***}	-3.98^{*}	-3.56^{*}
	(0.01)	(0.01)	(2.02)	(2.02)
River Density	0.00^{***}	0.00^{**}	-0.73^{***}	-0.49^{**}
	(0.00)	(0.00)	(0.24)	(0.24)
Latitude	0.00	0.00	1.32	2.22
	(0.01)	(0.01)	(1.81)	(1.60)
Longitude	0.03^{**}	0.03^{***}	-10.25^{***}	-10.33^{***}
	(0.01)	(0.01)	(3.34)	(3.32)
Iron Production	−0.10	−0.11	−15.66	−16.20
	(0.17)	(0.17)	(28.52)	(29.68)
Agricultural Suitabilit	0.02	0.02	11.27	0.93
	(0.05)	(0.04)	(13.83)	(11.48)
Terrain Ruggedness	0.00^{**}	0.00^{**}	-0.92^{***}	-0.91^{***}
	(0.00)	(0.00)	(0.32)	(0.31)
Intercept	-2.21^{***}	−1.11	2997.68^{***}	2503.25^{***}
	(0.77)	(0.78)	(208.66)	(204.49)
N	420	420	420	420

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 14. This table gives the effect of historical borders treating ethnicity as a pre-treatment confounder. We operationalize ethnicity as the number of ethnic groups on a given unit Columns 1-2 regress our measures of border density and variability on the total number of claims, 3-4 on the number of years with an unresolved claim, and 5-6 on time until the first claim. All standard errors robust to arbitrary heteroskedasticity.

The Effect of Historical Borders: Ethnicity as a Mediator

Outcome:	Number of Claims		Duration Until Claim	
Total Effect *Historical Border Density*	0.296	.	-173.096	.
	(0.086)	.	(0.00)	.
Direct Effect *Historical Border Density*	0.289	.	-171.84	.
	(0.085)	.	(0.00)	.
Total Effect *Historical Border Variability*	.	0.343	.	-93.858
		(0.00)	.	(0.00)
Direct Effect *Historical Border Variability*	.	0.308	.	-88.376
	.	(0.00)	.	(0.00)
Mediating Effect Ethnic Number	0.007	0.035	-1.256	-5.481
	(0.814)	(0.00)	(0.858)	(0.044)

Table 15. This table gives the effect of historical borders treating ethnicity as a post-treatment mediator. We operationalize ethnicity as the number of ethnic groups on a given unit Columns 1-2 regress our measures of border density and variability on the total number of claims, 3-4 on the number of years with an unresolved claim, and 5-6 on time until the first claim. p-values produced by quasi-Bayesian simulation. Controls for Urban Population, River Length, Lat/Long, Iron Production, Agricultural Suitability, and Terrain Ruggedness are included but not show.

The Effect of Historical Borders on Total Time w/ an Unresolved Dispute

	1.	2.	3.	4.	5.	6.
Historical Border Density	28.57**		−3.50	30.64		3.81
	(13.26)		(14.81)	(22.11)		(31.05)
Historical Border Variability		35.43***	36.49***		20.74***	19.85*
		(7.46)	(8.47)		(6.38)	(10.25)
Urban Population				2.80**	2.70**	2.72**
				(1.20)	(1.25)	(1.19)
River Length				0.16*	0.11	0.12
				(0.09)	(0.09)	(0.10)
Latitude				0.92	0.71	0.77
				(0.83)	(0.93)	(0.83)
Longitude				−1.01	−0.76	−0.76
				(0.90)	(0.93)	(0.93)
Iron Production				6.90	7.18	7.02
				(19.92)	(20.36)	(20.07)
Agricultural Suitability				−5.61	−3.35	−3.71
				(4.28)	(3.04)	(4.60)
Terrain Ruggedness				0.38**	0.38**	0.38**
				(0.18)	(0.17)	(0.17)
Intercept	46.95**	178.72***	186.18***	−9.13	79.27	70.59
	(18.62)	(30.50)	(42.66)	(67.23)	(84.52)	(77.99)
N	466	466	466	466	466	466

Table 16. OLS estimates of the effect of border density and variability on the total number of months a given grid-square had an unresolved territorial dispute. Robust standard errors in parentheses.

The Effect of Historical Borders on the Timing of the First Territorial Claim by Precedent

	First Claim that Follows Precedent						*First Claim that does not Follow Precedent*					
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Historical Border Density$_i$	−109.55***	0.47***			19.62	−0.06	−102.36***	0.57***			−120.46***	0.07
	(35.14)	(0.13)			(46.60)	(0.17)	(35.14)	(0.13)			(46.60)	(0.17)
Historical Border Variability$_i$			−90.99***	0.43***	−95.56***	0.45***			−14.69*	0.44***	13.39	0.41***
			(14.73)	(0.07)	(19.49)	(0.09)			(8.03)	(0.07)	(10.67)	(0.09)
Urban Population	−3.73**	0.01**	−3.43**	0.01**	−3.33*	0.01*	1.58*	0.01*	2.12**	0.01	1.52	0.01
	(1.74)	(0.00)	(1.70)	(0.00)	(1.72)	(0.00)	(0.95)	(0.00)	(1.04)	(0.00)	(0.94)	(0.00)
River Length	−0.68***	0.00***	−0.47**	0.00	−0.44**	0.00	−0.27*	0.00***	−0.16	0.00**	−0.31*	0.00**
	(0.22)	(0.00)	(0.21)	(0.00)	(0.22)	(0.00)	(0.16)	(0.00)	(0.15)	(0.00)	(0.17)	(0.00)
Latitude	−1.75	−0.01	−1.35	−0.02**	−1.03	−0.02**	−0.14	−0.01	1.74*	−0.02**	−0.24	−0.02**
	(1.37)	(0.01)	(1.17)	(0.01)	(1.37)	(0.01)	(1.01)	(0.01)	(0.96)	(0.01)	(1.02)	(0.01)
Longitude	−3.40	0.01	−4.60*	0.02	−4.60*	0.02	1.46	0.01	1.66	0.02	1.63	0.02
	(2.30)	(0.02)	(2.38)	(0.02)	(2.38)	(0.02)	(1.56)	(0.02)	(1.62)	(0.02)	(1.64)	(0.02)
Iron Production	−19.10	0.02	−18.89	0.02	−19.69	0.02	−43.70*	0.05	−48.49*	0.03	−43.62*	0.03
	(28.45)	(0.05)	(27.89)	(0.05)	(28.17)	(0.05)	(24.71)	(0.05)	(26.78)	(0.05)	(24.54)	(0.05)
Agricultural Suitability	30.10***	−0.34***	22.80**	−0.25***	20.97*	−0.24***	11.01*	−0.35***	1.07	−0.25***	12.29*	−0.26***
	(11.61)	(0.09)	(9.97)	(0.08)	(11.35)	(0.09)	(5.98)	(0.09)	(4.96)	(0.08)	(6.46)	(0.09)
Terrain Ruggedness	−0.51**	0.00***	−0.49*	0.00**	−0.49*	0.00**	0.06	0.00***	0.04	0.00**	0.05	0.00**
	(0.26)	(0.00)	(0.25)	(0.00)	(0.25)	(0.00)	(0.17)	(0.00)	(0.17)	(0.00)	(0.17)	(0.00)
Intercept	2542.64***		2203.54***		2158.85***		2333.70***		2113.06***		2387.47***	
	(152.41)		(152.55)		(153.06)		(118.03)		(126.29)		(111.64)	
N	466	466	466	466	466	466	466	466	466	466	466	466
Model	OLS	Cox	OLS	Cox	OLS	Cox	OLS	Cox	OLS	Cox	OLS	Cox

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Robust standard errors in parentheses.

Table 17

SUR Estimates of Historical Border Effects on Claims with Precedent/No Precedent

	1.	2.	3.	4.	5.	6.
	Number of Claims Based on Precedent			*Time Until First Claim Based on Precedent*		
Historical Border Density	0.10		-0.38	-109.50**		10.12
	(0.14)		(0.20)	(34.85)		(44.17)
Historical Border Variability		0.21***	0.27***		-63.74***	-65.35***
		(0.04)	(0.06)		(9.50)	(13.02)
Urban Population	0.03**	0.03**	0.03**	-3.73*	-3.31	-3.26
	(0.01)	(0.01)	(0.01)	(1.73)	(1.71)	(1.76)
River Length	0.00**	0.00**	0.00*	-0.68***	-0.49*	-0.48*
	(0.00)	(0.00)	(0.00)	(0.20)	(0.19)	(0.22)
Latitude	0.02**	0.02***	0.02**	-1.76	-2.03	-1.88
	(0.01)	(0.01)	(0.01)	(1.37)	(1.16)	(1.31)
Longitude	0.00	0.01	0.01	-3.40	-4.62*	-4.62
	(0.01)	(0.01)	(0.01)	(2.28)	(2.35)	(2.37)
Iron Production	-0.15	-0.16	-0.14	-19.09	-21.30	-21.78
	(0.11)	(0.12)	(0.11)	(26.41)	(26.25)	(25.62)
Agricultural Suitability	-0.06	-0.08*	-0.05	30.12**	27.63**	26.80*
	(0.04)	(0.04)	(0.04)	(11.08)	(9.93)	(11.14)
Terrain Ruggedness	0.00**	0.00**	0.00**	-0.51*	-0.50*	-0.51*
	(0.00)	(0.00)	(0.00)	(0.25)	(0.22)	(0.24)
Intercept	-0.54	-0.01	0.83	2542.33***	2227.34***	2205.00***
	(0.58)	(0.56)	(0.65)	(147.02)	(146.91)	(148.82)
	Number of Claims Not Based on Precedent			*Time Until First Claim Not Based on Precedent*		
Historical Border Density	0.10**		0.11**	-102.23***		-117.13**
	(0.03)		(0.04)	(30.39)		(39.29)
Historical Border Variability		0.01*	-0.00		-10.43*	8.14
		(0.01)	(0.01)		(7.18)	(39.29)
Urban Population	-0.00	-0.00*	-0.00	1.58	2.14*	1.52
	(0.00)	(0.00)	(0.00)	(0.89)	(1.01)	(0.87)
River Length	0.00	0.00	0.00	-0.27	-0.17	-0.30
	(0.00)	(0.00)	(0.00)	(0.15)	(0.15)	(0.15)
Latitude	0.00	-0.00	0.00	-0.14	1.62	-0.13
	(0.00)	(0.00)	(0.00)	(0.96)	(0.90)	(0.97)
Longitude	-0.00	-0.00	-0.00	1.46	1.65	1.61
	(0.00)	(0.00)	(0.00)	(1.52)	(1.61)	(1.70)
Iron Production	0.06	0.07	0.06	-43.70	-48.87*	-43.36
	(0.04)	(0.04)	(0.04)	(23.06)	(24.65)	(22.73)
Agricultural Suitability	-0.01*	-0.00	-0.01*	11.02	1.87	11.43
	(0.00)	(0.00)	(0.00)	(5.87)	(5.32)	(6.06)
Terrain Ruggedness	-0.00	-0.00	-0.00	0.06	0.04	0.05
	(0.00)	(0.00)	(0.00)	(0.16)	(0.16)	(0.15)
Intercept	0.04	0.26*	0.02	2333.23***	2116.69***	2375.28***
	(0.09)	(0.11)	(0.09)	(112.42)	(122.43)	(108.79)

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 18. This table gives seemingly unrelated regression estimates for the outcomes separating claims that have historical basis in precedent from those that do not. Bootstrap standard errors in parentheses.

Non-Parametric Estimation of the Effect of Historical Border Variability	
Outcome:	Number of Claims
Historical Border Density	0.14
	(0.17)
Urban Population	0.01
	(0.01)
River Density	0.003^{***}
	(0.001)
Latitude	-0.02^{*}
	(0.01)
Longitude	-0.03
	(0.02)
Iron Production	-0.11
	(0.08)
Agricultural Suitability	0.04
	(0.10)
Terrain Ruggedness	0.0016^{**}
	(0.0015)
Intercept	-0.80
	(1.67)
Approximate Significance of Smooth Terms	
Historical Border Variability	p-value$< 0.000027^{***}$
N	466

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 19

Approximate Latitude and Longitude Fixed Effects with Splines	
Outcome:	Number of Claims
Historical Border Density	0.16
	(0.20)
Historical Border Variability	0.40^{***}
	(0.11)
Urban Population	0.009^{*}
	(0.005)
River Density	0.001^{**}
	(0.0006)
Iron Production	0.03
	(0.06)
Agricultural Suitability	0.53^{***}
	(0.16)
Terrain Ruggedness	-0.004^{**}
	(0.002)
Intercept	-7.34^{***}
	(2.51)
Approximate Significance of Smooth Terms	
Latitude	p-value$< 2e-16^{***}$
Longitude	p-value$< 1.69e-7^{***}$
N	466

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 20

Average Border Duration, Average Precedent Age and the Emergence of Claims - Negative Binomial Specification

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Avg. Border Duration	-0.09***	-0.05**					-0.11***	-0.03		
	(0.02)	(0.02)					(0.02)	(0.03)		
Avg. Time Since Last Change			-0.01	-0.02*					0.02*	0.03**
			(0.01)	(0.01)					(0.01)	(0.01)
Border Changes					0.16**	0.18***	-0.11	0.05	0.41***	0.44***
					(0.05)	(0.04)	(0.07)	(0.08)	(0.09)	(0.06)
Avg. Border Duration × *Border Changes*							0.03***	0.02*		
							(0.01)	(0.01)		
Avg. Time Since Last Change × *Border Changes*									-0.00***	-0.00***
									(0.00)	(0.00)
Urban Population		0.01		0.01*		0.01		0.01		0.01
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
River Length		0.00***		0.00**		0.00**		0.00**		0.00*
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Latitude		-0.03*		-0.05***		-0.03*		-0.03		-0.04**
		(0.01)		(0.01)		(0.01)		(0.02)		(0.01)
Longitude		0.02		0.02		-0.02		-0.02		-0.02
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Agricutural Suitability		-0.12		-0.04		-0.01		0.08		-0.00
		(0.10)		(0.08)		(0.08)		(0.09)		(0.08)
Terrain Ruggedness		0.00		0.00		0.00		0.00		0.00
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Iron Production		-0.14*		-0.14*		-0.15*		-0.12		-0.13
		(0.06)		(0.06)		(0.06)		(0.06)		(0.07)
Historical Border Variability		0.12		0.01		-0.23		-0.19		-0.42
		(0.30)		(0.28)		(0.28)		(0.30)		(0.27)
Intercept	1.21***	-0.03	0.52	0.36	-1.57**	-0.81	-1.20	-1.68	-3.40***	-1.90
	(0.26)	(1.53)	(0.32)	(1.57)	(0.55)	(1.65)	(0.90)	(1.95)	(0.94)	(1.75)

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

This table reproduces Table 4. from the main text. Robust standard errors in parentheses.

Table 21

Average Border Duration, Average Precedent Age and the Timing of Claims

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Avg. Border Duration	9.709***		1.474			1.409		4.169		
	(1.453)		(3.469)			(1.789)		(2.920)		
Avg. Time Since Last Change				3.531**	−5.345***				4.183*	−3.918
				(1.576)	(2.047)				(2.136)	(3.557)
Border Changes		−46.899***	−26.997		−99.394***		−24.805***	10.364		−67.717***
		(5.938)	(17.720)		(15.683)		(7.435)	(17.740)		(21.040)
Avg. Border Duration × Border Changes			−3.853**					−4.292***		
			(1.508)					(1.482)		
Avg. Time Since Last Change × Border Changes					0.972***					0.885**
					(0.273)					(0.374)
Historical Border Density						−190.960***	−106.101**	−144.750**	−152.284***	−102.708**
						(49.070)	(45.412)	(57.535)	(40.446)	(44.758)
Urban Population						−3.107*	−3.114*	−2.637	−3.442*	−2.896
						(1.777)	(1.818)	(1.752)	(1.797)	(1.794)
River Length						−0.825***	−0.712***	−0.734***	−0.737***	−0.631***
						(0.223)	(0.221)	(0.218)	(0.224)	(0.221)
Latitude						−2.981*	−2.930*	−3.170**	−0.477	−1.103
						(1.572)	(1.497)	(1.548)	(1.773)	(1.820)
Longitude						−1.828	−1.264	−3.338	−2.632	−1.267
						(2.912)	(2.398)	(2.592)	(2.659)	(2.508)
Agricultural Suitability						41.093***	36.923***	24.503*	35.835***	34.380***
						(13.080)	(11.669)	(13.175)	(11.928)	(11.787)
Terrain Ruggedness						−0.762**	−0.689***	−0.758**	−0.595**	−0.565**
						(0.298)	(0.261)	(0.299)	(0.260)	(0.265)
Iron Production						−27.979	−25.725	−30.899	−26.745	−23.990
						(26.453)	(26.720)	(26.775)	(26.707)	(25.960)
Historical Border Density						−190.960***	−106.101**	−144.750**	−152.284***	−102.708**
						(49.070)	(45.412)	(57.535)	(40.446)	(44.758)
Intercept	1913.822***	2465.266***	2685.355***	1895.834***	2783.864***	2534.157***	2620.071***	2936.745***	2252.844***	2737.179***
	(49.633)	(40.005)	(98.398)	(92.391)	(120.238)	(173.529)	(179.902)	(208.433)	(192.966)	(271.328)

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

This table reproduces Table 4. treating the dependent variable as the number of months until the first claim made. Robust standard errors in parentheses.

Table 22

Average Border Duration, Average Precedent Age and the Timing of Claims - Cox Models

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Avg. Border Duration	-0.054^{***}		-0.039			-0.025		-0.052		
	(0.015)		(0.035)			(0.019)		(0.041)		
Avg. Time Since Last Change				-0.020^{***}	0.024^{***}				-0.020^{***}	0.017^{*}
				(0.007)	(0.009)				(0.006)	(0.010)
Border Changes		0.221^{***}	0.102		0.378^{***}		0.133^{***}	-0.023		0.276^{***}
		(0.028)	(0.064)		(0.055)		(0.033)	(0.074)		(0.065)
Avg. Border Duration × Border Changes			0.012^{***}					0.015^{***}		
			(0.004)					(0.005)		
Avg. Border Duration × Border Changes					-0.003^{***}					-0.003^{***}
					(0.001)					(0.001)
Urban Population						0.008^{*}	0.007	0.007	0.009^{*}	0.006
						(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
River Length						0.002^{***}	0.001^{***}	0.002^{***}	0.001^{***}	0.001^{**}
						(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0005)
Latitude						-0.001	-0.006	-0.005	-0.015^{**}	-0.016^{*}
						(0.008)	(0.008)	(0.009)	(0.008)	(0.009)
Longitude						0.010	0.003	0.016	0.015	0.0001
						(0.016)	(0.016)	(0.017)	(0.016)	(0.016)
Agricultural Suitability						-0.383^{***}	-0.300^{***}	-0.260^{***}	-0.340^{***}	-0.296^{***}
						(0.090)	(0.088)	(0.096)	(0.088)	(0.089)
Terrain Ruggedness						0.004^{***}	0.004^{***}	0.004^{***}	0.004^{***}	0.003^{***}
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Iron Production						0.045	0.016	0.039	0.035	-0.012
						(0.048)	(0.050)	(0.050)	(0.049)	(0.054)
Historical Border Density						0.627^{***}	0.308^{**}	0.403^{**}	0.546^{***}	0.302^{*}
						(0.143)	(0.149)	(0.166)	(0.132)	(0.156)

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 23. This table reproduces Table 4. treating the dependent variable as the number of months until the first claim made. Robust standard errors in parentheses.

Alternative Measures of Historical Borders

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
			Number of Claims						*Time Until First Claim*			
% Annual Border Change	-2.78^{***}			-2.06^{***}			733.54^{***}			422.75^{***}		
	(0.42)			(0.49)			(92.33)			(105.01)		
Cumulative Stability		-0.13^{***}			-0.08^{***}			44.09^{***}			16.83^{**}	
		(0.02)			(0.03)			(0.03)			(7.48)	
Time Since Last Change			-0.35^{***}			-0.29^{***}			95.62^{***}			63.57^{***}
			(0.05)			(0.05)			(10.07)			(11.44)
Historical Border Density				0.03	0.09	-0.06				-117.67^{***}	-147.59^{***}	-113.85^{**}
				(0.16)	(0.18)	(0.15)				(44.11)	(55.31)	(45.94)
Urban Population				0.02^{**}	0.02^{**}	0.02^{**}				-2.96^{*}	-3.15^{*}	-2.80
				(0.01)	(0.01)	(0.01)				(1.69)	(1.78)	(1.73)
River Length				0.00^{**}	0.00^{***}	0.00^{**}				-0.55^{**}	-0.75^{***}	-0.57^{***}
				(0.00)	(0.00)	(0.00)				(0.22)	(0.23)	(0.21)
Latitude				0.01^{**}	0.02^{***}	0.01^{*}				-1.55	-3.57^{**}	-1.68
				(0.01)	(0.01)	(0.01)				(1.48)	(1.57)	(1.55)
Longitude				0.00	0.00	0.00				-1.68	-0.24	-2.18
				(0.01)	(0.01)	(0.01)				(2.50)	(2.72)	(2.82)
Agricultural Suitability				0.00	-0.10^{**}	-0.01				20.16	41.11^{***}	21.38^{*}
				(0.04)	(0.04)	(0.04)				(12.50)	(12.22)	(12.10)
Terrain Ruggedness				0.00^{**}	0.00^{***}	0.00^{**}				-0.46^{*}	-0.76^{**}	-0.68^{**}
				(0.00)	(0.00)	(0.00)				(0.25)	(0.30)	(0.29)
Iron Production				-0.10	-0.09	-0.15				-25.43	-25.75	-12.37
				(0.16)	(0.16)	(0.16)				(26.44)	(26.34)	(25.19)
Intercept	3.03^{***}	3.38^{***}	3.51^{***}	1.24^{*}	1.42^{*}	1.82^{**}	1531.30^{***}	1243.88^{***}	1386.80^{***}	2132.78^{***}	2108.04^{***}	2014.92^{***}
	(0.40)	(0.45)	(0.41)	(0.70)	(0.82)	(0.76)	(86.13)	(0.82)	(86.06)	(166.59)	(210.85)	(181.28)
N	466	466	466	466	466	466	466	466	466	466	466	466

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 24. This table produces our main results with three alternative measures of historical border variability.