What's in a Line?

Natural Experiments and the Line of Demarcation in WWII Occupied France[1]

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SSRN first draft: January 26, 2015
This draft: July 31, 2015

Abstract: In "Political Devolution and Resistance to Foreign Rule," Ferwerda and Miller (FM) use a natural experiment during WWII France to argue that devolution of authority to local elites mitigates resistance to foreign rule. We dispute FM's claims on four levels. First, the Line of Demarcation dividing France was delineated with the goal of keeping strategic railways under direct German control, invalidating FM's natural experiment research design. Second, the higher level of resistance they observe in directly occupied France results from the Resistance's efforts to target these strategic railways. Third, FM's argument is not supported by the overall pattern of resistance in metropolitan France between 1940-44. Finally, FM's data is unsuitable for testing theories connecting the location of an attack with its perpetrators' precise geographic origins. These problems lead us to argue for the epistemic priority of treatment-assignment causal process observations over balance checks on pretreatment covariates when validating natural experiments.

[1] For research assistance, we thank Bernard Bèzes, Benjamin Billingsley, Gabriel Botelho, Austin Carder, Fabiola Davila, Ajua Duker, Sarah Holder, Cassidy Lapp, Aube Rey Lescure, Sona Lim, Nils Metter, Usha Rungoo, Lyndon Sam, Angelina Xing, and, especially, Michael Repas. For comments and suggestions, we thank Kate Baldwin, David Collier, Allan Dafoe, Samuel DeCanio, Thad Dunning, Francesca Grandi, Greg Huber, Sigrun Kahl, Stathis Kalyvas, Audrey Latura, Adria Lawrence, Luis Schiumerini, and the participants in the Program on Order, Conflict, and Violence at Yale.

Note to the reader

On July 8, 2015, Ferwerda and Miller replied to our rejoinder by posting "Rail Lines and Demarcation Lines: A Response" ((http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2628508). We include our assessment of their response as Appendix C at the end of this document.

New Haven, July 31, 2015

In "Political Devolution and Resistance to Foreign Rule: A Natural Experiment," Jeremy Ferwerda and Nicholas L. Miller (FM) examine the determinants of resistance to foreign occupation by focusing on the case of World War II France. The Franco-German armistice of 1940 provided for the partial occupation of metropolitan France, alongside a reconfigured French state ruled from Vichy. A Line of Demarcation (LoD) divided the two. FM argue that this LoD was placed in a locally arbitrary way, creating a natural experiment in which each commune (France's smallest administrative unit) in proximity of the LoD was as-if randomly assigned to conditions of direct (occupied) or indirect (Vichy) rule. Using data from four of thirteen French departments intersected by the LoD in a regression discontinuity design (RDD), FM find that communes within 5-, 10-, and 20-kilometer bandwidths inside the occupied zone exhibited higher levels of violent resistance than communes within those same bandwidths on the Vichy side. FM argue that this difference resulted from Germany's cooptation of the right-wing elites that dominated Vichy, while in the occupied zone French citizens generally opposed direct German rule (644). Consistent with this conjecture, FM find that resistance was associated with political partisanship only in Vichy (655).

We show that FM's claims are empirically unfounded. The differences in resistance levels FM find result from disparities in the availability of strategically valuable targets on either side of the LoD, not from differences in occupation regimes. We present our evidence in four stages. First, we use historical documents and maps, as well as difference-of-means tests, to show that the LoD was not locally placed as-if randomly. In fact, German leaders explicitly defined its local placement so as to keep strategic railways in the directly occupied zone. Second, we reconstruct FM's dataset from the same sources they use and show that, conditional on the location of strategic railroads, there are no statistically significant differences in the magnitude of violence between Vichy and the occupied zone within the departments FM analyzed. Third, we introduce a new department-level dataset of resistance events to test FM's argument for the entirety of metropolitan France. We find that railroad sabotage – the main form of violent resistance in wartime France – was much more common in Vichy than in the occupied zone, the opposite of what FM anticipate. Furthermore, while we find (like FM) that left-right political cleavages were strongly associated with violent resistance, we find no evidence of this association being stronger in the occupied zone. Fourth and finally, we turn to the chronology of FM's data and local histories of the Resistance in the departments FM studied in order to establish that FM's data are unsuitable for testing theories that infer the geographic origins and motivations of the perpetrators of anti-German attacks from their location. A majority (59%) of anti-German attacks in these departments took place after the D-Day Allied landings, in a context of conventional war, not stable political order. Furthermore, during this period, target selection for Resistance attacks was coordinated by the Allies as part of the overall effort to liberate France.

We conclude with recommendations for strengthening the scholarly criteria for validating natural experiments. We argue that treatment-assignment causal process observations (Dunning 2012: 212-218) have epistemic priority over balance checks on pretreatment covariates. Without qualitative evidence on the process of assignment to treatment, researchers cannot identify the relevant covariates for which balance should be checked. As Brady and Collier (2010) put it, good qualitative evidence is an essential component of successful quantitative analysis and, therefore, of the quest to improve our standards of causal inference.

The Line of Demarcation: a natural experiment?

The key identifying assumption of FM's research design is that French communes in close proximity to the LoD were "assigned on a quasi-random basis to the Vichy or German zones" (642). Given the centrality of this assumption to their study, it is important to scrutinize the argument FM present in its support. They

acknowledge that Germany had strategic objectives in the overall design of the LoD, "most notably, seizing the Atlantic coast and capturing large provincial capitals" (647). Yet, FM maintain, the LoD "followed an arbitrary course at the local level, cutting across preexisting administrative borders and splitting departments (provinces), cantons (counties), and communes (municipalities)" (645), adding that the "motivation behind the precise location of the line remains opaque" (*Idem*, fn. 9). FM's article includes no additional historical evidence on the local placement of the LoD.

There is, however, overwhelming evidence that the LoD was not as-if randomly placed at the local level. A combination of documentary and cartographic evidence shows that the German rationale for the specific placement of the LoD was to keep major double-track railroad lines running from the German border to the Atlantic coast, and along the Atlantic coast to the Spanish border, under their direct control. This policy was formulated by Adolf Hitler himself, formalized in the Franco-German armistice, and implemented on the ground by the German occupation authorities with remarkable faithfulness.

The proposed path of the LoD was discussed on 17 June 1940 in a meeting attended personally by Adolf Hitler and two of his top military commanders, Generals Alfred Jodl and Wilhelm Keitel.[2] The location of strategic railways was a central concern during discussions at this planning meeting:

> The envisioned demarcation line between occupied and unoccupied territory was drawn on a map by General Jodl. In the course of doing so, attention was paid to ensuring that the East-West connection through central France that went from Belfort through Dôle-Le Creusot-Moulins-Bourges-Tours to Nantes, and the North-South connection from Tours through Angoulême-Bordeaux to the Spanish border, would run within the territory to be occupied. (Böhme 1966: 21)[3]

In the same meeting, Hitler explained that the boundary of the French "sovereign domain" would be determined by the military necessities of the continuing war with Britain (Böhme 1966: 20-21). Direct control over the major railroad lines running through *Mittelfrankreich* from Germany to the Atlantic coast and Spain gave the Wehrmacht the mobility to take the fight to Britain and, if necessary, meet emerging seaborne threats.

Indicative of the strategic importance of controlling these particular French railroads, the topic percolated through other meetings Hitler had around this time. The intention to maintain direct control over a railway link to the Spanish border was discussed explicitly between Adolf Hitler and Benito Mussolini during their meeting in Munich the following day, June 18. Specifically, "[t]he Führer proceeded to discuss in detail the conditions of an armistice. With the aid of a map, he indicated the form the occupation of French territory should take. … In the country's interior, the occupation would be designed in such a way that under all circumstances the railway leading to Spain via Irun is located completely inside the occupied zone"

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[2] Jodl was Chief of the Operations Staff of the Armed Forces High Command (*Oberkommando der Wehrmacht*, or OKW). Keitel was a member of the German delegation to the Armistice negotiations and signed the instrument for Germany.

[3] Our translation. The author of the report is General Hermann Böhme, who was, in June 1940, a top staff officer in the OKW bureau that did the preparatory work for this meeting. Planning for the LoD – which mapped France "perfectly in its smallest details with its police stations, its industrial bakeries, its steel mills, etc." – began prior to the invasion (Alary 2007: 7).

(Germany, Auswärtiges Amt 1961: 334).[4] Hitler had also insisted on his determination to maintain a railway route to Spain in a meeting with the Spanish General Vigon on 16 June 1940, two days before his conversation with Mussolini.[5] Vigon had conveyed to Hitler General Franco's concern that a prolonged war could lead to the invasion of either Portugal or Morocco by American forces. Hitler reassured Vigon that Germany would use this railway route through occupied France to act against any such invasion.

Implementing Hitler's directives, Article II of the Armistice specifies that "French state territory north and west of the line drawn on the attached map will be occupied by German troops" (Am. J Intl. Law 1940: 173). The original text of the Armistice, including this map, was retained by Hitler and subsequently lost. However, an Associated Press wire originating in Berlin and containing the text of the Armistice was widely published in international newspapers on June 26 and June 27, 1940.[6] In lieu of a map, this report included a note on the LoD's placement that read:

> The line mentioned in Article II of the armistice agreement begins in the east on the French-Swiss border at Geneva and runs thence nearly over the villages of Dole, Paray, Le Monial [sic] and Bourges to approximately twenty kilometers (about twelve miles) east of Tours. From there it goes at a distance of twenty kilometers east of the Tours-Angouleme-Liborune [sic] railway line and extends through Mont de Marsan and Orthez to the Spanish border (New York Times, June 26th 1940).[7]

This note, published prior to the implementation of the LoD, explicitly situates it relative to the position of the Paris-Bordeaux railway (which ran through Tours, Angoulême, and Libourne), while Dôle, Paray-le-Monial, Bourges, and Tours all lay along the double-track line connecting Tours with Germany that ended up just inside the German-occupied zone (see Map 1).

--- Map 1 ---

The German rationale for the local position of the line makes eminent strategic sense. Double-track railroads were critical for WWII combatants, because they permit more than twice the throughput of single-track lines (van Creveld 1977: 158). The Wehrmacht relied particularly heavily on rail transportation due to a well-documented shortage of trucks during WWII (van Creveld 1977: 142 – 147; Overy 1973). As one historian of the occupation of France put it, "[t]he primary means of locomotion for the German army is its use of railways" (Veyret 2001: 24). As a sovereign neutral, the planned French rump state would not be in a position to lend its transportation system to the Germans. Hence, German logistical needs required the direct control of these strategic railways.

The documentary evidence presented above, together with railway maps from the period, permits us to describe the approximate path of the rail connection the Germans intended to keep in their zone of

[4] This quotation is from a French translation of the original German report on the meeting prepared for the German foreign ministry.
[5] See Germany, Auswärtiges Amt 1961: 305.
[6] See *New York Times*, 26 June 1940; *Le Matin*, 27 June 1940.
[7] A French version of the AP report, also including this note, was published by *Le Matin* on June 27, 1940. Multiple transcriptions of the Armistice agreement currently available online include this note as an Appendix. See, for example: http://avalon.law.yale.edu/wwii/frgearm.asp. Accessed August 6, 2014. See Texte de l'armistice signé à Rethondes le 22 Juin, 1940. http://mjp.univ-perp.fr/france/1940armistice.htm. Accessed August 6, 2014.

occupation (see Map 1). The north-south route described in Hitler's 17 June meeting is the Paris-Bordeaux line, which departed from Paris, ran through Tours, Poitiers, Angoulême, and Libourne, to Bordeaux, whence it continued south to the Spanish border. Map 2 shows the route of the Paris-to-Bordeaux railroad through the Charente and Vienne, along with the specified 20-kilometer "buffer" to its east. The eastern edge of this buffer closely tracks the LoD, indicating that the implementation of the occupation zones followed the description in the AP report cited above.

--- Map 2 ---

The east-west route described in the 17 June meeting was, at the time, the southernmost double-track railroad that cut across France north of the Mediterranean coast (see Map 1). It began in Nantes, near the Atlantic coast, intersected the Paris-to-Bordeaux railroad at Tours, then zigzagged East through Bourges, Moulins, and Paray-le-Monial (with Le Creusot lying just off this route). East of Chagny, in eastern France, the French rail network became denser, with several double-track routes to the Northeast and Germany inter-connecting at multiple points, generating several possible itineraries for German logistical needs that satisfied Hitler's requirement that they run entirely within the occupied zone. For the vast majority of its East-West path across central France, the LoD ran just to the south of this railway, normally within five kilometers. As shown in Maps 3 and 4, such was the case for the portion of this route that cut through the Cher and Saône-et-Loire, the remaining two departments in FM's study.[8] Again, there is a remarkable overlap between the contours of the LoD and the guidelines issued from Berlin as reported in the AP wire above.

--- Maps 3 and 4 ---

This strategic rationale behind the placement of the LoD has serious consequences for FM's research design. If the LoD is not locally quasi-random, then the communes on either side of it may be unbalanced on a key confound: double-track railways the Germans deemed strategically significant and, therefore, the Resistance would have good reason to attack. FM compute balance statistics and match on a variable they label *Train distance*, which measures each commune's distance to the nearest train station. The French railway network of the 1940s was extensive, however, and it included many strategically irrelevant single-track and narrow-gauge branch lines connecting small towns to the national network. Thus, *Train distance* does not capture the strategically significant double-track railways that directly influenced German plans for the LoD.[9]

[8] Why did the Germans maintain a 20 kilometer buffer around the Paris-Bordeaux line while situating the LoD just to the south of the Nantes-Tours-Belfort railroad? The documentation we consulted does not provide an answer, but some intuitive conjectures are compatible with this decision. First, since the British threat was primarily air- and sea-borne, the danger that lines of communication closer to the coast could be cut was more acute. Second, the relevant portion of the Paris-Bordeaux railway is exposed on its eastern side and does not follow any natural defenses. (Most main rivers in this part of France run East-West.) Therefore, it made sense to add a defensive buffer east of the railway. In contrast, the Nantes-Tours-Belfort line is, for the majority of its length, placed immediately to the north of a river and canal system, making it easier to defend and, therefore, requiring no buffer (Veyret 2001, 13). In any case, both railroads are included within the geographical scope of FM's study, which implies that strategic selection based on their location has the potential to bias their findings.

[9] FM control for major railways in a robustness check mentioned in their footnote 30. Although their supplemental materials include no numerical tests on this particular point, they provide the 1920s-era maps of passenger train routes based on which they performed this analysis on p. 18 of their supplemental materials. Based on these maps, their analysis appears to have focused on the distinction between "express" and "regional" trains. These categories are

To examine covariate balance on strategic railways, we code a binary variable – *Double track* – that equals one for communes intersected by a double-track railroad and zero otherwise.[10,11] We assume random assignment of communes to the directly occupied and Vichy zones, and we conduct difference-of-means tests comparing *Double track* across these two conditions. In effect we ask: is assignment to either side of the LoD a good predictor of the location of double-track railways? Since we know that these railways were built long prior to WWII – in other words, that their location was determined pre-treatment – if these statistical tests are highly significant, the assumption of as-if random assignment is undermined.

--- Table 1 ---

Table 1 shows two sets of comparisons. The top half follows FM's procedure by dropping all communes that intersect the LoD. This is a highly conservative assumption – i.e., it biases the analysis against finding an association between treatment assignment and the presence of double-track railroads – because for most of the communes intersected by the LoD *and* a double-track railroad, the railroad ran on the occupied side of the LoD.[12] For the four departments as a whole, and for 20- and 10-kilometer bandwidths around the LoD, treatment assignment (i.e., assignment to either side of the LoD) is a statistically significant predictor of railroad location. The t-test at the 5-kilometer bandwidth is not significant, probably because so many of the communes in this tighter bandwidth were dropped from the analysis.[13] Since we know on which side of the LoD the railroad ran for every commune intersected by the LoD, however, we can retrieve these observations. In the bottom half of Table 1, we do so by splitting each of the communes intersecting the LoD into an occupied and Vichy portion and coding *Double Track* for each of the resulting parts. When we do this, treatment assignment becomes a highly significant predictor of double-track railway location at the 5-kilometer bandwidth as well as at an even tighter bandwidth of 3 kilometers.

Precisely where an RDD expects to maximize covariate balance – close to the discontinuity – we find strategic railways highly unbalanced. Returning to Maps 2 – 4, it is easy to see why: within these four departments, the double-track railroads in the occupied zone tend to run roughly parallel to the LoD, while

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largely irrelevant for German logistical needs and map very imperfectly to the distinction between single- and double-track railways that we identify. Several double-track railways in the four departments in FM's analysis were used only by regional trains; several express trains in these departments seem to have run on single-track railways. It is possible that this discrepancy accounts for why FM did not identify the role of double-track railways in determining the placement of the LoD or the frequency of sabotage attacks.

[10] While the railroads the Germans intended to keep in their zone of occupation account for a substantial percentage of all double-track railroads in Charente, Cher, Vienne, and Saône-et-Loire, other lines existed. In particular, multiple lines that ran south to Lyon and Marseille became strategically important to the Germans once they occupied southern France at the end of 1942.

[11] Our analysis is based on a GIS constructed from two principal sources: the GADM database of Global Administrative Areas (http://gadm.org/home), and a 1:200,000-scale map series depicting the LoD that we obtained from the Cartothèque of the Institut Géographique Nationale (IGN) of France.

[12] To be precise, 24% (31/130) of communes intersected by the LoD were also intersected by a double-track railroad. Of those, a double-track railroad ran only on the occupied side in 68% (21/31).

[13] As robustness tests, we first computed the length of double-track in the occupied and Vichy zones for FM's four departments, as well as for 5-, 10- , and 20-km bandwidths around the LoD. Within these departments, the occupied zone had more than twice the length of track as the Vichy zone; within the 5-km bandwidth, the occupied zone had nearly four times as much double-track as did Vichy. For details, see Appendix A, Table A.1. In addition, we replicated the difference-of-means tests in Table 1 using the shortest distance between each commune's boundary and the closest double-track railroad, with similar results (see Appendix A, Table A.2).

those in Vichy tend to run roughly perpendicular to it. Thus, the construction of tight bandwidths around the LoD systematically captures more of the double-track railways on the occupied side and less on the Vichy side, thereby *reinforcing* the association between double-track railroads and the occupation zones, rather than ameliorating it.

Does FM's theory explain variation in resistance during WWII France?

We now turn to an analysis of the determinants of violent resistance in wartime France. First, we condition on strategic railroads in FM's four departments to see if railroads can account for the higher incidence of violent events in the occupied zone that FM observe. Second, we test FM's key hypotheses on a new, department-level data set of sabotage events.

Violent resistance in proximity to the LoD in the Charente, Cher, Saône-et-Loire, and Vienne

That strategic railroads are much more common on the occupied side of the LoD need not affect FM's estimates if railroads are uncorrelated with the incidence of violent events. If, however, places with strategic railroads were more likely to have violence than those without, and railroads were systematically more common in the occupied zone than in the Vichy zone, then FM's estimate of the local average treatment effect (LATE) is biased due to unobserved heterogeneity.[14]

To test this conjecture, we reconstructed FM's dataset of resistance events from its original source, the maquis (rural guerrilla bands of French Resistance fighters) histories compiled by the French *Service Historique de l'Armée*.[15] Whenever possible, we identified the commune in which each event occurred. FM analyze two types of events: *Sabotage* and *Fighting* (648).[16] They report statistically and substantively significant results for *Sabotage*. For *Fighting*, their findings are substantively weaker and fail to reach conventional levels of statistical significance in several tests (650). Consequently, we focus on sabotage. Similar results for the *Fighting* dependent variable are reported in the Appendix, Tables A.3 and A.4.

Prima facie we would expect the location of railroads to be an important determinant of sabotage attacks, given that 66% (378) of the sabotage events reported in FM's four departments were coded as "railway sabotage."[17] We begin by generating color-coded maps of the distribution of sabotage across the territory of these departments, together with the path of all double-track railroads. See maps 5 – 7 below.

[14] Formally, let Y be the number of resistance events, let X be the indicator of assignment to the occupied zone or the Vichy zone, and let R indicate the presence of a strategic railroad. FM observe $Y_i|X_i = 0$ and $Y_j|X_j = 1$. To recover the local average treatment effect (LATE), the key assumption is that $E(Y_i|X_i = 0) = E(Y_j|X_j = 0)$. That is, in the absence of treatment, the treated units would have had the same incidence of violence as the units that were in fact treated.

[15] FM's replication data became available after we completed our analysis. We have since examined the two datasets side-by-side. See Appendix B for a discussion of some discrepancies as well as a replication of our key results on FM's data. All our key criticisms of their analysis are supported by statistical tests using FM's data.

[16] We were able to assign communes to 573 *Sabotage* events and 613 *Fighting* events, while FM identified 598 *Sabotage* events and 595 *Fighting* events. Roughly 7% of the *Sabotage* events and 10% of the *Fighting* events could not be assigned to a unique commune.

[17] Information on the type of sabotage is missing for an additional 23% (130) of these events, leaving open the possibility that as many as 89% of the total events were directed at railroads.

--- Maps 5 – 7 ---

Two points emerge. First, the pattern of sabotage closely follows the railways. Second, while there were many attacks in the Vichy zone, because they targeted routes that ran roughly perpendicular to the LoD, tight bandwidths around the LoD tend to exclude them from the analysis. By contrast, attacks on the occupied side are systematically more likely to be selected into the analysis, because they targeted railways running roughly parallel to the LoD. In other words, the RDD itself creates a biased selection of cases.

As a formal test of the pattern identified in these maps, we compute differences-of-means in commune-level sabotage counts between the occupied and Vichy zones, conditional on whether or not the communes' boundaries intersected a double-track railroad. Our results are reported in Table 2.[18] We consider all three of FM's bandwidths, as well as the entire departments. In general, the differences are small; in no case do they achieve statistical significance at the 95% level. For six of the eight comparisons, higher sabotage counts were observed in Vichy. Contrary to FM's expectation, there is no evidence that a commune's assignment to either side of the LoD affects the incidence of sabotage when one controls for the presence of double-track railroads.

--- Table 2 ---

In Table 3, we reverse this procedure, taking the difference-of-means between communes intersected by double-track railways and communes without such railways, conditional on occupation zones. All eight of these comparisons are substantively large and have the sign we expect. Across the various subsets of data created by conditioning on bandwidth and occupation zone, communes with double-track railroads had, on average, multiples of the number of attacks that occurred in communes without them. Of the eight comparisons, six are statistically significant at the 95% confidence level. Within the Vichy zone, the comparisons at the 5- and 10-kilometers bandwidths are not significant at the 95% level of confidence. However, it is important to keep in mind that by excluding communes intersected by the LoD, we omit some of the highest sabotage counts in the dataset. The significance of this point is indicated by the penultimate line in Table 3, which makes the same comparison, but only for communes intersected by the LoD (the communes dropped in the previous eight comparisons). In substantive terms, the difference of means is enormous: at the LoD, communes with a double-track railroad had, on average, 33 times as many sabotage attacks as those without one. In the last line of Table 3, we give the unconditional difference-of-means between communes with and without double-track railroads: the former had, on average, sixteen times as many sabotage attacks as the latter.[19]

--- Table 3 ---

[18] See Appendix A for: (i) our formal estimand, (ii) our defense of the difference-of-means estimator instead of local linear regression, and (iii) local linear regression estimates as a robustness check.

[19] Double-track railroads are likely associated with additional covariates that are themselves likely to be associated with resistance, worsening the problem of unobserved heterogeneity in FM's study. For example, Route Nationale 10, the most important motorway in Charente and Vienne, followed the path of the Paris-Bordeaux railroad, which connected the regional hubs of Châtellerault, Poitiers, and Angoulême. The Vichy zone in these departments contained no cities of comparable size. In the Saône-et-Loire, the town of Le Creusot, located near the Tours-Belfort railway, contained France's most important munitions factories, which Hitler explicitly wished to keep inside the occupied zone (Böhme 1966: 22).

In sum, our evidence shows that, in the departments FM studied, sabotage occurred at systematically higher rates in communes with double-track railroads. Some of these railroads were precisely the ones German decision makers shaped the LoD to control. When we condition on double-track railroads, there is no evidence that violent resistance in wartime France was higher in the occupied zone than in Vichy.

Violent resistance throughout France

When their assumptions are satisfied, RDDs permit *locally* credible estimates of causal effects, but they can only indirectly estimate the causal effect of the treatment far from the discontinuity. In wartime France, the causal effect of indirect versus direct rule in small rural hamlets in the vicinity of the LoD may or may not have been similar to the effect on large cities such as Paris or Marseille. The effects of different strategies of political rule at the national level lie beyond the scope of FM's research design, but there is no reason to suppose that those effects were less important in the remainder of metropolitan France than they were in the vicinity of the LoD. In this section, we take a brief look at the broader pattern of violent resistance in France to see if it is consistent with FM's overall account.

To do this, we built a department-level database from a variety of sources. Since FM's data cover only 22 departments, we compiled a count of sabotage events in 88 of the 90 departments of metropolitan France from sources held by the French National Archives.[20] We coded two indicators, one for departments entirely in the Vichy zone (*Vichy*) and a second for the thirteen departments intersected by the LoD (*Line*); the occupied zone is the implicit category. To measure partisanship, we consulted returns from the first round of the 1936 parliamentary elections. Since the party system of the late Third Republic was highly fragmented, we followed Lachapelle's (1936: viii, 328-332) classification of ideological groupings and calculated the 1936 vote share for both the left and right. These two variables are highly collinear, so we use the vote share for the right minus the share for the left (*Vote share difference*). Given that votes not cast for the right (left) might go to centrist parties rather than the left (right), we control for the vote share of moderate parties (*Vote share center*). We also control for the log of department population (*Log population*), taken from the 1936 census. Given the importance of major railroads demonstrated above, we calculated the length of double-track line in each department (*Double track length*).

We include a variable for the percentage of each department's land area that is mountainous (*Rough terrain*). Mapping the data suggests a clear pattern of higher sabotage counts in proximity to Germany; we include the distance of each department's centroid to the German border to capture this pattern (*Germany distance*).To capture FM's conjecture that partisanship affected resistance only in the Vichy zone, we interact *Vichy* and *Line* with *Vote share difference*. We fit a poisson regression to the data. We report our estimates in Table 4. (Appendix A includes descriptive statistics on the variables in Table A.5.)

--- Table 4 ---

If the cooptation of the French right through indirect rule had, as FM maintain, suppressed resistance, we would expect to observe a negative coefficient for *Vichy* (indicating a lower intercept for the Vichy zone) and a negative coefficient on the interaction term between *diff_share* and *Vichy* (indicating a stronger relationship between partisanship and resistance in Vichy than in the occupied zone).

[20] The sources we consulted include no data on the island of Corsica or the Bas-Rhin, which was annexed to Germany in 1940.

This is not what we find. Figure 1 plots (from left to right) the predicted sabotage count ranging from departments dominated by the political left to departments where the right was stronger in the 1936 elections, separately for the occupied zone and the Vichy zone, with all other variables at their means. The predicted counts for the Vichy zone are higher across the board. The slope of the relationship between partisanship and sabotage is roughly similar for the two zones, with tight confidence bands, suggesting that the left-right cleavage was associated with resistance for France as a whole.[21]

--- Figure 1 ---

In sum, by delineating the LoD so that strategic railways were placed just inside the occupied zone, German decision-makers created an environment that was richer in targets for the Resistance in the territory they occupied directly, but only in close proximity to the LoD. By focusing their analysis just on this small region of France and not taking account of the strategic process through which communes were selected into the two zones, FM mistakenly concluded that whatever difference of occupation regimes between the two sides of the LoD may have existed played a causally important role in determining the geographical pattern of violence. Our results indicate otherwise.

The strategic logic of violent resistance in WWII France

FM's theoretical claim is that the political cooptation of local elites through devolution of power will dampen resistance to foreign rule (643). Connecting FM's independent variable – the level of devolution of political power to local elites – with their dependent variable – violent resistance to foreign rule – requires a causal mechanism of considerable complexity. At the same time, *any* possible causal mechanism consistent with their theory necessitates that attacks occurring within one of the zones (Vichy or the territory that was initially directly occupied by the Germans) be perpetrated by Resistance groups based in that same zone. For FM's theory to be testable using their data, spillover between the two zones must be limited or, at any rate, zone-neutral, i.e., with no significant net effect in one direction. If it turns out to be the case that (i) it is impossible to ascertain the geographic origins of the groups perpetrating the attacks and (ii) there are good strategic reasons consistent with a higher level of resistance attacks in one of the zones, then FM's theory cannot be tested using locally disaggregated data such as theirs.

To evaluate whether these conditions hold, we turn to the timeline of FM's data. We highlight the inadequacy of an empirical analysis of resistance in the vicinity of the LoD for testing theories that, like FM's, infer the geographic origins and motivations of the perpetrators from the precise location of an attack.

Per our replication of FM's data, fewer than 5% (26) of the 547 sabotage observations for which we can establish both year and month took place between the implementation of the LoD in July 1940 and its abolition in March 1943, following the total occupation of France by German forces in November 1942. From March 1943 onwards, the LoD – with its obstacles and checkpoints – was physically eliminated (Alary 2003: 269-273). Thenceforth, the LoD was a legal abstraction, and no particular hurdles precluded a maquis based in one zone from conducting actions in the other.

Furthermore, during the first half of 1943, the existing panoply of local Resistance groups had to a great extent been consolidated under the aegis of the National Resistance Council (*Conseil National de la*

[21] These results are consistent with the argument advanced in Kocher et al. (2013).

Résistance), the organization that from that moment on attempted to direct the French Resistance efforts in coordination with the Allies and the Free French government in London (Kedward 1993: 291). Thenceforth, it is difficult to circumscribe the decision-making process leading to target selection to the local level. Taken together, these two developments put in place in 1943 – and therefore affecting 95% of the events in FM's analysis – should lead us to question the use of FM's data to test the validity of arguments that infer the geographic origin of the perpetrators in relation to the LoD from the precise location of sabotage attacks vis-à-vis the LoD.

This is another instance in which FM's RDD aggravates instead of ameliorating the problem: whereas it is implausible that most attacks deep in the directly occupied zone were perpetrated by maquis originating deep in Vichy – and vice versa – in the areas FM analyze it is highly plausible that the LoD placed no constraints on the actions of local maquis. This might not be considered problematic if the territorial units on either side of the LoD were balanced in respect to key covariates, as FM claim. But, as we demonstrated above, the zone that was initially directly occupied by the Germans possessed a greater availability of strategic targets. It is therefore highly plausible that many of the attacks perpetrated on that side of the former LoD were conducted by groups originating from across the LoD, making it impossible to test FM's theory using FM's data.

These problems are compounded by a second feature of the timeline of FM's data. Attacks perpetrated between April 1943 and D-Day (June 6, 1944, when the Allies landed in Normandy, reopening conventional land warfare in France) account for only an additional 36% (199) of the sabotage events in FM's four departments. The remaining 59% (322) took place from June to September 1944, as the battle for the liberation of France was underway.[22] For a chronology of the sabotage data, see Table 5 and Figure 2 below.

--- Table 5 and Figure 2 ---

In this post-D-Day period, there are two additional reasons why it is difficult to trace any causal mechanism compatible with FM's theory connecting differences in political rule to levels of resistance. First, political rule was in flux. German forces were gradually being pushed back by the Allies or retreating ahead of them in order to avoid being cutting off, making it extremely difficult to pinpoint the type of rule that prevailed in each location at each moment. This volatility in rule often compounded the analytical problems produced by the above-mentioned abolition of the physical LoD. Second, by then resistance efforts were largely integrated into overall Allied war plans. This coordination effort stemmed from the importance of railway sabotage for the overall Allied effort to liberate France. Beginning in late 1943, the British Special Operations Executive (S.O.E.) and the *Bureau Central de Renseignement et d'Action* of de Gaulle's Free French government devised a plan aimed at hindering railway traffic in France in the aftermath of the envisioned landings through a combination of aerial bombardment and sabotage attacks conducted by the French Resistance (Veyret 2001: 49-51). This effort eventually led to Plan *Grenouille*, aimed at sabotaging railway depots (Veyret 2001: 51), and Plan *Vert*, aimed at sabotaging railway lines. Their purpose was to delay the transfer of reserves from cantonments in the interior of France and on the Mediterranean coast to the Atlantic coast following D-Day (Calmon 2000: 53; Durand 1968: 426-427; Veyret 2001: 129-132). Liberman (1996: 48-49) argues that this campaign, which made "thousands of railway cuts and disabl[ed] locomotives at a rate comparable to Allied bombing," was the only militarily significant act of resistance in all of occupied Europe. To this effect, "[c]ontainers marked 'Plan Vert' were parachuted into each region to

[22] These percentages exclude an additional seventeen events dated 1944 but with no mention of the month.

be allocated according predefined priorities. … Each [French Resistance] team received one copy of a map … containing the approximate location of the point to be attacked" (Durand 1968: 428). Given the limited means at the disposal of the Resistance, "only the most important itineraries, given the Allies' knowledge of the enemy's order of battle, were selected" (Durand 1968: 427). In sum, over half of the sabotage events used in FM's study were part of a centralized plan directed against key railways chosen based on the overall strategic goals of the Allies, and were implemented in a context of fluid political rule by groups acting across a by-then abstract LoD.[23]

The fluidity of political rule and the importance of central organization by the Allies can be seen in greater granularity by turning to the specific events in the Vienne and the Saône-et-Loire departments, which together account for 80% of FM's data. In the Vienne, the first maquis emerged only in the spring of 1944 "in anticipation of the approaching landing" of Allied forces (Calmon 2000: 50). Starting in May that year, 125 Allied aerial missions dropped over four thousand containers with weaponry and ammunition for the maquis Viennois, along with instructions to stock them "awaiting the insurrection that should follow the landings" (Calmon 2000: 52). In exchange for these supplies, local Resistance groups (integrated across the former LoD) agreed to be placed under the unified control of major Maingard de la Ville, alias "Samuel," himself part of a chain of command ultimately directed by General Pierre-Marie Koenig, de Gaulle's delegate to the Allied headquarters and, after D-Day, commander of the French Forces of the Interior [FFI] (Calmon 2000: 50-52). In order to increase the Resistance's intervention potential, "'Jedburgh' teams, composed of three men (one American, one English, one French) were sent in to help familiarize the new combatants in weapons manipulation and guerrilla techniques. On the ground, the FFI received reinforcements from the [British] SAS (Special Air Service): 56 men were parachuted in between June 6 and 10, 1944, [and] four Jeeps arrived by air." Their purpose was "to multiply the sabotage" against major transportation axes, "with the goal of delaying as much as possible the routing of [German] troops, materiel, and munitions (Calmon 2000: 52-53).

Increasing the great strategic importance of double-track railways in the Vienne, the August 15 Allied landings in the Mediterranean coast of France required German armies deployed in the Southern section of the Atlantic Wall to redeploy to Dijon through Tours and Bourges (see Map 1). As Calmon writes (2000: 53-54), it was "a matter of escaping the two jaws that are closing" as Allied forces moved East from Normandy and North from the Provence. Overall, then, the Resistance in the Vienne was now acting as a behind-the-enemy-lines force supporting the conventional offensive launched by the Allies, sabotaging railways across the department in a coordinated effort directed and equipped centrally from London.

The situation was similar in the Saône-et-Loire, a department that accounts for 65% of FM's sabotage data and that was crisscrossed by some of the most strategically important railways in the whole of France, connecting German forces deployed in the Mediterranean coast and Marseille, through Lyon, to both Germany and the area where the new front evolved as Allied forces advanced from their beachheads in Normandy (Veyret 2001: 49-50). Given this strategic importance, the British S.O.E. supplied the maquis in the department with thousands of weapons and explosive devices in preparation for the invasion (Veyret 2001: 82-83). With these supplies, the Resistance managed to liberate soon after the Allied landings a triangle of territory delimited by Chagny, Mâcon, and Paray-le-Monial – i.e., straddling the LoD (Veyret 2001: 120). From this territorial base, and within the purview of General Koenig's overall leadership, Resistance operatives launched repeated raids against the key Paris-Lyon-Marseille and Strasbourg-

[23] For an analysis similar to the one conducted above in Tables 2 – 3, but restricted to the pre-D-Day period, see Tables A6 – A7 in Appendix A. There are no important differences in the results.

Bordeaux railway lines *on both sides of the LoD* (Veyret 2001: 110, 120). The most targeted railway line during June 1944 in the Saône-et-Loire, according to the Central Movement Service of the French railway company (the S.N.C.F.), "was the [North-South] Paris-Lyon line … with around thirty sabotage events [or] three times more on average" than any other line in the department (Veyret 2001: 130).

As in the Vienne, impeding railway communications from the Mediterranean coast through the Saône-et-Loire would become an even more important objective in preparation for the August 15, 1944 Allied landings on the Mediterranean coast, which opened a second land front. In an attempt to avoid the extrication of the German forces placed in the area (which might if mobile be pulled back to Germany instead of cutoff by the Allied forces advancing eastward from Normandy), the S.A.S. parachuted teams of saboteurs into the Cluny area in the Saône-et-Loire on 13 and 14 August 1944. Their objective was to "accelerate the rhythm of the operations against the communication lines taken by the retreating German forces" (Veyret 2001: 111-112). According to a French military commander, "the withdrawal of the 1st German army to Dijon, ordered before the Allied landings in the south of France, was made almost impossible by the actions of the F.F.I. and there too the sabotage of the railway network played a decisive role; only a quarter of the German forces stationed in the South-West succeeded in reaching Dijon" (quoted in Veyret 2001: 132).

In sum, the violent resistance actions captured by FM's data were mostly targeted at sabotaging key railway lines in the context of a centrally coordinated Allied effort to liberate France more than a year after the LoD had been dismantled as a physical barrier. The differences FM observe across the LoD are therefore largely driven by the unbalanced presence of double-track railroads on either side of the LoD in its close proximity. Put simply, the patterns of violent resistance FM detect in close proximity to the LoD reveal the strategic logic of targeting underpinning the Resistance efforts, not the local political motives underlying individual decisions to resist against, or collaborate with, the German occupiers.

Conclusion

By approaching experimental standards of causal identification using historical data, natural experiments are uniquely alluring. But the entire weight of their causal power relies on what Dunning (2014: 227) calls their "Achilles heel": the assumption that the treatment-assignment process was as-if random. According to the literature, the plausibility of this assumption can be tested in two ways. First, by deploying treatment-assignment causal process observations (CPOs), consisting of "pieces or nuggets of information about the process by which units were assigned to treatment and control conditions" (Dunning 2014: 218). Second, by testing for balance on *relevant* pretreatment covariates (Dunning 2012: 239-243). We argue for the epistemic priority of the former over the latter.

The basic problem with FM's study is its lack of treatment-assignment CPOs. In lieu of direct evidence of the treatment assignment process, FM advance three claims supporting the as-if random assumption: (i) existing work on the LoD does not explain its local placement (645, fn. 9); (ii) the LoD cut across pre-existing boundaries (645); and (iii) balance statistics on observed pretreatment covariates find no significant differences (647-648). In the absence of evidence against the as-if random assumption (claims i and ii), FM rely on balance tests to support it (claim iii). But balance can only be established on measured covariates. Without knowledge of the treatment-assignment causal process, how can we identify the *relevant* covariates? In FM's case, balance checks on a set of causally irrelevant variables generated undue confidence in the as-if random assumption. Insufficient attention to the role played by double-track railways in German decision-making about the location of the LoD led FM not to test for balance on this particular covariate, which

would have invalidated the assumption of as-if random assignment to treatment. As we have shown, historical documents on the process of drawing the LoD focusing "on the information, incentives, and capacities of key actors with control over processes of treatment assignment" (Dunning 2014: 212) reveal that German decision-makers had *information* about the location of French strategic assets, strong *incentives* to retain control over those essential to their war aims, and the *capacity* to shape the LoD. In sum, absence of evidence undermining the as-if random assumption is not sufficient to support it, even when combined with exhaustive balance tests on observed pretreatment covariates. Treatment-assignment CPOs and balance tests are complementary, not fungible. There is no statistical replacement for what Freedman (1991) called "shoe leather research."

This argument has implications for what should be our set of "best practices" to adjudicate the value of design-based research. We put forth a two-step process. First, we should not accept a natural experiment as valid in the absence of CPOs showing either that the treatment-assignment process was as-if random or that the strategic dynamics in play did not operate at the relevant level. If the information, incentives, and capabilities of the agents deciding which units are assigned to treatment might lead them to conduct this process in pursuit of strategic goals, then the researcher must show that, at the relevant level of analysis, the treatment-assignment process was arbitrary, in that there were multiple plausible ways of assigning units to treatment that would have equally satisfied the decision-makers' strategic aims. In FM's case, this would have involved documenting the process through which German leaders shaped the LoD, showing either that they designed it in a random fashion or that their strategic considerations had no local impact.

Second, we second Dunning (2014: 229) in arguing that we should attribute greater value to "scholarly adversarialism, … [which] may provide the most feasible way of confronting the problem that absence of evidence is not evidence of absence." Scholars with substantive knowledge of a natural experiment's empirical domain should scrutinize the plausibility of its as-if random assumption. This is particularly important given the risky trade-off in design-based research. If properly designed, natural experiments allow for the empirical testing of causal relationships using small samples of the population of relevant cases. If improperly designed, however, the causal claims at stake will be supported by what amounts to a very thin observational test, likely to be rejected as valid. (In FM's study, a general theory connecting political rule with resistance to foreign occupation is supported by evidence coming from less than 5% of metropolitan France's territory.) So far, however, we have failed to generate this sort of cross-polinization between design-based research and deep historical knowledge. As Dunning (2014: 231) points out, "[c]ritiques of as-if random have tended not to draw extensively on qualitative evidence … because of the extensive case knowledge and detailed information required to do so." This article demonstrates the value of revisiting existing natural experiments with a critical eye.

The causal identification revolution in political science entails a certain irony. To solve the inferential problems inherent in qualitative research, the postwar behavioral revolution turned to quantitative work (Russett 1969). More recently, to solve the inferential problems with observational quantitative work, the experimental framework was proclaimed as the gold-standard of causal identification in political research (Gerber, Green, and Kaplan 2004). At least in what concerns natural experiments, however, the whole weight of their ability to identify causal relations rests on the assumption of as-if random assignment to treatment. Proper validation of this assumption necessarily depends on qualitative research.

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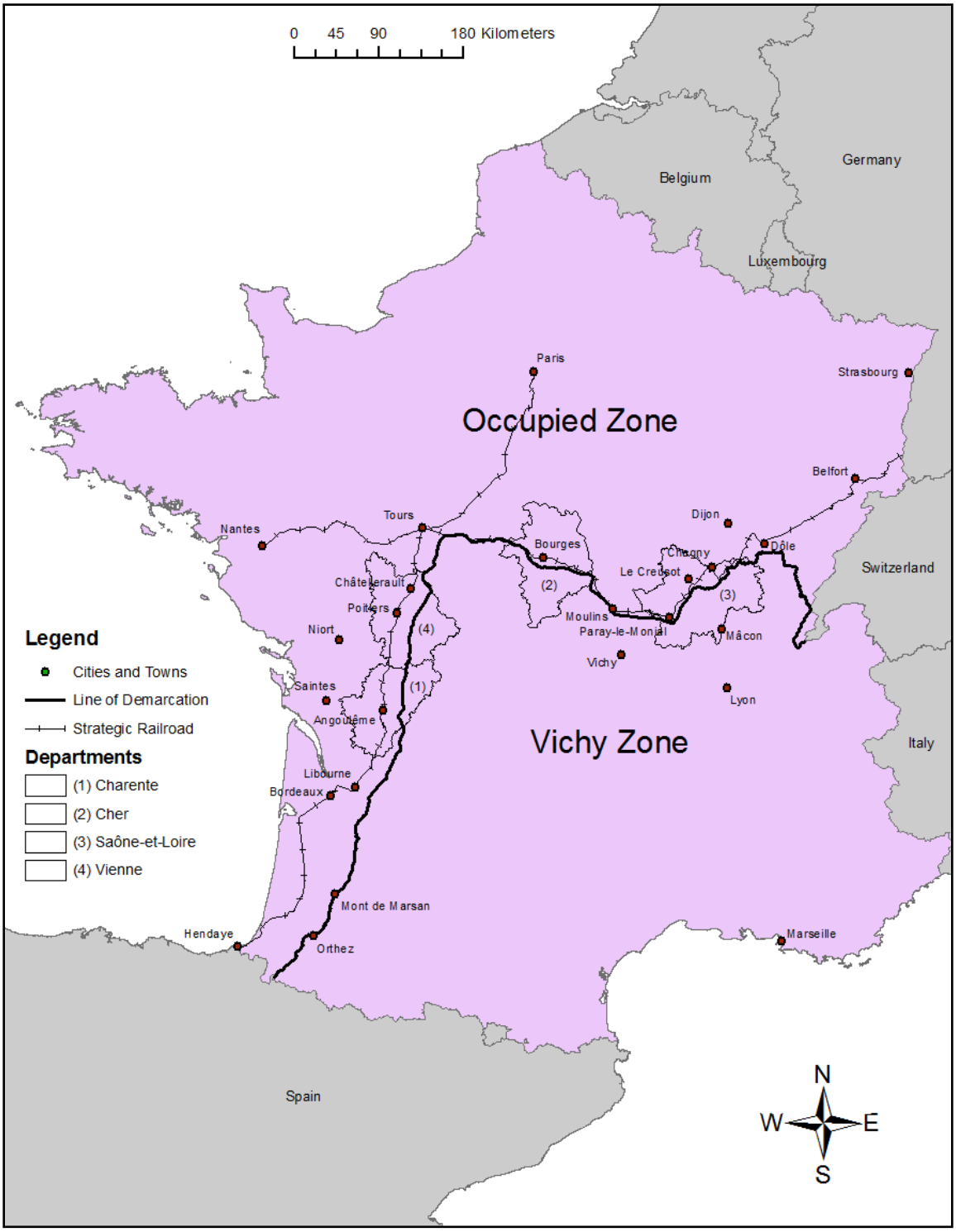
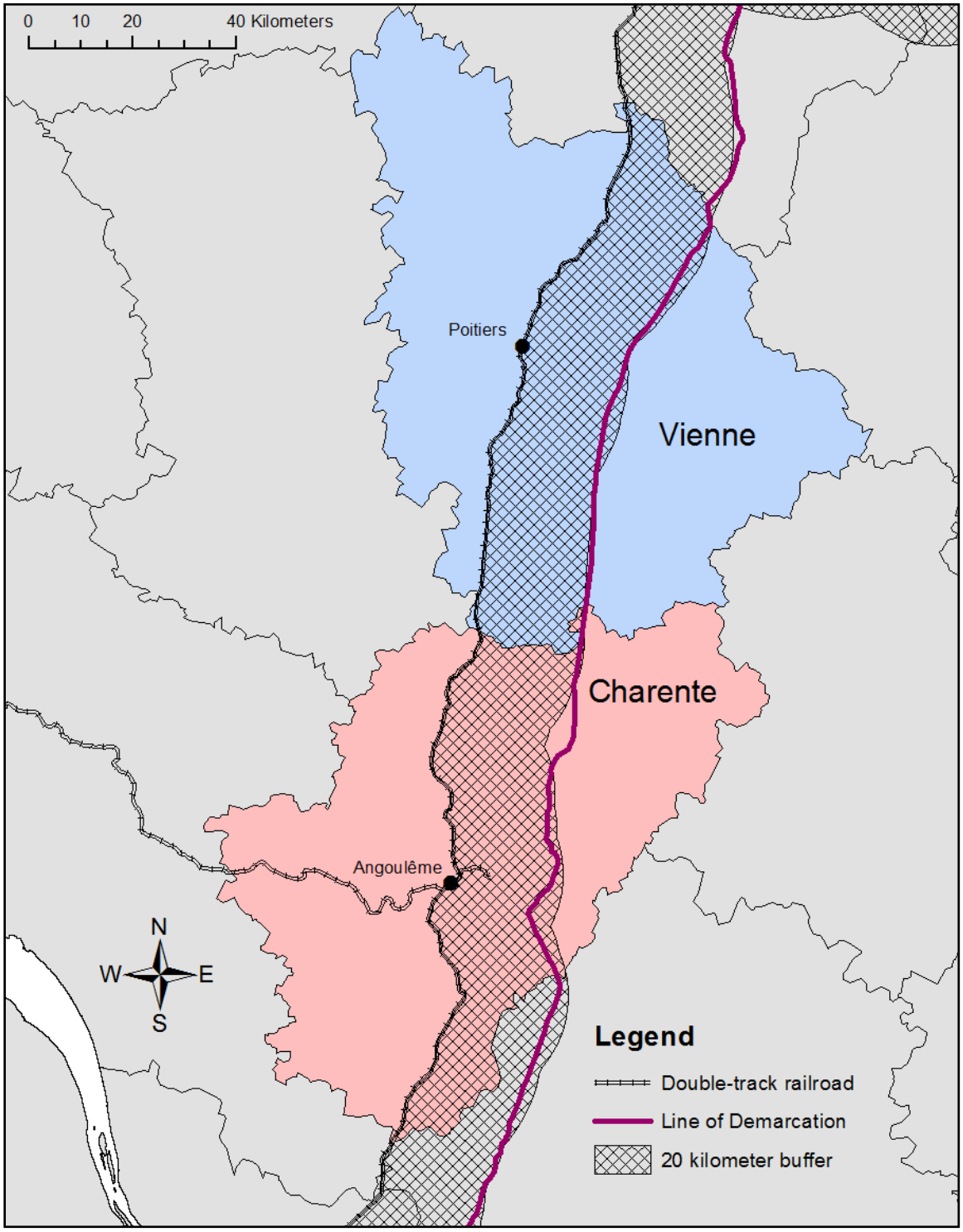
Maps, Tables, and Figures

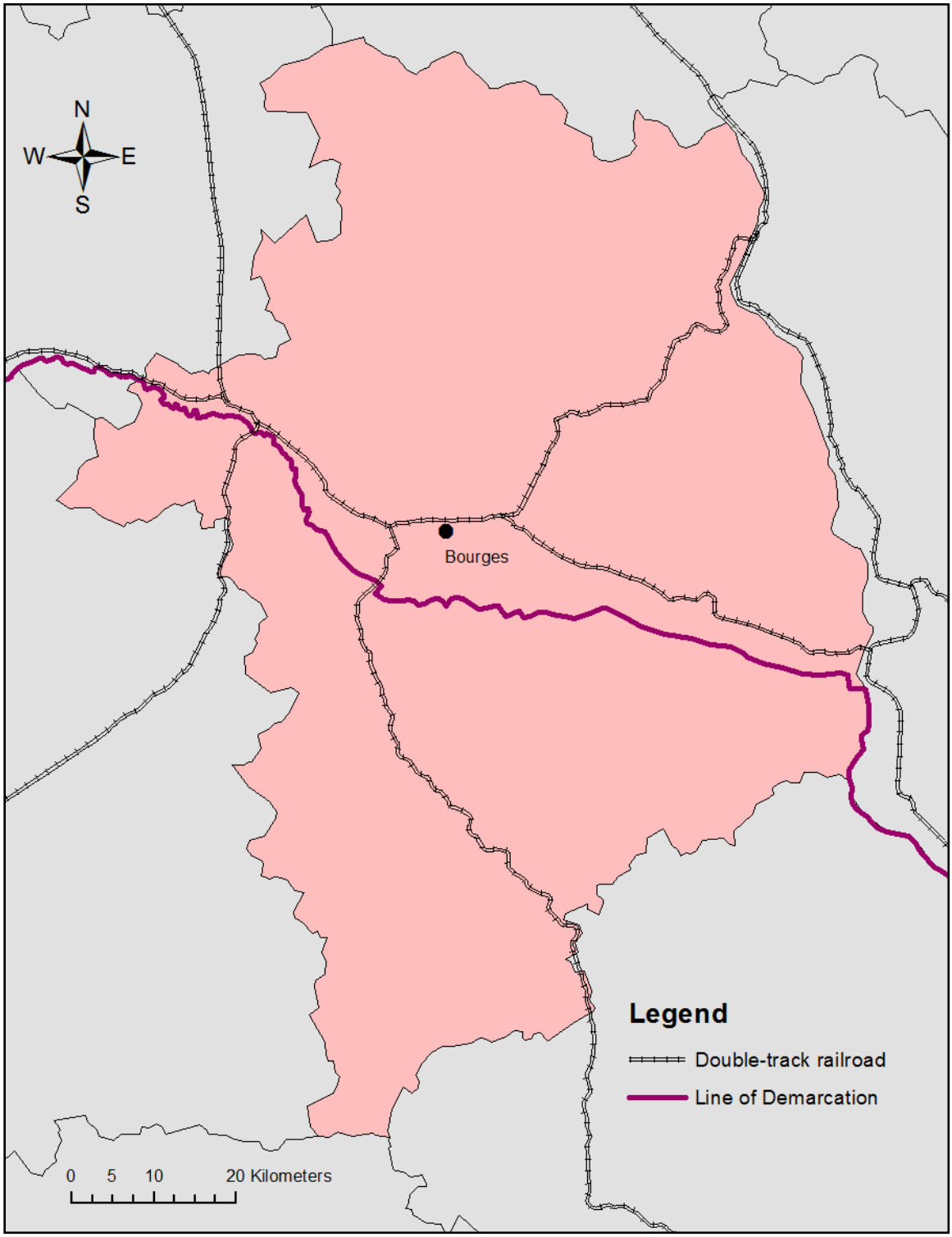
Map 1: The Line of Demarcation (LoD) and Nantes-Tours-Belfort and Paris-Tours-Bordeaux Railroads



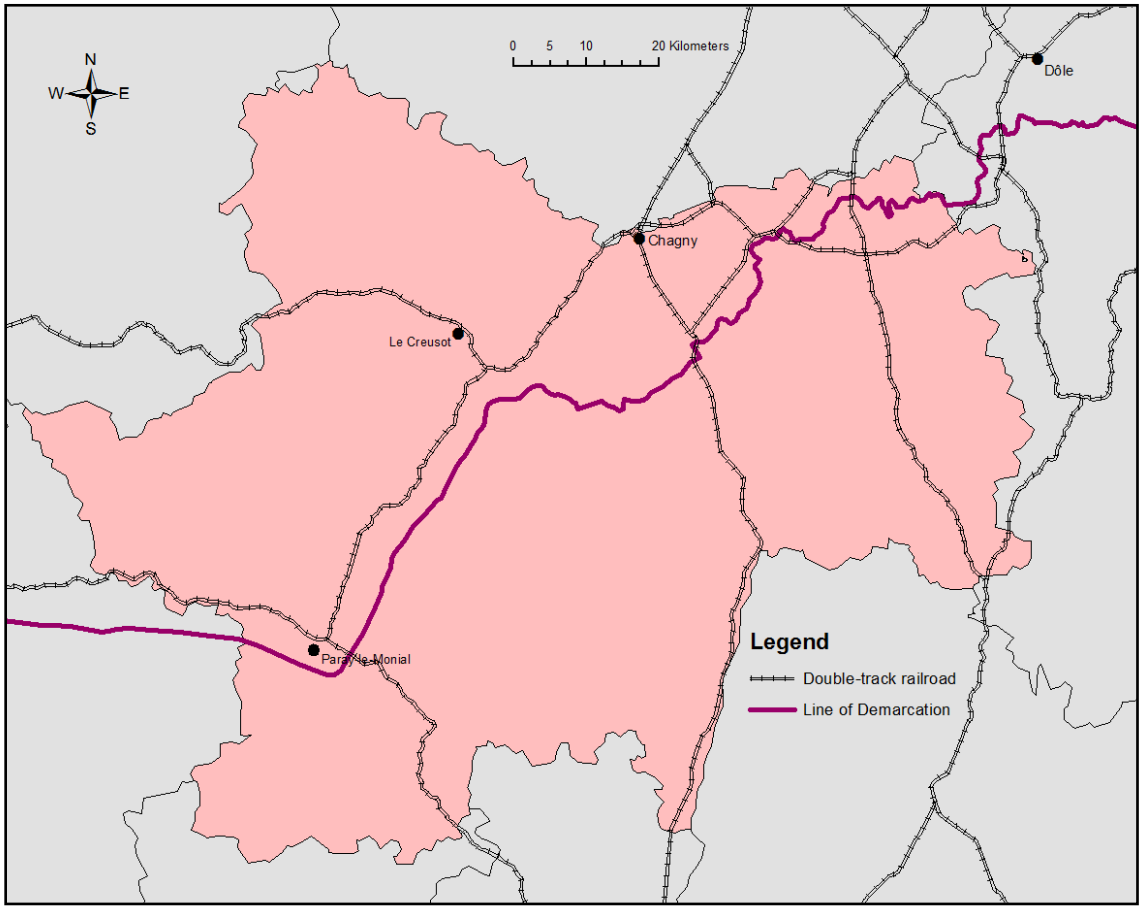
Map 2: Double-track Railways and Line of Demarcation in Charente and Vienne



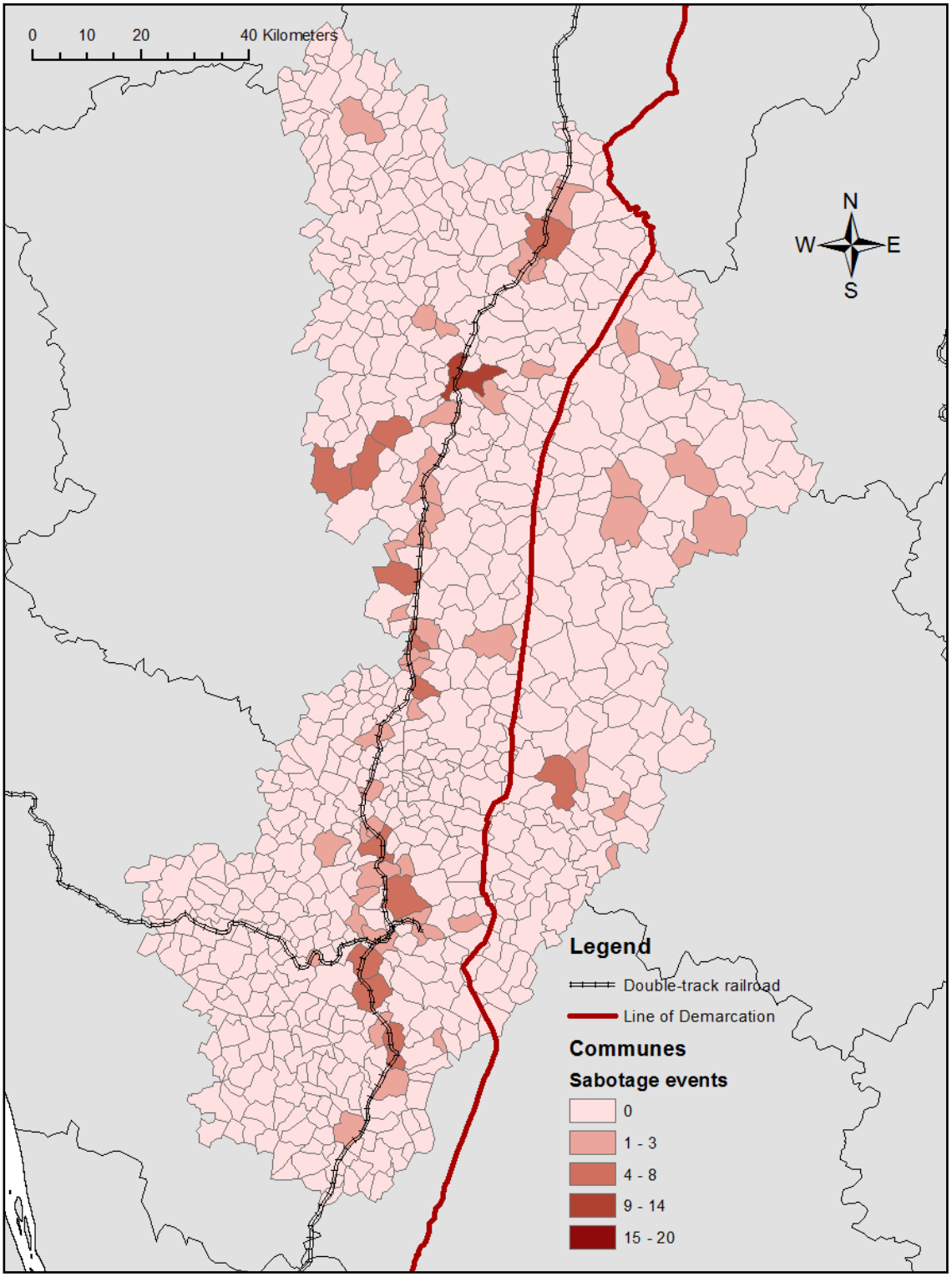
Map 3: Double-track Railways and Line of Demarcation in Cher



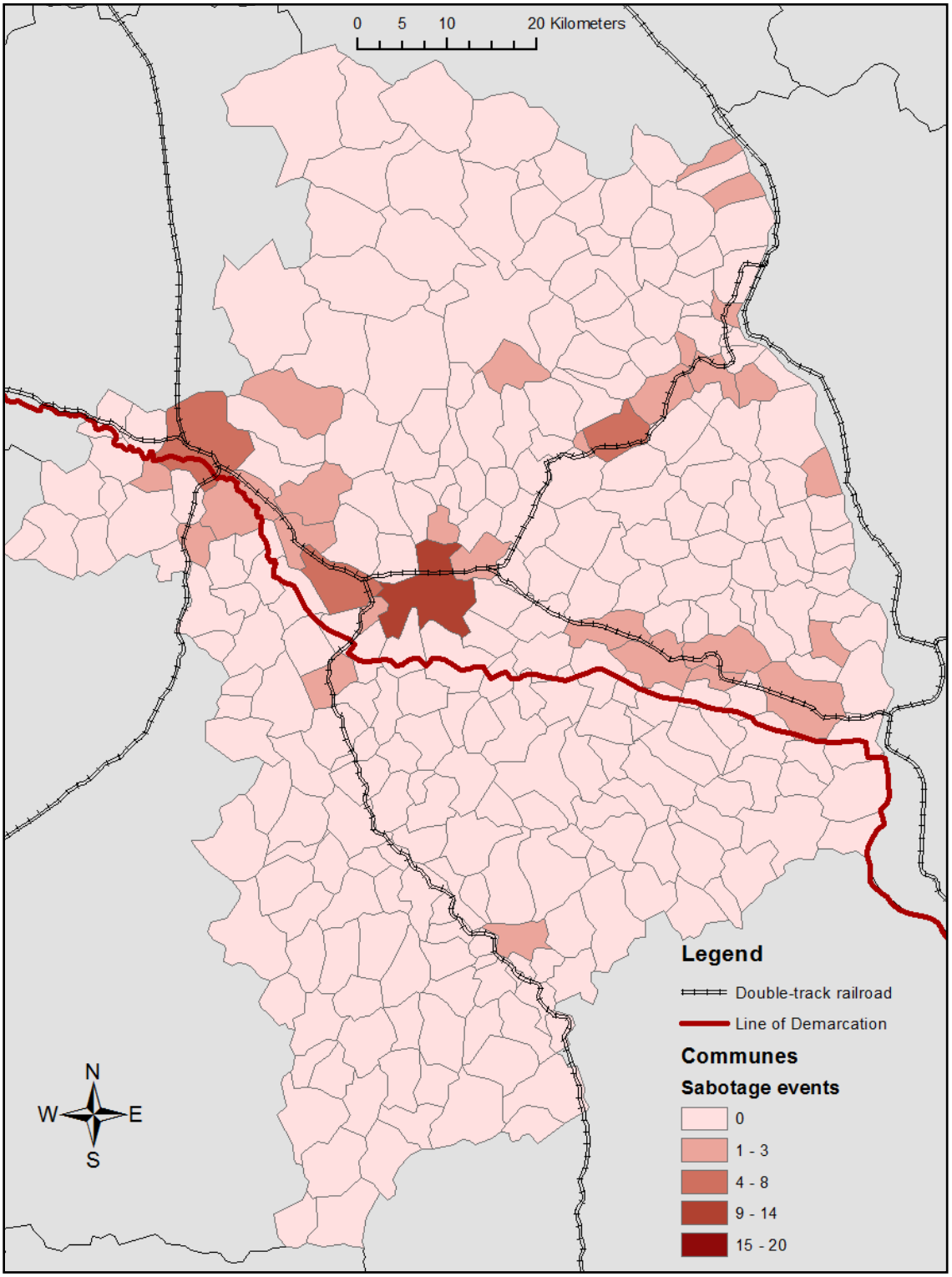
Map 4: Double-track Railways and Line of Demarcation in Saône-et-Loire



Map 5: Double-track Railways and Sabotage in Charente and Vienne, 1940-44



Map 6: Double-track Railways and Sabotage in Cher, 1940-44



Map 7: Double-track Railways and Sabotage in Saône-et-Loire, 1940-44

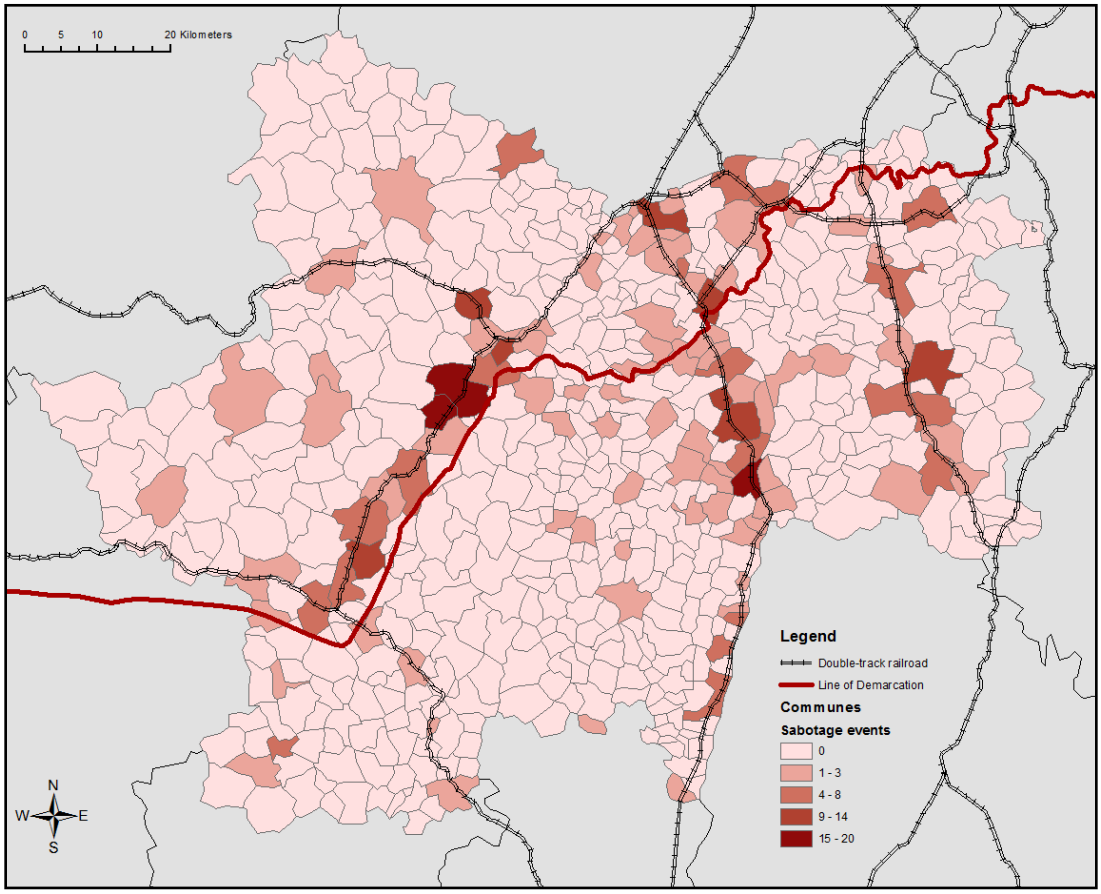


Table 1: Difference of means test, commune intersects double-track railway conditional on treatment assignment

		Mean of *Double track*, Occupied Zone (std. dev.)	Mean of *Double track*, Vichy Zone (std. dev.)	Difference of means, Occupied – Vichy	T-test (unequal variances)
Communes intersecting the Line of Demarcation dropped					
(1)	All Communes (N = 1418)	0.186 (0.390)	0.130 (0.336)	0.057	t = 2.922 p < 0.01
(2)	20Km Bandwidth (N = 691)	0.264 (0.442)	0.104 (0.305)	0.161	t = 5.610 p < 0.0001
(3)	10Km Bandwidth (N = 294)	0.277 (0.449)	0.144 (0.352)	0.133	t = 2.832 p < 0.01
(4)	5Km Bandwidth (N = 95)	0.327 (0.474)	0.239 (0.431)	0.087	t = 0.941 p = 0.349
Communes intersecting the Line of Demarcation split					
(5)	5Km Bandwidth (N = 353)	0.247 (0.433)	0.120 (0.326)	0.127	t = 3.123 p < 0.01
(6)	3Km Bandwidth (N = 253)	0.209 (0.408)	0.089 (0.285)	0.121	t = 2.731 p < 0.01

Two-tailed tests.

Table 2: Difference of means in *Sabotage* events between occupied and Vichy zones, conditional on intersection with a double-track railroad, with communes intersecting the LoD dropped

		Mean of *Sabotage*, Occupied Zone (std. dev.)	Mean of *Sabotage*, Vichy Zone (std. dev.)	Difference of means, Occupied – Vichy	T-test (unequal variances)
5km Bandwidth	*Double track* = 1 (N = 27)	3.125 (5.018)	0.727 (1.489)	2.398	t = 1.799 p=0.088†
	Double track = 0 (N = 68)	0.030 (0.174)	0.114 (0.323)	-0.084	t = -1.346 p=0.184†
10km Bandwidth	*Double track* = 1 (N = 62)	2.122 (3.964)	1.429 (3.234)	0.693	t = 0.739 p = 0.464
	Double track = 0 (N = 232)	0.084 (0.391)	0.120 (0.502)	-0.036	t = -0.611 p = 0.542
20km Bandwidth	*Double track* = 1 (N = 130)	2.063 (3.497)	2.235 (4.593)	-0.173	t = -0.200 p = 0.843
	Double track = 0 (N = 561)	0.082 (0.359)	0.129 (0.513)	-0.047	t = -1.263 p = 0.207
All Communes	*Double track* = 1 (N = 231)	1.497 (2.944)	1.684 (3.480)	-0.187	t = -0.404 p = 0.687
	Double track = 0 (N=1187)	0.096 (0.506)	0.108 (0.454)	-0.012	t = -0.423 p = 0.672

Two-tailed tests. † A randomization test (10000 simulations) at this bandwidth gives $p > .10$. At larger bandwidths, randomization inference reliably converges to the value of the t-test.

Table 3: Difference of means in *Sabotage* between communes with and without double-track railroads, conditional on zone of occupation

		Mean of *Sabotage*, *Double track* = 1 (std. dev.)	Mean of *Sabotage* | *Double track* = 0 (std. dev.)	Difference of means, *[Double track = 1] – [Double track = 0]*	T-test (unequal variances)
Communes intersecting the LoD dropped					
5km Bandwidth	Occupied (N = 49)	3.125 (5.018)	0.030 (0.487)	3.095	t = 2.466 p < 0.05
	Vichy (N = 46)	0.727 (1.489)	0.114 (0.323)	0.613	t = 1.355 p = 0.204
10km Bandwidth	Occupied (N = 148)	2.122 (3.964)	0.084 (0.391)	2.038	t = 3.286 p < 0.01
	Vichy (N = 146)	1.429 (3.234)	0.120 (0.045)	1.309	t = 1.851 p = 0.079
20km Bandwidth	Occupied (N = 363)	2.063 (3.497)	0.082 (0.359)	1.980	t = 5.538 p < 0.0001
	Vichy (N = 328)	2.235 (4.593)	0.129 (0.513)	2.106	t = 2.672 p < 0.05
All Communes	Occupied (N = 832)	1.497 (2.944)	0.096 (0.506)	1.401	t = 5.904 p < 0.0001
	Vichy (N = 586)	1.684 (3.480)	0.108 (0.454)	1.576	t = 3.943 p < 0.001
Communes intersecting the LoD included					
Communes intersecting the Line only	(N = 130)	2.742 (4.195)	0.081 (0.340)	2.661	t = 6.299 p < 0.0001
All Communes	(N=1548)	1.698 (3.282)	0.100 (0.474)	1.598	t = 16.658 p < 0.0001

Two-tailed tests.

Table 4: Determinants of sabotage in wartime France

	Poisson model
Line	1.024***
	(.046)
Vichy	0.917***
	(.063)
Vote share difference	-1.757***
	(.089)
On line X vote share difference	1.362***
	(0.132)
Vichy X vote share difference	1.218***
	(0.119)
Vote share center	0.389*
	(0.115)
Log population	0.062
	(0.033)
Germany distance (km)	-0.003***
	(0.0001)
Rough terrain (% of land area)	-0.007***
	(.0009)
Double track length (km)	0.002***
	(0.0002)
Constant	3.804***
	(0.397)
Pseudo-R^2	0.596
Observations	86

* p < .05, ** p < .01, *** p < .001

Table 5: Distribution of *Sabotage* per Department per Period

	July 1940 – March 1943	**April 1943 - May 1944**	**June - September 1944**	***Total***
Charente	0	12	48	***60***
Vienne	17	11	52	***84***
Cher	4	13	42	***60***
Saone-et-Loire	5	163	180	***372***
Total	***26***	***199***	***322***	***547***
Monthly Rate	***0.79***	***14.21***	***80.5***	

Figure 1: Marginal effect of partisanship on sabotage in wartime France

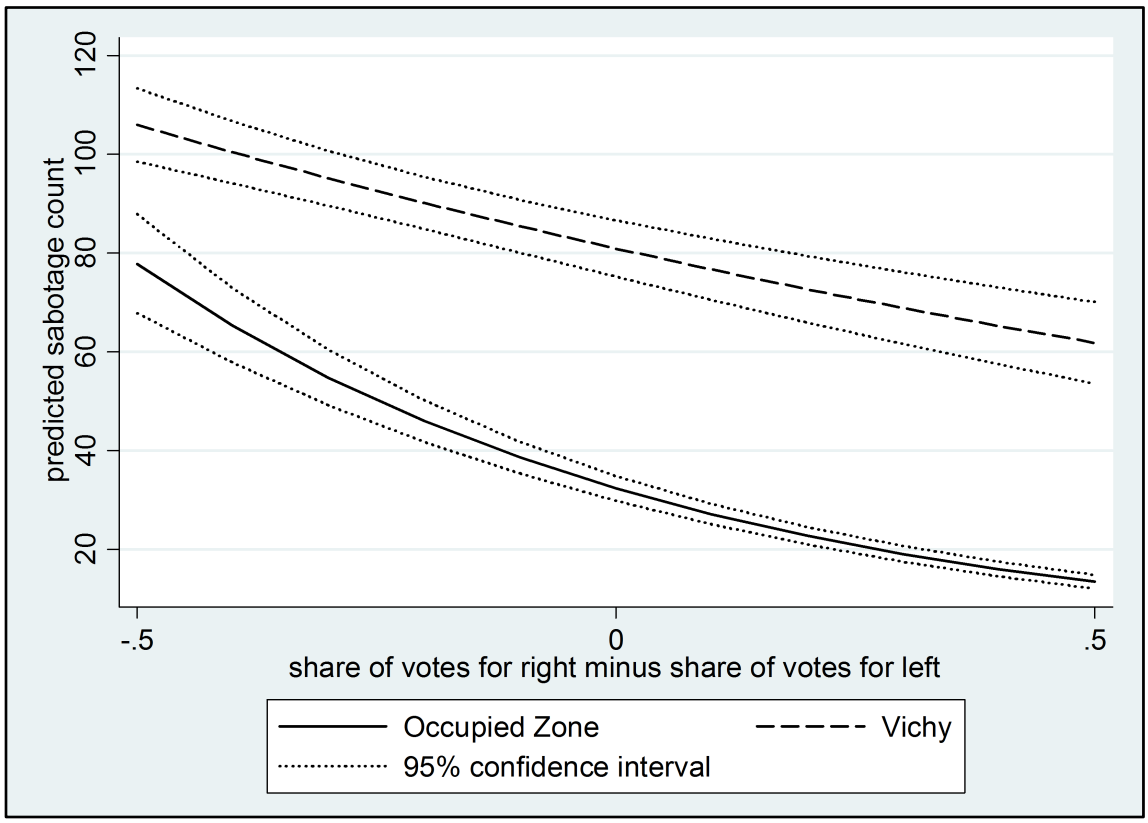
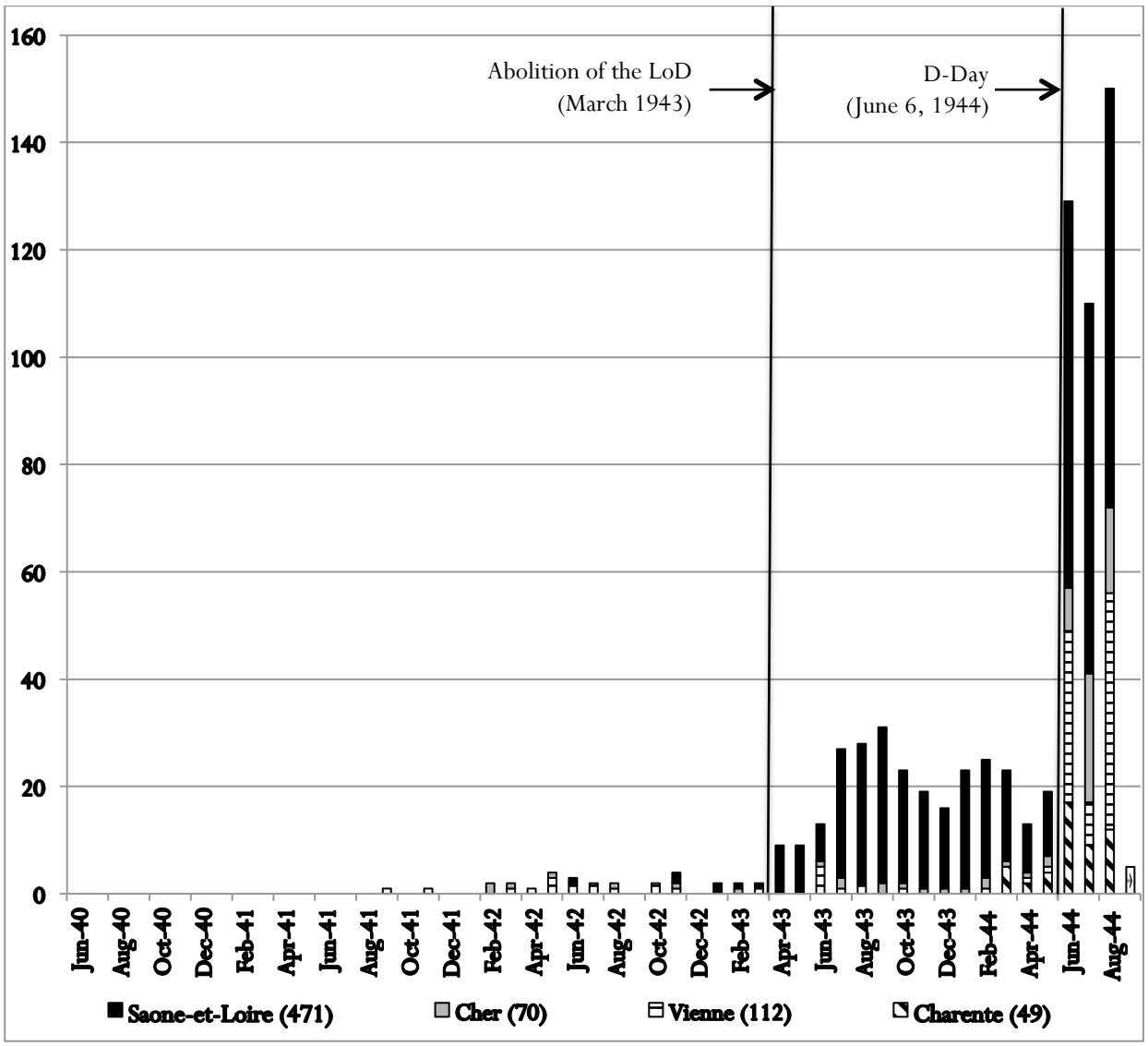


Figure 2: Chronology of Sabotage Data per Department (Charente, Cher, Saône-et-Loire, and Vienne), June 1940 – September 1944



Appendix A: Robustness tests

Table A.1: Kilometers of double-track railway in Vichy and the occupied zone by department

	All Communes		20Km Bandwidth		10Km Bandwidth		5Km Bandwidth	
	Occupied	Vichy	Occupied	Vichy	Occupied	Vichy	Occupied	Vichy
Charente	174.7	0	98.4	0	0.6	0	0	0
Cher	121.1	73.8	121.1	39.4	111.6	23.9	94.9	13.9
Saône-et-Loire	212.8	187.2	199.0	98.9	164.0	63.6	115.1	41.0
Vienne	143.7	0	109.4	0	0	0	0	0
Total	*652.3*	*261.0*	*527.9*	*138.3*	*276.2*	*87.5*	*210*	*54.9*

Table A.2: Difference of means in commune distance to closest double-track railroad, conditional on treatment assignment, with communes intersecting the LoD dropped

		Mean commune distance to closest double-track railroad (meters), Occupied Zone (std. dev.)	Mean commune distance to closest double-track railroad (meters), Vichy Zone (std. dev.)	Difference of means, Occupied - Vichy	T-test (unequal variances)
(1)	All Communes (N = 1418)	6891 (8394)	11307 (11144)	-4415	t = -8.107 p < 0.0001
(2)	20Km Bandwidth (N = 691)	3609 (3902)	11671 (9990)	-8062	t = -13.701 p < 0.0001
(3)	10Km Bandwidth (N = 294)	4511 (4722)	8624 (7408)	-4113	t = -5.668 p < 0.0001
(4)	5Km Bandwidth (N = 95)	4915 (5315)	7227 (6864)	-2312	t = -1.827 p = 0.071

Two-tailed tests.

Fighting

Tables A.3 and A.4 replicate the analysis found in Tables 2 and 3 in the main body of our paper for a distinct dependent variable: armed attacks by the Resistance against German personnel and their collaborators, which FM call "*Fighting*." Table A.3 shows the difference of means between the occupied and Vichy zones holding constant the presence of double-track railroad lines. As before, we perform the analysis for 5-, 10-, and 20-kilometer bandwidths around the line, and for all communes, in the departments of Charente, Cher, Saône-et-Loire, and Vienne. Only two of the eight comparisons have the sign FM anticipate; not one is statistically significant at the .05 threshold.

Table A.3: Difference of means in *Fighting* events between occupied and Vichy zones, conditional on intersection with a double-track railroad (communes intersecting the LoD dropped)

		Mean of *Fighting*, Occupied Zone (std. dev.)	Mean of *Fighting*, Vichy Zone (std. dev.)	Difference of means, Occupied - Vichy	T-test (unequal variances)
5km Bandwidth	*Double track* = 1 (N = 27)	1.875 (3.202)	0.364 (0.674)	1.511	t = 1.830 p = 0.085
	Double track = 0 (N = 68)	0.121 (0.331)	0.286 (0.750)	-0.165	t = -1.181 p = 0.244
10km Bandwidth	*Double track* = 1 (N = 62)	1.244 (2.508)	0.571 (0.978)	0.672	t = 1.508 p = 0.137
	Double track = 0 (N = 232)	0.178 (0.670)	0.328 (0.770)	-0.150	t = -1.591 p = 0.113
20km Bandwidth	*Double track* = 1 (N = 130)	0.865 (1.901)	1.088 (2.366)	-0.224	t = -0.497 p = 0.621
	Double track = 0 (N = 561)	0.255 (0.886)	0.347 (1.059)	-0.092	t = -1.123 p = 0.262
All Communes	*Double track* = 1 (N = 231)	0.703 (1.676)	0.921 (1.853)	-0.218	t = -0.866 p = 0.388
	Double track = 0 (N=1187)	0.238 (0.952)	0.347 (1.133)	-0.109	t = -1.759 p = 0.079

Two-tailed tests.

Table A.4 shows the difference of means between communes with and without double-track railway lines, but with the zone of occupation held constant. The top half of the table follows FM's procedure and drops communes intersected by the LoD; the bottom half recovers those communes but does not condition on occupation zones. It should not be at all surprising that the location of strategically valuable railroads does a better job of accounting for the geographical distribution of railroad sabotage than for the geographical distribution of attacks on personnel; this is, indeed, what we find. The last row of the table indicates that there were almost three times as many *Fighting* events in communes with double-track railroads as there were in communes without. Overall, even when we condition on the occupation zones and drop many observations (i.e., those communes intersected by the LoD or located outside of FM's bandwidths) the location of double-track railroads continues to be an important predictor for the location of *Fighting* events.

Table A.4: Difference of means in *Fighting* events conditional on intersection with a double-track railroad, within categories of FM's treatment assignment

		Mean of *Fighting* | *Double track* = 1 (std. dev.)	Mean of *Fighting* | *Double track* = 0 (std. dev.)	Difference of means, *[Double track = 1] – [Double track = 0]*	T-test (unequal variances)
Communes intersecting the LoD dropped					
5km Bandwidth	Occupied (N = 49)	1.875 (3.202)	0.121 (0.331)	1.754	t = 2.186 p < .05
	Vichy (N = 46)	0.364 (0.674)	0.286 (0.750)	0.078	t = 0.325 p = 0.749
10km Bandwidth	Occupied (N = 148)	1.244 (2.508)	0.178 (0.670)	1.066	t = 2.686 p < 0.05
	Vichy (N = 146)	0.571 (0.978)	0.328 (0.770)	0.243	t = 1.085 p = 0.289
20km Bandwidth	Occupied (N = 363)	0.865 (1.901)	0.255 (0.886)	0.610	t = 3.028 p < 0.01
	Vichy (N = 328)	1.088 (2.366)	0.347 (1.059)	0.741	t = 1.806 p = 0.080
All Communes	Occupied (N = 832)	0.703 (1.676)	0.238 (0.952)	0.465	t = 3.337 p < 0.01
	Vichy (N = 586)	0.921 (1.853)	0.347 (1.133)	0.574	t = 2.628 p < 0.05
Communes intersecting the LoD included					
Communes intersecting the LoD only	(N = 130)	1.387 (2.380)	0.535 (1.918)	0.852	t = 2.034 p < 0.05
All Communes	(N=1548)	0.847 (1.828)	0.304 (1.128)	0.543	t = 6.293 p < .0001

Two-tailed tests.

Table A.5: Descriptive statistics

Variable	Mean	Standard deviation
Vote share difference	0.032	0.343
Vote share center	0.177	0.108
Log population	12.824	0.732
Germany distance (km)	413.087	199.045
Rough terrain (% of land area)	16.999	25.025
Double track length (km)	207.886	150.375

Table A.6: Difference of means in pre-D-Day *Sabotage* events between occupied and Vichy zones, conditional on intersection with a double-track railroad, with communes intersecting the LoD dropped

		Mean count of sabotage events, Occupied Zone (std. dev.)	Mean count of sabotage events, Vichy Zone (std. dev.)	Difference of means, Occupied - Vichy	T-test (unequal variances)
5km Bandwidth	Intersects Railroad (N = 27)	1.5 (2.805)	0.182 (0.603)	1.318	t = 1.820 p=0.087
	~ Intersect Railroad (N = 68)	0.000 (0.000)	0.114 (0.323)	-0.114	t = -2.095 p < .05
10km Bandwidth	Intersects Railroad (N = 62)	0.878 (1.965)	0.381 (0.865)	0.497	t = 1.380 p = 0.173
	~ Intersect Railroad (N = 232)	0.037 (0.191)	0.064 (0.277)	-0.027	t = -0.863 p = 0.389
20km Bandwidth	Intersects Railroad (N = 130)	0.938 (2.423)	0.794 (1.647)	0.143	t = 0.382 p = 0.704
	~ Intersect Railroad (N = 561)	0.041 (0.234)	0.068 (0.323)	-0.027	t = -1.133 p = 0.258
All Communes	Intersects Railroad (N = 231)	0.594 (1.956)	0.711 (1.590)	-0.117	t = -0.486 p = 0.628
	~ Intersect Railroad (N = 1187)	0.035 (0.221)	0.053 (0.272)	-0.017	t = -1.187 p = 0.237

Two-tailed tests.

Table A.7: Difference of means in pre-D-Day *Sabotage* between communes with and without double-track railroads, conditional on zone of occupation

		Mean count of sabotage events, commune intersects railroad (std. dev.)	Mean count of sabotage events, commune ~intersect railroad (std. dev.)	Difference of means, RR - ~RR	T-test (unequal variances)
Communes intersecting the LoD dropped					
5km Bandwidth	Occupied (N = 49)	1.5 (2.805)	0.000 (0.000)	1.5	t = 2.139 p < 0.05
	Vichy (N = 46)	0.182 (0.603)	0.114 (0.323)	0.068	t = 0.356 p = 0.728
10km Bandwidth	Occupied (N = 148)	0.878 (1.965)	0.037 (0.191)	0.841	t = 2.735 p < 0.01
	Vichy (N = 146)	0.381 (0.865)	0.064 (0.277)	0.317	t = 1.666 p = 0.111
20km Bandwidth	Occupied (N = 363)	0.938 (2.423)	0.041 (0.234)	0.896	t = 3.619 p < 0.001
	Vichy (N = 328)	0.794 (1.647)	0.068 (0.323)	0.726	t = 2.564 p < 0.05
All Communes	Occupied (N = 832)	0.594 (1.956)	0.035 (0.221)	0.558	t = 3.547 p < 0.001
	Vichy (N = 586)	0.711 (1.590)	0.053 (0.272)	0.658	t = 3.597 p < 0.001
Communes intersecting the LoD included					
Communes intersecting the Line only	(N = 130)	1.097 (2.508)	0.030 (0.172)	1.066	t = 4.235 p < 0.0001
All Communes	(N=1548)	0.687 (1.932)	0.042 (0.240)	0.645	t = 11.558 p < 0.0001

Two-tailed tests.

Discussion of FM's Use of Local Linear Regression as their Estimator

In section 2, we estimated the effect of assignment to different sides of the LoD on sabotage, conditional on the presence of double-track railroads. Formally, our estimand is:

$$\tau_1 = E[Y_i|D = 1, R = 1, X_i \leq z] - E[Y_i|D = 0, R = 1, X_i \leq z]$$
$$\tau_2 = E[Y_i|D = 1, R = 0, X_i \leq z] - E[Y_i|D = 0, R = 0, X_i \leq z]$$

τ indicates the effect of being assigned to the occupied zone rather than the Vichy zone, Y_i is the number of sabotage events observed in commune i, D is the treatment indicator ($D = 1$ when communes are assigned to the directly occupied zone; $D = 0$ when communes are assigned to Vichy), R indicates whether or not a commune's boundaries intersect with a double-track railroad, X_i is the distance from the centroid of each commune polygon to the LoD, and z is a bandwidth. We estimate τ separately for $R = 1$ (communes that intersect a double-track railroad) and $R = 0$ (communes that do not intersect a double-track railroad).

Best practices in estimating the causal effect of the forcing variable in RDDs are currently the object of a methodological dispute. Imbens and Kalyanaraman (2012: 938) prescribe the use of local linear regression with a triangular kernel, which assigns increasing weight to observations as the value of the forcing variable approaches the discontinuity. In the RDD context, Imbens and Lemieux (2008: 623-624) argue, the difference-of-means is a biased estimator of the average treatment effect if the slopes of the regression of the outcome variable on the forcing variable diverge in the vicinity of the discontinuity. In contrast, Dunning (2012: 158) argues in favor of using the difference-of-means estimator, noting that

> if the conditional expectation of the potential outcomes under treatment (or control) on either side of the regression discontinuity is much different, for units included in the study group, the natural experiment has failed—for it has not in fact generated as-if random assignment to treatment conditions. In this case, the assigned-to-control group is not a valid counterfactual for the assigned-to-treatment group.

We follow Dunning's advice, for two reasons. First, because FM cannot determine whether sabotage events occurring in communes intersected by the LoD took place in Vichy or the directly occupied zone, they drop from the analysis all such communes. Figure A.1 below shows the distribution of communes by distance to the LoD. Of 119 observations within 2.5 kilometers of the LoD, 106 or 89% are excluded from the analysis. Consequently, FM's analysis is not really an instance of estimating a regression function at a boundary point. This accounts for FM's puzzling choice of estimator: local linear regression with a *rectangular* kernel, which weighs all observations equally regardless of their distance to the discontinuity, in essence replicating the issue that leads Imbens and colleagues to criticize the use of the difference-in-means estimator and endorse local linear regression in its stead. As FM recognize, the triangular kernel recommended by Imbens and Kalyanaraman (2012), "assigns large weights to areas without substantial support" (Ferwerda and Miller 2014: 650). Under such circumstances, we contend, local linear regression presents no advantages vis-à-vis a simple difference-in-means estimator.

Second, and more importantly, we believe the FM study is a nearly perfect example of Dunning's conjecture. As FM show (651, Figure 3), the slope of the regression of *Sabotage* on distance to the LoD (the forcing variable) diverges sharply near the LoD, with a steeply positive slope on the occupied side and a near-zero slope on the Vichy side. Our maps clearly indicate the reason: in the departments of Cher and

Saône-et-Loire (which together account for over 70% of the sabotage events in their study) this is precisely where the LoD runs close to the railroad, with many high-sabotage communes lying right along it. In other words, the divergence of slopes FM observe at the discontinuity is capturing the confounding effects of double-track railroads.

Despite these arguments in favor of the difference-of-means estimator, and as a robustness check on our difference-of-means estimation, we present in Figure A.2 below local linear regression point estimates and confidence bands of the sabotage and fighting dependent variables on the occupation zone treatment, controlling for intersection with double-track railroads. Estimates were computed for 5 – 25 kilometer bandwidths. While controlling for railroads improves the efficiency of the estimates, the occupation zone treatment indicator is insignificant at the 90% confidence level for 45 out of 50 estimates.

Additional References

Imbens, Guido and Karthik Kalyanaraman. 2012. Optimal bandwidth choice for the regression discontinuity estimator. Review of Economic Studies 79(3): 933 – 959.

Imbens, Guido and Thomas Lemieux. 2008. Regression discontinuity designs: A guide to practice. Journal of Econometrics 142(2): 615 – 635.

Figure A.1: Distribution of communes within the 5km bandwidth by distance to the LoD (Y axis indicated number of communes)

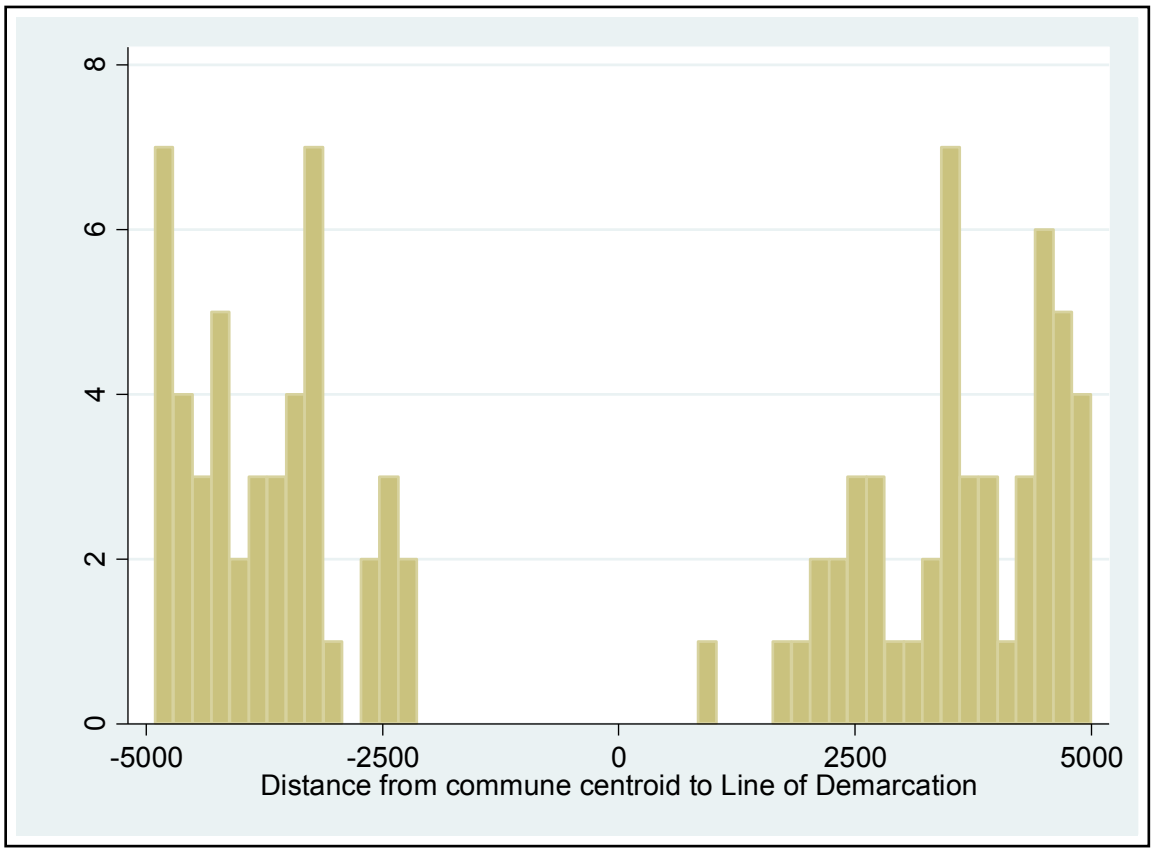
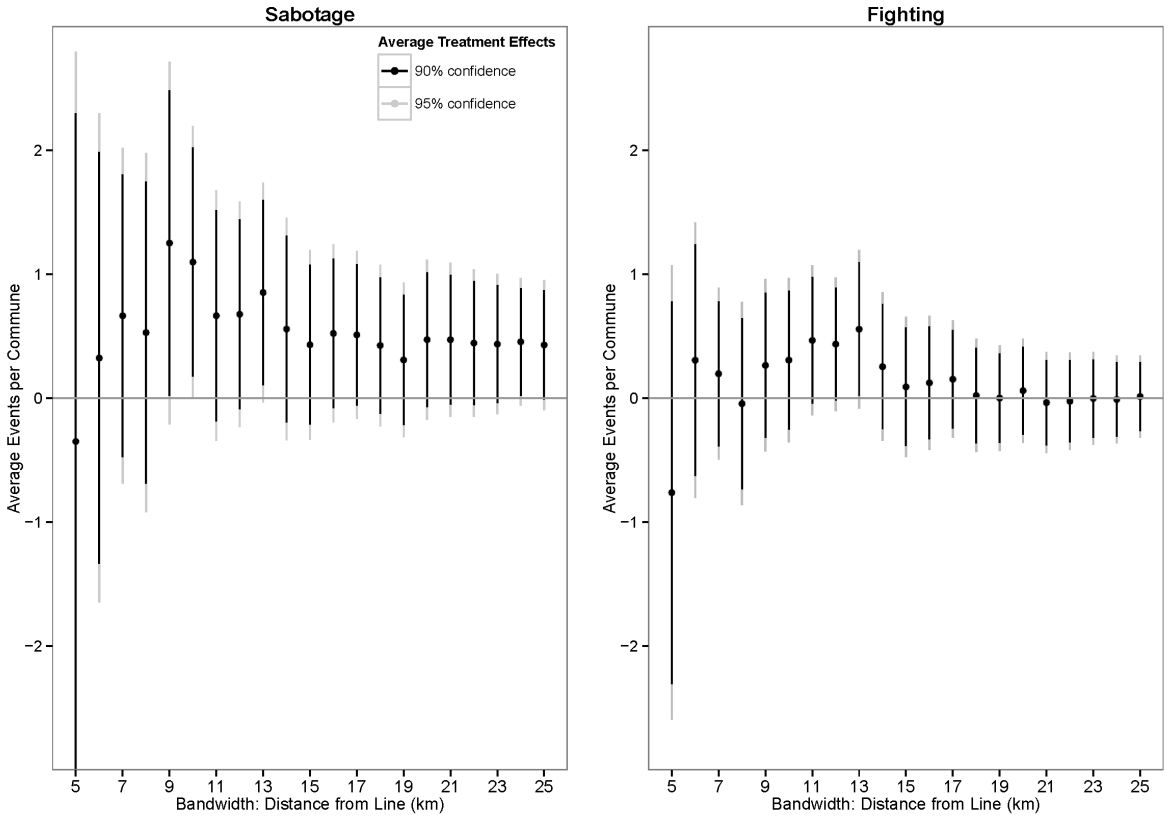


Figure A.2: Local linear regression, point estimates and confidence bands for occupation zone treatment



Appendix B: **Discussion of differences between our data and FM's replication data**

The replication data for Ferwerda and Miller (2014) became available in January 2015. As a robustness check on our results, we here discuss some discrepancies between our data and FM's data. We also replicate some of our key results using their data.

Placement of the Line of Demarcation

The first thing to note is the high degree of overlap between the two datasets. Despite the fact that FM's trace of the LoD was based on maps different from the ones we used, the coefficient of correlation between our respective indicators of the distance from commune centroids to the LoD is 0.998. Furthermore, Table B.1 below shows the correspondence between the two datasets' codings of the occupation zones. The two datasets have identical values on 1501 out of 1548 (97%) of the communes in the Charente, Cher, Saône-et-Loire, and Vienne departments. Not a single commune is coded by the two datasets as lying on opposite sides of the LoD. Every discrepant commune was coded by one research group as having boundaries intersected by the LoD, while the other group thought the entire commune lay inside either the occupied zone or the Vichy zone. The relatively small number of discrepancies (47 out of 1548, or 3%) is not surprising, given that we found several cases in which the LoD intersected with only a small sliver of a particular commune's territory; consequently, differences of 100-200 meters in the trace of the LoD could result in distinct codings. A truly complete diagnosis of the differences between our datasets is not possible, given that FM's replication materials do not include their GIS data.

--- Table B.1 ---

We are also certain that FM made several coding mistakes. Map B.1 below shows a detail from our GIS of the Departments of Charente and Vienne. We highlight the communes in these departments that FM drop from their dataset because they intersect the LoD. Given that the LoD itself was continuous, the set of communes intersected by the LoD should also have no discontinuities. But, as we can see, there are three gaps among the communes FM list as intersecting the LoD. (Two similar gaps are detectable in FM's coding of communes in the Saône-et-Loire). In other words, we can say for sure that FM included in their analysis at least five communes that, by their own coding criteria, should have been excluded. We say "at least five" because our own trace of the line intersects eight communes in these gaps.

--- Map B.1 ---

Sabotage and Fighting Events

In addition to differences in the trace of the LoD, the two datasets have somewhat different counts for sabotage and fighting events. Over 1429 common observations, the two sabotage variables are correlated at 0.891, while the two fighting variables have a correlation coefficient of 0.795. One important difference in the coding rules for the fighting variable is that we included as instances of "fighting" all events designated in the sources as "embuscade," "ataque," or "combat." It appears that FM tallied events labeled "embuscade," "ataque," and "accrochage," while excluding events labeled "combat." The source material we both worked from does a poor job of explaining how events were sorted into these categories; FM did not explain their coding protocol in sufficient granularity for us to know which set of events they considered to represent "fighting"; we were able to determine this only by examining their replication data. Consequently, we arrived at somewhat different operationalizations.

Beyond differences in the coding rules we applied, however, there are some differences between our counts and the ones FM arrived at. To some extent, such discrepancies are inevitable. The location information provided in the source material is sometimes inexact and calls for a degree of guess work. For instance, we encountered a number of events for which the location specified was not a commune. We researched those place names using a variety of sources. In some instances we were able to find villages in the specified departments with those names; in such cases, we assigned the events to the communes that contained those villages. It is easy to imagine that one group, consulting somewhat different sources, would come up with somewhat different information, resulting in slightly different counts.

Regardless of these particular issues, and given the nature of the sources, we are unlikely to generate a final consensus on coding. Thus, it makes sense to replicate our analysis using FM's data. Tables B.2 and B.3 are identical to Tables 2 and 3 from our main text, but in place of our data, we use FM's sabotage measure, their coding of the occupation zones, and their coding of the distance from commune centroids to the LoD. Since FM did not code double-track railroads, we use our own variable. There are some differences, but overall the results are quite similar to the ones we obtained using our own data. Holding double-track railroads constant (Table B.2), there is no evidence that the occupation zones explain variation in commune-level rates of sabotage. In contrast, when we hold the occupation zones constant (Table B.3), communes with double-track railroads had a vastly higher rate of sabotage than communes without them.

--- Tables B.2 and B.3 ---

In short, while there are some coding differences between FM's data and ours, the differences are, for the most part, consistent with what one would expect from different research teams working from imperfect primary sources. Overall, there is no indication that our results are sensitive to coding differences. The criticisms we laid out in this paper are also supported by results based on FM's replication data.

Map B.1: Communes identified by FM as intersecting the LoD and therefore dropped from their analysis in the departments of Charente and Vienne

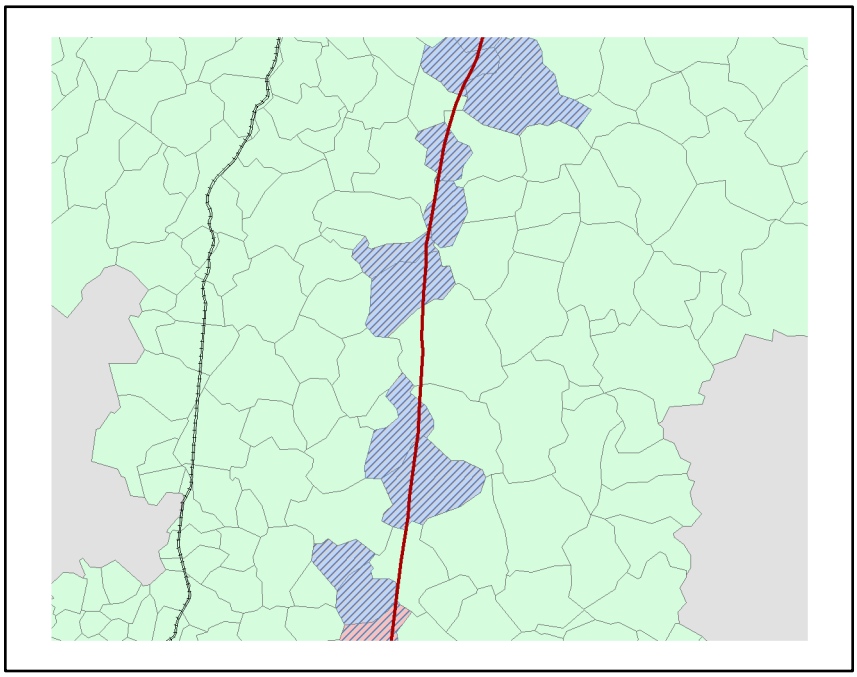


Table B.1: Correspondence of occupation zone coding between Ferwerda/Miller (FM) and Kocher/Monteiro (KM) datasets.

	Occupied (KM)	Vichy (KM)	Intersecting (KM)	Total
Occupied (FM)	819	0	20	839
Vichy (FM)	0	581	9	590
Intersecting (FM)	13	5	101	119
Total	832	586	130	1548

Table B.2: Difference of means in *Sabotage* events between occupied and Vichy zones, conditional on intersection with a double-track railroad, with communes intersecting the LoD dropped (FM's replication data)

		Mean of *Sabotage*, Occupied Zone (std. dev.)	Mean of *Sabotage*, Vichy Zone (std. dev.)	Difference of means, Occupied – Vichy	T-test (unequal variances)
5km Bandwidth	*Double track* = 1 (N = 31)	3.409 (5.612)	2.333 (3.606)	1.076	t = 0.634 p=0.532
	Double track = 0 (N = 72)	0.242 (0.902)	0.026 (0.160)	0.217	t = 1.362 p=0.182
10km Bandwidth	*Double track* = 1 (N = 71)	2.811 (5.382)	2.611 (3.712)	0.200	t = 0.175 p = 0.862
	Double track = 0 (N = 237)	0.123 (0.547)	0.092 (0.381)	0.031	t = 0.495 p = 0.621
20km Bandwidth	*Double track* = 1 (N = 139)	2.566 (5.020)	2.758 (4.381)	-0.192	t = -0.212 p = 0.833
	Double track = 0 (N = 571)	0.102 (0.435)	0.154 (0.595)	-0.053	t = -1.215 p = 0.225
All Communes	*Double track* = 1 (N = 240)	1.813 (4.167)	1.813 (4.167)	-0.254	t = -0.494 p = 0.622
	Double track = 0 (N=1187)	0.132 (0.687)	0.128 (0.524)	0.004	t = 0.123 p = 0.902

Two-tailed tests.

Table B.3: Difference of means in *Sabotage* between communes with and without double-track railroads, conditional on zone of occupation (FM's replication data)

		Mean of *Sabotage*, *Double track* = 1 (std. dev.)	Mean of *Sabotage* | *Double track* = 0 (std. dev.)	Difference of means, *[Double track = 1] – [Double track = 0]*	T-test (unequal variances)
Communes intersecting the LoD dropped					
5km Bandwidth	Occupied (N = 55)	3.409 (5.612)	0.242 (0.902)	3.167	t = 2.624 p < 0.05
	Vichy (N = 48)	2.333 (3.606)	0.026 (0.160)	2.308	t = 1.920 p < 0.1
10km Bandwidth	Occupied (N = 159)	2.811 (5.382)	0.123 (0.547)	2.689	t = 3.628 p < 0.001
	Vichy (N = 149)	2.611 (3.712)	0.092 (0.381)	2.520	t = 2.877 p < 0.05
20km Bandwidth	Occupied (N = 372)	2.566 (5.020)	0.102 (0.435)	2.465	t = 5.047 p < 0.0001
	Vichy (N = 338)	2.758 (4.381)	0.154 (0.595)	2.603	t = 3.411 p < 0.01
All Communes	Occupied (N = 839)	1.813 (4.167)	0.132 (0.687)	1.681	t = 5.180 p < 0.0001
	Vichy (N = 590)	2.068 (3.442)	0.128 (0.524)	1.940	t = 4.840 p < 0.0001

Two-tailed tests.

Appendix C

In July 2015, Ferwerda and Miller (FM) released a detailed response to our criticisms, titled "Rail Lines and Demarcation Lines: A Response" (henceforth, FMR). Here we evaluate the arguments they make in their reply.

The Line of Demarcation: a natural experiment?

The basic strategy of FM's research design will be familiar to anyone acquainted with the fundamentals of design-based inference. FM identify a sharp discontinuity in the data, in this case a spatial one, that corresponds to the Line of Demarcation (LoD) created by the Franco-German Armistice of 1940. They assert that the route of the LoD was locally arbitrary, such that assignment of French communes to either side was as good as random. Consequently, in close proximity to the LoD, comparing the post-treatment values of variables on one side of the line to the post-treatment value of the same variables on the other side of the line constitutes a natural experiment.

This research design would be fatally compromised if the discontinuity at the LoD happens to correspond to one or more sharp discontinuities in pre-treatment variables. We identified just such a break in the data: double-track railway lines that ran close to the LoD on one side, but not the other. Our maps (KM, Maps 2 – 4) make it clear that the LoD closely followed the contours of the double-track railway network, and our statistical tests show that assignment to the German side of the LoD is an excellent predictor of (i) whether or not a commune intersected a double-track railroad (KM, Table 1), and (ii) how far each commune lay from the nearest double-track railroad (KM, Table A.2).[1] If the location of the LoD had been assigned randomly, the probability that we would observe these statistical associations with a pre-treatment variable is extremely small. Moreover, the new data FM present in their response to our critique (FMR, Table A.1) shows that multiple-track railroads were massively unbalanced, with 2.5 to 4 times as many kilometers of track on the German-occupied side as on the Vichy side of the LoD. FM do not dispute any of these results; on the contrary, they bring entirely new sources of data to bear in support of these claims.

In our critique, we argue that the local association between double-track railroads and the LoD was not an accident. We identified multiple historical sources indicating that the Germans had a keen interest in keeping specific railway connections in the part of France they would occupy directly. For strategic-level consultations, these references are remarkably specific. In the crucial meeting between Hitler, Keitel, and Jodl documented by Böhme – just three days before the German and French delegations met to discuss terms – Hitler laid out seven "guidelines" (*Richtlinien*) for an armistice. Only one of these conditions concerned the specific shape of the planned occupation zones, and it focused solely on the maintenance of "connections" (*Verbindungen*) through central France and down the French Atlantic coast. Specifically, Böhme (21; quoted in KM, 4) writes: "The envisioned demarcation line between occupied and unoccupied territory was drawn on a map by General Jodl. In the course of doing so, attention was paid to ensuring that the East-West connection through central France that went from Belfort through Dôle-Le Creusot-Moulins-Bourges-Tours to Nantes, and the North-South connection from Tours through Angoulême-Bordeaux to the Spanish border, would run within the territory to be occupied." Within days of this meeting, Hitler made it clear in diplomatic exchanges with Mussolini and agents of the Spanish dictator Francisco Franco that the connections he placed such a high priority on were, in fact, railroads. He specifically mentioned the Paris-Tours-Bordeaux line. Also, as we show in our rebuttal, the press reporting

[1] Citations to "KM" refer to the body of this rejoinder

on the Armistice that appeared before the LoD was implemented is quite specific that the boundary was to be placed approximately 20 kilometers east of the Paris-Tours-Bordeaux railroad.

FM concede that the North-South "connection" was a railroad, but they insist that we don't really know if the East-West connection through central France was intended to be a railroad: "[I]t may be reasonably interpreted as a railway, but it is also possible that Boehme was referring to a line of communication or road connection" (FMR, 9, fn. 23). Why would Böhme use the same word (*Verbindung*), *in the same sentence*, to refer to a North-South railroad and some other kind of East-West connection? Since all of the towns mentioned (Belfort, Le Creusot, Moulins, Bourges, Tours, Nantes) lie on the network of double-track trunks lines, *and* they form an almost-direct route across central France, *and* there is no motorway that could serve this purpose, *and* the WWII German military transported their armies and material over rail networks at strategic distances, by far the most straightforward interpretation is that Böhme was talking about a railroad.

The foregoing makes it clear that keeping these two railroad connections inside their occupation zone was a priority for the German high command in shaping the LoD. In their response, FM point to evidence from several sources indicating that the LoD was adjusted locally by German commanders on the ground or altered over time by negotiations through the Franco-German Armistice Commission in Wiesbaden. They assert that "a range of idiosyncratic factors determined the line's placement in the departments we study" (FMR, 6), and conclude that "the demarcation line was not *singularly* determined by the location of strategic railroads" (FMR, 4, emphasis added).

We agree that strategic railroads were not *the only* factor that determined the local placement of the LoD. The strategic priorities articulated by the OKW had to be implemented by lower-ranking officers. The minor modifications FM identified might have made the LoD less faithful to Hitler's "master plan," but they might also have reflected local commanders' attempts to better execute the Führer's eminently rational objectives on the ground. Here it is crucial to keep in mind that the precise contours of the LoD were not *uniquely* well-suited to maintaining these specific railroad links through France; *any* route that moved the boundary further south or east, and some routes that nudged the boundary further west, would also have kept these railroads inside the German zone. However, the Germans wanted to maintain a sovereign France. To do that, they needed the French to agree to an armistice. Thus, it was in their interest to grant as large an unoccupied territory as would be consistent with their other strategic aims. Minor adjustments to the route of the LoD of a few kilometers here or there, of which there were a number we know about, would not and did not change its overall shape.[2] That *other* priorities came into play is not terribly important unless FM can show that those other rationales overruled German concerns about railroads. They cannot, which fatally undermines their identification strategy.

Finally, on this point, FM call our attention to the fact that the Paris-Tours-Bordeaux railway – which we claimed determined the LoD's placement in Charente and Vienne – lay 20 kilometers away from the LoD. Since some of FM's tests use bandwidths narrower than 20 kilometers, they argue that this cannot have affected their overall results (FMR, 15). In response, we make two points. First, in their original article, FM include a number of statistical tests using "several bandwidths ranging from a distance of 10 km to 25 km from the demarcation line" (FM, 650). The reason they do this is, as they acknowledge, that "regression discontinuity designs are highly sensitive to the choice of bandwidth" (FM, 650). One of the apparent

[2] This is confirmed by the two examples of "vagaries" of the local placement of LoD that FM include in their response as figure A3. In neither of these situations did the strategic railway lines change side.

strengths of their original findings is the consistency of their results over these multiple bandwidths. If, as their reply now suggests, some of their tests at wider bandwidths should be discarded because of the imbalance we detected in the presence of strategic railways, the overall strength of their findings would be considerable weakened.[3] Second, although, as FM claim in their response (FMR, 15), communes within 10 kilometers of the LoD in the Charente and the Vienne did not intersect double-track railroads, it does not follow that the imbalance in these railroad does not affect FM's results. As we discuss in detail below, a location closer to major railroads is likely to be associated with many additional pre-treatment variables for which FM do not control. Therefore, proximity to the strategic railways we identified might have led to higher incidences of violent resistance activity. In short, to accept that the LoD in Charente and Vienne was placed with the Paris-Tours-Bordeaux railroad in mind, as FM now do, is not a trivial concession on their part.

Summing up, the evidence that the LoD was designed to keep important railroad connections inside the German zone is overwhelming. But, what if FM are right in spite of the evidence? What if the local placement of the LoD was really the result of a welter of small, "idiosyncratic," and non-systematic causes? In point of fact, their research design would still be invalidated if, by a highly improbable accident of fate, the LoD *just happened* to have ended up located right along double-track railroads that were prime targets of the Resistance. The maps and statistical tests – not only ours but also FM's – are clear that *at least* the latter scenario must be true. This is a problem, not only because of the presence of these railroads, but also because their placement is almost certainly associated with the location of a great many other things – population centers, waterways, roads, industrial facilities, etc. – that might influence the occurrence of violent resistance. So our criticism of FM's research design is twofold. First, we can show that the LoD corresponds to at least one important pre-treatment discontinuity. Our statistical tests show that the association between the LoD and double-track railways is extremely unlikely to have occurred by chance. Second, given the historical *and* statistical evidence, it is highly improbable that the Germans had no systematic rationale for the local placement of the LoD.

Does FM's theory explain variation in resistance during WWII France?

Even though the failure of their identification strategy greatly undermines the confidence we can have in their causal claims, FM could of course still be right that the difference in political institutions between the two zones was an important determinant of violent resistance. In their response, they attempt to make this case by controlling statistically for single- *and* double-track railroads and showing that, when they do so, they "observe significantly elevated levels of resistance activity within the German zone" (FMR, 3).

Before we move on to discuss FM's specific analysis, let us review what we know so far about the role of railroads in shaping the patterns of violent resistance. None of these points are disputed by FM. First, we know that a large percentage (at least 66%) of the sabotage attacks FM studied were directed against railroads. Second, we know that the Resistance (and the Allies with whom they were cooperating) had good reason to attack the strategic railroads that were the Germans' principal means of moving troops and supplies around France. We know that, in preparation for the D-Day landings, explicit plans were put in

[3] As is clear from the maps, the LoD also bulged in two locations to within much closer than 20 km of the railroad in the Charente and the Vienne. Consequently, a number of communes located less than 20 kilometers from the LoD did intersect the Paris-Tours-Bordeaux railroad. This is an additional cause for concern regarding FM's local linear regression estimates at bandwidths between 10 and 20 kilometers.

place to attack French railroads in order to cut off the Normandy beaches from reinforcement and resupply, and to prevent German troops from escaping France when the front collapsed. Third, we know that double-track railroads were massively unbalanced in the vicinity of the LoD in the departments FM studied (KM, Table A.1; FMR, Table A.1). Finally, we know from both maps (KM, Maps 5 – 7) and statistical tests (KM, Table 3) that communes with double-track railroads running through them were *vastly* more likely to have sabotage attacks than communes without such railroads. These differences are gargantuan, dwarfing the effects FM identify for political institutions. Although only 17% of communes in FM's four departments had double-track railroads going through them, 78% of the sabotage attacks happened in those communes.

In our critique of FM's article, we compare the rate of sabotage on the two sides of the LoD while controlling for the presence of double-track railroads. We find little evidence of more attacks on the directly-occupied side of the LoD conditional on intersection with double-track railroads (KM, Table 2). FM also test for a difference in the rate of sabotage per kilometer of multiple-track railroad (FMR, Table A.1) and find that differences between the two zones are not statistically significant at any of their bandwidths.[4]

The crux of FM's response to our rebuttal is to assert that, if we control statistically for something *else*, something we (Kocher and Monteiro) have explicitly asserted is unlikely to matter, then sabotage attacks "remain elevated" on the German side of the line. FM's response involves three steps. First, rather than conditioning on whether or not each commune intersected a railroad, they measure kilometers of track running through each commune. This is a reasonable alternative measurement. Second, they examine only sabotage against railroads, rather than all sabotage attacks as they did in their original article; they offer no explanation for changing the dependent variable and no tests with the original dependent variable. We are agnostic about this change; we were reluctant to base our own analysis solely on railroad sabotage because FM's article examined sabotage against all targets. Third, FM carry out a whole series of statistical tests conditional on *total* kilometers of railroad track. To account for the difference between single- and double-track railroads, they multiply double-track kilometers by two.

We should start by revisiting why we took the approach we did: conditioning on the presence of double-track but not single-track railways. In 1940-44, France was a country with a very extensive rail network. Not all of the lines in this network were of equal importance. Some were intended to carry passengers and freight over long distances, while others linked smaller population centers to the national network. The most important lines in the network, its "trunk" lines, tended to be double-tracked. The crucial point here is that these railways were not important because they had two tracks. Rather, they had two tracks because of the economic or strategic important of the places they connected. The route through central France that Hitler directed his generals to keep in the occupied zone began in Belfort (on the German frontier) and terminated in Nantes (near the Atlantic coast, at the base of the Breton peninsula). It was therefore useful in a quite general way for controlling the French Atlantic coast. It also intersected with multiple north-south rail lines that were useful for controlling southern France down to the Spanish border and the Mediterranean.[5] By contrast, the single-track railroad between St. Saviol and Le Vigeant in the Vienne was useful only for controlling those two strategically inconsequential towns.

[4] To be more specific, their t-values imply that a higher sabotage count on either side of the LoD is statistically credible. In addition, their point estimate for the 20 kilometer bandwidth indicates a higher sabotage rate on the Vichy side.

[5] One implication of this point is that down-scaling a double-track railroad to single-track did not necessarily make it strategically unimportant. If the railroad section running from Paray-le-Monial to Montchanin was downgraded (FM

Double-track lines were not, therefore, "twice as important" as single-track lines, as implied by FM's decision to multiply their length by two. It would be closer to accurate to say that double-track lines mattered a lot and single-track lines hardly mattered at all.[6] Multiplying double-track length by two is thus a totally arbitrary decision with no basis in theory or the available evidence. Furthermore, since single-track railways were relatively ubiquitous in the territory FM examine, including their track length in the analysis dilutes the strategic importance of the main railway lines for the Germans and, consequently, for the Resistance.

FM defend their decision to pool all railroads in the departments of Charente, Cher, Saône-et-Loire, and Vienne by pointing out that single- and double-track lines are likely to be statistically associated, because they are part of a network – i.e., they must connect at some point. Thus, they say, controlling for double-track lines while failing to control for single-track lines will introduce bias. After all, in communes where double-track and single-track railways co-occur, it is impossible to know if a Resistance attack was directed at the big railway to somewhere or the little railroad to nowhere (FMR, 14).

There are certainly places where double- and single-track railways co-occurred in these departments. However, given that most of the double-track was on the German-occupied side, most of the communes with both types of railroads were *also* on the German-occupied side. For instance, in Charente and Vienne, there were literally *zero* kilometers of double-track on the Vichy side of the line, from which it follows that all communes with more than zero kilometers of single-track *and* more than zero kilometers of double-track were also on the German-occupied side of the LoD. According to our data, of the 18 communes in FM's four departments in which a single-track line intersected with a double-track line, 14 were located in the German-occupied zone. These communes had a very high rate of sabotage, which reflects the strategic value of junctions. Thus, explicitly conditioning on the coincidence of single-track and double-track lines gives additional evidence *against* FM's argument, rather than in favor of it.

There is a more fundamental problem, however. Given the evidence we presented in KM about the strategic rationale behind the location of the LoD, it is important to keep in mind that we are no longer in the Elysium of design-based inference; we've gone to messy-regression-land. Railroads could be the only confounding variable to account for, but how likely is that? The whole thrust of design-based inference is to avoid having to make such tendentious assumptions. Although in FMs view there were multiple factors determining the placement of the LoD, they now want to assume that all of those reasons are small, "idiosyncratic," and non-systematic. How likely is that?

If FM's goal is to create a statistical model of sabotage in WWII France, then there is much more work to be done, because many variables are statistically associated with double-track railroads, and a number of those variables also probably account for some portion of the variation in sabotage attacks. We offer some examples in our rebuttal: "Route Nationale 10, the most important motorway in Charente and Vienne, followed the path of the Paris-Bordeaux railroad, which connected the regional hubs of Châtellerault,

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say this was planned, but they do not know if it ever happened), its position in the network could still have made it vital.

[6] The deeper problem is that, of course, all double-track lines were not equally important for all strategic and tactical purposes. Likewise, some single-track lines may have been of great strategic importance in some parts of France at particular points in time. Thus, double-track lines are an imperfect quantitative proxy for the strategic problem faced by a continental power attempting to hold France against a maritime invader.

Poitiers, and Angoulême. The Vichy zone in these departments contained no cities of comparable size. In the Saône-et-Loire, the town of Le Creusot, located near the Tours-Belfort railway, contained France's most important munitions factories, which Hitler explicitly wished to keep inside the occupied zone" (KM, 9, fn. 19). It is not difficult to think of many more. Railway lines are not built just anywhere; they often connect existing cities and towns. They influence economic and population growth as people and industries move to locations where transport and shipping is convenient. They require extensive rights-of-way, which then tend to accrue additional linear infrastructure: fuel and water pipelines; electricity and telephone wires. Moreover, natural geography dictates that some routes are more feasible and less costly than others. Civil engineers try to route them next to water courses and along the natural contours of the land to avoid building costly tunnels and bridges. Thus, the plausible target set for sabotage is endogenous to railways, which are in turn endogenous to other variables that themselves plausibly influence the target set. Controlling for single-track railroad in addition to double-track railroads is not going to solve this problem. FM promised us clean identification, because the LoD was supposedly "as good as random." But clearly it was not. Because the Germans cared about key strategic railroads, they positioned the LoD along a route that created many important differences between the two zones.

In our rebuttal, we did not pretend for a moment to have provided the true causal model of sabotage in WWII France – or even a plausible identification strategy. At the same time, we have extremely good evidence that the Resistance intended to sabotage important railways, and that the vast majority of their sabotage attacks in fact happened in communes through which these important railways passed, during a time at which railway sabotage was of great military value to the Allies. FM are certainly correct that, in the vicinity of the LoD, more of these attacks occurred on the German side than on the Vichy side. But, as our maps show very clearly, these attacks were also clustered in a peculiar way, in spatial "strips." These strips just happen to correspond to the routes of some of the double-tracked trunk lines of the French railway system. In the vicinity of the LoD, lines of that type just happen to be far more prevalent (2.5 to 4 times as prevalent, depending on the bandwidth) on the German-occupied side. To convincingly rebut our critique by showing that something *else* (not double-track railroads) can explain the data, FM should ideally be able to account for this odd pattern of spatial clustering. Single-track railroad lines are not the answer, because they did not follow the same strips of land. The effects of the LoD itself might be a candidate, except that the strips of sabotage sometimes lie close to the LoD and sometimes do not. In point of fact, the strips lie close to the LoD only when a double-track railroad line *also* runs close to the LoD.

In sum, pooling single- and double-track railroads into a single variable, as FM have done, is an arbitrary modeling choice that obscures a crucial relationship we identified in the data. More importantly, once FM concede the necessity of controlling statistically for this particular variable, one may reasonable ask why we should stop there. FM dismiss the usefulness of the statistical model of railroad sabotage we provide for all France, writing that "findings on overall resistance levels can only be interpreted as causal if we believe they have controlled for all relevant confounders associated with respect to occupation zone and resistance" (FMR, 4). This objection applies with equal force to the analysis they conduct in their response.

The strategic logic of violent resistance in WWII France

In the third and final section of our rebuttal, we showed that the overwhelming majority (95%) of FM's data recorded events that occurred after the LoD was dismantled in early 1943, when Resistance groups could operate unimpeded by any physical barriers separating the two zones. We also demonstrated that more than half (59%) of the events recorded in FM's data occurred after the D-Day landings, when political rule was in flux and Resistance efforts were coordinated with and by the Allied powers. We showed this by

deploying a monthly chronology of the data as well as detailed local histories of two of the four departments FM study – the Vienne and the Saône-et-Loire – which together account for 80% of FM's data (Calmon 2000; Veyret 2001). These histories provide substantial detail on both (i) the role played by attacks on strategic railways and (ii) that played by Allied coordination efforts. When taken together, these factors show that FM's data was ill suited to test theories that connect the location of a Resistance attack with the geographic origins and motives of their perpetrators. Much of the violence FM observe is part of a coordinated effort to hit strategic railways in order to hinder the movement of German troops and materiel once conventional fighting in France resumed in June 1944.

Responding to this rich local evidence, FM make three claims. First, they argue that resistance in their departments of interest "was driven by local factors and began before D-Day" (FMR, 4). Specifically, they argue, "locally rooted resistance groups had emerged across France long before [the Allied landings of June 1944] and before the resistance was nationally unified" (FMR, 18). In support of this claim, FM cite two works. The first is Kedward's (1993, 163) general study of rural resistance in southern France, which emphasizes the role of "local objectives, local organization, and still more, local constraints, remained paramount" even in 1944. The second is a study of the Gironde – a department that is not included in their analysis or our rebuttal – where "more than 80% [of resistance members] were local residents, with the majority organized within local units" (FMR, 19).

Let us start by noting that, in KM, we did not dispute that the Resistance had emerged locally long before D-Day and consisted mostly of local members. Most Resistance groups were in fact formed earlier in the war; and most Resistance members, unsurprisingly, joined the Resistance where they lived. These facts are hardly relevant for the matter at hand. What matters is that, in the four departments FM study, the Resistance did not conduct much violent activity before the LoD was abolished in early 1943. Per our count, only 26 of the 547 sabotage attacks recorded in the data took place during this early period. The overwhelming majority of the attacks were perpetrated when the LoD was no longer a physical impediment to Resistance forces acting across the two zones. This trend in violent activity supports our view that the Resistance grew engaged in violent action in preparation for, and particularly in the aftermath of, the Allied landings of 1944, benefiting from Allied support and coordination. Since we have established that within the bandwidths FM study, the German zone was vastly richer in targets that were of great strategic importance for the Allied effort to liberate France, FM's counter depends on their ability to establish that despite this imbalance, Resistance efforts were so "local" that operatives would be unwilling or unable to travel a few kilometers from within Vichy territory to German-occupied territory in close proximity to the LoD in order to perpetrate an attack. For FM's counter to work, the local objectives, organization, and constraints that Kedward mentions must be "local" at a scale that prevents them from operating across the small distances that separate localities within FM's bandwidths across the LoD. Is this plausible?

To answer this question we must examine FM's second counter, their claim that "even in 1944, the border between zones still affected resistance activity" (FMR, 17). How so? Other than an orthogonal claim about how in the Gironde – again, a department not included in their original study or our rebuttal – "the demarcation line represented an ideological break as well as a frontier for the organization of the Resistance," all FM have to offer about the four departments they study is that in the Saône-et-Loire "the resistance was organized differently in the two zones, even after 1942" (FMR, 17, citing Veyret 2001, 16, 46).

We have little doubt that being under direct German occupation or a subject of Vichy France produced until the end of the war some ideological and organizational differences between Resistance groups

depending on where they were based. But, once again, this is largely irrelevant for the matter at hand. What matters is whether the higher level of railway sabotage we observe in the German zone – where the railways were located within FM's narrow bandwidths – can be imputed to local groups originating on the same side of the LoD. It cannot. Given that the LoD no longer posed a significant physical hurdle and that, after D-Day, conditions on the ground were fluid, it is implausible that the sabotage attacks against these railways – which drive FM's findings – can be imputed to Resistance groups originating solely on the German side of the line.

This poses a serious problem for testing FM's theory. In their response, FM object that "it is incorrect that our theory strictly requires that attacks by perpetrated by groups originating in the same zone" (FMR, 17). As they wrote in their original article, because "effective insurgency requires the aid of the local population," it is no problem "if some spillover did occur *due to increased motivation to resist in the German zone*" (FM, 652, emphasis added). The problem for FM stems from the fact that, as our criticism conclusively establishes, the spillover did *not* occur due to increased motivation to resist in the German zone. Rather, the spillover was the result of a great relative abundance of strategic targets in that zone, namely, the presence of railway lines vital to the German war effort. It is impossible to tell whether these lines were being hit by Resistance operatives based in their immediate vicinity or a few kilometers farther away, in Vichy territory.[7]

Finally, FM present what they consider their "most important" objection to our claims on the role of Allied coordination of Resistance attacks in the territory they study. They object that "Plan Vert, the major coordinated campaign for destroying French railways that Kocher and Monteiro specifically reference, identified relatively few targets in the four departments we analyze" (FMR, 19). They cite a list of targets found in Durand's (1968, 432-434) history of French railroads during the war, which includes only two communes (Poitiers and Angoulême) in the sample of localities on which they tested their argument. Since both of these towns are located approximately 15 kilometers from the LoD, they were not included in FM's 5- and 10-kilometer bandwidth estimates.[8] Hence, they conclude that Allied coordination is "insufficient to explain the patterns of resistance" observed within their sample.

As we mentioned in KM (12), the Allies developed two plans organizing Resistance efforts against French railways in the aftermath of the D-Day landings. The first was *Plan Vert*, which targeted railway lines. The second was *Plan Grenouille*, which aimed to sabotage hoists (*appareils de levage*) and turntables (*plaques*

[7] Attempting an intellectual jujitsu move, FM find the criticisms we lay out in KM against using the location of Resistance events to infer the political motivations of the insurgents to be "puzzling" (FMR, 18), since in our own work (Kocher et al. 2013) we do the same. FM are correct that in Kocher et al. (2013) we use the location of attacks to infer their political motivation. There is, however, a crucial difference between FM's use of location to infer motivation and ours. As we argued in KM (12), the difference has to do with the scale of the study. FM use a micro slice of territory – a narrow bandwidth around the LoD. We cover the entirety of French territory. At this macro level, "it is implausible that most attacks deep in the directly occupied zone were perpetrated by maquis originating deep in Vichy – and vice versa" (KM, 12). Clearly, most Resistance groups were not traveling hundreds of miles across France to conduct their business. At the micro scale FM use in their study, however, it is a highly plausible conjecture that attacks occurring on one side of the LoD (which was by the time the overwhelming majority of these attacks took place an administrative border but not a physical barrier) were perpetrated by Resistance groups originating from across the LoD *particularly in areas where strategic targets were vastly more available on one side*, as is the case with the territory FM study.

[8] A third commune, Vierzon, was dropped from FM's analysis because their maps indicate that its boundaries intersected the LoD.

tournantes) in selected railroad depots. Durand's target list refers to *Plan Grenouille*; he does not provide a list of the targets included in the much broader *Plan Vert*. Nevertheless, Durand does quote extensively from an assessment of the effects of *Plan Vert* produced by the French General Bondil, who says that important objectives were met in Charente, Vienne, and Saône-et-Loire (Durand 1968, 438-439): "In the Charente and the Vienne, 200 trains were immobilized on June 21 [1944] for lack of locomotives. … In the Saône-et-Loire, the important railway centers of Chalon-sur-Saône and Paray-le-Monial were rendered unusable." (Both these railroad hubs are located in the formerly occupied territory FM study, in communes intersected by the LoD, and therefore dropped from FM's analysis.) Durand's text also gives ample evidence of systematic coordination between the Allies and the Resistance in their efforts to sabotage French railways in the immediate aftermath of D-Day, as we indicated in our rebuttal.

FM are certainly correct that the Resistance had a great deal of local agency in selecting targets. As Durand (1968, 426-428) notes, the locations chosen for sabotage as part of the Allies' *Plan Vert* were "susceptible of modification" on the ground and "a great deal of initiative would be left to the operators," particularly as to where exactly to hit a railway line the *Plan* deemed a sabotage target. In this sense, *Plan Vert* was more "a rational orientation than an imperative order" (Durand, 1968, 426). In other words, the Allies provided assistance and coordination to local agents in pursuing the mutually useful objective of limiting German supply lines and troop movements. In the territory FM study, however, the railroad targets that fulfilled these common strategic objectives were much more likely to be available on the formerly occupied side of the LoD. Hence, that is where the *maquis* tended to strike. In the presence of such unequivocal evidence of the importance of a coordinated effort to target major railway lines, vague claims about the role of local agency are not particularly relevant to evaluating our counter to FM's study. Unless FM can show that local motivations systematically led Resistance groups to pursue agendas that were orthogonal to the Allied plans, our criticism stands.

Conclusion

In the conclusion to their response, FM offer a general defense of the value of design-based inference with which we broadly concur. In particular, we agree that natural experiments are valuable, in part, precisely because their "underlying assumptions are explicit and transparent" (FMR, 20) enough to be susceptible to clean refutation, which is often not the case with observational research. Where we differ is more a matter of degree than kind. FM imply that an "imperfect" natural experiment is better than no natural experiment at all. All we can say is that it depends on the degree of imperfection. If a massive discontinuity in important pre-treatment variables lies in very nearly the same place as the discontinuity that determines treatment assignment – as is the case with FM's study – then a natural experiment is worse than no natural experiment because it will produce false certainty on highly biased estimators of causal parameters.

In our view, FM put the challenge of design-based inference in terms that are too stark. They say that researchers have to choose between controlling for all possible confounding variables and exploiting an exogenous source of random variation. We think researchers have more options, and we believe they can exercise more than one of them in the same study. In our rebuttal, we use several distinctive types of evidence (documents, secondary histories, maps, and statistical tests at multiple levels of aggregation) to produce a consistent and highly credible account of the distribution of violent resistance in the section of WWII France that FM analyzed in their paper. This account demonstrates that FM's argument is not supported by the data, a conclusion we maintain.

It is true that our approach does not allow us to precisely estimate the causal effect of a specific treatment: the presence of double-track railways. In this instance, however, it is not entirely clear what would be the use of such an estimate. Presumably, the strategic importance of railways and other kinds of infrastructure to armed groups varies tremendously by conflict, region, technology, era, and type of warfare, among other factors. We suspect this is also true for institutions of the sort FM investigate. If this is the case, then we have even more reason to be catholic in our approach to method.