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War and city size: The asymmetric effects of the Spanish Civil War

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Abstract

Populations are affected by shocks of different kinds, and wars, a priori, may be among the most prominent. This article studies the effect of the Spanish Civil War (1936–1939) shock on the distribution of population, especially on cities. One of the main contributions of this study is that it underlines the importance of distinguishing between winning and losing sides, an aspect which until now has been largely overlooked. While previous research on war shocks has also tended to be concerned with inter-state wars, this paper concentrates on a civil war. We take advantage of a new, long-term, annual data set. Our results show that, overall, the Spanish Civil War did not have a significant effect on city growth. However, we also find a significant and negative effect in the growth of cities that aligned themselves with the losing side. These results are robust to heterogeneity in the effect of the war shock, measured as war severity and duration. Although short lived, the temporary effect on growth results in a permanent effect on the size of cities on the losing side.

KEYWORDS

asymmetric effects, city growth, city size, severity and duration of war, Spanish Civil War, war shocks

1 | INTRODUCTION

Recently, there has been growing interest in the effect of different types of shocks on city populations, such as those produced by biological and climatological disasters, economic crises, and high immigration flows (e.g., Bakker et al., 2020; Baltagi et al., 2023; Bosker et al., 2008a; Dijkstra et al., 2015; Glaeser, 2011; Sastry & Gregory, 2014; Xu & Wang, 2019).[1] However, few studies have attempted to analyze the impact of war on population, especially on urban structure (see Hanlon & Heblich, 2022). The key question is to what extent war shocks have transitory or more permanent effects on the distribution of urban populations. In other words, are major events such as war able to alter city size distribution, which appears to emerge as remarkably stable in most countries? (e.g., Black & Henderson, 2003; Eaton & Eckstein, 1997).

Davis and Weinstein's (2002) study on the bombing of Japanese cities during World War II first proposed that the impact on relative city size was temporary, as most cities returned to their prewar position in the distribution within about 15 years. Similar results were found by Brakman et al. (2004) in their study on the bombing of German cities during World War II. Although the re-examination of this case by Bosker et al. (2007) and, principally, Bosker et al. (2008b) gave support to the existence of permanent effects.[2] Ciccone (2021) has also shown a persistent effect of at least 15 years after World War I in the historical German state of Württemberg. Moving from inter-state, 20th-century, wars to the US Civil War (1861–1865), Sanso-Navarro et al. (2015) findings suggest that the effects of the shock on city growth were transitory. Whereas Hanlon's (2017) analysis of the consequences of the US Civil War on British cities shows that the exogenous shock was of a persistent nature. Therefore, the few existing studies, which have concentrated on only a few wars, turn up ambiguous results, and a consensus is still far from being reached.[3]

The main contributions of this study are twofold. First, it underlines the importance of distinguishing between winning and losing sides, an aspect that has been largely overlooked when analyzing war shocks in the previous literature. We evaluate to what extent the shock was asymmetric. Directly after the outbreak of the Spanish Civil War (hereafter SCW) in 1936, the capital cities, our main spatial unit of analysis, were taken by one side or the other. As the war progressed, the frontlines inevitably shifted in favor of the, ultimately, winning side. Our hypothesis is that the SCW could have affected those capital cities that the losing side was unable to hold and those secured by the winning side from the very beginning of the war differently. The latter would have tended to be farther from the advancement of the victors and the battleground. Our identification strategy is based on the cross-sectional variation in the timing of the SCW, as each city was taken by the winning side in a particular year. To complete the assessment of the asymmetric impact, our empirical model also allows the possibility of a heterogeneous effect of the war shock (e.g., Brakman et al., 2004; Davis & Weinstein, 2002). We consider differences in the shock across cities in the form of the severity and duration of war (and other war-related indicators).

In relation to the aforementioned, our second contribution concerns the analysis of a civil war. It would be fair to say that civil war may be one of the most disruptive events in a nation's history, likely to greatly influence its development at different levels (Blattman & Miguel, 2010). However, only two previous studies, à la Davis and Weinstein (2002), refer to the effect of civil war on urban structure. Sanso-Navarro et al. (2015), for the United States, consider a war that took place in the early stages of the urbanization process and where the battles were essentially fought in the open field. González-Val and Silvestre (2022) in fact refer to the SCW. Still, their cross-sectional regression includes only a low number of observations. The SCW fits the general portrait of one of the most, if not the most, critical phenomenon in modern Spanish history.[4] As the SCW stands out for the toll it took on the population, there are good reasons to expect a permanent effect

[1]See also Lin and Rauch (2022) for a review of studies.
[2]Related literature addresses the refugee shock in Germany produced by the expulsion of ethnic Germans from East and Central Europe after World War II (Braun et al., 2021; Schumann, 2014; Wyrwich, 2020; see also Vonyó, 2012).
[3]The replacement of population with measures of economic activity also leads to different conclusions (Bosker et al., 2007; Davis & Weinstein, 2008; Miguel & Roland, 2011; Vonyó, 2012).
[4]According to accounts by historians and economists (e.g., Martín-Aceña, 2006; Moradiellos, 2004; Oto-Peralías, 2015; Preston, 2012; Rosés, 2008; Tur-Prats & Valencia Caicedo, 2020).

of the war shock.[5] However, in comparison with other well-studied international wars, the levels of material destruction in the SCW do not seem to be extraordinary, which could also lead us to anticipate a temporary effect.

From a methodological standpoint, our research also adds to the literature by using annual data. Since Davis and Weinstein (2002), the standard empirical specification, with minor modifications, has consisted of a cross-sectional regression in which the variation of a city's population between two selected, postwar, points in time is regressed on the variation of a city's population during the war years. However, this represents a "static" approach in which estimates may also be sensitive to the choice of the postwar period considered in the dependent variable (Chang et al., 2015; Head & Mayer, 2004). Hanlon's (2017) proposal is more dynamic, since decennial (census) shock-specific time effects interact with the variable of interest. Whereas Bosker et al.'s (2008b) fully dynamic strategy relies on a time series analysis and the estimation of unit root tests for each individual city as well as a panel. From this point on, our dynamic specification, in the vein of Acemoglu et al.'s (2005), allows us to capture the effect on an annual basis.[6] To support our approach to the dynamics of a shock, our model can be extended to estimate an event-study that captures possible pre-existing trends in city growth before the war.

As a first step, our empirical analysis starts with the estimation of unit root tests for a panel of cities, to check whether city growth is random or trend-stationary and, thus, whether the effect of any shock would be permanent or transitory (Bosker et al., 2008b). Panel data unit root tests, first suggested by Clark and Stabler (1991) in this context, have been proposed to be more powerful than those based on individual time series.[7] We, therefore, favor the use of a panel data model, which accounts for city and time-fixed effects, to then estimate the extent of the average dynamic response of city growth after its being taken by the winning side. Unlike in most previous research, the long-term nature of our data set minimizes the bias of dynamic panel estimators (see Ciccone, 2021).

We do not find a significant, persistent, effect of the SCW when considering growth rates for all the cities. We do, however, find a negative and significant effect when considering growth rates for the cities that aligned themselves with the losing side from the very beginning of the war, albeit the effect of the shock was temporary. Thus, differences between sides dissipated in around 12 years. These results are robust when we account for heterogeneity in the severity and duration of war. Additionally, the effect on growth of cities that belonged to the losing side, even if temporary, caused a permanent effect on their size. This finding would support the random growth theory, as these cities were not able to return to their initial size and growth path.

The rest of the paper is organized as follows. In Section 2, we review the literature on war shocks and city size distribution. Section 3 briefly describes the outbreak, evolution, and important effects of the SCW. Section 4 presents the data sources and the methodology. Section 5 provides analyses of the effect of the war shock and several robustness checks. Section 6 delves into the possible heterogeneity in the effect of the war shock across cities. Section 7 sets out our conclusions.

2 | WAR SHOCKS AND CITY SIZE

The effect of wars on the distribution of urban populations may be interpreted through the lens of three theories in the fields of urban economics and economic geography, based on locational fundamentals, random growth, and increasing returns to scale (e.g., Bosker et al., 2008b; Davis & Weinstein, 2002). First, according to the locational fundamentals theory, exogenous, fixed, natural-resource endowments, or geographical advantages (i.e., "first nature geography") not only play a crucial role in early settlements, but also determine the evolution of populations (e.g.,

[5]Perhaps our setting being comparable to that recently described by Ciccone (2021) with regard to the effect of WWI military casualties in a German state, although our records also include civilians.
[6]Models of this kind have been used to assess different effects (e.g. Cuberes & González-Val, 2017; Hanlon, 2017; Sánchez-Vidal et al., 2014; Sanso-Navarro et al., 2019).
[7]See, especially, Baltagi et al. (2007), and the works cited therein.

Ellison & Glaeser, 1999; Picard & Zeng, 2010).[8] These permanent characteristics make particular locations excellent sites for economic activity and, therefore, this theory predicts that shocks, even large ones, would be temporary, provided they do not alter the fundamental characteristics of cities.

Second, the random growth theory is based on stochastic growth processes and probabilistic models. These models are able to replicate the empirical regularity known as Gibrat's law (or the law of proportionate growth). Contrary to the location fundamentals theory, the random growth theory argues that the growth of cities is a random variable independent of their initial size (e.g., Cordoba, 2008; Gabaix, 1999; Simon, 1955). As a result, exogenous shocks can have permanent effects on particular cities, and consequently on their relative position within the distribution.

Finally, the increasing returns theory is supported by, mainly, New Economic Geography models that display nonlinear behaviors and multiple equilibria, as a consequence of their basic assumptions on "second nature geography". On city growth, the literature on urban increasing returns, i.e., agglomeration economies, is extensive (e.g., Duranton & Puga, 2004; Melo et al., 2009; Rosenthal & Strange, 2004). Within this framework, the main factor behind whether different types of shocks have either temporary or permanent effects on the city size distribution would be the degree of stability of the initial equilibrium. The extent of the impact of a shock, then, would remain unclear a priori. New Economic Geography models can also feature path dependence, which will arise if historical advantages attract activity to a particular location and returns to scale rise enough to sustain density there (Bleakley & Lin, 2012). If both conditions were met, then the effect of a shock would be transitory.

Figure 1 illustrates the path that city size may follow after a war shock, depending on the different theoretical mechanisms at work.[9] One particular city is considered in the figure, but note that the effect of the war shock can vary across cities because cities can follow different pre- and postwar paths. Let *a-b* be the initial increasing path of city size before the shock.[10] For the sake of simplicity, let us assume that the war shock lasts only one period. The different trajectories from *b* onwards represent the possible patterns of growth in the aftermath of the war. *a-b-d-e* represents the trend when there is no effect of the shock on city size. As expected, the immediate consequence of the war shock is a decrease in city size (i.e., negative growth) from *b* to *c*. However, there are three possible paths for city size afterward. First, in *b-c-d-e* city size exhibits a V-shape with reversion back to the original prewar trend (*a-b-d-e*). Therefore, the negative effect on both growth and city size is fully transitory, a scenario that could be explained by the locational fundamentals theory.[11]

Second, in *b-c-f* the negative effect on growth is transitory, as city size resumes its prewar growth rate after the shock. However, city size remains on a path (*c-f*) that is parallel to, but below its prewar trend (*a-b-d-e*), meaning that the effect on the level of city size is permanent. This case could be explained by the random growth theory: if growth is independent of initial size, once the negative effect of the war shock (*b-c*) vanishes, growth returns to its initial pattern but without the mean reversion observed in *c-d*.[12]

Finally, the war shock is able to shift city size onto a new growth path, *c-g*, which is both lower and less steep. In this case the shock has negative permanent effects on both the growth rate and the level of city size. In fact, city population may take a very long time to recover its prewar level (*b*), which could be considered as an example of (negative) path dependence. The war shock, although temporary, is able to alter the dynamics of city size for all the following periods.

[8]See also Lin and Rauch (2022) on second nature, human-made, factors.
[9]This figure is an adaptation of fig. 2 in Martin and Gardiner (2019, p. 1806), which refers to the effect of economic recessions.
[10]Figure 1 depicts an increasing city size over time to emphasize the effect of the shock on growth. However, the argument would also be valid for a city with a size around a steady state value.
[11]For example, the path *a-b-c-d-e* is similar to that followed by real data compiled by Davis and Weinstein (2002, fig. 2).
[12]Theoretically, an upper and lower threshold determine the stability of the equilibrium. If the war (negative) shock is sufficiently large to push city size below the lower threshold, then city size moves to a new (lower) equilibrium size and does not return to the initial path. Otherwise, as for the path *b-c-d-e*, it can be argued that the negative shock is not strong enough to push city size below the lower threshold, so the initial deviation is undone in the second period (*c-d*).

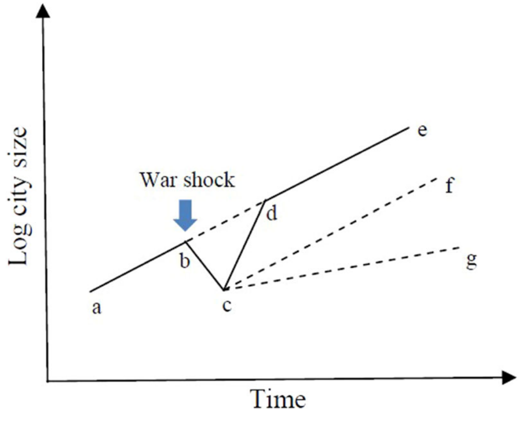


FIGURE 1 Types of recoveries from war shock. Based on Martin and Gardiner's (2019) representation of the response of cities to recessions.

Part of the research on the effect of wars on city growth has mainly adhered to the location fundamentals theory (Brakman et al., 2004; Davis & Weinstein, 2002, 2008; Gónzalez-Val & Silvestre, 2022; Sanso-Navarro et al., 2015). Although the presence of increasing returns and multiple equilibria or the possibility of random growth processes has received support from the studies by Bosker et al. (2007, 2008b), Hanlon (2017) and Ciccone (2021). On the other hand, recent related research proposes theoretical models that focus on demographic factors, such as fertility (Allen & Donaldson, 2022; Ogasawara & Komura, 2022).

3 | THE SPANISH CIVIL WAR (1936–1939)

The SCW started in 1936 as a result of a failed military coup, by an extreme right-wing faction of the army, against the legally constituted *Republican* government formed in 1931. The war came after a period of heightened social and political polarization, which, in spite of internal differences, led to a clear-cut divide: Left, that is, Republican/loyalist, versus Right, that is, Nationalist/rebel (e.g., Balcells, 2017; Domenech, 2013; Simpson & Carmona, 2020). The coup rapidly split the army and the territory into two camps. In the words of one historian, "less than a week was needed to clear the picture" (Casanova, 1999, p. 57; see also Prada, 2010).

At first (in 1936), the Republican territory represented 60% of the total population of the country. The coup failed in four out of the six largest cities (above 200,000 inhabitants): the capital cities of Madrid, Barcelona, Valencia, and Málaga; and was only successful in two: Sevilla and Zaragoza.[13] However, the rebellion soon secured 32 out of 49 capital cities, which constituted the focal points of political, economic and military power in the 49 Spanish provinces (Ortega & Silvestre, 2006). Over nearly 3 years the frontlines progressively shifted in favor of the Nationalist army. The war ended in 1939, and General Francisco Franco, the leader of the rebel army, put himself at the head of a right-wing dictatorship that lasted until 1975.

In terms of total (GDP) destruction, the SCW hovers in the mid-to-high range, as compared to other civil wars (Martín-Aceña, 2006; Rosés, 2008). The loss of physical capital was similar to the Western European average during World War II (Prados de la Escosura, 2017). The greatest destruction of physical capital was that of the transport system (Prados de la Escosura, 2017; Rosés, 2008). Generally, cities were not bombed heavily, and the housing stock was only slightly reduced (Carreras & Tafunell, 2004; Catalán, 1995; Rosés, 2008). Although there were a few

[13]For the spatial location of provinces and their capital cities, see Appendix A. All Appendices mentioned in this and following sections are available online in the supporting information of the article.

exceptions, such as Madrid (the nation's capital city) and several capital cities situated along the east (Mediterranean) coast, which suffered large-scale bombing by the Nationalist army and its German and Italian allies (Balcells, 2017; Solé-i-Sabaté & Villarroya, 2003).

Excess mortality caused by the SCW is closer to 350,000 deaths and amounts to around 500,000 deaths when including the years immediately following, until 1942, roughly double that caused by the Napoleonic Wars in Spain (Ortega & Silvestre, 2006; Prados de la Escosura & Santiago-Caballero, 2018).[14] Of the second total, around 200,000 would be the result of political repression: 50,000 in the area under the control of the loyalists and nearly 150,000 in the area gradually falling under the control of the rebels (Balcells, 2017; Espinosa, 2021; Juliá, 2008; Preston, 2012). In an international comparison of civil wars, the SCW is one of the most severe, as shown in Appendix B. Moreover, excess mortality tended to be much higher than that resulting from World War II in France and Italy (Ortega & Silvestre, 2006).

4 | DATA AND EMPIRICAL STRATEGY

4.1 | Data on population

Long-term population datasets are mostly based on decennial (or roughly decennial), census-type data, which limits the scope of research by missing all the intercensus years' population dynamics. As far as we know, only the data set of 62 large West German cities for the 1925–1999 period constructed by Bosker et al. (2008b) has been used to assess the effects of shocks. In our research, we used a new, long-term, annually and spatially disaggregated population database, as introduced in González-Val and Silvestre (2020). Based on three official statistics, the decennial censuses of population, the yearly *Movimiento natural de la población* (natural growth of population = births minus deaths) and (from 1996 onwards) the annual revision of the municipal register (*Padrón continuo*), the authors established a straightforward method to provide annual estimates at the urban and provincial levels for the 1900–2011 period. The approach outlined therein goes further than the use of simple (usually linear) interpolation of population figures at census dates.

At the urban level, González-Val and Silvestre (2020) propose the provincial capital (which would represent a selection of LAU II/NUTS V units). Further research also refers to the capital cities as reflecting urban demographic behavior, even if they do not represent the entire urban population (Pérez-Moreda et al., 2015). As the traditionally most important centers of power (as argued above), during the SCW the capital cities played a key role in the military strategies of both sides (Casanova, 1999; Seidman, 2002). The number of provinces and, therefore, the capitals, 49, has remained unchanged.[15] A fundamental feature of long-term population estimations is the homogenization of spatial units, since geographical boundaries may have changed over time. Using the 2011 census as reference, the estimates accounted for (13 types of) changes in boundaries since 1900, as a result, predominantly, of annexations and, often more recently, segregations of usually neighboring municipalities.

During the 1900–2011 period, the populations of the capital cities have represented between 18% and 37% of the total national population. The number of inhabitants of the capital cities throughout the whole of the period covered is above 10,000 (except for Soria before 1927), which is a standard urban threshold in Spain (Tafunell, 2005). A second unit of analysis is the province (NUTS III regions), which is used as a robustness check to complement the analysis of cities. This is important, as the use of different spatial units may lead to different results

[14]Excess mortality is expressed as additional deaths compared to a baseline period, which is provided by the extrapolation of figures for the 1926–1935 period. We will return to the issue of postwar deaths in Section 6.

[15]The two provinces corresponding to the Canary Islands are joined together due to census reporting criteria, and the Spanish enclaves of Ceuta and Melilla in northern Africa have been excluded.

(Braun et al., 2021; Wyrwich, 2020). The number of observations totals 5439 (49 capital cities or provinces × 111 years).

4.2 | Empirical strategy

Let S_{it} be the share of city i's population over the total population of the country at time t. The most general form for the growth equation of cities within a panel data framework is (Favaro & Pumain, 2011):

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \varphi_t + u_{it}, \tag{1}$$

where s_{it} is the natural logarithm of S_{it}; $s_{it+1} - s_{it}$ is the annual growth, from 1900 to 2011, in the relative size of city i at time t; and α_i and φ_t are city and time (annual) fixed effects, respectively. The parameter β determines the relationship between size and annual growth. A significant estimated coefficient $\beta < 0$ indicates mean reversion and, thus, city share would be stationary. While a nonsignificant estimated coefficient provides support to Gibrat's law for cities, that is to say, city growth would be independent of initial size.[16]

With $u_{it} = \rho u_{it-1} + \varepsilon_{it}$, ρ allows for serial correlation in u_{it}—the error term in the growth equation (Chesher, 1979). In Davis and Weinstein's (2002) cross-sectional model, ρ is called the "persistence parameter", and measures the length of the war shock. ρ can be estimated using a unit root test provided that the shock is correctly instrumented. Our Equation (1) can easily be adapted to test for unit roots adding augmenting lags of growth. For a panel of cities, unit root tests offer a first glimpse at whether city growth is random or stationary. Stationarity would imply that the panel of cities recover after the war shock. Whereas if city growth follows a unit root the consequence of the war shock would be a permanent effect on city shares, therefore a random growth pattern.

We run two-panel unit root tests, the Levin et al. (2002) test and a Fisher-type panel data test. In both tests, the null hypothesis states that all series are nonstationary (each time series contains a unit root).[17] The Fisher-type test is similarly estimated to the Im et al. (2003) test and, as in our setting, is especially suitable with samples comprised of a finite number of panels (Choi, 2001).[18] Results are reported in Table 1. Panel A considers all the cities in our sample. With regard to the Levin et al. (2002) test, the null hypothesis is clearly rejected in all cases in which different lags for the Augmented Dickey–Fuller regressions were considered. The null hypothesis is also rejected for the Fisher-type test. Therefore, the results in panel A point to stationarity in the entire panel of cities. Panels B and C consider two subsamples of cities, according to their behavior during the war. We will discuss these results below.

Mean reversion, as shown in Table 1, panel A, indicates that city shares returned to their prewar trend after the shock, the pattern *b-c-d-e* in Figure 1. However, even if the war shock did not have permanent effects on population—so far, considering all the cities—how long the adjustment took is still a relevant issue. To estimate the length of the SCW shock on city growth, we propose the following extension of Equation (1):

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \sum_{k=1}^{16} \delta_k \text{Civilwar } d_{kit} + \varphi_t + u_{it}, \tag{2}$$

where we expect $\hat{\beta}$ to be significant and negative.

The key element in Equation (2) is $\sum_{k=1}^{16} \delta_k \text{Civilwar } d_{kit}$, which represents a dynamic set of city-specific time dummy variables. Thus, Civilwar d_{1it} is a dummy variable that takes a value of 1 the year immediately after city i was won for the Nationalist, that is, rebel, camp, and zero otherwise; Civilwar d_{2it} is a dummy variable that takes a value of 1 in the second year after; and so on. Here, we take advantage of the cross-sectional variation in the timing

[16]On this matter see, e.g., González-Val et al. (2014).
[17]On Levin et al. (2002) unit root test applied to city growth see, e.g., Resende (2004), Henderson and Wang (2007) and Bosker et al. (2008b).
[18]See one recent application in Ogasawara and Komura (2022, p. 1053, f.n. 19).

TABLE 1 Panel data unit root tests on city shares.

Augmenting	Levin et al. (2002)		Fisher-type test	
Lag	Test statistic	*p*-Value	Test statistic	*p*-Value
Panel A: All capital cities (49)				
1	−54.730	0.000	180.761	0.000
2	−62.827	0.000	147.193	0.001
3	−71.135	0.000	127.683	0.023
Panel B: Nationalist capital cities				
1	−5.027	0.000	142.720	0.000
2	−5.257	0.000	115.743	0.001
3	−5.775	0.000	99.880	0.003
Panel C: Republican capital cities				
1	−2.275	0.011	38.042	0.290
2	−3.443	0.000	31.449	0.593
3	−4.193	0.000	27.803	0.764

Note: Natural logarithms of city shares. 32 Nationalist and 17 Republican capital cities. 1900–2011 period. Levin et al. (2002) adjusted t^* test statistic and *p*-value. The *P*-statistic (inverse chi-squared) and the *p*-value are reported for a Fisher-type test based on Phillips–Perron unit-root tests on each panel. The null hypothesis for both tests states that all series are nonstationary (each time series contains a unit root). Time trends are not included.

of the SCW as each city was taken by the, ultimately, winning side in a particular year.[19] Each city, then, has its own set of time dummy variables. For example, in the case of Sevilla, which was won for the rebels in 1936, Civilwar $d_{1it} = 1$ refers to 1937, Civilwar $d_{2it} = 1$ refers to 1938, and so on.[20] In short, as we consider that the war may have affected cities differently according to the year in which they were taken by the winning side, the δ_k coefficients reflect the average dynamic response of growth to the SCW. The model in Equation (2) enables the effect of the shock to vary annually. A negative (positive) sign of δ_k indicates that growth in city i has decreased (increased) after k years since the end of the war. So, a reasonable question would be: did the effect of the war on growth increase or decline over time? In the final results presented, we consider the period up to 16 years after each city was taken. We experimented with longer time periods, but the estimated coefficients of the time dummy variables from the 16th year onwards were not significant in any case.

The focus of our method (i.e., Equation 2) is placed on the period after the taking of cities by the winning side. In other words, we analyze the dynamics of growth in the period *after* the war rather than the period *during* the war; and those periods vary by city. During the SCW years (1936–1939) the urban hierarchy was barely affected by the shock. Transition matrices were calculated to illustrate the extent of change in the city size distribution over the prewar, war and postwar periods (Bosker et al., 2008b). Appendix C suggests that, compared to the pre- and postwar periods, city size distribution remained highly persistent during the SCW years. The diagonal elements of the matrix corresponding to the war period are equal to 1 in all states but that of the smallest cities. Therefore, to find an effect of the war shock we should examine the post-war period rather than the war period itself. In any case, we will return to postwar versus during war effects below, in Section 6, when addressing the reasons for city size evolution.

[19]Such a short treatment timing (1936–1939) reduces potential biases of settings of this sort (Baker et al., 2021).
[20]We mostly used the annual classification proposed by Ortega and Silvestre (2006), based on Thomas (1978). There are four cities (Málaga, Gerona, Barcelona and Tarragona), though, that were taken at the very beginning (January or February) of the year, and we, therefore, assigned them to the previous year.

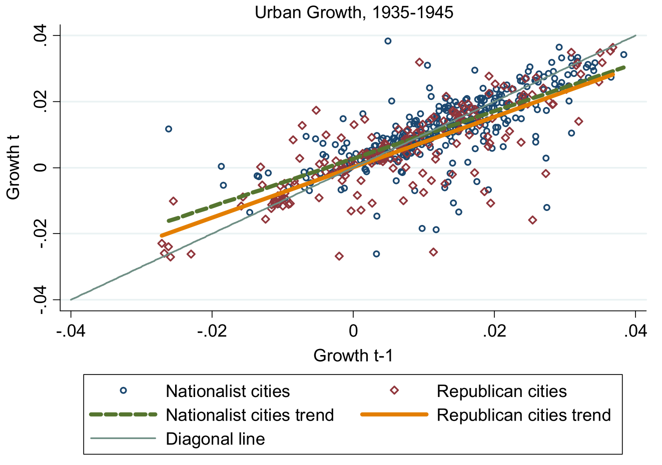


FIGURE 2 Population growth rates of capital cities. Annual growth rates of relative populations from 1935 to 1945. Nationalist cities are those that aligned themselves with the rebel camp from the very beginning of the war (year 1936).

We also aim to go beyond the average effect for *all* cities. One of the most noteworthy findings of Brakman et al. (2004) is the existence of asymmetries in the effect of the war shock on East German cities versus West German cities. In this vein, some descriptive evidence displayed in Figure 2 certainly points to differences across two groups of cities, those held by the Republican government (until their fall) and those secured by the Nationalists from the very beginning of the war in 1936. Growth in year *t* is plotted against its lagged value *t-1* throughout the 1935–1945 period, 1945 marking the first turning point towards the postwar economic recovery (Rosés, 2008). Most of the observations corresponding to both sides in the war are below the diagonal, which indicates that most city shares decreased during the war and postwar sub-periods. However, even in such a, generally low-growth context, linear trends show that growth in the Republican cities tended to be lower than that of the Nationalist ones.[21]

In fact, the results of the panel unit root tests show that different growth patterns could have had different effects on city size. In Table 1, panels B and C refer to the unit root test for the Nationalist and Republican cities, respectively. The results in panel B, for the Nationalist cities, confirm the stationarity of city sizes. However, in Panel C, for the Republican cities, the results are contradictory. The Levin et al. (2002) test rejects non-stationarity for any lag, but the Fisher-type test supports a unit root in all cases. The mixed evidence implies that, if growth in the Republican cities in the aftermath of the war was significantly lower, we cannot rule out a permanent effect on their size as a result.

In this regard, Equation (2) can be modified to delve deeper into asymmetries in the war shock. The distinction between treated and nontreated, by war, cities is not possible, since the SCW affected the whole country. However, we can certainly consider the 32 (out of 49) capital cities that aligned themselves with the Nationalist camp from the very beginning of the war (year 1936) as the control group. In Equation (3), we add a Republican dummy variable REP_i, which is set to 1 if city *i* was held by the Republican government (until their fall); and set to zero for the Nationalist cities. REP_i interacts with the dynamic set of city-specific time dummy variables:

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \sum_{k=1}^{16} \delta_k REP_i \cdot \text{Civilwar } d_{kit} + \varphi_t + u_{it}. \quad (3)$$

[21]Differences in the evolution of size between the two groups of cities are confirmed by means of (unreported) empirical density functions using adaptative kernels for 1935 and 1945. All unreported analyses mentioned in this and following sections are available from the authors upon request.

This way, the model in Equation (3) turns to a difference-in-differences approach in which the set of time dummy variables would measure the average dynamic effect of SCW on the growth of the Republican cities (the treatment group) compared to the growth of Nationalist cities (the control group).[22] A negative (positive) sign of δ_k indicates that growth in the Republican city i has decreased (increased) after k years since the end of the war, compared to growth in the Nationalist cities. This is a similar specification to the one used by Hanlon (2017), in his study on the consequences of the US Civil War on British cities.

The findings from our approach so far may be biased if variations in city size after the Nationalist takeover were in some way associated with specific prewar trends. An extended version of the previous model leads to Equation (4), which represents an event-study specification (Jacobson et al., 1993). Pre- and posttreatment is defined by dummy variables that measure the time relative to the SCW (i.e., "event-time"). Therefore, $k = 0$ corresponds to the year of the end of the war, a date anywhere between 1936 and 1939 that varies by city. In Equation (4), the τ_k coefficients are meant to capture the average dynamic effect of the SCW k years before the end of the war. We consider a prewar period, up to 16 years, of the same length as the postwar period.[23]

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \sum_{k=-1}^{-16} \tau_k \text{Civilwar } d_{kit} + \sum_{k=1}^{16} \delta_k \text{Civilwar } d_{kit} + \varphi_t + u_{it}. \quad (4)$$

Accordingly, the difference-in-differences version of model (4) includes the Republican dummy variable REP_i which multiplies both pre- and posttreatment dummy variables:

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \sum_{k=-1}^{-16} \tau_k REP_i \cdot \text{Civilwar } d_{kit} + \sum_{k=1}^{16} \delta_k REP_i \cdot \text{Civilwar } d_{kit} + \varphi_t + u_{it}. \quad (5)$$

Our empirical analysis, on the one hand, will also include a series of robustness checks to confirm the findings (Section 5.2). We first estimated placebo regressions. Next, we ran models excluding large cities, and then we examined the only case in which a city switched sides and, additionally, looked at different time periods. We also accounted for public efforts to influence the rate of growth of cities. Finally, we considered the province as an alternative spatial unit of analysis to the capital city. On the other hand, a further series of specifications will examine the determinants of the dynamic response of city growth to the SCW considering heterogeneity in the war shock across cities (Section 6).

5 | RESULTS

5.1 | Main results

The results for Equations (2) and (3) are shown in Table 2. Robust standard errors are clustered by city to deal with serial correlation. Adding the initial city shares$_{it}$, ((ln(Relative population)$_{it}$), in columns 2 and 4, does not alter the main results. As expected, the significant and negative effect of the estimated coefficient ofs_{it} on growth points to mean reversion. "Years" indicate the average effect on growth k years after cities turned to the Nationalist side (at different times). That is to say, these coefficients report the effect of the set of time dummy variables $\sum_{k=1}^{16} \delta_k \text{Civilwar } d_{kit}$. Years are grouped in pairs to avoid multicollinearity, because time, annual, fixed effects are also included in all regressions. In the first two columns, which consider the impact of the SCW on growth for all cities, we found that, although the estimated coefficients of Years present a negative sign until Years 3–4, they are not

[22]In the treatment group, three, four, and 10 capital cities were won for the Nationalist in 1937, 1938 and 1939, respectively. (The total is 17, instead of 18, due to one of the year reassignments. See footnote 20 in this regard).
[23]This option also permits us to exclude most of the effect of the Spanish flu (Pérez Moreda et al., 2015).

TABLE 2 Dynamic effects of the Spanish Civil War (SCW) shock on relative city growth.

	All capital cities		Republican capital cities	
	(1)	(2)	(3)	(4)
ln(Relative population)$_{it}$		−0.005**		−0.005**
		(0.003)		(0.003)
Years 1–2	−0.002	−0.002	−0.011***	−0.011**
	(0.004)	(0.004)	(0.004)	(0.004)
Years 3–4	−0.001	−0.001	−0.010**	−0.010**
	(0.007)	(0.007)	(0.004)	(0.004)
Years 5–6	0.001	0.001	−0.011***	−0.011***
	(0.004)	(0.004)	(0.004)	(0.004)
Years 7–8	0.003	0.003	−0.010***	−0.010**
	(0.004)	(0.004)	(0.004)	(0.004)
Years 9–10	0.005	0.004	−0.010***	−0.010***
	(0.004)	(0.004)	(0.003)	(0.003)
Years 11–12	0.006	0.006	−0.006**	−0.006**
	(0.004)	(0.004)	(0.003)	(0.003)
Years 13–14	0.005	0.005	−0.003	−0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Years 15–16	0.003	0.003	−0.003	−0.003
	(0.002)	(0.002)	(0.003)	(0.003)
Constant	0.025***	−0.007	0.025***	−0.009
	(0.002)	(0.016)	(0.002)	(0.016)
City fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R^2	0.340	0.342	0.348	0.350
Observations	5,439	5,439	5,439	5,439

Note: 1900–2011 period. Dependent variable: (ln)Growth in relative population, or ($s_{it+1} - s_{it}$). Robust standard errors clustered by city in parentheses. In columns 3 and 4, Nationalist cities act as the control group.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

significant at the usual levels. Therefore, it seems the war did not have a significant effect on subsequent urban growth. On average, then, the growth of city shares did not deviate from the prewar trend (pattern *a-b-d-e* in Figure 1; see also Table 1, panel A).

However, in the difference-in-differences specifications (Equation 3) reported in columns 3 and 4, a negative and significant effect of the SCW shock on population growth is now found in the Republican cities relative to the Nationalist ones, the latter acting as the control group. The average negative effect of the war shock on the Republican side was at any rate temporary, since it clearly lasted until Years 11–12 after. It should be remembered that the significant average effect *k* years after the end of the war refers to cities for which the Years variables are in the range between a) Years 1–2 = 1938–1939, if they were won for the Nationalists in 1937,[24] and Years 11–12 = 1948–1949; b) Years 1–2 = 1939–1940, if they were won for the Nationalists in 1938, and Years 11–12 = 1949–1950; and c) Years 1–2 = 1940–1941, if they were won for the Nationalists in 1939 (the last year of the war), and Years 11–12 = 1950–1951. The difference in growth tended to diminish over time, according to the size of the estimated coefficients.

These results are validated by Figure 3, which depicts the event-study estimates of the τ_k (prewar) and δ_k (postwar) parameters, along with confidence intervals at the 5% level. Again, we estimate two versions of the model (Equations 4 and 5). Figure 3.1 refers to the specification considering the war effect on all cities. Whereas Figure 3.2 refers to the specification focusing on the war effect on Republican cities relative to Nationalist ones.[25] There seems not to be evidence of the existence of significantly different prewar trends in urban growth in either of the two figures. Moreover, the estimated coefficients for the post-war time dummy variables are consistent with those shown in Table 2 (columns 2 and 4). The results hold when using the recent estimator proposed by Callaway and Sant'Anna (2021).[26] These (unreported) results show no significant different prewar trends either.

To recapitulate, results in Table 2 first suggest that the SCW had a nonpermanent effect when considering the average effect on growth for all cities, but they do also show a negative, significant, and temporary effect, of around 12 years, on Republican cities relative to Nationalist ones. So, the distinction between the two groups of cities leads to the finding of an asymmetric shock.

The pattern of estimated coefficients for Republican cities (in Table 2, columns 3 and 4) has two implications. Firstly, in terms of Figure 1, path *b-c-d-e* is discarded for this group of cities, as no evidence of a recovery period is obtained. To put it another way, no estimated coefficient presents a positive sign. Secondly, as suggested in Table 1, panel C, a temporary, and negative, effect on city growth may have a more permanent effect on the level of city size, as shown in path *b-c-f* in Figure 1 (which is parallel to, but below the prewar trend). Based on the significant estimated coefficients until year 12, as reported in Table 2, column 3, it is possible to estimate a counterfactual city share for the Republican cities.[27] Thus, the average city share of Republican cities would have been 5.97% bigger had the SCW not occurred.

A further calculation (holding the total population of the country to be constant) indicates that this figure would be equivalent to around 16,000 inhabitants (16,174, to be precise), on average. As a reference, the average population of the 17 (latterly considered) Republican cities immediately before the war (in 1935) was 215,532.

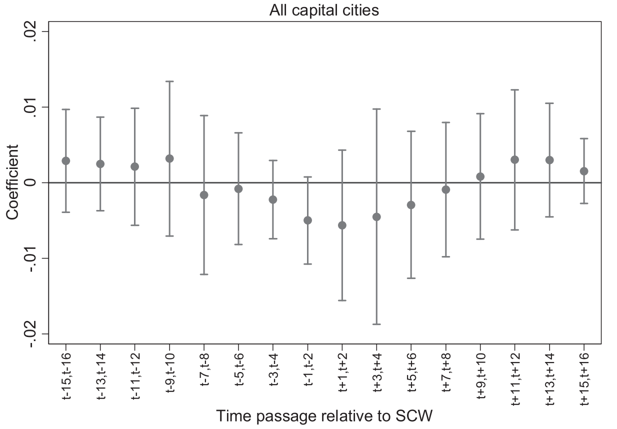
5.2 | Robustness checks

5.2.1 | Placebo test

A possible concern with our results is that the negative and significant post-war Years dummy variables, when considering the effect on one group of cities relative to the other, may, in fact, be capturing some other factor that affects city growth at the same time, but which is not related to the war—in spite of city and time-fixed effects also being included. To offset this possibility, we run placebo regressions. We drew random war dates for all the capital cities from a discrete uniform distribution of the interval 1936–1939, (which is the SCW time frame). With these random dates, we then defined a new set of Years dummy variables for the entire sample of cities or the two city groups, Republican and Nationalist, and re-estimated Equations (2) and (3). We repeated this exercise 2000 times. Appendix D.1 reports the

[24]As those won for the Nationalists in 1936, the first year of the war, belong to the control group.
[25]Also, in both models, the initial city share is included, and time dummy variables are grouped in pairs.
[26]The doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares (see Sant'Anna & Zhao, 2020). Specifications include city and time-fixed effects.
[27]Specifically, we used this formula: $s_{i\,SCW+12} = s_{i\,SCW} + \sum_{k=1}^{12}((s_{i\,SCW+k} - s_{i\,SCW+k-1}) + |\hat{\delta}_k|)$.

3.1. All capital cities



3.2. Republican capital cities

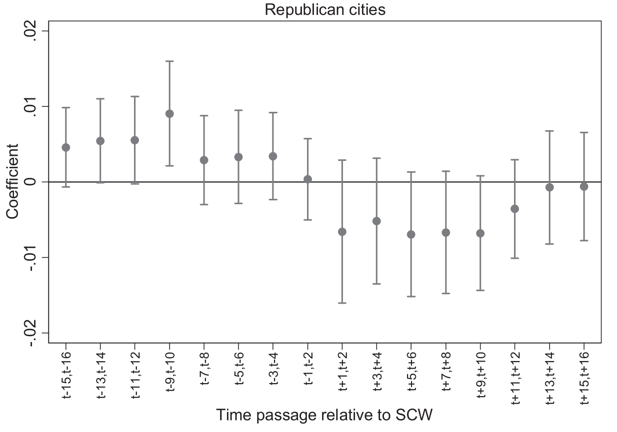


FIGURE 3 Event-study. Robust standard errors clustered by city. Period: 1920–1955. Significant coefficients at the 0.05 level. SCW, Spanish Civil War.

empirical distribution of the estimated coefficients. Finally, from these 2000 replications we obtained the bootstrap average and standard deviation of all the estimated coefficients for different random dates.

We might expect that if we are truly capturing the effect of the SCW shock, and therefore the set of dummy variables created according to the historical dates is unique, we should obtain no significant estimated coefficient when running the same regression using random dates. The results are reported in Appendix D.2. The estimated coefficients of the new Years variables are not significant in any case, while the coefficients for the initial city share (columns 2 and 4) are identical to those obtained in Table 2. In other words, random dates lead to no effect on city growth, as does any other combination of cities within the control and treatment groups determined by the random dates.

5.2.2 | Alternative subsamples

Here, from our data set we removed, first individually and then together, the two largest cities of Madrid (1,276,073 inhabitants in 1936) and Barcelona (1,069,919) (compare with Valencia, 403,443, the third city in size). Then, in another specification, we removed Teruel, as the only capital city that temporarily switched sides during the war—when it was taken by the Republican (loyalist) army during the first 2 months of 1938. The results, which are reported in Appendices E.1–E.4, vary slightly. The general picture remains one of essentially no effect on growth for all the cities and a temporary effect on Republican cities relative to Nationalist ones.[28] The main results were also robust to the (unreported) estimation of Equations (2) and (3) for shorter periods, 1900–1980, 1900–1970, and 1900–1960 (instead of 1900–2011).

5.2.3 | Government interventions

The long-term evolution of relative city size may also potentially be affected by public efforts to address cities' backwardness—although US and Japanese government reconstruction programs seemed to have a modest, if any, impact on city growth (Bosker et al., 2007; Brakman et al., 2004; Davis & Weinstein, 2002, 2008). In Spain, two government interventions, which appeared at either end of the Franco dictatorship, should be considered: the *Dirección de Regiones Devastadas* (Office of Devastated Regions) (1939–1957) and the creation or consolidation of "growth poles", as part of the *Planes de Desarrollo* (Development Plans) (1964–1975).[29]

The scant availability of sources made it difficult for us to accurately reflect the role played by these government actions. As far as we know, there is a lack of systematic and detailed, spatially disaggregated, information on expenditures. Hence, an alternative, elementary though it may be, is the use of two sets of dummy variables. We re-estimated the effect of the SCW (Equations 2 and 3) with the addition of two dummy variables identifying the cities that benefited and those that hardly benefited from reconstruction programs at all. The procedure and sources are explained in Appendix F, which also includes the results.

The first public intervention, the Devastated Regions Program, has no significant effect in any case, and the second public intervention, Growth poles, is significant at the 10% level in only two specifications (columns 11 and 12). The lack of more precise information is a limitation to be borne in mind when interpreting our evidence. However, the finding of an absence of a clear impact of these programs is in line with previous research done for Japan and Germany, as well as research carried out for Spain, especially regarding the program implemented immediately after the war (Carreras & Tafunell, 2004; Comín & Vallejo, 2009; De la Torre & García-Zúñiga, 2013; De Terán, 1999; López-Díaz, 2003).

In any case, as compared with Table 2, the main results hold: in columns 1–6 of Appendix F, the Years variables display no SCW effect on relative city growth for all cities; while in columns 7–12, which refer to the effect on growth in Republican cities relative to Nationalist ones, the effect is negative, significant, temporary and of the same duration.

5.2.4 | The province as spatial unit of analysis

Our final robustness check replaces the capital city with the province (NUT III regions) as a spatial unit. Given that the (49) provinces comprise the entire Spanish territory and population, here not only urban but also rural areas are considered. Appendix G refers to the estimation of the main model in Equations (2) and (3). Compared with Table 2, a foreseeable difference is that the initial share ($\ln(\text{Relative population})_{it}$) has now a positive, instead of negative, significant effect, which points to a divergent behavior of relative provincial growth, while we found mean reversion for all the capital cities. This discrepancy between spatial units in Spain is well-known in the literature (see González-Val & Silvestre, 2020; and the works cited therein).

[28]The exclusion of both Madrid and Barcelona (Appendix E.3) leads to a somewhat longer, asymmetric, shock.
[29]A brief description of specific interventions for both programs is included in Appendix F.

Apart from that, the main results concerning the SCW shock are largely similar to those obtained previously using the capital city as a spatial unit. As in Table 2, Appendix G does not seem to show a significant effect of the SCW shock when considering all the provinces (columns 1 and 2), but it does show a negative, significant, and temporary effect of the same length on the Republican provinces relative to the Nationalist ones (columns 3 and 4), although the magnitude of the estimated coefficients for the Years variables is smaller. It would seem that the broader the spatial scope, the weaker the shock (Braun et al., 2021; Wyrwich, 2020).

We also performed additional analyses.[30] Thus, the placebo regressions again suggested that the Years variables are truly detecting the effect of the SCW shock (because no estimated coefficient was significant). The specifications that include the variables reflecting the reconstruction programs also showed that, when focusing on the Republican provinces relative to the Nationalist ones, the average length of the shock was the same as in Appendix F.[31] The event-study, however, in addition to post-war differences, pointed to prewar trends in relative provincial growth, so the effect of the war is unclear. This result may be associated with the aforementioned long-term process of divergence between populations at the provincial level (e.g., González-Val & Silvestre, 2020).

6 | THE DETERMINANTS OF ASYMMETRIC SHOCKS: A CLOSER LOOK AT THE EFFECT OF WAR

In Section 5, estimations include a specific set of dummy variables for each city to incorporate the variation in the timing of the SCW. Our hypothesis was that cities could have been affected differently by the war shock depending on the year in which they were taken by the Nationalist, that is, winning, side. As a matter of fact, historical research points to higher numbers of deaths in combat in the areas still held by the Republican government after 1936, given their proximity to the frontlines (e.g., Alcaide, 2008; Ortega & Silvestre, 2006; Prada, 2010).

However, there could be some potential issues with our identification strategy if (part of) the SCW effect continued after the war was over. For example, in the form of political repression as a consequence of the Nationalist takeover, and differences in economic and health repercussions. Thus, registrations of violent deaths by the Nationalist side, although imprecise and under debate, suggest the existence of a first peak occurring in the first 2 years of the war, 1936 and 1937; and also a second peak taking place around the end of the war and the subsequent years, in particular from 1939 to 1942, and lasting (albeit decreasingly) until the middle of the 1940s. Postwar repression was not only directed at locations which had been under Republican control throughout most of the war, but they tended to be primary targets (Babiano et al., 2017; Casanova, 1999; Espinosa, 2021; Prada, 2010).[32]

Moreover, the area controlled by the Republican government displayed poor resource management. The Nationalist government demonstrated a much more efficient, centralized, and military-oriented administration of available resources, as opposed to the diffuse and disorder-prone political and economic decision-making exhibited by the loyalists, which included the administration of the transport system (Cayón-García & Muñoz-Rubio, 2006; Martín-Aceña, 2006). In the Republican territory, in fact, the main subsistence products, such as wheat, potatoes and legumes, as well as livestock, were in short supply as the war progressed; yields probably also fell (Martínez-Ruiz, 2006). This was not a desirable position to be in the context of a weakened health system and the spread of contagious diseases, both during the war and in the very first years thereafter (Galofré-Vilà & Gómez-León, 2022; Ortega & Silvestre, 2006).

[30]Another robustness check referred to a different assignation of SCW dates for five provinces, as the taking of the capital city did not always align with that of the rest of the provincial territory. The alternative assignment criterion of provinces provides nearly the same results as in Appendix G.
[31]The effect of the Growth poles variable was now more significant. This finding makes sense due to the fact that regional policy in the 1960s and early 1970s was targeted at an area beyond the capital cities.
[32]We also owe part of these remarks to historian Francisco Espinosa (Personal communication by email, 21 July 2021).

In this section we propose a model to account for potential endogeneity associated with the use of a (post-war) set of time dummy variables. Using Equation (2) as a basis, Equation (6) adds a Y_i, constant and city-specific, variable representing the extent of the war shock exclusively between 1936 and 1939, which interacts with the set of dummy variables. The resulting interaction term, $\sum_{k=1}^{16} \delta_k Y_i \cdot \text{Civilwar } d_{kit}$, refers to the average dynamic effect of the SCW conditional upon the variation in the war impact. This way, the new model considers heterogeneity at the city level in the war shock.[33] This is a similar specification to those recently used by Ogasawara and Komura (2022) and Gindelsky and Jedwab (2023).

$$s_{it+1} - s_{it} = \alpha_i + \beta \cdot s_{it} + \sum_{k=1}^{16} \delta_k Y_i \cdot \text{Civilwar } d_{kit} + \varphi_t + u_{it}. \tag{6}$$

We regard the city-specific war shock in terms of its severity and duration. Based on historical research, which has emphasized excess male mortality during the SCW (Galofré-Vilà & Gómez-León, 2022; Pérez-Moreda et al., 2015; Ortega & Silvestre, 2006), the severity of the SCW can be measured as the number of male deaths during the 1936–1939 period divided by the number of female deaths during the same period. We used the definitive figures published in 1943—in the *Movimiento natural de la población* (natural growth of population).[34] As an alternative to this ratio, we look at the duration of war, measured as the (natural logarithm of the) difference between the year of being taken by the winning side and 1935.[35]

We tried several other indicators related to the effect of the war, as set out in Appendix H. In the end, however, we decided to stick with excess male mortality, which has been constructed from population statistics; and the duration of war. As an option, we considered two available accounts of war casualties at city level: *Political repression (a)*, which is based on data on mass graves recently collected under the leadership of the Government of Spain (see Tur-Prats & Valencia Caicedo, 2020); and *Political repression (b)*, which is based on revised calculations by the late political scientist at the University of Kansas Ronald A. Francisco, of previous research done by historians (see González-Val & Silvestre, 2022). However, accounts of this sort, although valuable for a number of reasons, are probably less comprehensive than estimates based on population statistics, at least for the case under study (Ortega & Silvestre, 2006). We also experimented with differences in two basic but informative socioeconomic characteristics of cities at the time, the literacy rate and the change in active agrarian population, which are only available at census dates. 1930 and 1940 are the closest dates both before and after the SCW. Likewise, we contemplated the use of two geographical distances, to Madrid (the Spanish capital) and to coast, as further proxies for the economic status of cities.[36] In all these cases, no dynamic effects were found (in specifications similar to those reported in Tables 3 and 4).

Turning to our preferred indicators, the results for Equation (6) are shown in Tables 3 and 4. In Table 3, column 1, conditional upon the extent of the war shock between 1936 and 1939 measured as the excess male mortality ratio, again shows a non-permanent effect of the SWC when considering population growth for all the cities (as in Table 2, column 2). Whereas column 2 shows a negative, significant, and temporary effect, of around 12 years, on Republican cities relative to Nationalist ones (as in Table 2, column 4), meaning that the higher the excess male mortality ratio during the war, the lower the city growth *k* years after (*k* ranging from 1

[33]In Equation (2) we *de facto* assumed that the shock is the same for all cities, given that the war impact was captured by a set of time dichotomous variables.
[34]Mortality refers to all causes of death for all segments of the population (see, e.g., Gindelsky and Jedwab (2023).
[35]On the sources, see footnote.[20]
[36]See, e.g., Ayuda et al. (2010).

TABLE 3 Dynamic effect of the severity of the Spanish Civil War (SCW).

	Population		Fertility		Mortality	
	All capital cities	Republican capital cities	All capital cities	Republican capital cities	All capital cities	Republican capital cities
Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
ln(Relative population)$_{it}$	−0.005**	−0.006**				
	(0.002)	(0.003)				
ln(Relative fertility)$_{it}$			−0.094***	−0.103***		
			(0.019)	(0.019)		
ln(Relative mortality)$_{it}$					−0.287***	−0.278***
					(0.050)	(0.049)
Years 1–2 $\times Y_i$	−0.002	−0.006***	0.067**	0.014	−0.098***	−0.097***
	(0.002)	(0.002)	(0.033)	(0.022)	(0.020)	(0.020)
Years 3–4 $\times Y_i$	−0.002	−0.006**	0.028***	−0.017*	−0.025	−0.012
	(0.003)	(0.003)	(0.006)	(0.009)	(0.017)	(0.009)
Years 5–6 $\times Y_i$	−0.002	−0.007***	0.027***	−0.016**	−0.018*	−0.010
	(0.002)	(0.003)	(0.008)	(0.007)	(0.010)	(0.010)
Years 7–8 $\times Y_i$	−0.002	−0.006**	0.025***	0.002	−0.017*	−0.026***
	(0.002)	(0.002)	(0.008)	(0.008)	(0.009)	(0.009)
Years 9–10 $\times Y_i$	−0.001	−0.006***	0.023***	−0.008	−0.004	−0.006
	(0.002)	(0.002)	(0.008)	(0.007)	(0.007)	(0.009)
Years 11–12 $\times Y_i$	0.000	−0.004***	0.017***	0.000	−0.002	−0.007
	(0.002)	(0.001)	(0.006)	(0.007)	(0.010)	(0.007)
Years 13–14 $\times Y_i$	0.000	−0.002	0.014**	0.003	−0.003	−0.002
	(0.001)	(0.001)	(0.006)	(0.008)	(0.005)	(0.006)
Years 15–16 $\times Y_i$	−0.000	−0.002	0.009	0.001	−0.000	0.001
	(0.001)	(0.001)	(0.005)	(0.008)	(0.006)	(0.006)
Constant	−0.009	−0.011	−0.613***	−0.668***	−1.778***	−1.725***
	(0.015)	(0.016)	(0.123)	(0.123)	(0.317)	(0.310)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.342	0.351	0.340	0.333	0.393	0.389
Observations	5439	5439	5439	5439	5439	5439

Note: Y_i = Excess male mortality, or Number of male deaths, 1936–1939/Number of female deaths, 1936–1939. Dependent variables: (ln)Growth in relative population, (ln)Growth in relative fertility, and (ln)Growth in relative mortality. 1900–2011 period. Robust standard errors clustered by city in parentheses. In columns 2, 4 and 6, Nationalist cities act as the control group.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

TABLE 4 Dynamic effect of Spanish Civil War (SCW) duration.

Dependent variable:	Population Republican capital cities (1)	Fertility Republican capital cities (2)	Mortality Republican capital cities (3)
ln(Relative population)$_{it}$	−0.005*		
	(0.003)		
ln(Relative fertility)$_{it}$		−0.103***	
		(0.019)	
ln(Relative mortality)$_{it}$			−0.289***
			(0.049)
Years 1–2 ×Y_i	−0.010***	−0.009	−0.115***
	(0.004)	(0.028)	(0.032)
Years 3–4 ×Y_i	−0.008**	−0.028***	−0.010
	(0.004)	(0.009)	(0.013)
Years 5–6 ×Y_i	−0.009**	−0.024**	−0.007
	(0.003)	(0.009)	(0.018)
Years 7–8 ×Y_i	−0.008**	0.002	−0.031**
	(0.003)	(0.010)	(0.014)
Years 9–10 ×Y_i	−0.008***	−0.015	0.002
	(0.003)	(0.012)	(0.014)
Years 11–12 ×Y_i	−0.005**	−0.002	−0.005
	(0.002)	(0.008)	(0.010)
Years 13–14 ×Y_i	−0.004	−0.002	−0.003
	(0.002)	(0.010)	(0.010)
Years 15–16 ×Y_i	−0.003	−0.002	0.002
	(0.002)	(0.010)	(0.010)
Constant	−0.008	−0.673***	−1.792***
	(0.016)	(0.121)	(0.313)
City fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R^2	0.351	0.333	0.380
Observations	5439	5439	5439

Note: Y_i = SCW duration, measured as ln(year in which a city was taken by the winning side − 1935). 1900–2011 period. Dependent variables: (ln)Growth in relative population, (ln)Growth in relative fertility, and (ln)Growth in relative mortality. Robust standard errors clustered by city in parentheses. In columns 2, 4 and 6, Nationalist cities act as the control group.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

TABLE 5 The effect of the Spanish Civil War (SCW) on the level of city shares with heterogeneity in the shock.

Republican capital city	Y_i = Excess male mortality	Y_i = SCW duration
Albacete	5.31	6.88
Alicante	4.94	6.88
Almería	4.69	6.88
Barcelona	4.43	5.41
Bilbao	6.47	3.38
Castellón	7.32	3.38
Ciudad Real	6.34	6.88
Cuenca	5.60	6.88
Gerona	6.78	5.41
Guadalajara	8.60	6.88
Jaén	4.82	6.88
Lérida	9.83	5.41
Madrid	4.73	6.88
Murcia	4.47	6.88
Santander	6.04	3.38
Tarragona	7.86	5.41
Valencia	4.87	6.88
Average	6.06	5.92

Note: Counterfactual estimates for city shares using the coefficients reported in Tables 3 and 4. Percent bigger that they would have been had the SCW not occurred.

to 12 years). In Table 4, column 1, conditional upon the extent of the war shock measured as war duration, the result is basically the same.[37] That is to say, the longer lasted the war, the lower city growth *k* years after.[38]

Tables 3 and 4 confirm the length of the dynamic response of growth to the SCW shock for the Republican cities: 11–12 years. Nevertheless, as in these specifications the shock varied by city, the final effect on city size may also be different. A similar counterfactual to that based on Table 2 can be estimated. Here, how much bigger would the city share of *each individual* Republican city have been had the SCW not occurred?[39] For our two city-specific war-shock indicators, averages shown at the bottom of Table 5, 6.06% and 5.92%, are very similar to the previous figure of 5.97.

Finally, with the aim of gaining insight into the temporariness of the shock, in the rest of specifications reported in Tables 3 and 4, the growth in relative population is replaced with the growth in relative fertility and the growth in relative mortality as dependent variables. In Table 3, column 3 suggests that the recovery in fertility was more intense in those cities that suffered most from the war, as indicated by the significant and positive effect of

[37]Only the effect on Republican cities can be shown in this table, because the duration of war for Nationalist cities = ln(year in which a city was taken by the winning side − 1935 = ln(1936–1935) = 0, so Nationalist cities become the control group.

[38]As in Appendix G, we replicated the analysis using the province, rather than the city, as spatial unit. Unreported results tended to be similar to those shown in Tables 3 and 4, although the conditional dynamic effects were significant for a shorter duration. Variation in the excess male mortality ratio at the provincial level, as a matter of fact, is lower than at the city level.

[39]Now, the formula is as follows:

$$s_{i\,SCW+12} = s_{i\,SCW} + \sum_{k=1}^{12}((s_{i\,SCW+k} - s_{i\,SCW+k-1}) + Y_i \cdot |\hat{\delta}_k|).$$

the estimated coefficients of the interaction variable. The higher the excess male mortality ratio during the war, the higher fertility growth *k* years after. This finding is in line with studies on war-related effects of imbalances in sex ratios.[40] In particular, to that by Ogasawara and Komura (2022), who find a higher fertility rate caused by the scarcity of men after World War II in Japan. Interestingly, the fertility adjustment did not clearly differ between the two groups of cities. In Table 3, column 4, the estimated coefficients tend to be not significant. Essentially the same result is found in Table 4, column 2. Indeed, the aftermath of the SCW has been depicted as one period of low but overall recovering fertility (Reher, 2003). As for mortality, columns 5 and 6 in Table 3, and column 3 in Table 4 do not show a clear pattern in the effect of the SCW shock on our indicator of population loss beyond the very first years (Years 1–2 $\times Y_i$).

7 | CONCLUSION

The distribution of populations across space, especially urban populations, is at the heart of research in economics, human geography and other social sciences. As explained by Hanlon & Heblich, 2022) in their "History and urban economics" review, in recent times, some researchers have turned to the use of historical data to analyze the causes of the existence or growth of cities (see also Nagy, 2022). A major strand of this literature considers whether shocks have transitory or more permanent effects on city size.

Compared to other shocks, war is apparently among the greatest potential contributors to the changing of city size distributions. However, although different types of shocks have been studied, surprisingly very little work has been done on the effect of war on city growth since the early study by Davis and Weinstein (2002). Moreover, results are mixed. The finding of war shocks of both temporary and permanent natures provides evidence for the three main theories of city growth. A related debate is that of methodology. There are shortcomings related to the use of cross-sectional specifications in the predominant method.

A major contribution of this paper is the distinction between two groups of cities, according to their initial alignment with the winning or losing side, which enables us to disentangle the asymmetric effect of the war shock. By focusing on a civil war, the article also extends a literature that had concentrated on a very few, usually inter-state, wars. Methodologically, this paper adds to the scant literature on war effects which provide a dynamic approach. The models used are based on a long-term, annual data set, which is disaggregated at the capital city level. Not only is the length of the shock more precisely assessed, but it is also possible to estimate the shock's year by year effect.

Two main conclusions should be drawn from this study. First, for the entire sample of capital cities it shows that the Spanish Civil War did not have a significant effect on city growth. Second, the population growth in cities that belonged to the losing side throughout most of the war did suffer from a significant negative effect of the shock, as compared to cities that belonged to the winning side. The war effect, at any rate, was temporary, lasting just over a decade (11–12 years), and it also tended to decline towards the end of that period. A series of robustness checks tend to confirm the results. Moreover, the results hold up when the empirical strategy is modified to account for the variation in the heterogeneity of the war shock at the city level.

Additionally, we should not discard the distinction between population growth and population in levels. Although temporary, the effect of the Spanish Civil War on the growth of cities that aligned themselves with the losing side brings about a more permanent effect on their size. Twelve years after the war, the average population size of those cities was 6% lower than if the war had not occurred. This effect may best be explained by the theories based on random growth. Locational fundamentals would not have been able to make this group of cities return to their prewar trend. While increasing returns explanations would fit better with longer adjustment periods in growth and ultimately greater final effects on size.

[40]See Battistin et al. (2022, p. 448) for a recent review.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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