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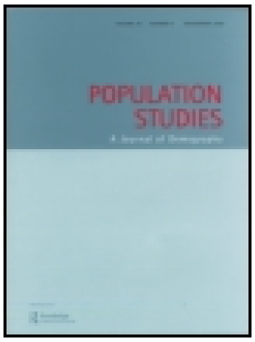

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Did the 1918 influenza pandemic cause a 1920 baby boom? Demographic evidence from neutral Europe

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In 1919–20, the European countries that were neutral in the First World War saw a small baby bust followed by a small baby boom. The sparse literature on this topic attributes the 1919 bust to individuals postponing conceptions during the peak of the 1918–20 influenza pandemic and the 1920 boom to recuperation of those conceptions. Using data from six large neutral countries of Europe, we present novel evidence contradicting that narrative. In fact, the subnational populations and maternal birth cohorts whose fertility was initially hit hardest by the pandemic were still experiencing below-average fertility in 1920. Demographic evidence, economic evidence, and a review of post-pandemic fertility trends outside Europe suggest that the 1920 baby boom in neutral Europe was caused by the end of the First World War, not by the end of the pandemic.

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Keywords: 1918 influenza pandemic; 1920 baby boom; historical demography; fertility; pandemics; mortality–fertility nexus; World War I

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Introduction

The classical framework of how human populations respond to mortality crises suggests that fertility should first fall and then rebound in the wake of mass death (Livi Bacci 2000; Heuveline and Poch 2007; Nobles et al. 2015). This is expected to be particularly the case for mortality crises that disproportionately affect young adults, such as the 1918–20 influenza pandemic (see Langford 2002; Saglanmak et al. 2011; van Wijhe et al. 2018). Therefore, just as that pandemic was followed by the Roaring Twenties—a decade of cultural innovation and relative economic prosperity in much of the Western world—we might also have expected it to be followed by a baby boom.

The 1920s was a time of historically unprecedented low fertility in Europe (Van Bavel 2010), but the basic demographic data do support the narrative of a short-term, post-pandemic bust–boom cycle. Figure 1 compares the relative crude birth rates (CBRs) in 1918, 1919, and 1920 (compared with 1915–17) in France, Hungary, Germany (countries involved in the First World War) and seven European countries militarily unaffected by the war. The belligerent countries saw very high upticks in fertility in both 1919 and 1920—because the war suppressed their fertility so gravely (see e.g. Vandenbroucke 2014)—but, with the exception of Spain, the neutral countries saw a clear bust–boom pattern over those years. The conclusion in the recent literature is that the First World War played little role in the neutral countries' fertility trends in this period and that their 1920 baby booms were due to post-pandemic fertility recuperation (Mamelund 2004, 2012; Bloom-Feshbach et al. 2011).

The mechanisms by which the influenza pandemic could have caused a baby boom are reasonable at face value. In populations across the world, there was a marked birth deficit roughly nine months after the pandemic's peak (Mamelund 2004; Bloom-Feshbach et al. 2011; Chandra and Yu 2015a, 2015b; Chandra et al. 2018; Dahal et al. 2018; Wagner et al. 2020; Kadt et al. 2021). This

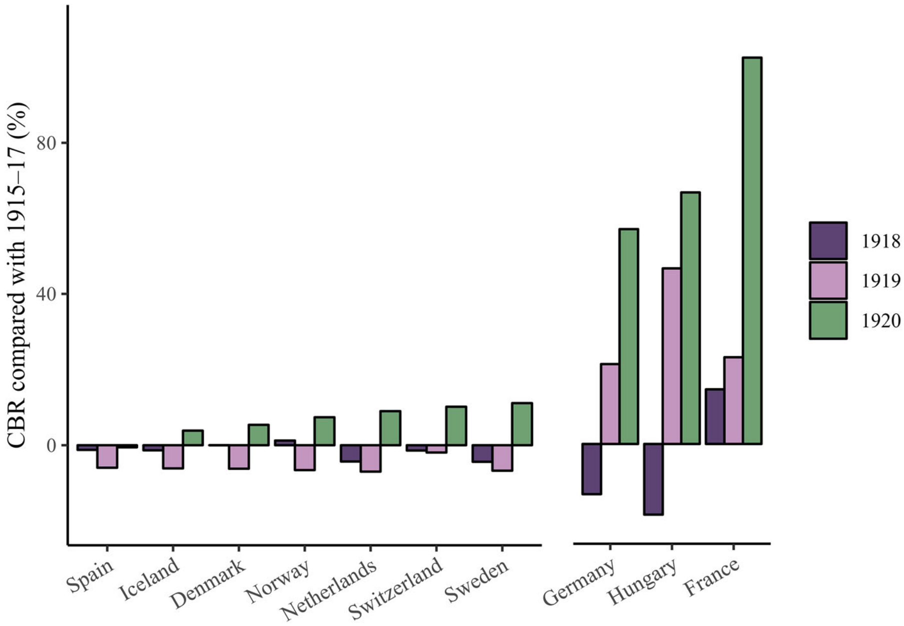

Figure 1 Relative crude birth rate (CBR) in 1918, 1919, and 1920 in seven neutral European countries (Spain, Iceland, Denmark, Norway, the Netherlands, Switzerland, and Sweden) and three belligerent European countries (Germany, Hungary, and France), using 1915–17 as a baseline
Source: Data used come from the Human Mortality Database (2023) and Jónsson and Magnússon (1997) for Iceland and from Chesnais (1992) for the other countries.

indicates that many conceptions were forgone during the pandemic, and we can assume that a sizable proportion of those conceptions were recuperated as the pandemic waned. There was also a surge in stillbirths around the peak of the pandemic (Reid 2005; Nishiura 2009; Chandra and Yu 2015b; Helgertz and Bengtsson 2019; Floris et al. 2022; see opposing evidence in Tripp et al. 2018; Khare et al. 2020), and this may have been associated with some post-pandemic live births: child death is known to increase the subsequent probability of maternal conception through the counterfactually early cessation of lactational amenorrhea and the desire to 'replace' the child lost (Palloni and Rafalimanana 1999; Lindstrom and Kiros 2007; Reher et al. 2017), and stillbirths may also trigger these two mechanisms. Additionally, there is some evidence that individuals can experience increased fertility in response to death in their communities, independent of how that mortality may have dampened their fertility (Rutayisire et al. 2013; Nobles et al. 2015; Broussard and Weitzman 2020; Smith-Greenway et al. 2022).

However, analyses of the 1918–20 pandemic and subsequent fertility trends are limited mostly to time series studies. In observing only that the 1920 boom followed the 1919 bust, these analyses do not make a strong causal case that the bust caused the boom (see Ní Bhrolcháin and Dyson 2007). In fact, two of the most detailed studies of the pandemic's impacts on family formation and fertility—Underwood's (1984) study of Guam (in Micronesia) and Herring's (1993) study of a Cree and Métis community in Manitoba, Canada—frame the post-pandemic recovery of fertility as a process of moderately paced adaptation, rather than as a one-year boom. Crucially, the existing literature does not test a plausible alternative hypothesis: that the pandemic caused the 1919 baby bust but some exogenous event, perhaps the end of the First World War, caused the 1920 baby boom independently of the bust.

The lack of suitable microdata available at present means that these competing hypotheses cannot be tested at the individual level, but they can be assessed at the aggregate level by comparing the magnitudes of the 1919 bust and 1920 boom across population subgroups (Mamelund 2004). On one hand, if we assume that the boom resulted from direct recuperation of births forgone in the bust (the *recuperation hypothesis*), relative fertility in 1919 and 1920 should correlate negatively: geographic and cohort-specific subgroups that experienced a stronger bust in 1919 should have seen a stronger boom in 1920. On the other hand, the relative birth trends could correlate positively: subgroups that experienced a stronger bust in 1919 may have also experienced a bust in 1920 because the pandemic had a persistent effect whereby it dampened fertility for at least two years (the *persistent dampening hypothesis*), whereas the net baby boom in 1920 was caused by a source exogenous to the pandemic. Mamelund (2004) reports evidence for the recuperation hypothesis based on geographic data from Norway but, to our knowledge, this method has not been used in any other analyses of the pandemic.

In this paper, we conclude that the persistent dampening hypothesis is a better interpretation of the available data from neutral Europe than the recuperation hypothesis is. Our analyses in support of this conclusion proceed in four stages, each with a different degree of certainty, but we find that they collectively provide strong support for this new historical narrative about the pandemic. First, in the Main results section, we test the recuperation and persistent dampening hypotheses using subnational fertility data from six countries and maternal cohort-specific fertility data from three countries. In doing so, we revisit Mamelund's (2004) work on Norway and extend it to five additional countries and an additional axis of population stratification. Second, we consider two alternative interpretations of these main results: that they are explained by the post-pandemic remarriage of widowers or by selective mortality during the pandemic. Third, we review what non-European demographic data show about fertility in the same period. Fourth, we use European economic data to bolster our suggestion that the 1920 baby boom in neutral Europe was caused by the economic effects of the end of the First World War, rather than the end of the pandemic. Finally, we discuss the relationship of our findings to other research on the pandemic.

Data and methods

For our geographic analysis, we obtained annual subnational live birth counts for 1916–20 for Norway, Sweden, Denmark, the Netherlands, Switzerland, and Spain: all the large European populations that were neutral during the First World War. Our focus on neutral populations is based on the lack of war as the predominant influential factor. Iceland was also neutral in the war, but subnational analysis for Iceland yields very small sample sizes. Iceland is also an unusual case because the pandemic was successfully confined to the west of the country through quarantines until 1921 (Summers et al. 2013). Subnational birth count data were available at municipal level for the Netherlands, at provincial level for Sweden, and for the urban (or capital) and rural (or non-capital) parts of provinces for the four remaining countries. For our cohort analysis, we obtained annual Lexis fertility rates for 1916–20 in Sweden and Denmark and converted age–period fertility rates for Norway to Lexis rates for those years.

Geographic fertility anomalies for 1919 and 1920 are established by standardizing the given annual birth count against the average annual birth count for 1916–18 in the same jurisdiction. Cohort fertility anomalies for 1919 and 1920 are established by subtracting the given period–cohort fertility rate (i.e. vertical Lexis parallelogram rate) from the average fertility in the same age range from Lexis parallelograms for 1916–18. In our geographic analysis of the Netherlands, we analyse only those municipalities with 250 or more births per year at baseline, in order to exclude the erratic annual fertility swings experienced by the country's smallest municipalities. However, we conduct sensitivity analysis to check whether this exclusion criterion affects our findings. Data for 1918 are included in the births baseline for the main analysis because the effect of the pandemic on live births seems to have been negligible in 1918, especially in comparison to 1919 (see Figure 1 and Chandra et al. 2018), but 1918 data are removed from the baseline in robustness checks. Additionally, we test whether adding stillbirths to our analysis affects our results.

At the time of the pandemic, vital registration completeness varied greatly within the countries of interest (see e.g. Ramiro Fariñas 1998), and we similarly expect subnational heterogeneity in the practices of recording a birth as live vs stillborn (see Davis 2009). However, these two sources of error should have remained fairly stable at the local level over 1916–20. Raw birth counts are used in the main geographic analyses instead of fertility rates because of denominator uncertainty. Even where annual subnational population estimates are, for example, reported in annual statistical yearbooks, the validity of these estimates is not always clear. Furthermore, interpolations between pre- and post-pandemic census counts can be distorted by any intercensal fluctuations in rates, including due to the pandemic itself. However, we test whether our geographic findings are robust to changes in subnational exposures to fertility by using published annual population counts as provisional denominators in the Netherlands and using exponential interpolations as provisional denominators in the other five populations.

All data and code used are provided in the supplementary material, with the exception of the data underlying our economic analysis for Spain. Those data can be requested from the researchers who digitized them (see Data notes for Spain in the supplementary material). Detailed information about our data sources (e.g. Brunborg and Mamelund 1994; Gómez-Tello et al. 2019; Boonstra 2021) and data handling is also provided in the supplementary material.

Main results

Figure 2 compares the relative subnational birth counts observed in Norway, Sweden, Denmark, the Netherlands, Switzerland, and Spain in 1919 and 1920 compared with 1916–18. In each country except Sweden, the anomalies correlate with each other strongly (adjusted $R^2 = 0.338–0.664$) and positively ($p < 0.001$, $n = 35–112$). This provides clear evidence in support of the persistent dampening hypothesis and clear evidence against the recuperation hypothesis. The regions that saw the strongest baby booms in 1920 are those that had seen minimal baby busts or fertility that was actually above average in 1919. In Norway, Denmark, the Netherlands, and Switzerland, those regions with the strongest 1919 baby busts tended to see a baseline number of births in 1920, rather than a baby boom. In Spain, the regions with the strongest 1919 baby busts tended to see baby busts also in 1920: in fact, busts of a similar magnitude as in 1919. In Sweden, the available geographic data—which are at the highest level of aggregation of any of the six countries of interest—provide support for neither the persistent dampening hypothesis nor the recuperation hypothesis ($p = 0.674$, $n = 25$).

Supplementary analysis indicates that these results are robust to excluding 1918 from the birth count baselines (Figure S1) and to weighting each regression by the absolute size of the annual birth baseline (Figure S2). In the case of the Netherlands, the significance of our finding is not sensitive to our small-municipality exclusion criterion (Figure S3). Additionally, the finding that the relative fertility anomalies in 1919 and 1920 are positively correlated holds if we use estimates of local CBRs instead of the recorded local birth counts (Figure S4). It also holds when adding stillbirths to the live birth counts in the countries with detailed stillbirth data available, namely Norway and Switzerland (Figure S5). We expect that this would also be the case in all other populations of interest, given that stillbirths tend to make up only a small proportion of all births, even in 1918–19.

Figure 3 shows the observed national cohort-specific fertility rates (CSFRs) for women in Norway, Sweden, and Denmark in 1919 and 1920 compared with 1916–18. In general, the 1920 baby boom in each country was concentrated among women who were aged 20–30 at the time (cohorts born around 1890–1900), whereas the 1919 baby bust was more concentrated among women who were aged 30–45 (born 1875–90). This is especially clear in Norway and Sweden. Women aged under 25 (or 30 in Sweden) tended to see a net boom over the two years, whereas women at older reproductive ages tended to see a net bust. In other words, the cohorts that saw the strongest baby busts in 1919 generally saw the smallest baby booms in 1920. This finding is also robust to using 1916–17 (Figure S6) or 1915–17 as the fertility baseline (Figure S7). This provides support for the persistent dampening hypothesis in the same way as our geographic results do.

Discussion

In all nine subgroup analyses—the panels of Figures 2 and 3—the evidence suggests that the 1920 baby boom was not a recuperation from the 1919 baby bust. Most regions and later (younger) reproductive cohorts experienced both a bust in 1919 and a boom in 1920, but populations with larger busts in 1919 did not see larger booms in 1920. In fact, the opposite seems to hold. In eight out of nine analyses, the populations whose fertility was hit hard in 1919 saw either the smallest baby booms in 1920 or yet another baby bust year in 1920 (as in the case of Spain). Therefore, the pandemic seems to have dampened the fertility of some groups in these countries for at least two years after its peak.

Our geographic results for Norway directly contradict those of Mamelund (2004). The conclusion in that paper in favour of the recuperation hypothesis unfortunately seems to have been drawn in error. Mamelund (2004, Table 8) reports that the subnational birth rate anomalies in Norway in the period January to September 1919 correlate positively with those in the period October 1919 to September 1920. This is consistent with our findings in Figures 2 and S4 and supports the persistent dampening hypothesis. Mamelund (2004) interprets the sign of the correlation incorrectly—as though it were negative—and therefore as support for the recuperation hypothesis.

Various mechanisms seem likely to have played some role in the persistent dampening we observe. Individuals whose spouses died in the pandemic may not have remarried until several years after the pandemic, if ever. Other individuals may have suffered pandemic sequelae (e.g. McCall et al. 2008; Strange 2022; see opposing evidence in Dourmashkin et al. 2012) or gained new care

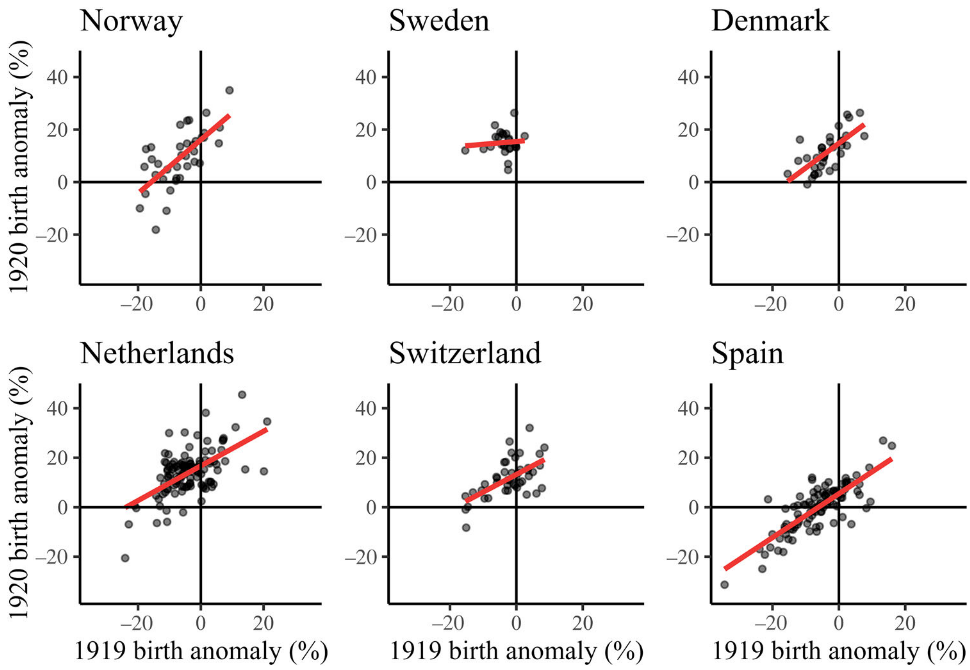

Figure 2 Correlation of relative subnational birth counts (anomalies) in 1919 and 1920 in Norway, Sweden, Denmark, the Netherlands, Switzerland, and Spain, using 1916–18 as a baseline
Source: See supplementary material for detailed information on data sources.

responsibilities for family members that caused them to delay marriage or childbearing, perhaps permanently. The pandemic may also have induced new perceptions in individuals that discouraged them from marrying or having children, at least in the medium term. Such perceptions could include existential uncertainty (see Outka 2019) or economic insecurity (see Karlsson et al. 2014; Galletta and Giommoni 2022). Mamelund (2004) finds that relative fertility in 1919 and 1920 still correlated strongly in Norway when controlling for excess mortality in the second half of 1918. This suggests that the pandemic dampened fertility largely through such perceptions, rather than through the impacts of its mortality. The fact that, in Sweden, Norway, and Denmark, 1919 fertility was dampened the least among the later (younger) cohorts (Figure 3) who faced the highest mortality from the pandemic also suggests an importance of such perceptions for the dampening observed.

Nevertheless, a 1920 baby boom did happen at the national level in five of the study countries and in much of Spain, the sixth country in our analysis. This raises the question of what caused that boom. France, Germany, the UK, non-Māori New Zealand, and Hungary also saw fertility rates rise significantly above their long-term baseline trends in 1920 (see Van Bavel 2010; Vandenbroucke 2014; Wilson et al. 2019). This phenomenon is poorly studied—especially in comparison to the baby boom that followed the Second World War (cf. Van Bavel and Reher 2013; Sandström 2014; Gauvreau et al. 2018)—but it seems likely that the baby booms in those countries resulted from recuperation following the severe impacts of the First World War on those countries' lives, families, and economies.

In the absence of a competing hypothesis, we suggest that the 1920 baby boom in neutral Europe was also a reaction to the end of the First World War. During the war, fertility rates in those countries did not fall much below their long-term trends, if at all (see Mamelund 2004; Van Bavel 2010), but the war still had impacts on individuals' lives and livelihoods. Increases in food prices and decreases in real wages were common in each country during the war, and their economies rapidly improved in the immediate post-war period (De Jong 2005; Ahlund 2012; Straumann 2015; Pedersen 2017; Gómez-Tello et al. 2019; Butie et al. 2020), although there was then a global deflationary crisis in 1920–21 (see e.g. Montgomery 1955; Velde 2022). Therefore, it is plausible that the neutral countries of Europe experienced a baby boom in 1920 as the result of a sudden improvement in relative economic conditions due to the end of the war. It is also possible that a post-war mood of celebration boosted fertility. In fact, it is thought that the celebrations of the November 1918 Armistice contributed to pandemic

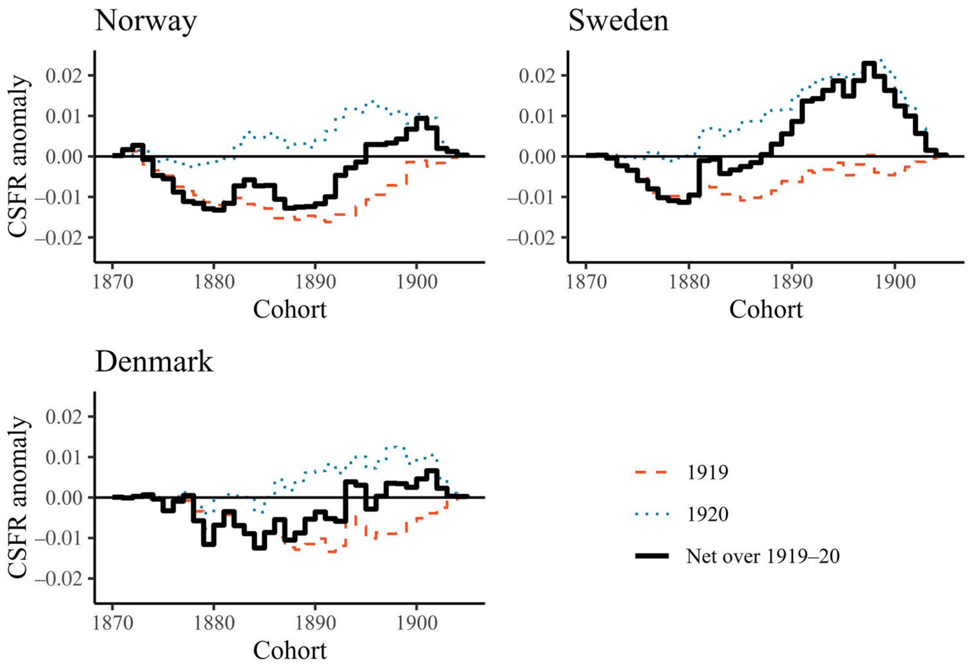

Figure 3 Cohort-specific fertility rate (CSFR) anomaly per woman in 1919 and 1920 in Norway, Sweden, and Denmark (using 1916–18 as a baseline) and net anomaly over 1919–20

Source: Data used come from the Human Fertility Database (2023) and Brunborg and Mamelund (1994).

caseloads in some locations (e.g. Crosby 1989, p. 85; Bulling 1991, pp. 16–17). However, quantitatively linking the high spirits at the end of the war in and of themselves to a baby boom is likely impossible. Suffice to say, the boom was due to the effects of the end of the war in general.

Alternative explanations

The role of widower remarriage

Previous work considering demographic trends following the influenza pandemic suggests that there was a large wave of pandemic widows and widowers remarrying in 1919 (Herring 1993; Muñoz-Pérez and Recaño-Valverde 2011). Additionally, two reviewers of this paper suggested that older widowers marrying younger women might explain the relatively high post-pandemic fertility of younger women (Figure 3). If this were the case, it would suggest a strong causal role of the pandemic in the 1920 boom, contrary to our overall argument. We suggest that widower remarriage (and nuptiality more generally) played a limited role in the 1920 baby boom in each of the six countries of study.

In the cases of Norway and Sweden, pre-existing literature on this topic supports our argument. In Norway, widows were constrained by law from remarrying for one year after the death of their husband, and widowers by custom did not usually remarry for one year after the death of their wife (see Mamelund 2004). This suggests a limited role for the remarriage of widows and widowers in Norway's 1920 baby boom. The fact that the number of marriages of non-widowed individuals in 1919 was down 15 per cent on 1917 suggests a limited role for nuptiality in the 1920 baby boom in Norway in general. In Sweden and Denmark, widow(er) remarriage customs were similar to those in Norway (Gaunt and Löfgren 1981, pp. 56–7). Indeed, Swedish crude marriage rates fell modestly from 1917 to 1919 and did so to an extent that appears to be uncorrelated with mortality during the main wave of the pandemic (Boberg-Fazlic et al. 2021, Figure C2). There was a marriage boom in 1920 in both Sweden and Norway, but the window for this to affect fertility in 1920 itself was limited.

Another line of evidence comes from the fact that in Spain and Switzerland, excess nuptiality in 1919 does not correlate with excess fertility in 1920. Assuming that the communities with the lowest relative fertility in 1919 experienced the highest rates of pandemic widowhood, we can deduce from Figure 2 that the communities with the most pandemic widows and widowers actually saw the smallest baby booms in 1920. However, directly testing this issue where possible is also fruitful. Figure S8 shows that the relative number of marriages in

1919 compared with 1917 does not correlate with the 1920 fertility anomaly across the rural and urban parts of Spanish provinces ($n = 98$). This is true whether we look at the 16 per cent of marriages that included at least one widow ($p = 0.586$) or at the 84 per cent of marriages that involved two non-widows ($p = 0.723$). In Switzerland, marriage counts by canton are not further disaggregated by civil status, but the relative number of all marriages in 1919 compared with 1917 does not correlate with relative fertility in 1920 across cantons either ($p = 0.507$; $n = 25$; Figure S9).

In Spain, Switzerland, Denmark, and the Netherlands, comparing the numbers of excess marriages in 1919 and excess births in 1920 also suggests that post-pandemic marriages, especially of widowers, could have caused only a modest proportion of the excess births in 1920. In Spain, there were only 10,002 more marriages involving widowers in 1919 than in 1917, compared with 21,200 more births in 1919 than 1917. Assuming that 20–40 per cent of those excess marriages yielded a birth within the first year (see Dribe et al. 2017, Table 2) and that none of the women in those marriages would otherwise have had children in 1920 (in an existing marriage, in a new marriage not involving a widower, or illegitimately), we can estimate that only 9–19 per cent of the excess births in 1920 in Spain were due to post-pandemic widower remarriage. This is a ceiling for this estimate, due to our second assumption being conservative and the fact that marriages are more likely to be officially recorded than births.

Meanwhile, applying our two simple assumptions to Switzerland suggests that only 2–4 per cent of the excess births in 1920 in that population were due to widower remarriage, based on only 970 excess marriages involving widowers in 1919 and 9,126 excess births in 1920. In the Netherlands, contemporary statistical yearbooks group remarriages of widowers and male divorcees together. However, even if we assume that there was only a negligible number of divorcees within that group, making our two assumptions leads us to estimate that only 2–3 per cent of the excess births in 1920 in the Netherlands were due to widower remarriage, based on 1,565 excess marriages involving widowers and male divorcees in 1919 and 19,875 excess births in 1920. In Denmark, contemporary statistical yearbooks do not report marriage counts by civil status. However, even if we assume that *all* 4,263 excess marriages in 1919 involved a widower, making the two assumptions leads us to estimate that widower remarriage can explain only 11–22 per cent of the 7,919 excess births in 1920.

This review of post-pandemic marriage patterns in neutral Europe is far from complete. Yet, it seems fairly clear that the 1920 baby boom cannot be explained by a surge in the reproductive output of newly widowed men. In fact, the literature on Norway and Sweden and our rough calculations for Denmark suggest that the baby boom in neutral Europe is unlikely to have stemmed in large part from post-pandemic nuptiality of any kind. If this is the case, it suggests that children born in neutral Europe in 1920 were disproportionately born to parents who had been married for longer. In turn, this could suggest that births in 1920 were disproportionately higher-order births, although the fact that the boom was concentrated among younger mothers (Figure 3) complicates this. However, this possibility is supported by the finding of Boberg-Fazlic et al. (2021, Table B5) that in Sweden's rural areas (where its baby boom was concentrated), fertility between September 1919 and September 1921 was disproportionately driven by higher-order births rather than first births. We return to this issue later in the discussion of our findings' relevance to other literature on the pandemic.

The role of selective mortality

It has also been suggested that selective mortality could explain our main result that relative fertility in 1919 and in 1920 correlate positively. In general, the selectivity of crisis mortality has important consequences for post-crisis population dynamics (Watkins and Menken 1985; Thornton et al. 1991). In the case of the 1918–20 influenza pandemic, we can envisage that it disproportionately killed individuals who would have been unlikely to have children in the post-pandemic period (due to their own age or health status), thereby boosting post-pandemic fertility rates purely by diminishing the denominator.

It is unlikely that this alternative story explains our result for three reasons. First, the positive correlation between relative subnational busts and booms is present whether we examine fertility rates or raw birth counts (Figures 2 and S4). Second, even when looking at rates, the elevated fertility in 1920 did not generally extend to 1921–22 (see Figure S10). If the baby boom in 1920 was largely a result of denominator change, the boom would have persisted into later years in the absence of another radical change in the denominator, all else being equal. Third, the selective aspects of the pandemic's mortality may have acted to decrease CBRs, rather than increase them. The pandemic's distinctive pattern was to kill individuals disproportionately at peak reproductive ages (see e.g. Saglanmak et al. 2011). Additionally, contrary to the previous narrative that the pandemic was 'egalitarian' with respect to socio-economic status (see Mamelund 2006), most quantitative analysis finds that the pandemic disproportionately killed people from poorer backgrounds (Klein 1973; Mamelund 2006; Grantz et al. 2016; Bengtsson et al. 2018; Wilson et al. 2018; Mamelund et al. 2021; Bakhtiari 2022; D'Adamo et al. 2023; see opposing evidence in Eiermann et al. 2022). Since the pandemic struck at a time when fertility tended to be higher among poorer populations (i.e. unskilled labourers) in at least some of the countries of interest (Bras 2014; Dribe and Scalone 2014; Dribe et al. 2014; Jaeggi et al. 2022), its socio-economic selectivity dimension likely further dampened fertility rates. Also, while there are suggestions that the pandemic disproportionately killed people infected with tuberculosis (Noymer and Garenne 2000; Noymer 2009; see opposing evidence in Sawchuk 2009; van Doren and Sattenspiel 2021), it is not clear that fertility was lower on average among people with tuberculosis than people without it, especially given the confounding of tuberculosis status with socio-economic status.

Further results

Post-pandemic fertility beyond Europe

Our suggestion that the 1920 baby boom in neutral Europe was a result of the end of the First World War is also supported by the near-complete absence of a 1920 baby boom outside Europe. Figure 4 displays the percentage change in CBR between 1915–17 and 1920 for a range of national populations throughout the world, including each of the non-belligerent European countries in Figure 1. The non-European populations shown consist of all 12 populations with data available in Chesnais (1992), plus five selected populations with data available in *International Historical Statistics* (Palgrave Macmillan 2013), Cyprus (Verropoulou 1997), and Korea (with rates calculated from Kim 1966, pp. 7, 21). Among the populations in neutral Europe, all but Spain display notably elevated fertility in 1920. Among the populations outside Europe, all but Japan and Egypt exhibit average or below-average fertility in 1920.

This geographic disparity is made even clearer by the fact that the elevated value for Egypt in Figure 4 may be only an artefact of changing birth registration completeness. Between the 1907 and 1917 Censuses in Egypt, the registered CBR and the census ratio of children aged under five to women aged 15–49 fell to a similar degree, but between the 1917 and 1927 Censuses this child–woman ratio stagnated while the registered CBR soared, especially in cities and beginning in 1920 (see Kiser 1944; see also El-Badry 1955). Assuming that coverage of children was not significantly poorer in the 1927 Census than the 1917 Census, a clear interpretation of this divergence is that the persistent rise in the Egyptian CBR beginning in 1920 was due to rapidly improving birth reporting rather than an increase in fertility.

In fact, we might interpret the large number of non-European populations with below-average fertility in 1920 as evidence for the persistent dampening hypothesis and its corollary that in the absence of the effects of the First World War, Europe would also have seen relatively low fertility in 1920 at the national level. Drawing this general conclusion seems premature at the moment. The post-war period saw rapid fertility declines in the United States (US), Canada, Australia, and New Zealand (Caldwell 2006; Van Bavel 2010) like in Western Europe; it is not clear how much of the difference between fertility in 1915–17 and 1920 in those and other populations was due to secular changes rather than any dampening effects of the pandemic.

The fact that the directly ruled parts of British India saw such unusually low fertility in 1920 (see also Mills 1986; Dyson 1989), as well as in 1921–22, may be more indicative. Although the pandemic death toll in British India is greatly disputed (Davis 1951; Mills 1986; Hill 2011; Chandra et al. 2012; Tumbe 2020), the various published estimates all suggest that it likely experienced the highest proportional death toll of any large national population in the world. Therefore, British India is exactly the type of population that we would expect to experience strongly below-average fertility after the pandemic if the pandemic did have persistent dampening effects on fertility. This argument is tentative, as it is not understood what effect the pandemic had on vital registration completeness in British India (Davis 1951, p. 237), nor how severely the Non-Cooperation Movement disrupted the quality of colonial statistics in 1921–22 (see Chandrasekhar 1972, p. 33). Indeed, the same concern can be raised about the effect of the Sam-il Movement for independence of vital statistics in Korea in 1919–20 (see Kim 1966, p. 8; Baldwin 1979, p. 149).

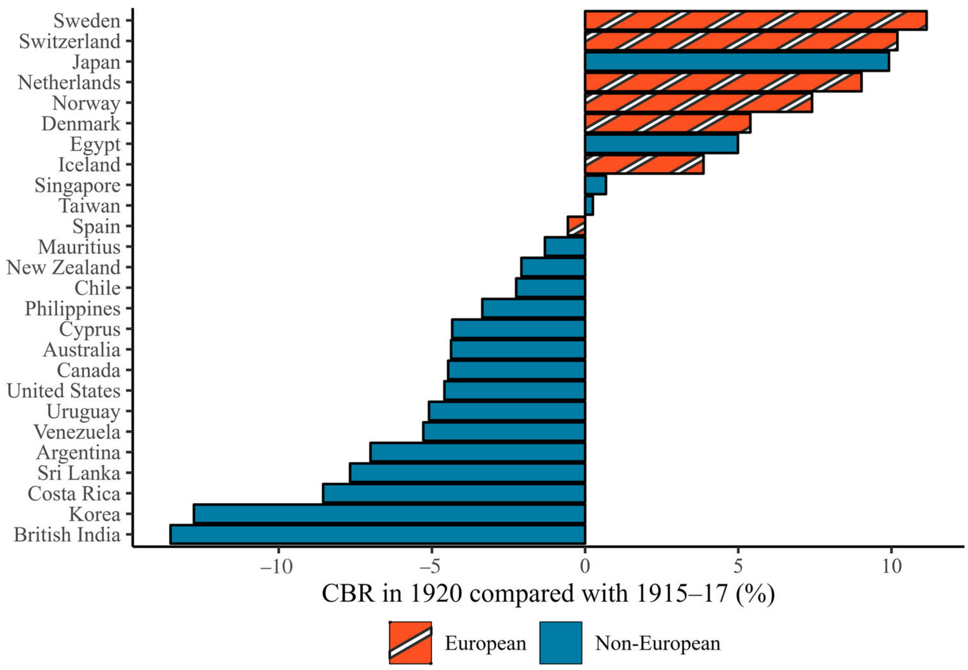

Figure 4 Relative crude birth rate (CBR) in 1920 in a range of non-European and neutral European countries, using 1915–17 as a baseline
Source: Data used come from Chesnais (1992), *International Historical Statistics* (Palgrave Macmillan 2013), Verropoulou (1997), and Kim (1966).

However, it is further indicative that the directly ruled districts of British India with the highest influenza-attributed mortality rates in 1918–19 reported significantly elevated women's labour force participation rates in the service sector at the 1921 Census (Fenske et al. 2022). Whether we interpret labour force participation as a cause or an effect of a slump in fertility, this result coheres with the suggestion that the pandemic dampened fertility in British India by a large amount and potentially for more than two years.

A few non-European populations clearly seem to have experienced abnormally high CBRs in 1920, in contrast to Egypt. Taeuber (1958, pp. 232–3) reports that evidence of a 1920 baby boom in Japan showed up both in official vital rates and in mid-century population pyramids, although she finds the boom 'puzzling'. Mid-century censuses from South Africa also indicate a relatively large 1920 birth cohort (Kadt et al. 2021), and *International Historical Statistics* reports that CBRs in the British Caribbean colonies of Barbados and Jamaica were more than 20 per cent higher in 1920 than in 1915–17 (Palgrave Macmillan 2013). Mexico also experienced a large baby boom starting in 1920 (Zavala de Cosío 1992), but this came at the end of a decade of revolution during which the pandemic contributed only a small proportion of the overall demographic disruption (McCaa 2003).

These cases warrant further investigation but may be attributable to the fact that all these populations were militarily involved in the First World War (either as independent powers or British colonies) or were experiencing other very exceptional circumstances. Additionally, Ohbuchi (1998, pp. 339–40) frames the CBR in Japan in 1920 as simply the peak of a gradual increase in that indicator that began around 1890 (see also Chandra and Yu 2015b). The 1918–20 pandemic was a global phenomenon, with almost no national exceptions (McLane 2013). Indeed, the havoc caused by the pandemic in each of the non-European populations in Figure 4 is at least partially documented (e.g. McQueen 1976; Langford and Storey 1992; Killingray 1994, 2003; Verropoulou 1997; Lee et al. 2007; Gealogo 2009; Hsieh 2009; Lim 2011; Maureira 2012; Humphries 2013; Hayami 2015; Botey Sobrado 2017; Summers et al. 2018; Cristina et al. 2019; Carbonetti and Rivero 2020; Tumbe 2020; Rose 2021). Any pandemic-induced baby boom should be visible on a global scale similar to that of the pandemic itself. Since the 1920 baby boom seems to have occurred almost only in Europe, we suggest that there must be a non-pandemic, Europe-specific explanation: the First World War.

Testing the First World War's effect on the baby boom

Our evidence that the 1920 baby boom was not caused by the pandemic serves as indirect evidence that the end of the First World War caused it instead. Our argument here is a tentative one that since the pandemic was not the culprit, the war must have been, as the only other likely suspect. Such arguments by exclusion can be correct, and they have played an important role in the field of historical demography (e.g. McKeown and Record 1962). However, in our case, some economic data from the end of the war and immediate post-war period can also be brought to bear on the new historical narrative we propose.

We contend that communities whose economies recovered fastest in the year after the end of the First World War are those which experienced the largest 1920 baby boom, controlling for the local 1919 baby bust. Unfortunately, the post-war economic recovery is difficult to measure at the local level. Price and wage data rarely seem to have been collected systematically. They also rarely seem to have been reported together, which is a problem because prices and wages jointly affect household purchasing power. Increasing wages could be a genuine sign of prosperity or simply a (partial) response to inflation. However, in the absence of simultaneous data on both variables, we assume here that relative price increases are a negative economic indicator and relative wage increases are a positive economic indicator. Therefore, we interpret a negative association between price increases in 1919 and relative fertility in 1920 as evidence that the prevailing economic fluctuation was a major determinant of fertility in 1920.

Spain presents a rare case for which price data for household goods were collected continuously and at local level during this period. Specifically, biannual price data are available for a range of 21 household goods (plus the cost of renting a single room) at the same geographic level as fertility data, that is, for the rural and urban parts of provinces (Gómez-Tello et al. 2019). Therefore, we test whether price changes between the winter of 1918–19 and the winter of 1919–20 correlate with relative fertility in 1920, controlling for relative fertility in 1919. Additionally, we test whether there is a statistically significant interaction between price change and 1919 fertility in their association with 1920 fertility. We would theoretically expect fertility to have been more responsive to price changes in communities where the pandemic did not dampen fertility strongly.

Figure 5 shows the most supportive results from this analysis. We find a clear interaction effect whereby increases in the price of bread ($p = 0.012$; $n = 73$), wheat ($p = 0.027$; $n = 67$), beef ($p = 0.040$; $n = 57$), mutton ($p = 0.034$; $n = 64$), fuel ($p = 0.017$; $n = 67$), and rent ($p = 0.067$; $n = 63$) correlate with relatively low fertility in 1920 in cases where local fertility was not strongly dampened in 1919. There are also interaction effects in the expected direction for potatoes ($p = 0.086$; $n = 70$) and green beans ($p = 0.021$; $n = 67$). At high levels of increase in these prices, there is relatively little association between relative fertility in 1919 and in 1920, as indicated by the narrowness of the difference between the predicted levels of relative 1920 fertility across the three terciles of relative 1919 fertility at the right-hand side of the subplots in Figure 5. In other words, communities with high inflation for these goods in 1919 saw relatively uniform relative fertility in 1920 without much respect to how strongly the pandemic seems to have dampened their fertility. At low levels of increase in these prices, there is a strong positive association between relative fertility in 1919 and in 1920. In other words, the 1920 baby boom in Spain was concentrated among communities that saw relative stability or decreases in the prices of these goods in the year after the end of the First World War. Additionally, non-interacting models indicate a negative association between price increases and 1920 fertility, controlling for 1919 fertility, when considering the cost of electric light ($p = 0.052$; $n = 62$) and milk ($p = 0.065$; $n = 72$). These results suggest that immediate post-war economic developments were very important in shaping fertility in 1920, at least in the case of Spain.

The results shown in Figure 5 are somewhat cherry-picked. Table S1 shows the coefficient estimates, standard errors, and significance levels of the model terms for all 22 household prices, when allowing and not allowing for interaction between 1919 fertility and price changes. However, we find them to be supportive of our overall argument. There are only two goods (soap and coffee) for which price increases are associated with higher fertility in 1920 at the $p < 0.10$ level of statistical significance; this compares with the 10 goods mentioned earlier for which the association is in the direction predicted by our hypothesis. The fact that the available data on household fundamentals such as bread, milk, fuel, and rent fit our hypothesis is also noteworthy.

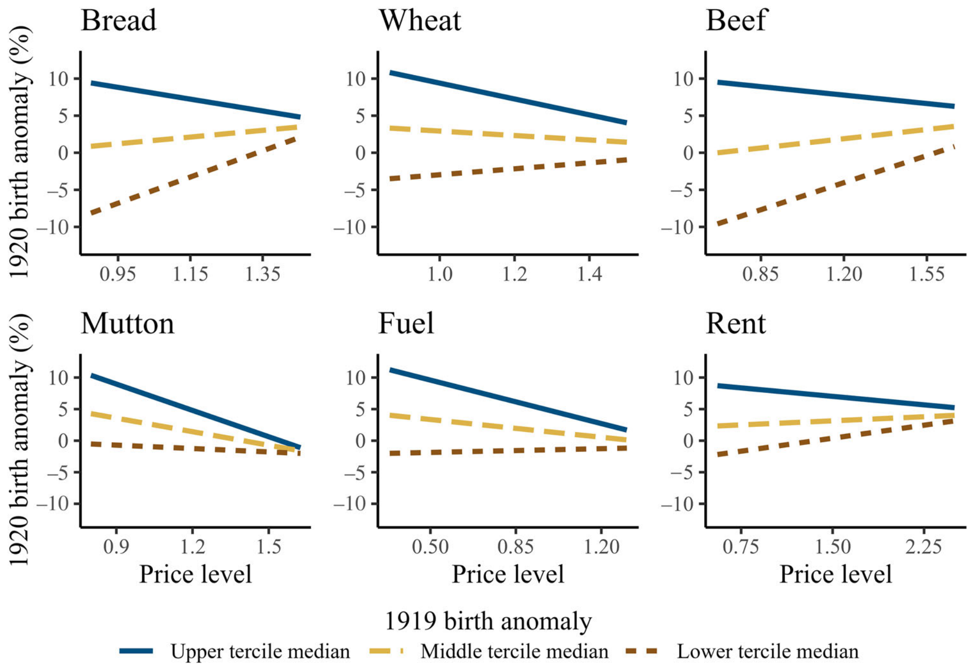

Figure 5 Interaction plots showing the linearly predicted value of the local 1920 relative birth anomaly in Spain (using 1916–18 as a baseline) based on the change in the price of bread, wheat, beef, mutton, fuel, and rent from winter 1918–19 to winter 1919–20 and different levels of 1919 relative birth anomaly
Source: See supplementary material for detailed information on data sources.

There seems to be no economic data set of comparable utility to that of Gómez-Tello et al. (2019) in any of the five other countries of interest, but as a second direct test of our hypothesis, we look at wage data from Norway. The wages of only six occupational groups in both cities and rural locations were measured by contemporary authorities. Additionally, it is not feasible to match wage data to the urban and rural parts of counties for which birth count data are available. However, averaging the relative changes in the available wage data between April 1918 and April 1919 at the county level yields further evidence supporting our hypothesis. The small sample sizes for each occupation (n = 17–18) make modelling an interaction effect impractical, but Table S2 shows that, when controlling for relative fertility in 1919, relative wage increases for two occupational groups are positively associated with relative fertility in 1920 at the $p < 0.05$ level. Wage increases for an additional two groups are significant at the $p < 0.10$ level.

These economic results are admittedly meagre, but we take them as support for our argument by exclusion that the boom was, in fact, caused by the end of the First World War, given that it does not seem to have been caused by the end of the pandemic. In future, these results should be reconsidered in quantitative and qualitative work that more closely considers the effect of short-term economic trends in this period on individuals' fertility behaviours.

Relevance to other research on the pandemic

Our key finding affirms the conclusion that different populations tend to be affected by pandemics in remarkably different ways. The 1918–20 influenza pandemic varied in its subnational severity (see e.g. Mamelund 2003; Hayami 2015; Bambra et al. 2021), but populations and families also varied in their capacity to cope with the pandemic. The historical literature reports that some community institutions reacted fairly effectively to the pandemic (e.g. Keeling 2010; McLane 2013; León-Sanz 2014; Gallardo-Albarrán and de Zwart 2021), whereas others were paralysed to the detriment of those they served (e.g. Ellison 2003; Feldman 2014; Sobral et al. 2014; Rao and Greve 2018). The pandemic's effect on severe mental health outcomes such as suicides seems to have been mostly null (see a review in Gaddy 2021; see also Chapelle 2022; Strange 2022), but its adverse effects were wide reaching in other domains. For example, recent econometric work concludes that the influenza pandemic was causally associated with the economic betterment of the wealthy in Italy at the expense of the poor (Galletta and Giommoni 2022).

On the specific front of post-pandemic fertility, Wagner et al. (2020) find that cities in the US that implemented longer periods of non-pharmaceutical intervention against the influenza pandemic saw their fertility rebound to pre-pandemic levels the fastest. Boberg-Fazlic et al. (2021, see Tables B5 and B6) also find that conceptions in the two years after the peak of the pandemic in Sweden were disproportionately driven by married women and women with high-social-status surnames. Like Covid-19, the 1918–20 influenza pandemic was not a great equalizer (Mamelund and Dimka 2021), and our findings show yet another way in which that was true. The pandemic did *not* result in a broad-based bust–boom cycle that would have meant its effects on fertility were effectively egalitarian.

Our findings are also relevant to the literature on the effect of in utero exposure to the 1918–20 pandemic. Several countries report that the 1919 birth cohort was disadvantaged in terms of health or economic status relative to surrounding cohorts (e.g. Almond 2006; see opposing evidence in Cohen et al. 2010). This can be interpreted as evidence for the pandemic influenza strain having adverse in utero effects. However, Beach et al. (2022) reports that this finding in the US may be entirely attributable to the 1919 birth cohort having been born into disproportionately disadvantaged households. Floris et al. (2022) find small-scale but similar results in Bern, Switzerland. Similarly, Smith-Greenway et al. (2022) find that the disruptive effect of death can increase bystanders' risk of unintended pregnancy far more than that of intended pregnancy, albeit in a contemporary context.

We additionally suggest that the 1920 cohort—which is used as part of the baseline comparison with 1919 in most studies considering the in utero question—may have been born into relatively advantaged households. The findings of Boberg-Fazlic et al. (2021) on the social correlates of fertility in Sweden in 1920 make this point clearly. In our own work, we find that in five out of the six national populations, children born in 1920 were disproportionately born into communities with relatively high fertility in 1919 and which can therefore be assumed to have been only mildly socially affected by the pandemic. As discussed in our subsection on widower remarriage, it also seems more likely than not that children born in neutral Europe in 1920 were born to parents who had been married for longer and may have had more experience in raising children than average. Understanding which families were and which families were not having children during and in the wake of the pandemic may contribute to the debate around the impacts of in utero exposure, especially on which baselines are appropriate for estimating the effects of exposure.

Conclusion

We offer the novel hypothesis that subpopulations whose fertility was hit hardest by the 1918–20 influenza pandemic saw below-average fertility in both 1919 and 1920. Subnational and cohort-specific data from six countries support this hypothesis. Our results suggested that the baby boom that occurred in much of neutral Europe in 1920 was caused by the end of the First World War, especially its economic impacts, and not by the waning of the pandemic. With the work of Mamelund (2004) cast in doubt, we know of no clear evidence, even at the ecological level, that individuals who postponed fertility during the main wave of the pandemic recuperated that fertility to a significant extent in 1920.

These findings are novel, and we hope they will prompt deeper research into the mortality–fertility nexus of the 1918–20 pandemic. As such, we would like to suggest several areas of research that might follow up on our work. To our knowledge, national CSFRs are not available for the period of interest in Spain, Switzerland, or the Netherlands, but subnational cohort-specific analysis in those countries could support or challenge our findings. Analyses of microdata may also contradict our conclusion, especially due to the possibility that some ecological fallacy lurks in our analysis. We discussed two mechanisms that have been suggested to us as alternative explanations for our main results, but we may have left other possible mechanisms unexplored. Microdata that would enable individual-level mechanisms to be studied more closely might be available for Iceland, where the families afflicted by pandemic mortality have already been mapped (see Gottfredsson et al. 2008). Additionally, there may have been a 1920 baby boom in some subpopulations of non-European countries, even though none was apparent at the national level; this would be like the case of Spain. It is also crucially important to study how fertility rebounded after the pandemic in indigenous and colonized populations, whose mortality from the pandemic was globally most severe by far (see e.g. Ohadike 1991; Adams 1997; Mamelund et al. 2013; Brady and Bahr 2014; Rice 2019; Gara 2020). In doing so, research can build on the work of Underwood (1984) and Herring (1993).

The mechanisms by which the pandemic affected individuals' fertility behaviours must also be better quantified and disentangled. The fact that among the neutral countries of Europe, Spain reported the highest 1918 excess mortality (Echeverri 2003; Ansart et al. 2009) but the lowest relative fertility in 1920 (Figure 1) could suggest the importance of the direct effects of mortality on fertility, or this may be a coincidence. Our cohort analysis and the geographic analysis of Mamelund (2004) tentatively suggest that psychological or cultural reactions to the pandemic may have been key to the post-pandemic dampening of fertility. However, it is therefore puzzling that the dampening seems to have been strongest at older reproductive ages. We would expect younger groups to have been more affected by any uncertainty or ideational change that the pandemic caused. It does not seem plausible that this effect was due only to a reduction in fecundity as a result of maternal ageing between 1919 and 1920.

To better disentangle how the pandemic impacted fertility in general, it will be necessary to understand the ways in which pandemic mortality, morbidity, and social disruption affected the ability of different subpopulations to either achieve or prevent childbearing. Doing so thoroughly will require interdisciplinary efforts and an integrated biosocial framework. All pandemic experiences are biosocial (see e.g. Fuentes 2020; Dimka et al. 2022), but a pandemic's effects on kinship and fertility are especially so. The 1918–20 influenza pandemic's effects on fertility are much less well understood than its death toll. Studying the former, especially through a biosocial lens, may yield key insights into both historical and modern populations' experiences of pandemics.

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No potential conflict of interest was reported by the authors.

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