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CDE Working Paper No. 96-28

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Paper presented at the preparatory meeting of the Twelfth International Economic History Congress (Sevilla, Spain) on Population and the Economy: From hunger to Modern Economic Growth, Osaka, Japan: January 7-10, 1997

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INTRODUCTION

This paper evaluates the relevance of a Malthusian framework in the effort to account for short-term fluctuations in vital rates in Latin America during the period 1800-1990. We have three goals. First, we evaluate short-term responses during the period 1920-1990. This period is relatively rich in data and contains a number of historical episodes with potentially strong impact on population. We show that even after the onset of transitions toward modern demographic regimes, there is evidence of a residual mechanism whereby marriages, births, and deaths react to changes in economic conditions. This evidence is not particularly strong but is consistent with expectations. Of some importance are the effects on mortality by causes of death during the period 1950-1990, a period punctuated by a prolonged crisis (the "debt crisis") that most of these countries experienced during the late seventies and throughout the eighties. Second, we estimate patterns of short-term demographic responses to changes in aggregate measures of socioeconomic well-being during the post-colonial period (roughly during 1820-1920) in some key cities (Buenos Aires (Argentina), Havana (Cuba), Mexico City (Mexico), Salvador (Brazil)), and in long time series for Chile. We identify strong responses during this early period in all five social settings. These responses are consistent with the presence of Malthusian short run fluctuations, and are strikingly similar to those observed during the 20th century in Latin America as well as those estimated in Western Europe during the 19th century. Third, we evaluate the impact of the estimated short-term fluctuations for the long run behavior of the demographic regimes in the areas under study.

MALTHUSIAN SHORT-TERM RESPONSES: A REVIEW.

It is no doubt true that, as suggested by Galloway (1988), the examination of the preventive and positive checks over the short run afford us with some unique opportunities for hypothesis testing. First, there is an important, though by no means exhaustive, set of propositions derived from the Malthusian framework that can be put forward to a rather rigorous test. Second, this can be done via formulation of suitable models that circumvent problems of simultaneity and joint causation of time series that otherwise abound in the analysis of the long term. Third, the data requirements for models in the short term are less stringent since, in most cases, observed population changes lead to marginal deviations from gradual trends in population counts. This makes it unnecessary to examine vital rates which are difficult to estimate without accurate yearly population counts.

But identification of short term responses, even if properly and convincingly demonstrated, is far from being a complete tool to verify the operation of a Malthusian regime. By all accounts, short-term demographic responses tend to have, in the long run at least, no more than a modest impact on population size, and appear to contribute little to alter the overall balance between standards of living, resources and population size (Watkins and Menken, 1985; Palloni, 1988). This theme is reviewed in more detail in the final section of the paper. Also, the relations between wages, technological shifts, and population density appear to be played out over lengthy periods of times, involving several generations, and it is in the corresponding dynamics where some of the most interesting relations of a Malthusian regime can be observed (Lee, 1996). Thus, short-term dynamics is probably a very partial albeit necessary ingredient of the workings of Malthusian regimes, and the overall accuracy or validity of the Malthusian model is not made or broken by the presence or absence of expected short-term regularities.

Despite these caveats, it is hard to dismiss the idea that to establish the applicability and credibility of Malthusian regimes in Latin America it is of some importance to investigate the nature of short-term responses. In particular, we intend to verify the following two propositions associated with the core of Malthus' population theory:

i) The preventive check: changes in real wages — or other measures of standards of living — ought to have a visible effect in the propensity to marry and in marital fertility rates;

ii) The positive check: changes in real wages — or other measures of standards of living — should induce changes in mortality at all ages, but particularly among young children and the elderly, the most vulnerable of all age groups.

It is well-established that in pre-industrial Europe, the empirical evidence for the so-called preventive check is fairly strong whereas that pertaining to the positive check is rather weak (Lee, 1996; Wrigley and Schofield, 1981; Galloway, 1988). Furthermore, empirical regularities associated with the preventive check indicate that the corresponding responses involve a short-term effect as well as a mechanism spread over 25 to 30 years, a pseudo-Easterlin cycle where the marriage pattern of one generation reflects labor market constraints determined by cohorts' size which are, in turn, affected by real wages experienced a generation before. In addition, several researchers have uncovered evidence suggesting the operation of short term responses — for both the preventive and positive check — in disparate places in developing areas, Latin America, Asia and Sub-Saharan Africa, verifying the operation not only of the preventive but also of the positive check (Lee, 1996; Bravo, 1992; National Research Council, 1993).

We will seek evidence for Malthusian short-term responses in Latin America at various stages of its history. The main questions that guide our investigation are the following: Are these responses present at all and, if so, do they change in character or magnitude as countries experience economic development and undergo, with variable timing and intensity, demographic transitions from high-mortality and high-fertility regimes to low-mortality and low-fertility ones? Is it the case that the preventive check overwhelms the positive check, much as it did in Western Europe? Are responses similar across countries or is there identifiable inter-country heterogeneity? Are responses comparable to those estimated in pre-industrial settings in Western

Europe or in other developing countries?

In the sections that follow we discuss expected patterns of responses, review data and data sources, introduce methods of estimation, and evaluate main results. Our discussion of expected responses of nuptiality, fertility and mortality follows established conventions and attempts to identify how marriages, births, and deaths respond to changes in economic conditions. To simplify the discussion, we refer throughout to responses to a hypothetical economic crisis or downturn and assume, as is normally done in the literature, that demographic responses are symmetric, namely, that an upturn or economic boom will produce responses equal in magnitude but opposite in sign relative to those associated with an economic crises.

a. The response of nuptiality.

Postponement of marriages, particularly first marriages, is a common response to economic changes. Indeed it is thought to be the only Malthusian mechanism of some relevance for the short run course of preindustrial populations. The main mechanism producing the response is related to the couples' capacity, real or perceived, to form and sustain viable households. To the extent that new couples rely on established households for the satisfaction of their immediate needs, at least during a short period after the union takes place, the response may be significantly attenuated. That is, one should expect that the number of marriages will be more sensitive to changes in social contexts where marriage initiation marks the formation of a new household. A second but less important mechanism involves increases (decreases) in remarriages made possible due to increases in widow(er)ing. Severe economic crises may increase adult mortality and widow(er)ing which will open marriage opportunities in the short run and therefore could trigger a larger than normal number of marriages.

The most typical nuptiality response to a strong economic crisis is an immediate drop in the number of marriages followed by a lagged increase above the number expected under normal conditions as postponed marriages are made up in a concentrated period of time. In social and economic contexts where anticipatory behavior is feasible, the number of marriages may begin to dip during or even right before the onset of the first clear signals of a crisis and responses will be visible without significant lags. An analogous pattern of response with reversed signs should be associated with economic upturns.

However, in societies such as those in Latin America where the prevailing nuptiality regime has always included heavy representation of consensual unions, it is not at all clear that actual responses should follow the expected patterns. This is because the linkage between union formation and economic requirements is, by and large, weaker for consensual unions than for more formal marriages. In fact, consensual unions frequently involve extended families and kin networks that erode somewhat the strong requirements of financial solvency before a couple can begin to live together. More generally, any dissociation between marriage and household formation could weaken the linkage between social and economic well being, on the one hand, and union formation on the other.

b. The response of fertility.

Delays in first (and higher order marriages) will reduce the number of births — particularly first births — within the year following the onset of a crisis. The fertility response, of course, may also be associated with changes in **marital fertility** as couples postpone planned births. In societies practicing contraception and where anticipatory behavior and adjustments are routine, the reduction of births may occur even earlier, and if access to and practice of abortion is widespread, the response can be simultaneous with the onset of the crisis. A similar result will take place if the crisis compromises the health and nutritional status of pregnant mothers, as stillbirths and spontaneous abortions will inevitably increase. As with marriages, the number of births tends to rebound and track the weakening of the crisis. This is due to the higher than normal number of new couples, to delayed births that are made up as soon as breathing room is found and, finally, to the more favorable distribution of women by fecundity status.

But the fertility response may also move in the opposite direction, that is, the number of births can increase as a result of a crisis. This is particularly true if mothers abandon traditional breastfeeding practices and if the crisis itself undermines norms that restrict sexual activity among the very young and among those who are widowed and separated. This is most likely to occur under conditions such as severe social stress frequently accompanied by political instability, warfare or massive displacement of populations.

In summary, following a crisis we would expect a drop in the number of births a year or so after the onset of the downturn followed by a gradual recovery, the result of increases in marriages and of second and higher order births that were postponed.

c. The response of mortality.

There are a number of ways in which economic crises can induce increases in mortality. First, the effect may be direct when a crisis produces starvation or acute nutritional deficiencies and their sequelae. More commonly, economic crises aggravate socioeconomic conditions among the poorest segments of the populations and increase exposure to diseases (through breakdown of infrastructure), erode population resistance to illnesses (lowering nutritional status, disrupting immunization programs) or, finally, compromise recovery (undermining access to health care). Of particular importance are the relations between nutritional status and about ten infectious diseases, including cholera, bacterial diarrhea, measles, respiratory tuberculosis, whooping cough, and some acute respiratory diseases. Regardless of the mechanism involved, infants and young children as well as the elderly are likely to be more affected than adult segments of the population.

Unlike nuptiality and fertility, the timing and direction of the expected effects on morbidity and mortality are difficult to pin down with precision. First, effects strongly mediated by nutritional status could lag by at least a year, except under wretched conditions. Shorter lags should be expected among infant and young children if patterns of breastfeeding are disrupted inducing increases mainly of waterborne diseases. Effects on neo- and post-neonatal mortality should be observable within one or two years after the crisis. Second, increases in respiratory ailments (except perhaps those affected by vitamin A deficiencies) are more likely to take place over a longer period of time, a year or more after the crisis. Recrudescence of respiratory tuberculosis may take even longer, except when its prevalence is already high and average nutritional status is close to a minimum.

As in the case of nuptiality and fertility, we expect to find 'echoes' in the mortality response. A sharp increase in mortality may be followed by an immediate decrease as the composition of the population by susceptibility to all or some diseases is sharply altered. 'Echoes' will be more visible in the most vulnerable age groups and in countries that have experienced the most recent mortality improvements since it is there where one will find higher variances of the population composition by frailty.

d. Selected contingencies.

The expected patterns described before apply in general but they involve important simplifications that obscure fine details. **First**, variability in the nature and duration of crises will produce heterogeneous responses. Recessions, such as those associated with the "debt crisis," that originate in international crises and are accompanied by draconian adjustments in consumption, purchasing power, and government spending will have different effects than those associated with downturns that are more or less localized and may be associated with temporary sagging demands for exports. Protracted crises are more likely to exhaust reserves and inventories or to outlast the shielding impact of public interventions. Longer exposure times are also more likely to trigger effects associated with thresholds (e.g., nutritional status).

Second, the presence or absence of certain social institutions or demographic conditions may amplify or minimize responses. Thus, as suggested before, decisions to marry will be less affected in societies where couple and household formation are dissociated from each other. Similarly, the response of marital fertility is contingent on the availability of or access to contraception, unless the crisis itself affects spouse separation and fecundity levels.

Finally, conditions present at the time of a crisis may modify the mortality response. For example, in societies with high prevalence of respiratory tuberculosis or bacterial diarrhea and weak or marginal nutritional status, one would expect to see a swifter impact of a crisis among the corresponding causes of death.

DEMOGRAPHIC RESPONSES DURING 1920-1990.

In this section of the paper we derive estimates of short-term effects of economic well-being on natality, nuptiality, and infant and adult mortality in ten countries during the period 1920-1990. The countries in our sample, albeit imperfectly, represent different types of demographic trajectories, styles of economic development and, to a lesser extent, political regimes.[1]

a. Data.

For the analysis we use the following country-years: Argentina (1910-1989); Chile (1908-1989), Colombia (1925-1988), Costa Rica (1925-1989), Guatemala (1930-1989), Mexico (1921-1989), Panama (1945-1989), El Salvador (1925-1989), Uruguay (1935-1989) and Venezuela (1936-1989). These countries represent fairly closely the entire spectrum of demographic regimes in Latin America. Argentina and Uruguay began their mortality and fertility transition during the last two decades of the 19th century and closely resemble demographic regimes prevailing in

[1] This section of the paper relies heavily on an article published elsewhere. See Palloni et al., 1996.

Western Europe and North America. Chile and Costa Rica began their transition later during the second or third decade of the 20th century and are perhaps slightly behind Argentina and Uruguay. The remaining five countries, particularly Guatemala and El Salvador, experience a very late onset of mortality decline and show only weak signs of sustained fertility decline.

b. Methods.

We use the following model (Lee, 1981; Galloway, 1988):

$$y_t=\alpha+\beta X+\delta Z+\varepsilon_t \qquad (1)$$

where y_t is a detrended demographic outcome for year t, X is a vector of detrended lagged socioeconomic indicators, Z is a vector of (possibly) detrended control variables, α is a constant and β and δ are vectors of effects. The results we present here were obtained using a first-order auto-regressive process and include lags 0 up to 4. As is well-known, the effects β can be conveniently interpreted as elasticities and adding the values of β_{t-k} for all lags k leads to an approximation of the total ('net') elasticity of the demographic outcome relative to the indicator of well-being.

To detrend our yearly series of indicators we use local least squares, a technique that provides a robust fit to the data without the need to impose a global functional form and without costly losses of degrees of freedom. Local least squares produces different results depending on the magnitude of the bandwidth (Cleveland, 1979). We experiment with values of bandwidth in the range between .20 and .90 and observe that results were very robust to changes. The results presented here correspond to a bandwidth of .90 although results with a bandwidth of .50 — the one we use for the post-colonial period — leads to identical inferences. Our demographic outcomes are reported number of yearly (legal) marriages, births, and non-infant deaths as well as yearly infant mortality rates. We also examine results obtained with a more fine-tuned breakdown of deaths by age and causes of deaths, but we are able to do this only for the period 1950-1990. We use infant mortality rates instead of number of infant deaths to circumvent the problem that the absolute number of infant deaths in one year changes sharply with changes in the number of births during the same year.

To the extent that completeness of vital events varies only gradually, the observed detrended series will be adequate for estimation. However, insofar as recessions themselves have non-negligible consequences on the smooth functioning of vital registration systems, it is likely that we will obtain lower than average completeness precisely during periods of social and economic hardship. The result will be to impart downward biases in estimated elasticities. Therefore, our results are likely to be **lower** bounds for the true elasticities.

As an indicator of well-being we use GDP per capita in constant 1970 US dollars. Although this choice is by no means ideal, we believe it has some important advantages over other equally plausible ones, at least during the period following World War II (Palloni and Hill, 1992; Palloni, Hill, and Pinto, 1996).

Some caveats are in order. First, in some countries the number of legal marriages amounts to a fraction — sometimes as low as 40 to 60 percent — of all unions. To the extent that union formation (as opposed to marriage formation) follows a different dynamic, our estimates will be misleading. Second, since the number of marriages (unions) can be affected by surges in adult mortality, we also estimate models with a control for lagged detrended adult mortality. However, the results change only trivially and we discuss only models without the controls for adult mortality. Third, to obtain estimates of marital fertility we experiment with a variety of procedures, all of which yield virtually identical results. The estimates we present here correspond to a model including a control for lagged number of detrended marriages.[2] Fourth, we model the effects of **historical period** by introducing a dummy variable attaining a value 1 if the year under observation is before 1955 and 0 otherwise. With this indicator we attempt to separate years before and after the onset of what turned out to be a rather sustained period of economic growth and of deep transformations in the demographic regimes of the region (at least until 1970). The indicator is entered additively as well as in interaction with the lagged values of GDP to detect changes in the magnitude and direction of responses.

c. Result I: nuptiality, fertility and mortality.

In what follows we summarize the results obtained from a pooled sample, containing all country-years listed above. These results are more robust than country-specific results assuming maximum intercountry heterogeneity of responses. Elsewhere[3] we show that a sequence of F-tests confirms that (a) the unconstrained models (country-specific) do not improve the explanatory power of the constrained model where all countries are assumed to have the same patterns of responses, and (b) that this applies to all demographic outcomes.

Table 1 displays the estimated elasticities and corresponding standard errors for alternative models for births, marriages, non-infant deaths, and infant mortality rate. For each demographic outcome (except births) there are two models, one with and one without the interaction term for time period. For births there is an additional model (Model 2) that includes a control for lagged marriages. The pattern of demographic responses is fairly regular and is also in satisfactory agreement with expectations. The initial response of marriages is very strong, properly signed and statistically significant. The 'echo' is protracted and dominates all other lags. The natality

[2] Lee (1981) raised well-justified objections against this procedure. He advocates the use of a standardized number of marriages instead. Since the results for our sample are virtually identical, we simply use the absolute number of marriages.

[3] See Palloni, Hill, and Pinto, 1996

response at lag 0 is very strong but, surprisingly, much stronger than at lag 1. The effects at higher lags are, as expected, negative but statistically insignificant. A control for lagged marriages changes results only trivially. The effects on infant mortality rate and on non-infant mortality are statistically significant but their magnitude is about a third of the effects on marriages and deaths.

Estimates from the model with an interaction effect reflecting time period suggest that before 1955 responses of births and marriages were **lower** and those of mortality were higher, but the models also show that the absolute differences (see estimated effect of interaction term) are low and only in the case of births is this effect statistically significant. The results of the fully interactive model are displayed in Table 2. Unlike results in Table 1, these models assume that the responses at every lag differ by time period and that these differences differ by lag. The new results, however, reinforce the impression that the time variance of responses for all outcomes is at best minor.

Are the net responses 'sizeable'? This question involves two separate issues. The first has to do with whether or not the net elasticities — the **sum** of lagged effects — are statistically significant from 0. This should be distinguished from the question of whether or not the overall contribution of the set of lagged independent variables to the explanation of variance in the demographic outcome is significantly different from 0. The second is whether or not responses are of any demographic significance. To address the first issue we develop a statistical test that reveals that only the net effects of marriages and births are statistically significant from 0 but that those for infant mortality are not at all, and those for non-infant mortality are statistically significant only according to very liberal criteria (see Palloni, Hill, and Pinto, 1996).

The answer to the second issue is fairly straightforward: we simulate scenarios where downturns (upturns) occur randomly over a period of 100 years and where demographic responses are equivalent to those estimated in Table 1 (models without interaction). Each downturn (upturn) is assumed to last a maximum of 5 years and involves drops (increases) of GDP of about 15 percent. We estimate that this regime will produce a deficit of births between 2 and 5 percent relative to a regime without crises. With less severe crises the deficit in births will fluctuate between .7 and 1.6 percent. Though not trivial, these are hardly the stuff of efficient population checks as they imply quite minor reductions (increases) in the natural rate of increase.

Are the responses comparable to those obtained elsewhere? Table 3 displays the estimated elasticities from our pooled sample and contrasts them with the median estimates of responses for 14 Western European countries prior to the demographic transitions (Galloway, 1988). The net responses of births and marriages are remarkably similar across samples but the net elasticity of non-infant mortality is less than in Europe. This cross regional similarity has also been recently documented by Lee (1990).

d. Results II: mortality by causes of deaths during the period 1950-1990.

A surprising result revealed by the examination of data by age and cause (Palloni and Hill, 1992) is that there is indeed a great degree of heterogeneity in the mortality response that remains concealed if one only uses total number of deaths. Even though the ultimate impact of economic recession is somewhat weak, the patterns of responses by age and cause are enlightening of the mechanisms that transmit the shock of oscillations in aggregate indicators to individual health.

Table 4 displays the estimated effects by lags and the net effects models where the demographic outcome is the (detrended) number of deaths by age and cause and the independent variables are the lagged values (up to four lags) of (detrended) GDP per capita. The models were estimated on a pooled sample of countries for the years 1950-1990.[4] By and large the mortality response follows a profile by age and cause consistent with theoretical expectations. **First**,

[4] The results by countries are discussed and analyzed elsewhere. See Palloni and Hill, 1992.

infectious diseases but particularly respiratory tuberculosis and diarrhea are the most responsive causes of deaths whereas infants, young children, the very young (5-14) and the elderly are the most affected age groups**.** **Second**, the causes of deaths that are most sensitive to economic recessions are diarrhea, respiratory tuberculosis and acute respiratory infections. The overall response of infectious diseases attains a maximum among infants and children whereas the overall response of respiratory tuberculosis attains a maximum among those aged 5-14 and 15-64. The strong response of respiratory tuberculosis (TB) is in accord with the many historical examples suggesting that TB is highly sensitive to changes in nutritional status and to sudden shifts in the redistribution of population (Palloni and Hill, 1992). It should be kept in mind though, that throughout Latin America the death rates associated with TB are fairly low and that deaths attributed to that cause are less than 5 percent of all deaths. **Third**, the pattern of responses by lags is remarkably consistent with expectations: with the exception of respiratory TB, deaths increase at lags 0 and 1 and thereafter experience a decrease, perhaps an 'echo' associated with changes in the composition by frailty. Respiratory TB responds at higher order lags, as it should if the mediating mechanism involves deterioration of already substandard nutritional status.

MALTHUS IN THE EARLY POST-COLONIAL PERIOD: 1820-1900.

1. General considerations

The goal of this section is to investigate the nature of short-term relations between demographic outcomes and economic well-being before 1910, during a more remote period of time which, in most cases, coincides with the latter stages of Colonial times. Although there very few studies of demographic short-term responses during this period in Latin America (for an exception see Reher, 1991) we begin utilize the framework summarized before to formulate two conjectures. First, the Malthusian mortality response should be more visible, sharper than what it appears to be in the post-1920 era. This should be the case since the post-1920 period is a time during which most of these countries develop the infrastructure and the institutional machinery, particularly the growth of a central state and bureaucracy, that act as buffers to shield and enhance the protection of the most vulnerable sectors of the population. Instead, the period prior to 1900 was one characterized by deep internal conflict, social and economic disorganization, and outright anarchy, where the most vulnerable social groups were left to fend for themselves. Inasmuch as urban areas are not only the most sensitive to and most affected by economic crises but also those where the poorest groups are concentrated, we should expect mortality responses to be larger there.

Second, we expect to identify marital fertility and especially marriage responses that are at least as large as those verified for the post-1920 period. The latter stages of the post-colonial period were characterized by a large inflow of migration from Spain and other European countries. Although there are exceptions (the case of Cuba during the first half of the 19th century), these migration flows were largely made up of unaccompanied individuals who came to the new continent to work in cities or in the countryside as laborers, small manufacturing enterprises, or in commercial ventures. Thus, it is likely that the bulk of union formation (unless it was consensual and informal) was fairly sensitive to valuation of short-term prospects and weakly dependent on the assets, wealth, and wherewithal of established households.

Undoubtedly there may be regional differences that exaggerate or attenuate the pattern of responses. Thus, in Cuba and other areas in the Caribbean with a large representation of slave and former slaves, consensual unions are dominant and it is there where one would expect a lesser response of marriage provided, of course, that what is being examined is the number of unions not the number of legal marriages. Similarly, one should expect the response of mortality to be sharper in cities receiving large influx of recently liberated slaves, such as in Havana in Cuba or Salvador in Brazil as these should be counted among the most vulnerable elements of the population.

2. Social, economic, and demographic settings.

Availability of data for entire countries during this period of time is a rare commodity. Indeed, we were able to secure the requisite data only for Chile during the years 1850-1910. What is fairly accessible in some cases is data for central cities, most of which kept various combinations of civil and parish registers of births, deaths and marriages. Joining these data together with information on economic indicators for the cities themselves (for the country in the case of Chile), results in a database that permits the types of analyses needed to identify demographic responses.

But matters are not so simple. In fact, there are very thorny problems surrounding the correct definition and description of the social and economic settings in each city, the comparability of the available economic indicators, and the uneven quality of recorded vital events. In this first exploration of the data we largely ignore these issues but must at least identify the associated problems well, even if briefly.

We were able to collect information for four of the six or seven most important cities during the period: Buenos Aires (Argentina), Havana (Cuba), Mexico City (Mexico), and Salvador (Brazil). We are in the process of gathering additional information for a number of other cities for the same period to extend our analysis. The cities we include in this paper occupy a central place in the economy of their respective countries, and Havana and Mexico City in particular serve until late in the 19th century as strategic loci of Spanish administration and domination. These cities are also very active commercial and administrative centers, the main destination ports of immigrants from Europe (and the hinterland) and, with the exception of Mexico City, the first entry port of major diseases and epidemics. Of the four, Salvador is perhaps the least advanced and of lesser importance as a commercial and administrative center, even though it was Brazil's capital until 1763. Buenos Aires and Havana are the most dynamic cities and experience very rapid demographic expansion during the period under study. Mexico City is always the largest in terms of population size but, as Mexico, its growth during the 19th century lags behind the growth of the other cities in the continent, particularly Buenos Aires and Havana.

A rough demographic profile characteristic of the various sites selected for analysis appears in the first part of Appendix 1. Since it is very difficult to estimate demographic parameters for the cities themselves we have sometimes resorted to the device of showing demographic parameters as they apply to the respective countries during the periods selected for analysis.

These cities, as all major cities in the colonial and immediate post-colonial period in Latin America, are heavily dependent on their agricultural hinterland for basic food supplies and staples and could suffer major setbacks during times of significant crop failures or when weather, political upheaval, or warfare disrupted routes of transportation for relatively long periods of time. More than anything, however, their normal functioning and ability to sustain large numbers of people depended heavily on their ability to carry out smooth and continuous trading with other Latin American cities, Spain, and some European countries. Thus, sharp downward fluctuations in rates of exchanges, breakdowns in the production or transportation of main exports, or disruption of international markets could have severe impacts on normal production and distribution of goods and services within major cities.

We thus expect that in the cities selected for analysis, fluctuations of price levels for strategic goods will be a good proxy for changes in underlying standards of living. Despite these broad similarities, however, it is likely that there is substantial variability with regard to the cities' sensitivity to crisis of comparable magnitude (see examples above), and that there could be important heterogeneities in each city's capability to mobilize stockpiles and inventories, access product substitutes, activate temporary subsidies for the poor, and use other mechanisms to fend off the most immediate and direct effects of crises. To the extent that we are unable to control for these sources of heterogeneities, they will be reflected in unexplained variance across cities in the estimated responses even if the true underlying regimes involve identical demographic responses.

3. Data and methods.

The data on demographic outcomes were retrieved from a variety of sources (including parish registers and vital statistics) which are described in the second part of Appendix 1. With the exception of Havana and Buenos Aires (1776-1811), we have information on births, marriages, and total deaths. For Havana we only have available yearly total deaths, and for Buenos Aires (1776-1811) we have information on yearly counts of total deaths and births but not for marriages.

All figures used in estimation are unadjusted counts. Except for the case of Chile where we use adjusted marriage counts, we have not yet investigated the need for adjustments, and are forced to rely on the assumption that relative undercounts coinciding with or triggered by crises will impart only minor distortions to trends. It is likely, however, that if these sources of errors do exist, they will generate a downward bias on the absolute value of estimated responses.[5]

The time series of economic indicators of well-being that we are able to estimate are exceedingly heterogeneous. They include such disparate constructs as terms of exchange in Buenos Aires for 1776-1811, a consumer price index for the later period in Buenos Aires, a general price index for Salvador, the yearly value of total fiscal revenues (in constant dollars) for Chile, the yearly value of total exports to the U.S. for Havana and, finally, a very unconventional

[5] In the case of Chile we present results calculated from unadjusted figures. The results from adjusted figures are very similar and lead to identical conclusions.

yearly volume of silver produced in Mexico. Thus, only in Salvador and Buenos Aires (during 1776-1811) are we able to calculate the more accepted and conventionally used consumer price indices. Most of these indicators are neither ideal indicators of well-being nor are they comparable across cities or consistent with indicators (normally real wages) used in Western Europe. This is a distinct limitation of our investigation since it may call into question not just the interpretation of effects for a single country but also our ability to place results in a comparative perspective. For example, it is not at all clear that fluctuations in fiscal revenue in Chile, mostly associated with the world price of nitrates, should be expected to have the same effects as changes in prices of a basket of basic goods in Buenos Aires. This is because erosion of a national government's capacity to raise revenues in a particular year will not affect day-to-day standards of living as it will a sharp increase in the price of grain or meat consumed by the population. It is not just that the lags involved may be quite different but that the expected total response may not be consistent across indicators. The same can be said about fluctuations in the volume of silver produced in Mexico. Although the volume of silver production may serve as a proxy for domestic gross product, it is unclear that its observed oscillations ought to have comparable effects to, say, oscillations in wheat prices or real wages. Until more research is done on the performance of the various indicators under different economic conditions, their exact meaning, and their relation to underlying shifts and jolts in the economy, our results for this historical period will remain fragile and tentative.

To estimate demographic responses we utilize a model exactly analogous to (1) but introduce a few modifications. First, in the application of local least squares to generate a predicted trend, rather than using a fixed bandwidth of .90, we estimate three models, one each for bandwidth .20, .50 and .80. Since differences in the corresponding estimates are quite trivial, we only show results with a bandwidth of .50 (although in the case of Chile we illustrate the nature and magnitude of differences). Second, rather than using Cochrane-Orcutt estimators of a first-order auto-regressive model, we generate Prais-Waiss estimates which utilize the data more completely, avoiding the losses of the first and last observations. The associated changes are, nevertheless, trivial. Finally, we carry out a battery of tests designed to evaluate the robustness of our estimates. These are explained in detail in (5) below.

4. Estimated responses.[6]

Figures 1a through 1f display detrended values of demographic outcomes and economic indicators for each of the six city-periods. These figures merely illustrate the nature of the data, and provide an idea about the magnitude of the frequency and size of fluctuations or deviations from trend. Table 5 displays the estimated elasticities of births, marriages, and total deaths relative to several indicators of well-being for six city-periods. To facilitate comparisons, the signs of the estimated elasticities for Buenos Aires (1776-1811) and Salvador (1800-1900), the two city-periods where we use price indices as economic indicators, have been reversed. With this change the Malthusian positive check is consistent with negative signs whereas the preventive check is consistent with positive signs.

Births react according to expectations in most places and responses are positive at lags 0 and 1 in all city-periods except Chile. Of a total of 10 estimates for lags 0 and 1, four are statistically significant and two of them occur in Mexico City. Also, as expected, the effects turn negative at lags 3 or 4 but only two of them turn out to be statistically significant. The net response varies from high of .45 in Buenos Aires during the earlier period to low of about .054 in Chile.[7]

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[6] Due to differences in the economic indicators, the expected responses for Buenos Aires during the first period and Salvador have opposite sign relative to those expected in the other city-periods.

[7] The equations for births do not include a control for marriages and, therefore, the corresponding estimates are not exact indicators of marital fertility response. Our previous experience with data for the post-1920 period suggests, however, that control for marriage fluctuations does little to reduce the observed response of births.

The response of marriages is even stronger than of births: all four elasticities at lag 0 are properly signed and three of them are statistically significant (at p>.05). At lag 1 the effects are positive and significant in Buenos Aires and in Chile (but only marginally so) whereas in Mexico City and Salvador the response turns negative (and significant) but the effects are statistically insignificant. The net response spans a broad range with the highest value in Buenos Aires (.82) and the lowest in Chile (.10).

Finally, the positive check appears to be in operation as well. In effect, in all cases except Chile, the elasticity at lag 0 is always negative although it is significant in only two of the six city-periods available to us. The patterns of response by lag follow expectations as the negative effects turn positive at higher lags or alternate with positive and negative effects. Of special importance are the cases of Buenos Aires (1776-1811), Havana and Salvador where we observe very large negative elasticities at lag 0 and/or 1 as well as sizeable echoes at lag 3 and/or 4. Havana and Salvador are the two cities with the largest representation of African slaves; this may partially explain the pattern of response observed. It is also interesting to note that the mortality response in Buenos Aires declines markedly between the two periods represented in the data.

How do these responses compare with responses estimated for the more recent, post-1920 period? With those estimated for pre-industrial Western Europe? With those for other localities in the area during an earlier period? Figures 2a through 2c display lag-specific elasticities for births, marriages and deaths for the city-periods in our sample, whereas Figures 3a through 3c displays lag-specific elasticities for the pooled sample of Latin American countries in 1920-1990 and the median values for Western Europe. Comparison of Figures 2a, 2b and 2c with corresponding Figures 3a, 3b and 3c shows the following: a) as expected, the pattern of birth response is stronger in the earlier than the latter period and, somewhat surprisingly, stronger also in early post-colonial Latin America than in Western Europe. The time(lag)-specific patterns are very similar across settings; b) the mortality response is larger in early post-colonial Latin America than in either Western Europe or Latin America after 1920. However, the pattern of response is more erratic and displays swings of wider magnitude at lags 2, 3 and 4, as well as a larger spread of net responses; c) finally, both the pattern and magnitude of the marriage response are very close across social and historical settings although the early post-colonial period appears to contain more noise at high order lags, and wider spread (in the expected direction) of net responses.

Finally, our estimated responses for Mexico City can be contrasted with estimates obtained by Reher (1991) for several localities in Guanajuato and Leon during an earlier period, the 17th century. Despite the fact that the models we and Reher use are not identical, we are surprised to verify that our estimated net responses for marriages and deaths are very close to those obtained by Reher whereas our estimates of net fertility responses are much lower than those that prevailed in the areas examined by Reher.

5. Are estimated effects robust?

In this section we briefly consider three issues that may affect the robustness of our findings. It is not our aim to offer an exhaustive technical treatment of each but merely to identify possible problems and shed light on possible solutions.

a. Detrending.

Since we have opted for a non-conventional procedure to detrend the data it is incumbent on us to reveal just how robust our results are to violation of assumptions or to changes in underlying parameters that need to be defined *a priori*. In particular, we examine the sensitivity of results to changes in the bandwidth of the local least squares procedures to identify time trend from yearly counts. Table 6 displays as illustration the results obtained for Chile utilizing three different bandwidths. Unsurprisingly the results vary but the heterogeneity they reveal is not at all a threat to internal validity since from each set of estimates we would have drawn identical inferences regarding the magnitude, sign, and statistical significance of responses. The outcomes for the other cases (not shown) reveal similar lack of sensitivity, and therefore confirm that results and inferences are quite robust to the detrending procedure.

b. Spuriousness.

Arguably all responses estimated before could be contaminated by spurious relations. The most obvious mechanism is one that involves the occurrence of epidemics. All cities in our sample — with the possible exception of Mexico City — are ports of entry for massive flows of products, people, and agents of disease. By now it is well known that these cities experienced frequent and recurrent epidemics of cholera, measles, typhoid and typhus, yellow fever, smallpox and various other scourges. As an illustration of the frequency and magnitude of epidemics, Figure 4 displays yearly counts of deaths for Havana during the 19th century, and identifies various agents of diseases responsible for peaks in deaths. It is well known that epidemics are a cause of social and economic disorganization that may lead to disruption in production, inefficient transportation and distribution of products, bottlenecks in labor supply and, more generally, to sharp distortions of credit and labor markets. Although the effects of the latter could result in increased returns to some factors of production at least in some sectors of the economy (and so could the emergence of black markets), the overall effects will be to generate an economic crisis of sorts. Since epidemics also cause increases in deaths and are likely to affect realization of marriages and marital fertility as well, conditions are set to find a relationship between an indicator of crisis and demographic response even though none exists outside the conditions generated by the epidemic.

The mechanism is plausible but three caveats are necessary. First, spuriousness will be important to the extent that the economic indicator we use is likely to reflect epidemics at all. In the case of Chile, for example, the economic indicator is fiscal revenues, and it is known that these were heavily dependent on nitrates exports. But nitrate production, processing and shipping was geographically concentrated in an isolated and remote enclave which was difficult to disrupt by epidemics of any sort. In this case, the occurrence of an epidemic is not as likely to affect estimation as, for example, in the case of Buenos Aires where the indicator of well being is the consumer price index.

The second caveat is that epidemics are more likely to have an effect on the death toll but their impact on marriages and births is probably minor, except under extreme conditions. This means that the magnitude of the bias associated with the mechanism producing spuriousness, and consequently the size of the adjustments required from suitable correction procedures, must be lower for births and marriages than they are for deaths.

The third caveat regards the possibility that an alternative and equally plausible mechanism relates economic performance, demographic outcomes, and epidemic without, however, producing spuriousness. Suppose that economic crises, most of which are accompanied by erosion in nutritional standards and by relatively massive displacement and movement of people, trigger some epidemics. If so, the estimated effects we obtain through a model like (1) are equivalent to reduced form (gross) effects but are devoid of upward biases. Regardless of the procedure we choose to apply to attenuate biases, we should always distinguish this from the mechanism producing spuriousness.

In summary we pose two broad conjectures. The first is that epidemics may lead to upwardly biased estimates of responses for mortality, marriages and fertility. The second is that the biases will be higher for deaths than they are for the other two demographic outcomes.

Although proper treatment of the problem should include all cities, we test our conjectures on Havana (1801-1900) and Buenos Aires (1864-1925) only, the city-periods where we have fairly reliable identification of epidemics. The drawback with the former case is that since Havana does not have information on marriages and births, we can only test the relevance of the first but not the second conjecture stated above. The drawback of the latter is that during the period under examination there were only two major epidemics, one of cholera in 1869-1871 and one of yellow fever in 1871-1873, and therefore the effects of mechanism producing spuriousness will be considerably diluted and not easy to identify reliably.

To reduce the effects of spurious relations we estimate a model like (1) including lagged values of a dummy variable designed to capture the exogenous effects of an epidemic. The dummy variable attains the value 1 for any year *t* during which there is evidence of an epidemic and 0 otherwise. The newly estimated model includes the value of the dummy at lags 1, 2, 3 and 4 but **not at lag 0**. This is because if epidemics do in fact cause economic disruption they will do so only after some time has passed, not immediately. The adjusted estimated elasticities, particularly those for deaths, should now be purged of contamination due to spuriousness.

Table 7 displays the results obtained from the new model for Havana and Buenos Aires and contrasts them with those obtained before. In all cases the estimated response is attenuated and, as suspected, the magnitude of the change is larger for deaths than it is for marriages and births. Despite the changes involved in the death response in Havana, however, there remain residual effects of considerable significance.

c. The mortality response.

Throughout we use the absolute number of total deaths rather than, as is more desirable, the number of deaths disaggregated by age groups. This could cause difficulties when, as in most demographic pre-transition demographic regimes, a high proportion of all deaths are infant deaths. In fact, if the number of births in year *t* contracts as a response to an economic crisis, so will the number of deaths in years *t* and *t*+1 since the exposed population at age 1 will be smaller.

Thus, if the crisis also affects mortality, the observed increase in the total number of deaths will underestimate the increase that would have taken place had the birth response been nil. In order to correct our estimates of the mortality response, we re-estimated the mortality equation including a control for the value of detrended births corresponding to lag 1. This creates collinearity between detrended prices (at lag 1) and the new control variable, and hence reduces the precision of our estimates. However, the resulting new estimates (not shown) differ very little from those obtained without the adjustment. We conclude, therefore, that the original estimates contain little if any downward bias in the estimated mortality response.

d. Homogeneity and symmetry.

There are two additional issues we wish to discuss which, if appropriately solved, could result in better or more efficient estimates. The first issue is that it is possible to estimate a more robust set of estimators for the period under investigation by pooling the samples for the various city-years. This will be inappropriate when the effects are different across city-periods, not just because each of them may involve different Malthusian responses, but also because the meanings of the economic indicators are, as discussed before, quite heterogeneous. Although we did not carry out a test of the null hypothesis of homogeneity, we present the results of estimation with a pooled sample in Table 8. Note that the responses are as expected for all demographic outcomes, and that the magnitude and pattern of estimated elasticities are now much closer to the ones in the samples of post-1920 Latin America or Western Europe. The one exception to this rule is that the net response of mortality is positive and fairly large. This is mostly due to a large positive effect at lag 4.

The second issue is that model (1), and all models related to it, are based on the rather odd assumption that positive deviations from trend of economic well-being have effects of the same magnitude but opposite in sign than negative deviations. There is little behavioral justification for this assumption other than that it simplifies estimation. A simple solution is to make the estimated elasticities a function of the sign of the deviation. This can be implemented by adding to the model interaction terms involving dummy variables proxying for the direction of deviations and the detrended (lagged) values of the economic indicator. However, a test carried out in the pooled sample of city-periods and for births only (results not shown) proved to be unsuccessful as it suggested that the interaction terms were all insignificant. We thus conclude that lack of symmetry of responses does not pose a significant threat to the internal validity of estimates.

THE DEMOGRAPHIC SIGNIFICANCE OF MALTHUSIAN RESPONSES

Although the foregoing analysis suggests that during both the late post-colonial period and the 20th century there was in place, albeit in a simplified form, a Malthusian mechanism involving short-term responses of marriages, births and deaths, it is still an unsatisfactory account. This is because the measures and framework we have chosen to assess the existence of demographic responses have little if any demographic meaning in their present form. Can one say, for example, that a total (net) response of mortality equivalent to .25 is demographically relevant? Whether or not that is the case will depend on a number of things: the frequency of crises, their duration and relative magnitude, the effects of crises on other vital rates, and on the nature of the relation over time between vital rates.

A feasible tool to assess the demographic significance of a regime of demographic responses is to simulate the trajectory of a total population with and without a regime of crises. Given a regime of crises and a stochastic component (pure white noise) affecting vital rates, the ultimate size of the population or its trajectory will depend in part on the estimated regime of responses to those crises. Keeping fixed the regime of crises but altering the numerical values of the responses (estimates of effects on births, marriages and deaths) it is possible to assess the relative magnitude of the impact of crisis with a common, easy to grasp metric related to population size or growth trajectory during a fixed period of time.

In what follows we design an exercise that will enable us to determine whether or not a given regime of crises and associated responses to those crises (as estimated in the previous sections) make any difference in the long term trajectory of a population.

1. A simple simulation exercise.

To assess the impact of a regime of crisis on the size and growth of a population we proceed to project a population over time using changing projection matrices. The nature of the matrices depends on the existence of crises and on the estimated response to those crises. To implement the simulation we require a few simplifying assumptions. **First,** we project forward a population containing three fifteen year age groups over fifteen year cycles. This simplification permits us to speed calculations and has little effect on substantive conclusions. **Second**, for each projection cycle we determine whether a crisis takes place within that projection cycle and then determine its magnitude. This is done by first randomly choosing the interwaiting times between crises with a Poisson distribution, and then using a multinomial distribution to allocate each crisis among categories we shall call small, large and medium. The Poisson rate (r) is variable and we will show the extent to which simulation results vary with changing rates. To simplify calculations we fix the multinomial probabilities for the magnitude of the crisis: .70 for medium size crises, .10 for large ones and .10 for small ones. A large crisis implies an increase in prices (or reduction in real wages) of 50 percent whereas the medium and small involve deviations from a secular trend of prices 20 and 10 percent respectively.[8] **Third**, we assume that a crisis lasts for a fixed period of two years and than its aftermath lasts four years, that is, the return to economic

[8] Values for the rates of crises and for the multinomial distribution used to determine their magnitude were fixed after estimating a range from the sample of cities. Since the simulation exercise is designed to determine the potential role of crises and not to verify the empirical existence of an actual mechanism, precision in the estimation of these quantities is not necessary. However, a more thorough validation exercise does require more careful estimation of these key parameters.

conditions that are on the secular trend takes four years during which prices descend to their normal levels in a linear fashion. **Fourth,** we assume that ours is an unlucky society where only periods of depression are possible but no periods of significant bonanza occur. This assumption simplifies the algebra considerably but leads only to a worst-case scenario and, therefore, to an upper limit of the ultimate impact of crises on demographic dynamics.

For each cycle of the projection we know *a priori* about the presence/absence of a crisis, time elapsed since the last occurred, and the exact magnitude of such crisis. To determine the vital rates associated with each projection cycle, that is, to choose the entire of the projection matrix appropriate for each cycle, we need to calculate proportionate changes in births and deaths associated with a particular projection cycle. These estimated changes are then applied to modify a baseline projection matrix to obtain the appropriate matrix for the projection cycle. The baseline projection matrix contains entries that roughly correspond to a population with a life expectancy of about 45 years and a pattern of mortality belonging to the Coale-Demeny female West model. The Gross Reproduction Rate (GRR) ensures a natural rate of increase of about .00099 per year. That is, if we applied the baseline matrix repeatedly with no crises and no random fluctuations the result would be a stable population with an intrinsic rate of increase equal to .00099. To calculate the proportionate changes in births and deaths within each cycle we use the estimated distributed lag models for the pooled sample of cities. We assume that errors in the equations for births and deaths are normally distributed with mean 1 and standard deviation .25. The autocorrelation (rho) is alternatively assumed to be -.50 and .50. In the absence of crises but with stochastic fluctuations, the sequence of matrices chosen for the projection will be related to each other by the parameter determining the autocorrelation function of the births and deaths series.

A final simplification is needed here. The distributed lag models enable us to compute estimated proportionate changes in births and deaths but these must be transformed into proportionate changes in **vital rates** that modify the entries of the baseline projection matrices. To do this we assume that the fertility scheduled is affected by a constant factor and that mortality changes are neutral, that is age invariant. In other words, we disregard the age differentials that may exist in the impact of crises on fertility and mortality.

2. Selected results of the simulation.

The simulation exercise described above depends on several parameters requiring a much more thorough review than what we have space for in this paper. To simplify the discussion we choose to focus on results obtained after changing only two of these parameters: a) the Poisson rate of the regime crisis (r), and b) the nature of the autocorrelation function (rho). Altogether we will review four different scenarios corresponding to a low and high rate of crisis and, for each of these, a negative and positive autocorrelation function with value .50. In each scenario we repeat the simulations 20 times and calculate the total population, rates of increase, and population age distribution for each of 200 cycles. Figures 5a through 5d display the **mean** population trajectories for a situation where only random fluctuations operate ("white noise") and for another where, in addition, we impose a regime of demographic crises ("white noise+crises"). The difference between the two reflects the ultimate effects of the Malthusian short-term response regime embedded in the simulations.[9] Table 9 displays population **growth ratios** after 50, 100, 150 and 200 cycles and the **cumulated** populations evaluated at those same four points in time.

First, the figures and the table show the influence of the stochastic component on the total population size. In fact, the magnitude of growth is governed by the degree and direction of the inertia of the system (measured by rho): keeping the regime of crises constant, a negative rho produces a trajectory that dampens overall growth. This is seen by comparing a given population

[9] To avoid cluttering, these figures do not display the associated confidence interval associated with the population trajectories. These confidence bands are plotted separately in figures 6a through 6d.

trajectory in Figures 5a or 5b with the corresponding one in Figures 5c or 5d. Second, within the boundaries of the influence of the stochastic component, the random regime of crisis makes an important difference. When crises are frequent enough, for example with an average waiting time of about 10 cycles, the corresponding population trajectory increasingly deviates from one where there is only a pure random component. This can be seen by contrasting the **differences** observed in Figure 5a and those in Figure 5c and those observed in Figure 5b and 5d. Also, according to Table 9, the relative differences in cumulated population between a regime with (frequent) crises and one without crises amounts to between 23 and 27 percent depending on the autocorrelation regime (580 vs 426 and 1050 vs 810). Similarly, the ratios of growth in the absence of crises is roughly about twice as large as in the regime with crises, independently of the autocorrelation regime (1.26 vs .54 and .04 vs .02).

But even in cases where crises are infrequent (with an average waiting time of 100 cycles) they can have some lasting influence in population dynamics. For example, the relative difference in the cumulated populations at the end of 200 cycles amounts to between 6 and 9 percent of the total cumulated population in the absence of crises (645 vs 605 and 1951 vs 1790) whereas the ratios of growth are between 24 and 44 percent larger in the absence of crises (.79 vs .63 and .34 vs .29).[10]

This exercise shows that the pattern of short-term demographic responses identified earlier in the late post-colonial period in Latin America had some demographic significance since, at least in the long run, its presence arrests the total growth of the population by more than a trivial amount. A rather benign regime of economic downturns would lead to reductions of the order of 6 percent in the total population growth over long periods of time. A more perverse regime could

[10] An important feature shown by the sequence of tables 6a through 6d is that the variance of the population trajectories increases as time passes. This ought to be expected since the variance of a random walk diverges asymptotically.

curtail such growth by close to 25 percent.

To conclude, it is important to emphasize what the simulation **does not tell us**: it does not prove or verify that the colonial regime of economic crises and associated (estimated) responses did in fact constitute the instrument whereby a population was kept in check. It only suggests that such a mechanism was possible if the regime of crises and associated responses had been in operation over long periods of time.

SUMMARY OF RESULTS AND CONCLUSIONS

This empirical review of Malthusian responses in the early post-colonial period in some cities in Latin America suggests the presence of three important regularities. First, the birth and marriage responses to economic fluctuations operate as expected. The response of mortality is quite large but exhibits an erratic pattern and rather perverse tendency to produce net effects contrary to expectations. These responses seem to be robust to control for spuriousness and to the detrending procedure. In summary, Malthusian short-term responses do exist early in the history of Latin America, although we have more confidence in the identification of the preventive than the positive check.

Second, the cities are quite heterogeneous and this is reflected in heterogeneity of responses. However, despite the variance contained in the estimates we find broad agreement of patterns and even relatively close range of responses. The pooled sample leads to (untested) estimates that suggest patterns of Malthusian response that are pervasive throughout the region.

Third, there is close agreement between the estimated patterns in early post-colonial Latin America and those estimated in post-1920 Latin America and Western Europe. This confirms once more that despite historical and institutional diversity, the Malthusian devils seem to find way to operate and produce empirical regularities of remarkable consistency and homogeneity.

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Table 1: Estimated Elasticities with a Pooled Sample of Countries: 1920-1990

Lag	Births			Marriages		Non-infant deaths		Infant Mortality Rate	
	(1)	(2)	(3)	(1)	(2)	(1)	(2)	(1)	(2)
0	.16(.03)	.13(.03)	.13(.03)	.34(.07)	.34(.07)	-.10(.04)	-.10(.04)	-.02(.01)	-.03(.01)
1	.00(.01)	-.05(.02)	-.05(.02)	-.04(.03)	-.04(.03)	.00(.02)	.00(.02)	-.01(.02)	-.01(.02)
2	.00(.01)	.01(.02)	.01(.02)	-.05(.03)	-.05(.03)	-.01(.02)	-.01(.02)	-.01(.02)	-.01(.02)
3	.01(.01)	.02(.02)	.02(.02)	-.02(.03)	-.02(.03)	-.01(.02)	-.01(.02)	-.06(.02)	-.06(.02)
4	.01(.01)	.01(.02)	.01(.02)	-.04(.03)	-.04(.04)	-.00(.03)	-.00(.02)	-.03(.02)	-.03(.02)
Inter	--	--	-.0070(.002)	--	-.005(.004)	--	.0001(.002)	--	-.0030(.002)
R^2 adj	.02	.10	.18	.06	.06	.03	.04	.00	.00
Net	.15	.12	.12	.19	.19	-.10	-.10	-.01	-.02

*See text for definition of Models

Table 2: Results with a Pooled Sample of Ten Countries
(Fully Interactive Model)

	Births	Marriages	Non-infant deaths	Infant Mortality Rate
Lag and interactions				
0	.004(.07)	.240(.15)	-.10(.10)	.10(.11)
1	.070(.08)	-.100(.17)	-.08(.12)	-.28(.14)
2	.070(.08)	.120(.17)	.14(.12)	.18(.14)
3	-.050(.08)	.100(.17)	-.05(.12)	.07(.14)
4	.060(.07)	-.140(.15)	-.00(.10)	-.10(.12)
Inter 0	.160(.07)	.130(.15)	-.00(.10)	-.14(.12)
Inter 1	-.140(.08)	-.070(.17)	.08(.12)	.27(.14)
Inter 2	.070(.08)	-.170(.17)	-.15(.13)	-.20(.15)
Inter 3	.060(.08)	-.110(.17)	.06(.12)	-.02(.15)
Inter 4	.050(.08)	.110(.15)	.01(.10)	-.07(.12)
R^2 adj	.18	.06	.03	.07

Table 3: Comparison of Estimated Elasticities by Lag and Net Elasticities

Demographic Outcome	Lag 0	Lag 1	Lag 2	Lag 3	Lag 4	Net
	Case I: Pooled Sample: Latin American Countries (1925-1990)					
Births	.16	.01	.00	.01	-.01	.15
Marriages	.34	-.04	-.05	-.02	-.04	.19
Non-infant deaths	-.10	.00	-.01	.01	.00	-.10
Infant Mortality	-.02	-.01	-.01	.06	-.03	-.01
	Case II: Medians of 14 European Populations					
Births	.05	.11	-.03	.01	.00	.14
Marriages	.13	.04	-.02	-.02	-.01	.13
Non-infant deaths	-.10	-.19	-.09	.01	.01	-.36
Infant Mortality	--	--	--	--	--	--

Sources: Panel I: Estimated effects (by Lag) on Births, Marriages, Non-infant deaths and Infant Mortality Rate (IMR)
Panel II: Results from Galloway (1988)

Table 4: Effects by Lag for Selected Causes of Death: Pooled Results for Nine Countries 1955-1990

Age Group	Lag	All	Infections	Respiratory tuberculosis	Violence	Ill-defined	Respiratory	Diarrhea
All(n=242)	0	-.05	-.45*	.08	.33	-.23	-.38	-.37
	1	-.12	-.65*	-.74*	.15	-.50	-.87*	-.81*
	2	.06	-.00	.08	.40	.49	.39	.02
	3	.02	-.14	-.08	.12	-.06	-.16	-.08
	4	-.04	.06	-.03	-.30	.14	-.19	-.05
	Adj R^2	.00	.08	.03	.05	.04	.04	.05
	Sum	-.13	-1.18	-.69	.70	-.16	-1.21	-1.29
0(n=249)	0	-.11	-.59*	-.05	-.46	-.56	--	-.55*
	1	-.34*	-.63*	-.83*	.64	.60	--	-.63*
	2	.04	-.09	.05	-.48	.09	--	-.08
	3	-.09	-.56	-.51*	-1.13	-1.95*	--	-.40
	4	.02	-.21	.07	.56	.33	--	-.15
	Adj R^2	.02	.08	.04	.00	.04	--	.05
	Sum	-.48	-2.08	-1.27	-.87	-1.49	--	-1.81
1-4(n=242)	0	.06	.17	.26	.69	.29	--	-.30
	1	-.37*	-.60	-.89*	-.57	-.46	--	-.77*
	2	.10	-.00	.62*	-.09	.62	--	.32
	3	-.30	-.69	-.44	.15	-.38	--	-.58*
	4	.32	.50	.14	.00	-.39	--	.02
	Adj R^2	.00	.00	.01	-.02	-.01	--	.02
	Sum	-.25	-.62	-.31	.36	-.32	--	-1.31
5-14(n=242)	0	-.06	-.10	-.21	.60	-.05	1.02	.80
	1	-.26	-.91*	-.66	-.49	-.16	-2.64*	-1.69*
	2	.10	.56	.82	-.06	-.09	1.16	.59
	3	-.17	-.80*	-.47	-.20	-.27	.91	-.87
	4	-.01	.21	.12	.52	-.20	-2.59*	-.22
	Adj R^2	.01	.04	.00	.01	-.02	.02	.01
	Sum	-.40	-1.04	-.40	.37	-.77	-2.14	-.95
15-64(n=242)	0	.04	-.24	.15	.23	-.13	-.31	-.31
	1	-.08	-.41*	-.50*	.07	-.34	-.77*	-1.06*
	2	.12	.13	-.31	.48	.56	.17	.54
	3	.02	-.14	.26	.14	-.02	-.11	-.41
	4	-.06	.03	-.09	-.31	.17	-.02	.03
	Adj R^2	.05	.03	.02	.04	.00	.02	.02
	Sum	.04	-.63	-.49	.61	.24	-1.04	-1.21
65+(n=242)	0	-.06	-.45*	.06	.52*	-.17	-.15	-.74*
	1	-.15*	-.03	-.73*	-.46	-.33	-.56*	-.44
	2	.05	.01	.04	.47	.51	.53	.03
	3	-.02	.20	-.11	-.08	.07	-.36	-.07
	4	-.04	-.06	-.07	-.05	.05	.18	-.26
	Adj R^2	.01	.01	.01	-.01	.02	.01	.04
	Sum	-.22	-.33	-.81	.40	.13	-.89	-1.48

* Significant at $p < .01$

Table 5: Estimated Elasticities by Lags[a,b]

A. Estimated Elasticities by Lag: Births (standard errors in parentheses)

Lag	Buenos Aires (1776-1811)	Buenos Aires (1864-1925)	Mexico City (1800-1900)	Havana (1801-1900)	Chile (1850-1910)	Salvador (1800-1900)
0	.028(.09)	.039(.04)	.110(.03)	--	-.017(.05)	.276(.16)
1	.180(.09)	.114(.05)	.124(.03)	--	.077(.05)	.193(.18)
2	.170(.09)	.068(.05)	-.023(.02)	--	-.026(.05)	.003(.18)
3	-.117(.10)	.036(.04)	.071(.03)	--	.005(.05)	-.026(.17)
4	190(.11)	.091(.04)	-.002(.02)	--	.015(.04)	-.014(.15)
Net	.45	.34	.28	--	.054	.43
R^2 adj	.24	.12	.41	--	-.04	.05
F value p	.03	.04	.00	--	.72	.13

B. Estimated Elasticities by Lag: Marriages (standard errors in parentheses)

Lag	Buenos Aires (1776-1811)	Buenos Aires (1864-1925)	Mexico City (1800-1900)	Havana (1801-1900)	Chile (1850-1910)	Salvador (1800-1900)
0	--	.290(.07)	.160(.13)	--	.023(.09)	.232(.11)
1	--	.250(.06)	-.016(.13)	--	.126(.09)	-.127(.10)
2	--	.089(.07)	-.092(.10)	--	-.016(.09)	.090(.10)
3	--	.173(.07)	.053(.10)	--	.038(.09)	.255(.10)
4	--	.015(.07)	.088(.09)	--	-.072(.09)	-.022(.11)
Net	--	.82	.19	--	.10	.38
R^2 adj	--	.46	-.02	--	-.05	.08
F value p	--	.00	.61	--	.77	.05

C. Estimated Elasticities by Lag: Deaths (standard errors in parentheses)

Lag	Buenos Aires (1776-1811)	Buenos Aires (1864-1925)	Mexico City (1800-1900)	Havana (1801-1900)	Chile (1850-1910)	Salvador (1800-1900)
0	-.40(.29)	-.10(.27)	-.127(.14)	-.300(.14)	.095(.05)	-.452(.12)
1	.86(.37)	.27(.35)	-.174(.14)	-.232(.15)	.002(.05)	.243(.13)
2	-.08(.38)	.064(.36)	-.135(.15)	-.158(.15)	-.012(.05)	-.006(.13)
3	-.51(.37)	-.010(.35)	.100(.14)	.649(.15)	-.043(.06)	-.001(.12)
4	-.21(.27)	.090(.28)	.586(.14)	.285(.15)	.002(.05)	-.064(.11)
Net	-.44	.21	.28	.25	.04	-.28
R^2 adj	.23	.06	.19	.24	-.03	.13
F value p	.04	.69	.001	.00	.71	.05

a: to facilitate comparisons, the signs for estimated elasticities for Buenos Aires (1776-1811) and for Salvador (1800-1900) have been reversed; b: at the end of each panel we display the probability associated with the test statistic F from the analysis of variance.

Table 6: Estimates for Chile with Alternative Detrending Procedures

	Bandwidth=.20	Bandwidth=.50	Bandwidth=.80
A. Estimated Elasticities by Lag: Births (standard errors in parentheses)			
Lag			
0	-.014(.05)	-.017(.05)	.005(.05)
1	.081(.05)	.077(.05)	.106(.05)
2	-.036(.05)	-.025(.05)	.028(.05)
3	-.006(.05)	.005(.05)	.038(.05)
4	-.020(.05)	.015(.05)	.035(.05)
Net	.005	.055	.212
R^2 adj	-.04	-.04	.12
F value p	.71	.72	.04
B. Estimated Elasticities by Lag: Marriages (standard errors in parentheses)			
Lag			
0	.030(.11)	.023(.09)	.062(.09)
1	.150(.10)	.126(.09)	.176(.09)
2	-.023(.10)	-.016(.09)	.073(.09)
3	.047(.10)	.038(.09)	.097(.10)
4	-.108(.11)	-.072(.09)	-.052(.10)
Net	.096	.099	.356
R^2 adj	-.02	-.05	.03
F value p	.59	.77	.29
C. Estimated Elasticities by Lag: Deaths (standard errors in parentheses)			
Lag			
0	.038(.05)	.095(.05)	.082(.06)
1	-.018(.05)	.002(.05)	.007(.06)
2	-.065(.05)	-.012(.05)	.008(.06)
3	-.088(.05)	-.043(.06)	-.041(.06)
4	-.040(.05)	.002(.05)	-.009(.06)
Net	-.173	.044	.047
R^2 adj	.04	-.03	-.05
F value p	.20	.62	.82

Table 7: Results for Havana(1801-1900) and Buenos Aires(1864-1925) Controlling for Epidemics

	Havana		Buenos Aires	
A. Estimated Elasticities by Lag: Births (standard errors in parentheses)				
Lag	with	w/out	with	w/out
0	--	--	-.021(.04)	.039(.04)
1	--	--	.090(.04)	.114(.05)
2	--	--	.022(.04)	.068(.05)
3	--	--	.019(.04)	.036(.04)
4	--	--	.089(.04)	.091(.04)
Net	--	--	.199	.34
R^2 adj	--	--	.06	.12
F value p	--	--	.20	.04
B. Estimated Elasticities by Lag: Marriages (standard errors in parentheses)				
Lag	with	w/out	with	w/out
0	--	--	.204(.07)	.290(.07)
1	--	--	.231(.07)	.250(.06)
2	--	--	.040(.07)	.089(.07)
3	--	--	.150(.07)	.173(.07)
4	--	--	-.002(.07)	.015(.07)
Net	--	--	.623	.82
R^2 adj	--	--	.30	.46
F value	--	--	.00	.00
C. Estimated Elasticities by Lag: Deaths (standard errors in parentheses)				
Lag	with	w/out	with	w/out
0	-.285(.15)	-.317(.15)	-.008(.21)	-.10(.27)
1	-.195(.16)	-.221(.15)	.220(.21)	.27(.35)
2	-.181(.16)	-.150(.15)	.023(.21)	.064(.36)
3	.656(.16)	.640(.16)	-.250(.21)	-.010(.35)
4	.292(.16)	.300(.14)	.158(.21)	.090(.28)
Net	.287	.25	.143	.21
R^2 adj	.22	.28	.59	.06
F valu p	.00	.00	.00	.69

Table 8: Results from Pooled Sample of City-Periods

	Births	Marriages	Deaths
Lag			
0	.050(.03)	.038(.04)	-.108(.07)
1	.091(.03)	.107(.04)	-.046(.07)
2	-.028(.03)	-.036(.03)	-.048(.07)
3	.054(.03)	.043(.03)	.100(.07)
4	.077(.03)	.002(.03)	.317(.06)
Net	.174	.154	.22
R2	.05	.04	.064
F value p	.001	.009	.00

Table 9: Simulated impact of crises on the trajectories of population growth

Scenario	Cycle	Growth Ratios[a]		Cumulated Populations[b]	
		white noise	white noise+crises	white noise	white noise+crises
rho=-.50	50	145	141	1.22	1.10
r=.010					
	100	300	289	1.15	1.04
	150	469	446	.99	.82
	200	645	605	.79	.63
rho=.50	50	212	199	1.31	1.27
r=.010					
	100	623	584	.36	.32
	150	1285	1198	.26	.22
	200	1951	1790	.34	.29
rho=-.50	50	142	131	.62	.49
r=.10					
	100	289	247	.58	.37
	150	432	342	1.04	.55
	200	580	426	1.26	.54
rho=.50	50	149	144	.21	.21
r=.10					
	100	347	314	.53	.48
	150	588	499	.09	.06
	200	1050	810	.04	.02

[a] Refers to the ratio of the total population at the end of a cycle to the total population at the beginning of the projection.
[b] Refers to the total number of persons-years lived from the beginning of the projection.

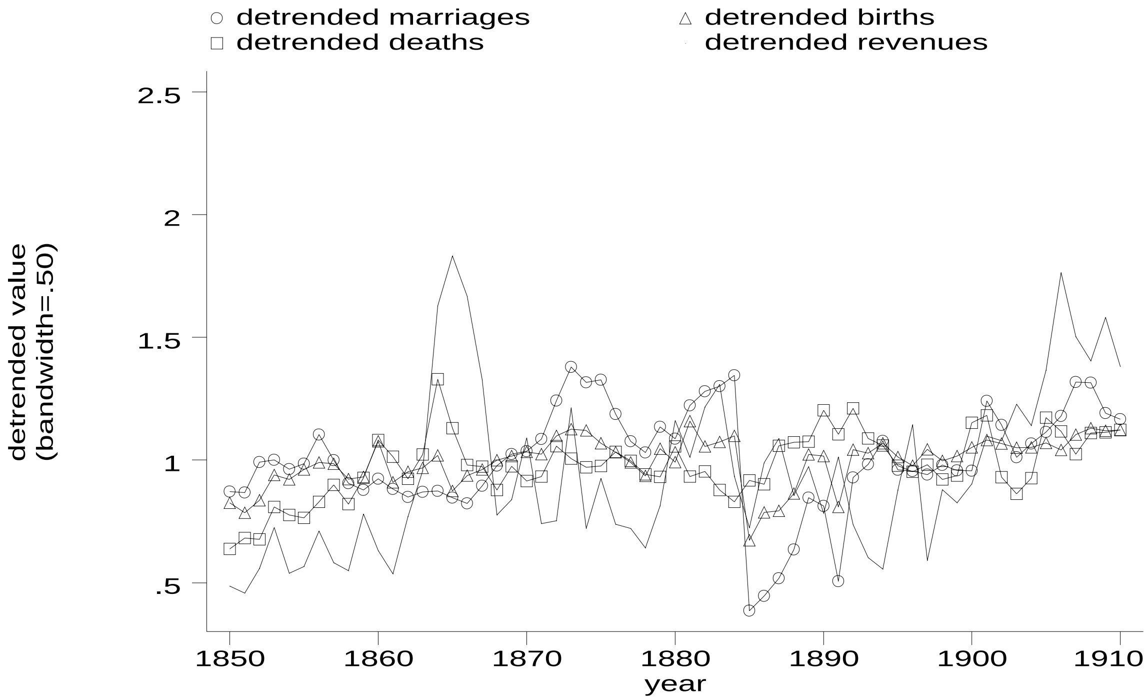

Figure 1a: Detrended Series for Chile, 1850-1910

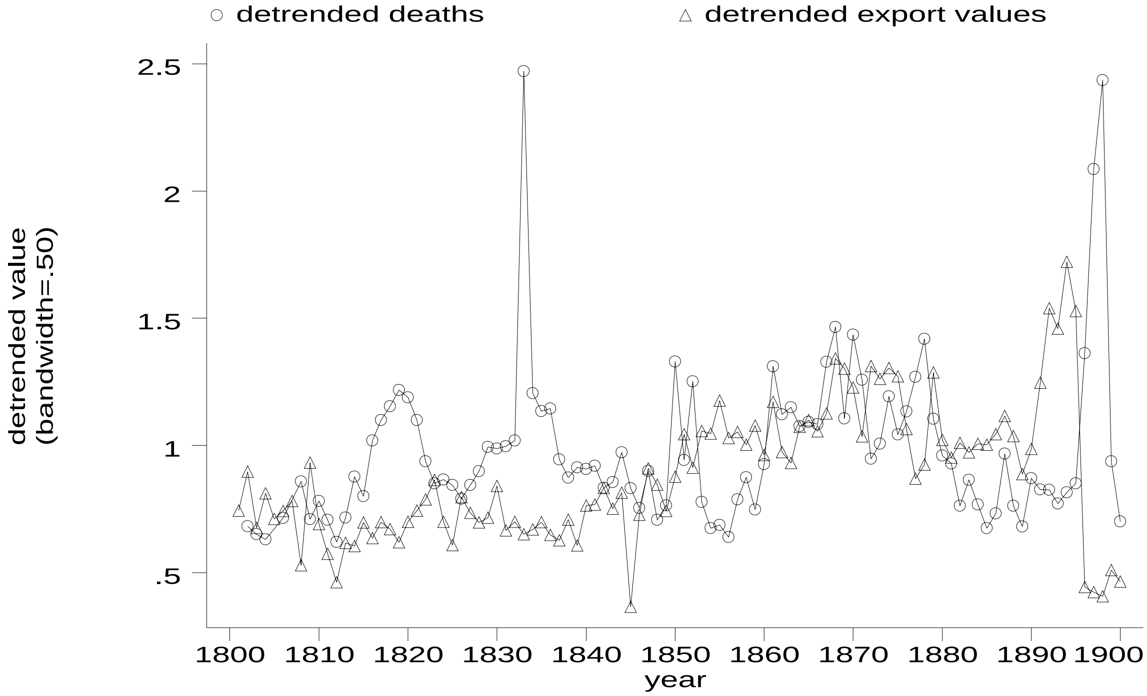

Figure1b: Detrended Series for Havana, Cuba, 1801-1900

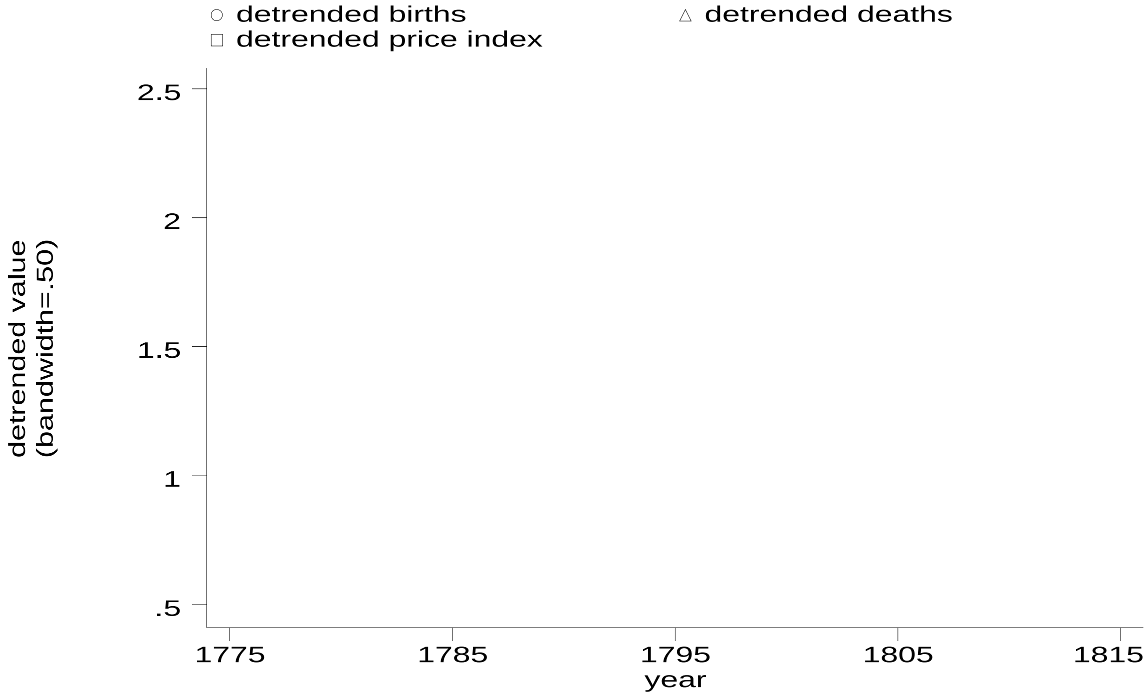

Figure 1c: Detrended Series for Buenos Aires, Argentina, 1776-1811

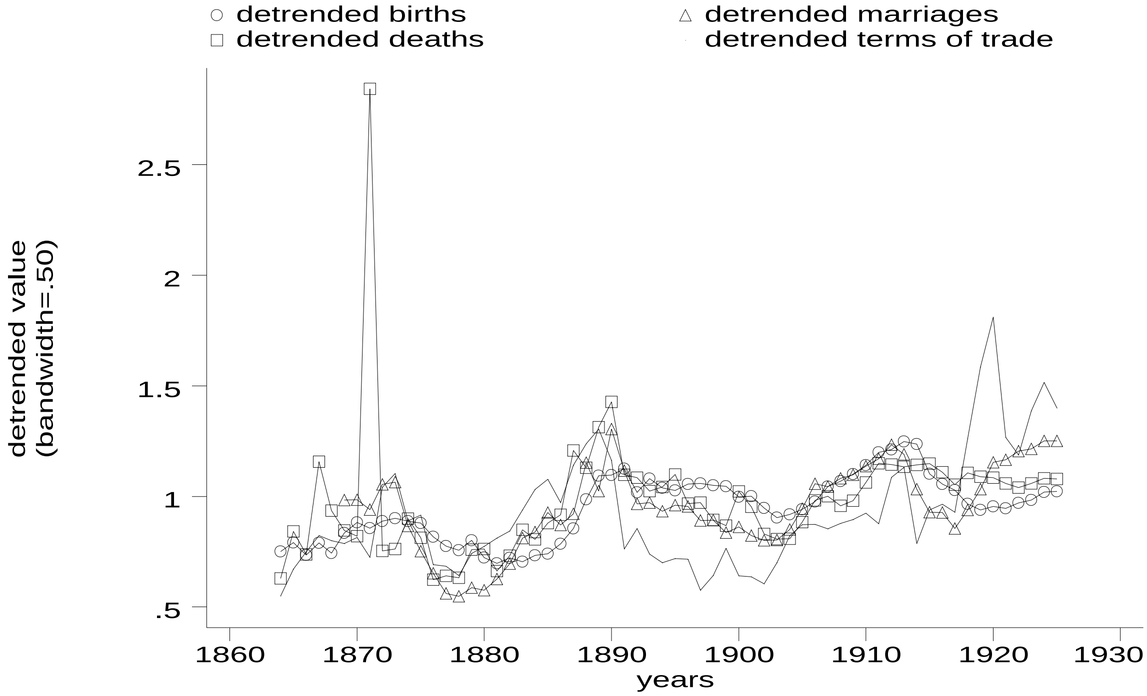

Figure 1d: Detrended Series for Buenos Aires, Argentina, 1864-1925

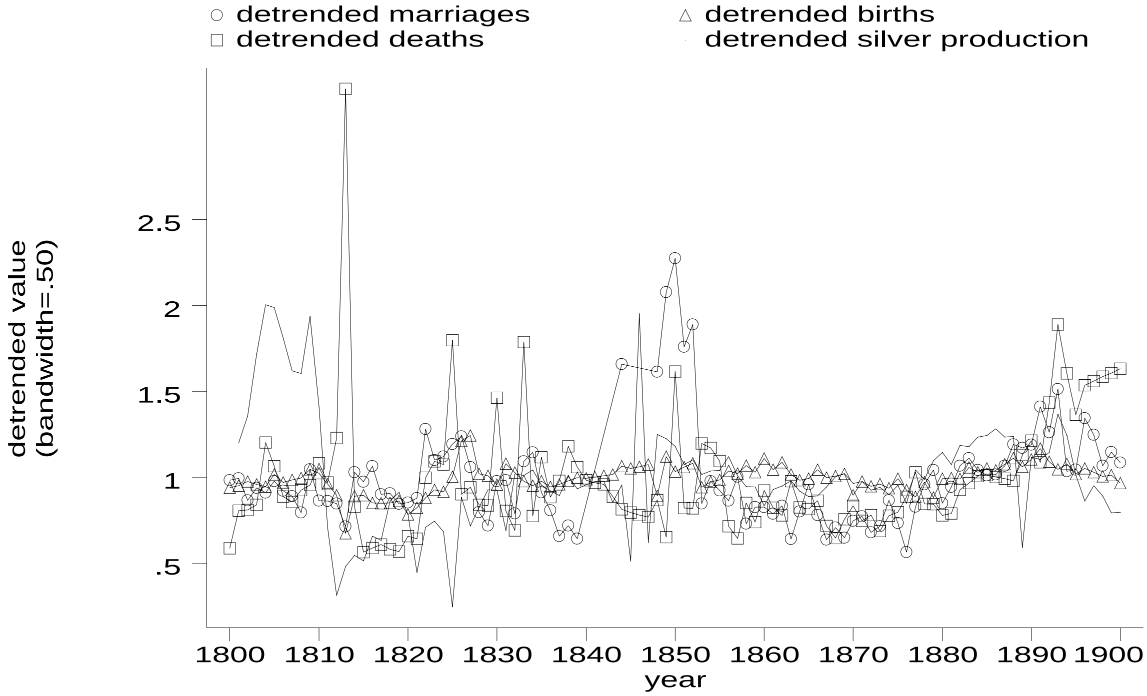

Figure 1e: Detrended Series for Mexico City, Mexico, 1800-1900

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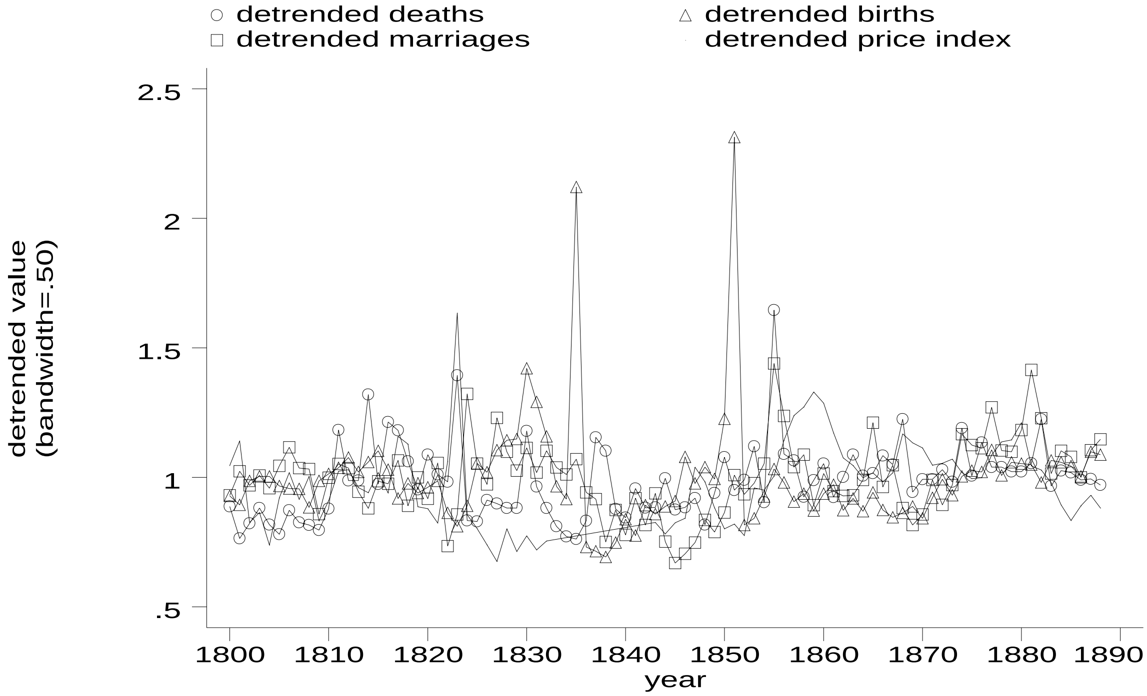

Figure 1f: Detrended Series for Salvador, Brazil, 1800-1890

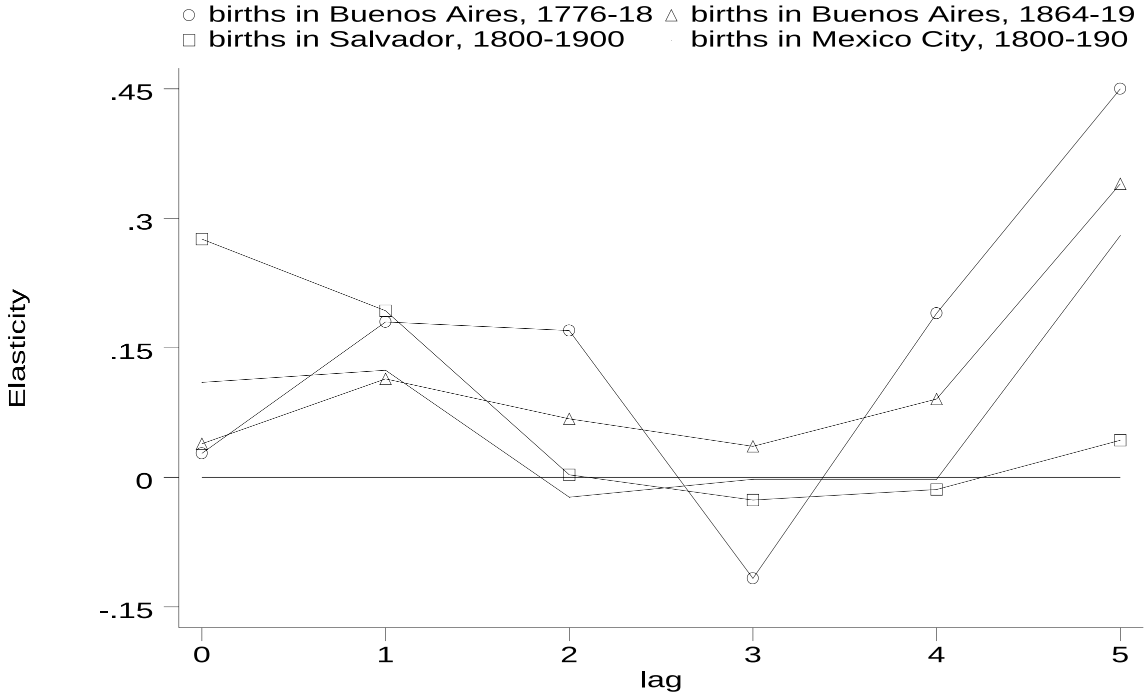

Figure 2a: Estimated birth response <lag 5=net response>

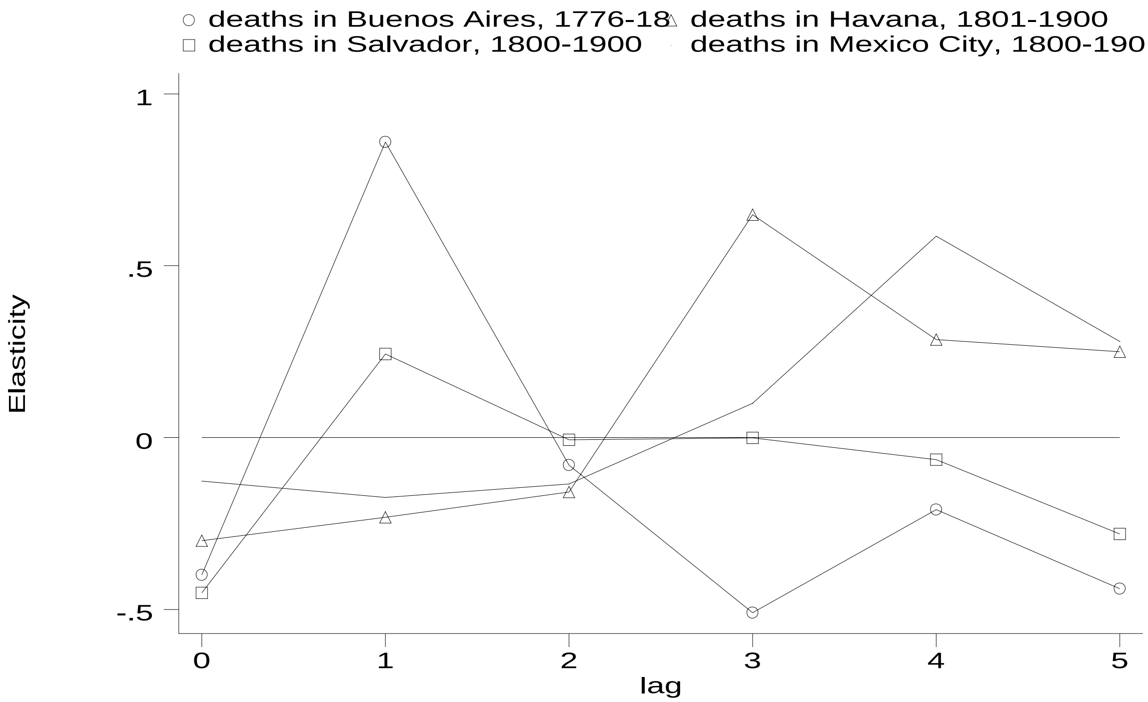

Figure 2b: Estimated death response<lag 5=net response>

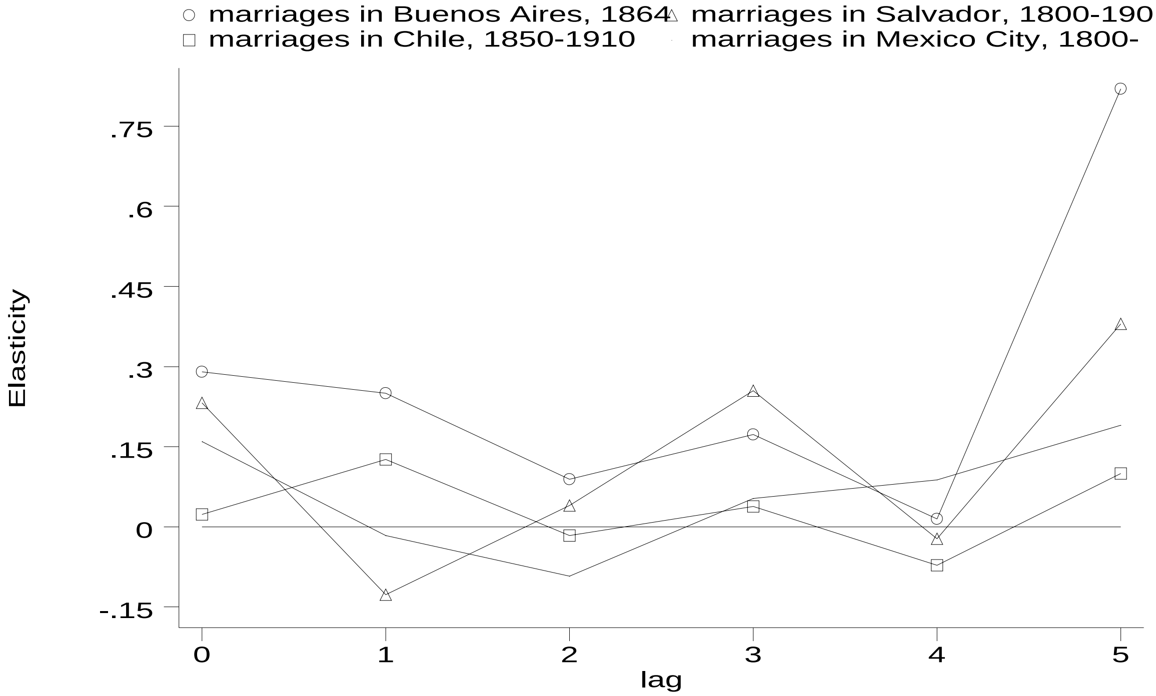

Figure 2c: Estimated marriage response <lag 5=net response>

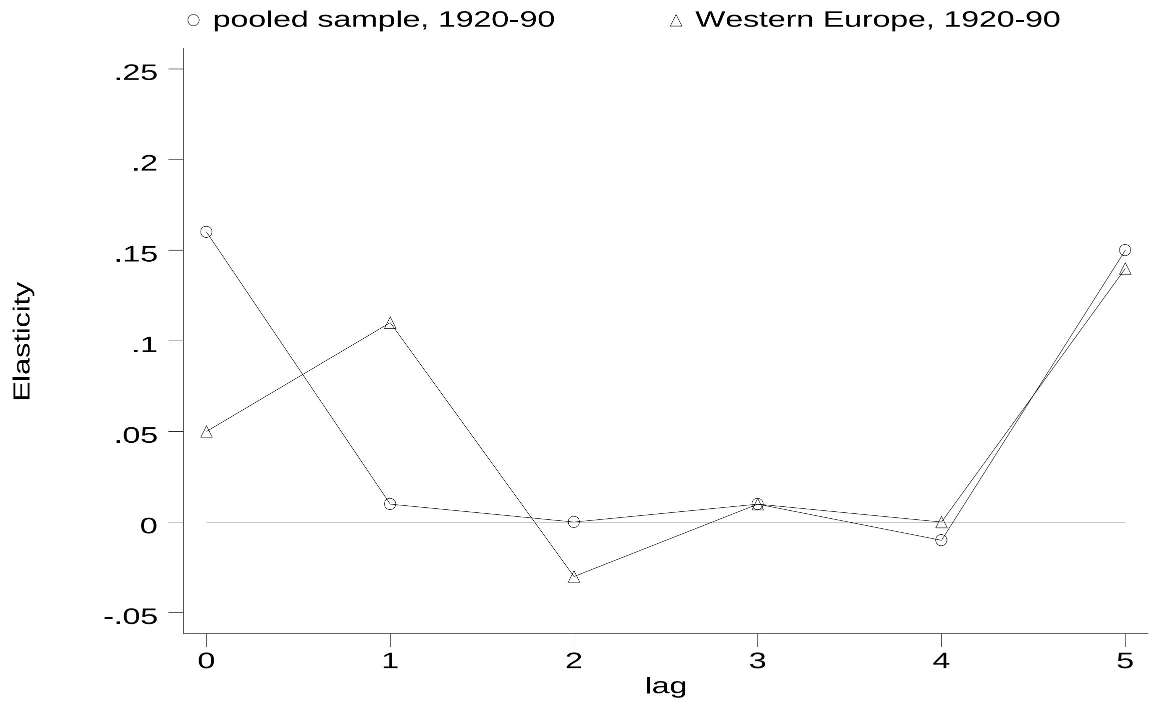

Figure 3a: Estimated birth response <lag 5=net response>

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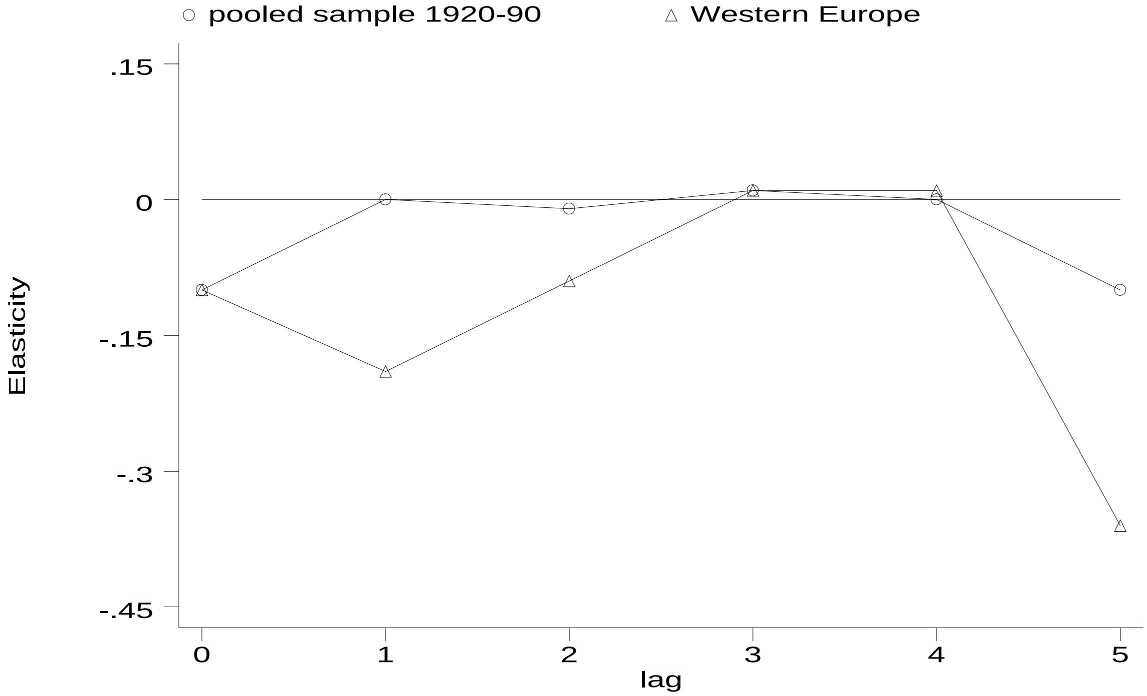


Figure 3b: Estimated death response <lag 5=net response>

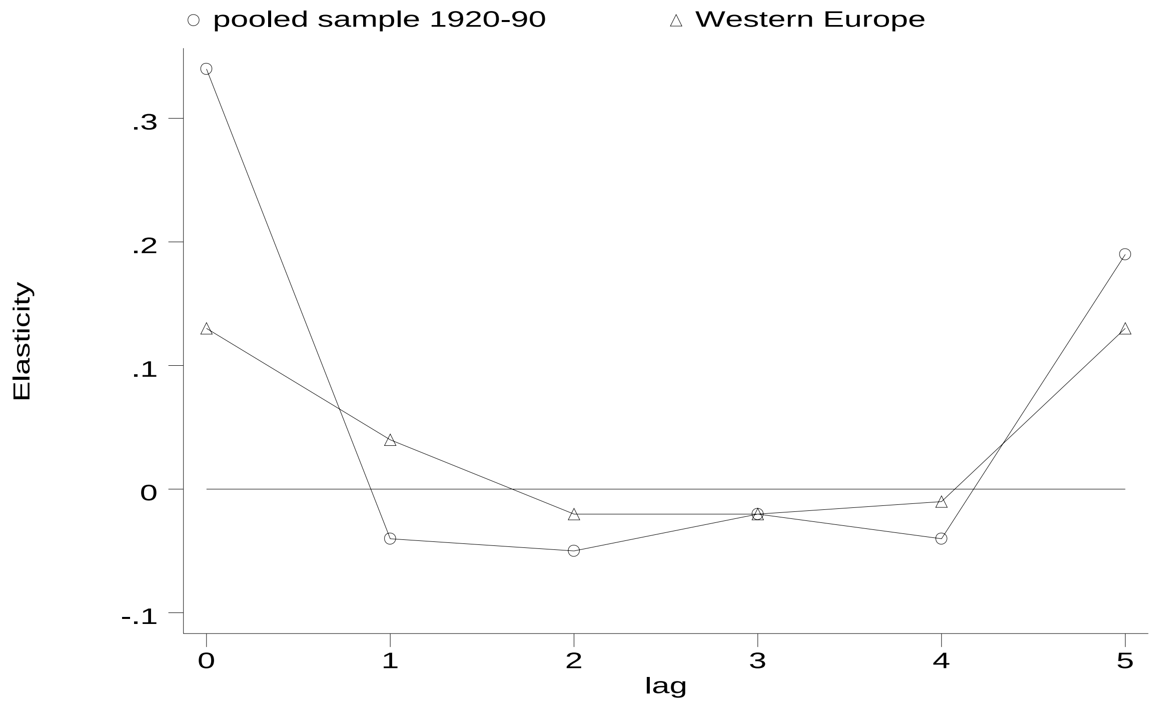


Figure 3c: Estimated marriage response <lag 5=net response>

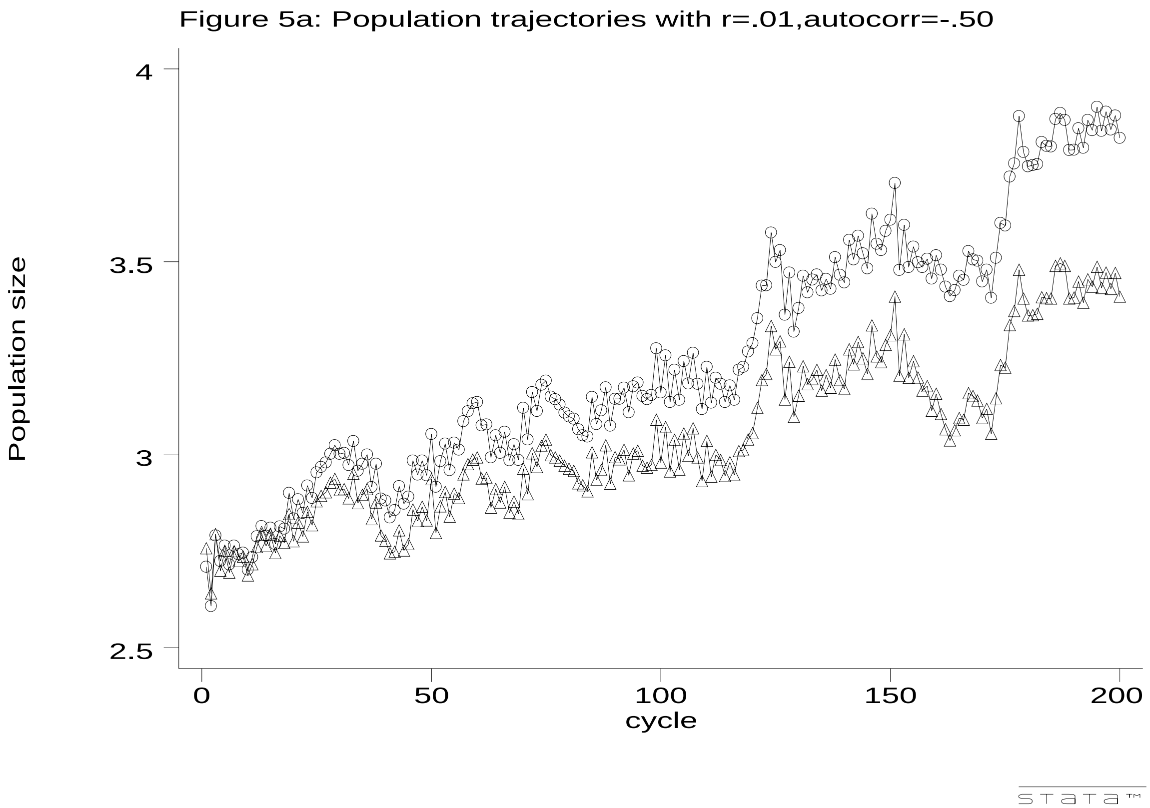


Figure 5a: Population trajectories with r=.01,autocorr=-.50
Population size
4
3.5
3
2.5
0
50
100
150
200
cycle
stata™

Figure 5b: Population trajectories with r=.01,autocorr=.50

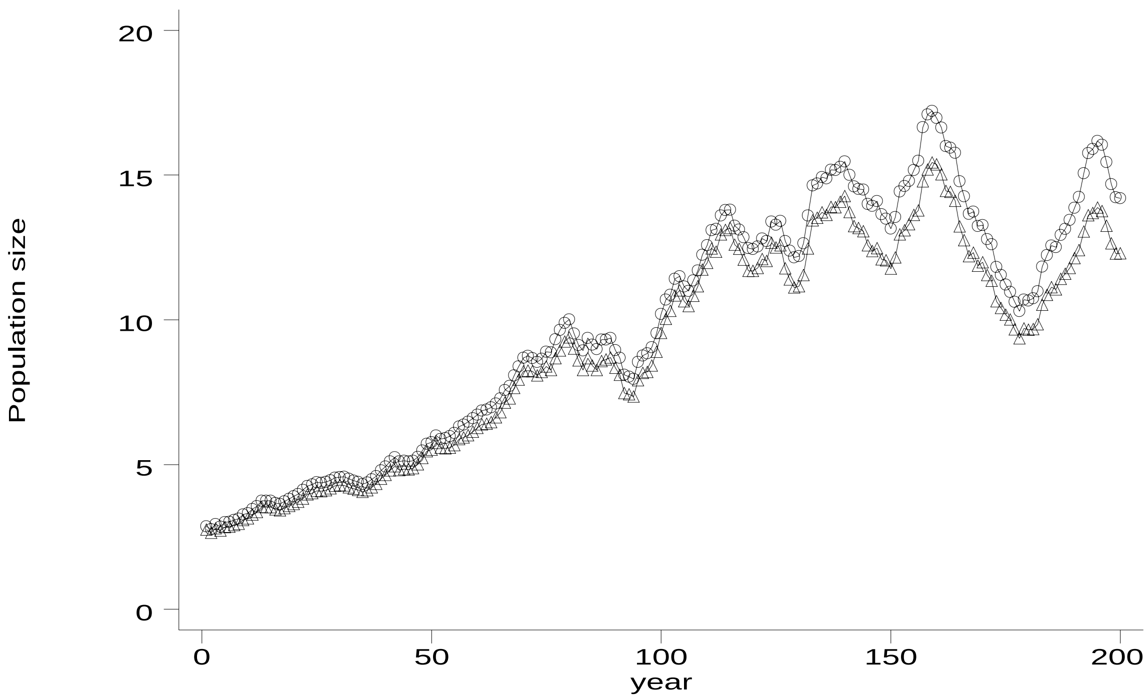


Figure 5c: Population trajectories with r=.10,autocorr=-.50

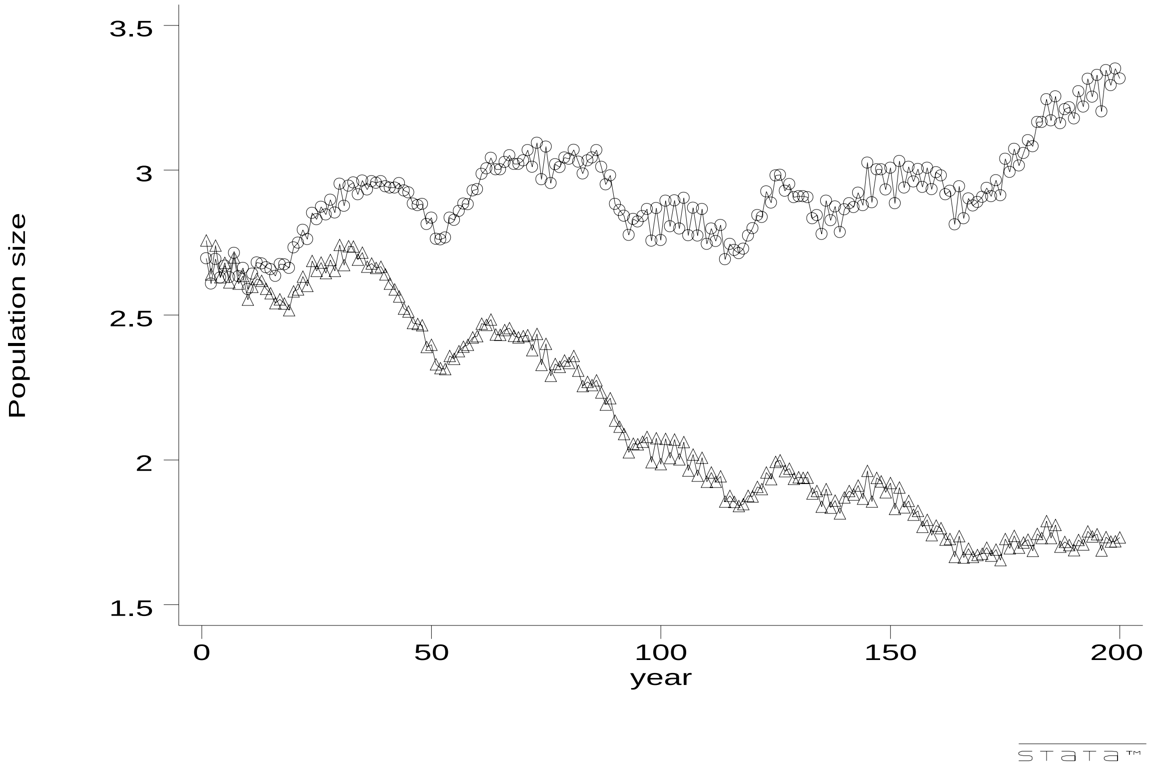


Figure 5d: Population trajectories with r=.10,autocorr=.50

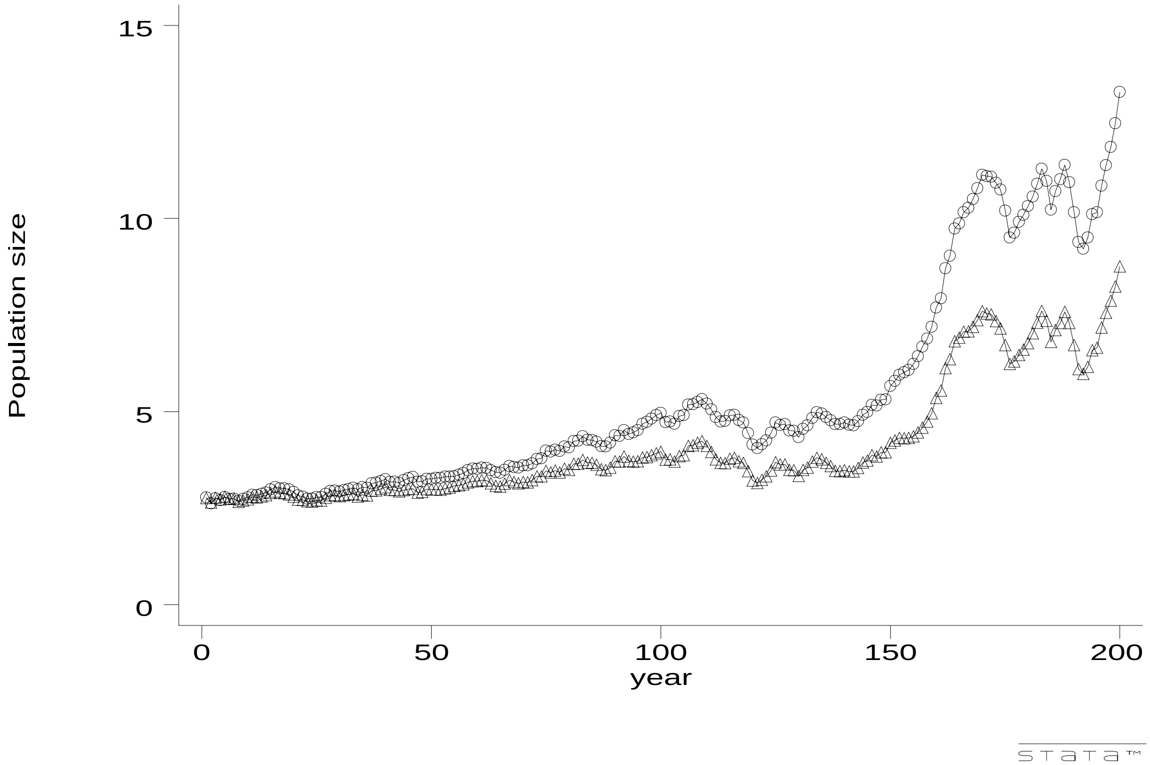


Figure 6a: Confidence bands for r=.01,autocorr=-.50

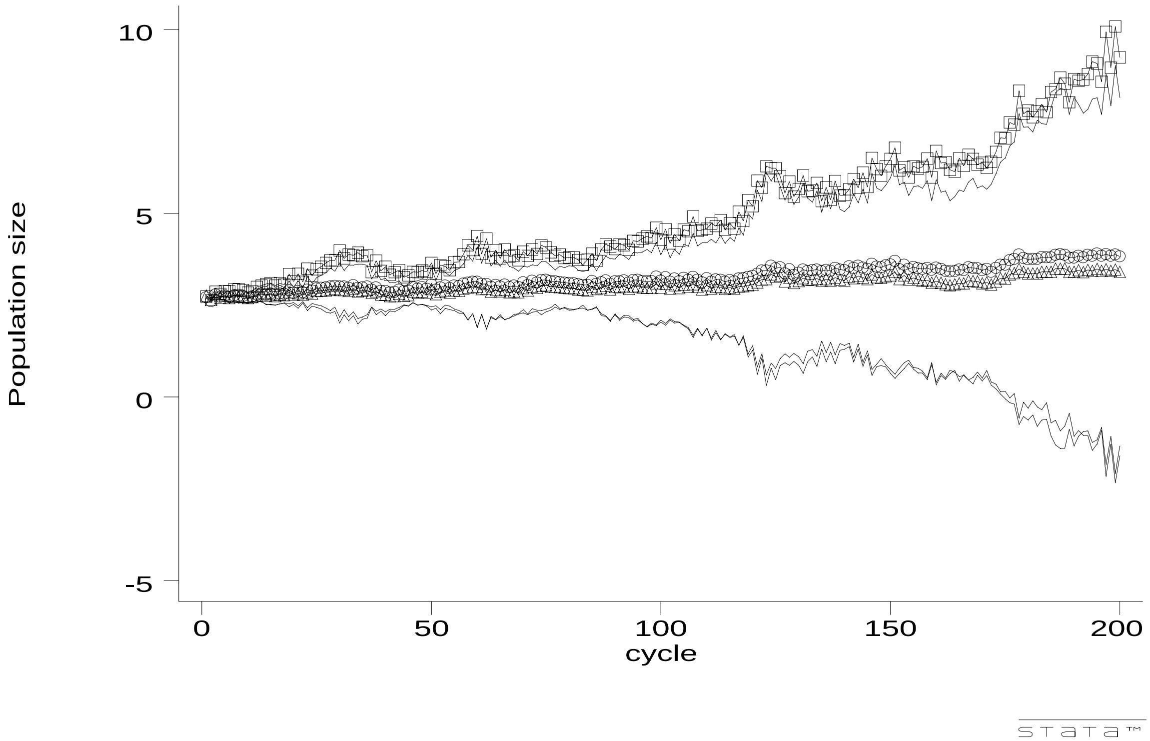


Figure 6b: Confidence bands for r=.01,autocorr=-.50

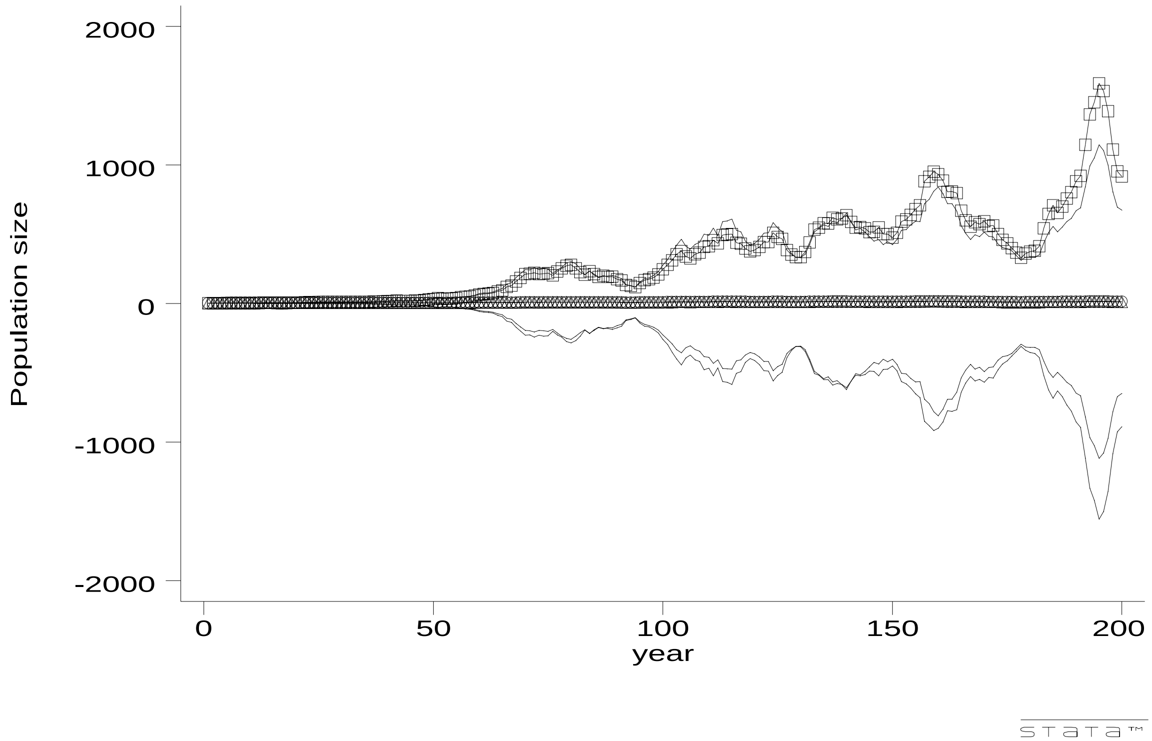


Figure 6c: Confidence bands for r=.01,autocorr=-.50

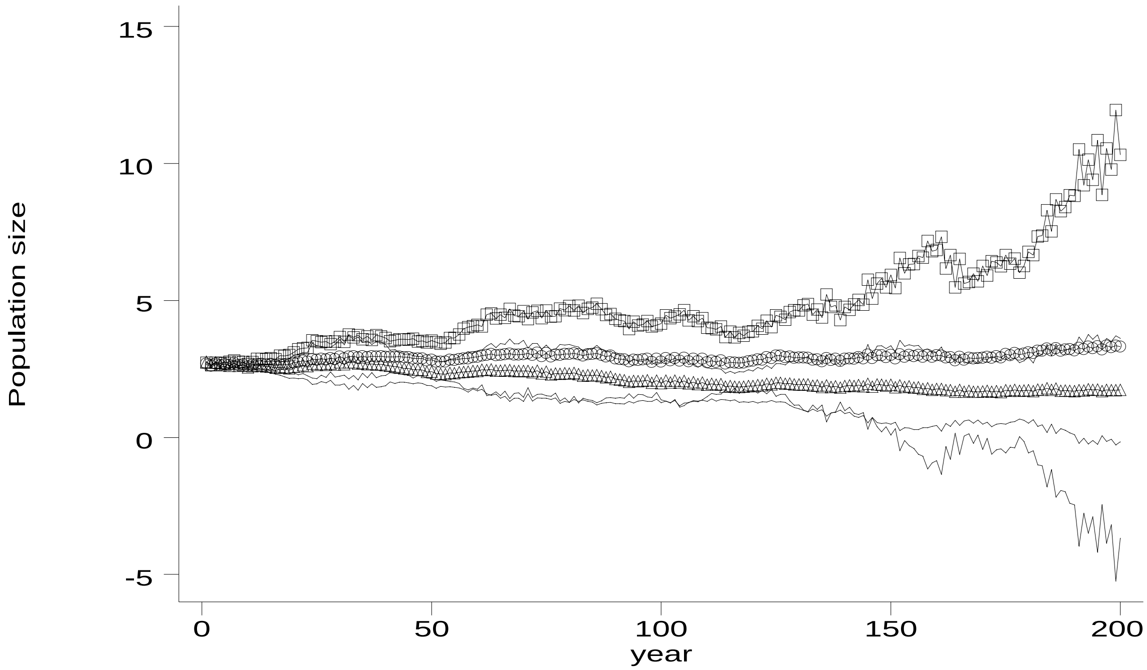
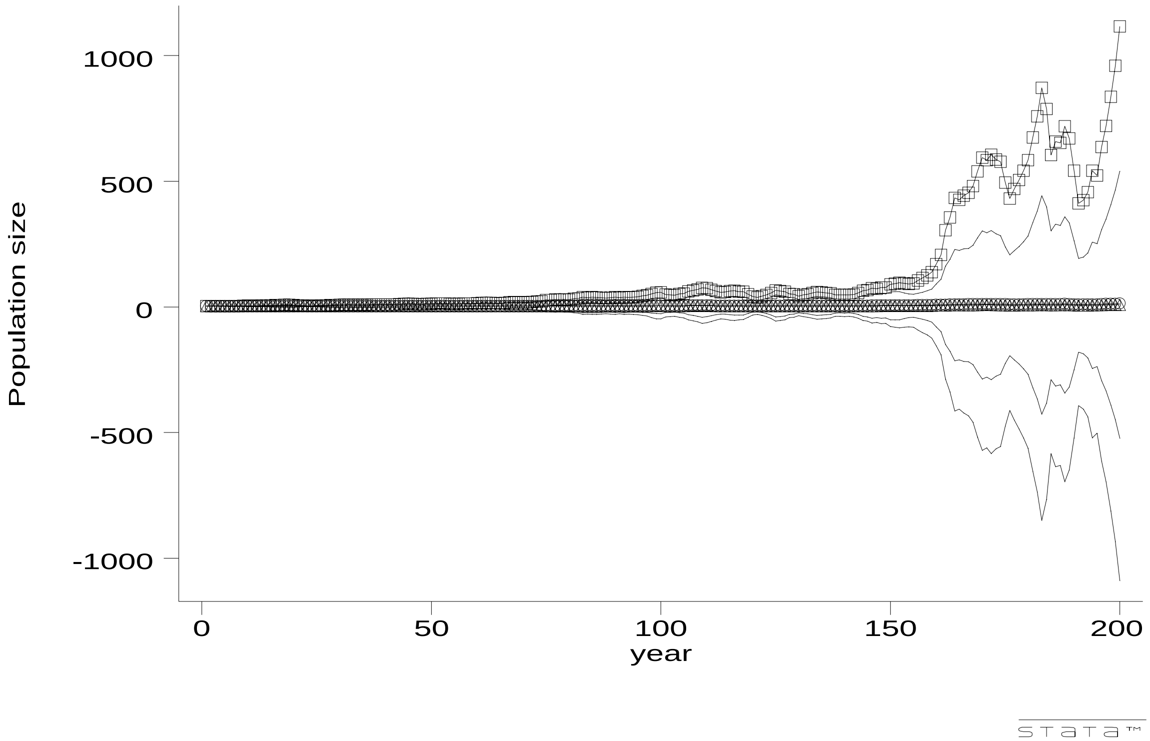


Figure 6d: Confidence bands for r=.01,autocorr=-.50



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