

Stop! Go! What Can We Learn About Family Planning From Birth Timing in Settler South Africa, 1835–1950?

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ABSTRACT We revisit the discussion on family limitation through stopping and spacing behavior before and during the fertility transition with a sample of 12,800 settler women's birth histories in nineteenth- and twentieth-century South Africa. Using cure models that allow us to separate those who stop childbearing from those who continue, we find no evidence of parity-specific spacing before the transition. We do find evidence of non-parity-based birth postponement before the transition. Increased stopping and parity-independent postponement characterized the beginning of the fertility transition, with increased parity-specific spacing following later in the transition phase.

KEYWORDS Settler South Africa • Fertility limitation • Cure models • Parity-dependent spacing

Introduction

Analyses of the drivers of fertility transitions often frame the discussion in terms of whether a society is ready, willing, and able (RWA) to begin limiting conception to limit family size (Coale 1973; Lesthaeghe and Vanderhoeft 2001: chapter 8). *Ready* implies that the economic, social, and cultural climate has an impact on a society's preference for children. Yet, although people may see the advantage of having fewer children, they are *willing* to limit family size only after that preference has become the norm in their society. Finally, they are *able* to implement actions to limit family size only if they know effective techniques. Typical evidence of readiness, willingness, and ability includes feelings regarding having a certain number of children (e.g., Fisher 2000a, 2000b; Fisher and Szreter 2003; Szreter and Fisher 2010: chapter 6) or knowledge of the kinds of birth control available (e.g., McDonald and Moyle 2018). Our ability to investigate these issues, however, is limited. McLaren (1984:3) noted that purely demographic studies cannot directly measure the contextual environment that would inform our knowledge of RWA. We may not be able to definitively outline the circumstantial impact on readiness, we may never know willingness without access to individual details, and we may not know ability without access to explicit mentions of it.

However, an empirical exploration of birth histories can shed light on characteristics of the fertility transition. Whether and how quickly couples continue to have chil-

dren following a first birth determine the final number of children they have. Changes to these patterns do inform our understanding of the fertility transition.

A robust debate in the historical demography literature concerns whether the first fertility transition was characterized by an increase in stopping behavior or an increase in the intentional spacing of subsequent births (Anderton and Bean 1985; Guinnane 2011; Knodel 1987). The dominance of one theory over the other has important implications for understanding the causes of the fertility transition. If the transition was dominated by stopping, it suggests an innovation in the approach to birth control that was absent before the transition as the main mechanism through which the transition was implemented: an increase in ability, with readiness and willingness potentially already present. Conversely, if spacing dominated the transition, it suggests increased use of known behaviors to adjust family size given some change in preferences: an adaptation in response to readiness and willingness, with ability already present (Carlsson 1966).

The consensus for many years had been that the transition was characterized and perhaps even defined by a new awareness of the ability to choose and manage family size as couples moved away from natural fertility (Coale 1986). New lower fertility rates were brought about by ceasing childbearing when the desired family size had been reached (Knodel 1987; Knodel and van de Walle 1979; van de Walle 1992), and it appeared that the fertility transition was implemented through birth control innovation. Later studies revisited this question. A substantial literature has argued for a change in intentional spacing behavior as a feature of the transition, thereby suggesting that the transition was a form of adaptation in response to a changing physical, social, economic, and cultural environment (Anderton and Bean 1985; Crafts 1984; David et al. 1988; David and Mroz 1989; Dribe et al. 2017; Garrett et al. 2001:288; Haines 1989; Szreter and Garrett 2000). These studies suggest that we think beyond intentional behavior and beyond the fertility transition as being a structural break in birth control behavior (Szreter and Garrett 2000).

Indeed, evidence points to geographic, economic, and social differences in fertility rates before the transition (Anderton and Bean 1985; Bengtsson and Dribe 2006; Dribe and Scalone 2010). Some researchers have argued for evidence of pretransitional parity-dependent fertility control through spacing (Anderton and Bean 1985; Cinnirella et al. 2017; David et al. 1988; van Bavel 2004; van Bavel and Kok 2010). Most have used a variety of techniques assuming that observed fertility outcomes are intentional (Johnson-Hanks 2007; Timæus and Moultrie 2008, 2013). Yet Fisher (2000a, 2000b), Fisher and Szreter (2003), and Szreter and Fisher (2010:236–242) found little communication between partners regarding family size outcomes, although they did find that partners used techniques to avoid conception—techniques that originated long before the transition.

Specifically, Timæus and Moultrie (2008), Moultrie et al. (2012), and Timæus and Moultrie (2013) discussed the importance of unintentional fertility control through postponement due to circumstances unrelated to family size as a third way that fertility outcomes may be eventuated. They noted that take-up of this idea in studies of historical fertility transitions has been fairly limited and that ignoring this option may result in misidentifying parity-dependent spacing both before and during the transition (Timæus and Moultrie 2013). The debate remains far from resolved, and our country-wide data set of complete birth histories of White South African women from 1835 to 1950 allows us to enter the fray.

Settler South Africa was in many respects similar to other settler communities on the periphery. South Africa during the nineteenth century was largely rural, land-abundant, and labor-scarce, much like the frontiers in the United States and Canada and like much of Australia and New Zealand. Industrialization began late in the nineteenth century, taking off in the early twentieth century. The fertility transition among the settlers in South Africa began in the late 1870s to women born in the 1850s, a timing similar to other settler fertility transitions as well as several European transitions (Cilliers and Mariotti 2019).

Our econometric technique is a mixed population model, known as a *cure model* in the biomedical literature. Cure models are increasingly popular in demographic analyses because they take into account that some women may never go on to experience an event, a point that traditional event-history models do not consider.

Our goal is modest. We use our data to analyze features of fertility behavior before and during the demographic transition in South Africa. We remain agnostic about intentions regarding family size beyond what our estimation technique is able to provide.

We stratify our sample into four periods: 1835–1859, 1860–1884, 1885–1909, and 1910–1950. Looking at only those women who have had at least one child, we find that before the transition, a woman's physiology was the prime determinant of the space between her consecutive children as well as when she had her final child. That is, age and fecundity mattered most. Social and economic characteristics arguably played a small role in all three types of fertility behavior: there is some evidence of postponement but no evidence of achieving desired family size through stopping or spacing. The incidence of stopping and postponement began to increase from 1885 onward, and parity-dependent spacing increased from 1910 onward. Crucially, this means that we do not find evidence of parity-dependent spacing before the fertility transition. Quite the contrary, women at high parities before the transition had the least variation in birth interval length, which we interpret as evidence of higher fecundity.

Although it is by now very clear that pretransitional couples were able to affect the timing of conception to avoid an inconveniently timed birth, they were not consistently using this strategy to control ultimate family size. Postponement will ultimately affect final family size because of women's finite childbearing window, but it does not imply conscious family planning. Regarding "ready, willing, and able," the evidence we find points to early readiness and ability at the onset of the transition, followed by an increase in willingness, which led to community-wide longer birth intervals later.

Intentional and Unintentional Family Size Control

If we can show that women/couples intentionally stopped having children at lower parities during the transition, then we will have some evidence of an improved ability to stop childbearing relative to pretransition periods. This evidence would suggest that a preference for smaller families may have always existed but that the transition was the first time that large sections of the population were able to successfully implement strategies to prevent childbearing (Carlsson 1966; Okun 1994).

If we do not find evidence of increased stopping—and instead see evidence of increased time between successive births—then, the literature argues, we are simply seeing a change in preferences being implemented with birth control techniques that

humankind has always known. Given women's finite childbearing window, increased birth spacing will also result in lower numbers of children ever born (Anderton and Bean 1985).

Both concepts—stopping and spacing—allude to conscious efforts to limit final family size, referred to as *parity-dependent fertility decisions*. Timæus and Moultrie (2008) noted a third possibility: postponing the birth of a child, not because of the number and ages of existing children but rather because of a preference not to have a child under the current circumstances (e.g., war, famine, health, other uncertainty, educational attainment, career, and so on; Johnson-Hanks 2004).

The investigation of birth stopping and spacing and, later, postponement before and during the demographic transition has evolved tremendously in the 60 or so years that demographers have been studying historical fertility. Methodological innovations, improvements in computing power, and access to better data have allowed a revisiting of this issue, and the debate remains far from resolved.

The earliest investigations concluded that couples made little attempt to limit fertility before the transition and that any deviations of the pretransition fertility rates between groups had more to do with factors such as cultural practices and seasonal migration (Henry 1961; Knodel and van de Walle 1979). It followed that the fertility transition must have been driven largely by an intentional increase in stopping rates once couples had achieved their desired family size. The Coale and Trussell *m* index (Coale and Trussell 1974) was instrumental in reaching this conclusion. The index, however, is limited in that it was constructed specifically to detect parity-dependent stopping rather than spacing (Knodel 1987; Okun 1994). Okun (1994) and Guinnane et al. (1994) noted that the Coale and Trussell index cannot accurately identify small proportions of fertility controllers in a population. Family limitation through spacing may well have been taking place but could not be detected. David et al. (1988) subsequently developed the cohort parity analysis technique specifically to identify spacing behavior, yet Okun (1994) noted that it, too, is unreliable in certain cases. This technique also has large data requirements.

Data advances, particularly the use of family genealogies and family reconstitution data, allow a more precise look at the length of time between consecutive births both before and during the fertility transition. A growing literature accepts that the transition was at least in part characterized by changes to birth interval length and not just by family limitation (Anderton and Bean 1985; Crafts 1984; David and Mroz 1989; Dribe et al. 2017; Garrett et al. 2001:288; Haines 1989; Reher et al. 2017; Reher and Sanz-Gimeno 2007; Szreter and Garrett 2000).¹

Another literature has examined evidence of parity-specific spacing before the transition (Cinnirella et al. 2017; van Bavel 2004; van Bavel and Kok 2010). This effort is important: evidence of spacing to limit family size would clearly show that couples already knew how to achieve their desired family size and that they were doing so before the transition. The conclusion would then be that the transition itself was a result of changing family size preferences in response to a change in external conditions in the late-nineteenth century.

Exploiting the use of event-history models—in particular, Cox proportional hazards models—and using family reconstitution data, these studies all found evidence of parity-dependent control: longer birth intervals at higher parities, after controlling

¹ This list is by no means comprehensive. See Dribe et al. (2017) for a deeper discussion of the findings.

for age, before the transition.² Moreover, they found differences in birth interval lengths based on occupation, region of residence, religion, language, and other variables that are unlikely to change much over the adult lifetime. The conclusion is that couples were able to limit family size before the transition.

A limitation of the search for pretransitional spacing is that it contains a sometimes implicit assumption that the observed outcome of smaller family sizes and longer birth intervals must be driven by an intention to limit family size. The debate doesn't allow for a change to birth interval lengths driven by something other than parity dependence (Johnson-Hanks 2007; Moultrie et al. 2012; Timæus and Moultrie 2008). Furthermore, the debate assumes some kind of explicit agreement within a partnership on the intended size of the family, something that Fisher (2000a, 2000b), Fisher and Szreter (2003), and Szreter and Fisher (2010: chapter 6) found not to be the case, even though they found evidence of pretransitional birth control.

In a recent dramatic twist in the debate, Clark and Cummins (2019) argued that the parity-dependent control that Cinnirella et al. (2017) observed is an artifact of the estimation method and that no parity-dependent control exists in pretransition England.³ Offering further support for this argument, Clark et al. (2019) argued that the unexpected arrival of twins led to an increase in the total number of children by almost one, concluding that couples did not respond to the additional unexpected birth.

Separately, Yamaguchi and Ferguson (1995), Li and Choe (1997), Alter et al. (2007), Gray et al. (2010), and Alter (2019) argued that Cox proportional hazards models are inappropriate for this type of event-history analysis because the technique assumes that all women will at some point experience a subsequent birth when in reality, some stop having children. The suggestion is that the data may not satisfy the proportionality assumption needed to obtain unbiased estimates using the Cox model.

A debate that began about whether the transition was driven by stopping or spacing, and that has not yet ended in consensus, has shifted to an investigation of whether couples practiced conscious spacing before the transition. This second debate has culminated in substantial disagreement on the appropriate use of methodology given limitations to family reconstitution data. It is at this stage in the debate that our study enters the fray. With data spanning 100 years and armed with event-history models that look simultaneously at stopping, spacing, and postponement, we investigate fertility outcomes both before and during the transition.⁴

Data

We use South African Families (SAF), a genealogical registry of all settler families living in the eighteenth, nineteenth, and early twentieth centuries.⁵ It is one of very few

² See Alter (2019) for a critique of these methods.

³ Cinnirella et al. (2019) provided a rejoinder to the Clark and Cummins (2019) argument.

⁴ Bengtsson and Dribe (2006) and Dribe and Scalone (2010) are among the few that looked at short-run shocks that may result in the postponement of a subsequent birth independent of the number and age of existing children.

⁵ The registers were compiled from the baptism records (which contain birthdates) and marriage records of the Dutch Reformed Church, marriage documents obtained from various courts and magistrates offices, and a number of notable genealogical publications and individual family histories. See Gouws (1987) for a more

registries in the world that is known to have documented a full population of immigrants and their families over several generations, and its vast scope over nearly three centuries is well suited to the study of demographic responses over the long run.⁶

Individuals in SAF are reported patrilineally, with children appearing exclusively in their father's lineage. As such, children are not directly linked to their mothers.⁷ Every male to immigrate to South Africa begins a new lineage in the data. These family trees contain as much as was known about each family member and typically include birth, baptism, marriage, and death dates and locations, as well as region of origin for progenitors. The spouse's name (maiden name, where applicable) and vital information are also listed. Ideally, all entries would be complete for all life history events, but this is not always the case.⁸

We restrict the sample to women with complete birth histories (i.e., we know the birthdates of the children they registered) and limit the analysis to marital childbearing, given that children born out of wedlock were rarely recorded in any of the source documents used by the genealogists who compiled the family lineages. The analysis does not include women with no children recorded under their husbands because it is unclear whether these women truly had no children or their children simply were not recorded.

Our data contain limited information on infant and childhood mortality. Some births in our data have no death dates, and it is feasible that some of these are infant or early childhood deaths that were not recorded correctly. We cannot exclude births for which we have no death date because doing so would require the exclusion of entire families, which would substantially reduce the sample. We expect the erroneous recording of infant deaths to decline over time with declines in the infant mortality rate.⁹ Later, in the discussion of our results, we consider the implications of a reduction in infant mortality or the reporting thereof for our model.

A woman's reproductive life is taken to be between the ages of 15 and 49. We include all women who we know lived at least until age 50 or for whom we have either their death date or their husband's death date. We exclude women for whom we have none of this information. We follow 12,800 women who gave birth to a total of 61,871 children between 1835 and 1950.

Representativeness

By virtue of the mere size and scope of SAF, many sources of potential bias are mitigated. However, some remain because of the missing or incomplete entries we

detailed description of the origins of these data. See Cilliers and Fourie (2012) and Cilliers and Mariotti (2019) for a full account of the transcription of the SAF into a database fit for use in demographic analysis.

⁶ Although the data contain all known families, they do not always contain all members of these families.

⁷ For example, if settler A1 (male) has two children, A1B1 (male) and A1B2 (female), both of whom then marry and have a number children of their own, only the children of A1B1 will appear in settler A1's lineage (because these children share their paternal grandfather's surname). The children of A1B2 will appear in her husband's lineage. See Cilliers and Mariotti (2019) for details on our construction of a data set of mothers from these patrilineages.

⁸ See Cilliers and Mariotti (2019) for an extensive discussion of the implications of missing data on SAF sample representativeness when studying fertility behavior in this population.

⁹ Simkins and Van Heyningen (1989), however, suggested White infant mortality rates that were substantially higher than in Australia during the fertility transition.

alluded to earlier. Chiefly concerning among these are (1) a misrepresentation of the age, regional, or socioeconomic distribution of women in our sample, and (2) the selective noninclusion of childless women and women with incomplete birth histories. Each of these warrants further discussion.

To discern any sample composition biases, Cilliers and Mariotti (2019) compared the SAF data with available aggregate census returns for the Cape Colony. The sample captures census distributions reasonably accurately. In general, we do not believe that the SAF systematically under- or oversamples certain groups of the population with respect to gender, and it likewise appears to provide an accurate representation of the age structure of the full population if not by district. Thus, we believe that any differential birth timing by age or region captured by our model is indicative of behavioral differences rather than a function of sample composition. To ensure that this is the case, we add controls for region in our estimation.

We also consider the socioeconomic structure of the sample, as proxied for by the household head's occupation.¹⁰ This variable is particularly poorly recorded, as we will show later. However, because the analysis does not predominantly focus on this variable, we include it purely to address correlation between occupation and other explanatory variables; we do not interpret it in the analysis.

A comparison between women dropped from our sample because they had no recorded children or because they had incomplete birth histories and the remaining women is informative to the question of bias arising from their exclusion. The former have poor records in general. It is unclear why they would have a substantially different number of children than the women in our sample and whether their birth timing would substantially differ from that of the women in our sample. For our findings to be incorrect, these excluded women would need completely different patterns of childbearing to shift the averages we find. We see no logical reason why their pattern of childbearing would be correlated with the quality of their other records.

Periodization

We split the sample into four periods to coincide with the timing of changes in settlement patterns and economic developments, as well as indicators of the onset of the fertility transition.¹¹ Cilliers and Mariotti (2019) showed that the onset of the settler fertility transition was driven by women born in the 1850s, who began having children in the 1870s. Our first period, 1835–1859, therefore consists of children born before the fertility transition began. These children were born predominantly in the Cape Colony; they resided either in Cape Town—which we denote *Old Urban*—or in what was then, and still largely is, a rural region that extended north and east from Cape Town, exceeding to some extent the boundary of the Cape Colony to the north—*Old Rural*.

This period coincides with the start of the Great Trek, the movement of Afrikaans speakers from largely the eastern portion of the Cape Colony into the interior of what was to become South Africa. We call these newly settled regions *New Rural*; they span

¹⁰ We use husband's occupation here because women rarely had reported occupations in the genealogical data and the censuses.

¹¹ Inclusion in a given period is conditional on a first birth for the couple in that period.

essentially all of modern South Africa that excludes Old Rural and the urban areas. That is, New Rural contains the two independent Boer Republics, the British colony of Natal, and the far northern districts of what came to be the Cape Colony under the British.

Our second period is 1860–1884, a combination of pretransition and transition births. With the Great Trek, we begin to see the establishment of small urban pockets along the east coast as well as in the interior during this period. We label these urban areas *New Urban*. These areas were initially small but began to grow with the development of the mining industry in the interior in the last quarter of the nineteenth century.

Our third period covers 1885–1909, when the fertility transition was well underway. These births coincide with the discovery of gold on the Witwatersrand, the slow advent of industrialization into the early twentieth century, and the Boer War (1899–1902). The settlers began a slow urbanization late in the nineteenth century, with the pace increasing because of the Boer War and the subsequent growth of the mining industry around Johannesburg in the early twentieth century (Giliomee 2003:323).

Our final period is from 1910 through 1950. The major economic developments during this time were the intensification of gold mining on the Witwatersrand coupled with industrialization and the growth of manufacturing in the 1920s and 1930s.

Sample Characteristics

We turn now to look at mother's and birth interval characteristics over our sample period, as shown in Table 1. Panel A reports means and standard deviations for the number of children ever born, age at marriage, first birth, and last birth, and birth interval lengths. The average number of children ever born is 7.61 in the first period, exhibiting a barely discernible decrease in the second period before it begins to decrease in the third period, culminating in 4.13 children for women giving birth in the last period. Figure 1 shows this result in more detail by looking at the distribution of children ever born by women giving birth in the four periods we examine. The first two periods exhibit largely the same distribution of children ever born, with most women having between 1 and 10 children. The third period shows higher shares of women with seven or fewer children and a faster decline in share as the number of children increased. We observe a big change in the final period: a large increase in the proportion of women with smaller families and much more concentration around low numbers of children.

Average age at marriage increased throughout the period of study: it increased by four years between the first period and the last, with the largest increases coming between the third and fourth periods. We see a similar increase in age at conception of the first child throughout the period of study. Age at last birth increased slightly before the transition before decreasing again slightly during the transition. Finally, looking at average birth interval lengths, we see increases in each period, with a final large increase between the third and fourth periods to an average interval length of 39.59 months.

We explore the increase in birth interval length in greater detail in Figure 2, which plots mean birth interval lengths and their standard deviations by period over parity.¹²

¹² Figure 2 does not take into account the number of children ever born, and hence at higher parities over time, it is dominated by an increasingly small proportion of women with large numbers of children—women we may deem to be highly fecund relative to the average.

Table 1 Descriptive statistics

	1835–1859	1860–1884	1885–1909	1910–1950
A. Means by Children’s Birth Years				
Children ever born	7.61 (3.48)	7.40 (3.30)	5.82 (2.93)	4.13 (2.18)
Age at marriage	19.73 (3.95)	20.21 (4.07)	21.76 (4.34)	23.78 (4.93)
Age at first conception	22.49 (4.16)	23.03 (4.62)	24.92 (4.76)	27.73 (5.38)
Age at last birth	37.79 (6.15)	39.26 (5.51)	38.52 (5.37)	37.92 (5.10)
Birth interval in months	27.83 (28.30)	27.92 (17.67)	30.61 (20.36)	39.59 (26.45)
B. Sample Share by Children’s Birth Years				
Mother’s age				
Age <25	33.84	30.25	22.93	14.17
Age 25–29	28.56	26.66	27.13	26.42
Age 30–34	20.78	21.42	24.67	27.78
Age 35–39	12.18	14.77	17.42	20.98
Age ≥40	4.64	6.90	7.85	10.65
Final births	10.34	12.04	17.62	32.11
Husband’s occupation				
White collar	7.56	8.13	7.80	10.79
Farmer	22.55	23.06	22.25	16.80
Skilled/semiskilled	3.09	2.79	3.03	3.34
Unskilled	3.69	3.42	1.38	0.68
Unknown	63.12	62.59	65.54	68.39
Region				
Old Urban	4.52	2.67	1.94	2.92
Old Rural	56.63	41.30	27.70	20.70
New Urban	1.37	2.44	3.99	5.58
New Rural	9.52	19.61	25.92	27.69
Unknown	27.96	33.99	40.45	43.10
Mother’s birthplace				
Born abroad	1.25	1.57	0.96	2.10
Born locally	56.22	51.83	45.64	46.84
Unknown	42.53	46.60	53.41	51.07
Mother’s home language				
English	17.55	20.08	20.60	20.43
Afrikaans	77.33	73.05	71.63	69.55
Unknown	5.12	6.88	7.77	10.02
Number of Mothers	1,674	2,532	3,785	4,809
Number of Birth Intervals	9,212	14,630	18,658	19,371

Notes: Standard deviations are shown in parentheses. Only women with known death dates who die after age 49 are included in the calculations of children ever born, age at last birth, and share of final births. Means and proportions are calculated on the basis of the period in which the first child was born.

In the two early periods, birth interval lengths were slightly shorter than 30 months—the same length that Menken and Sheps (1972) predicted for a pretransitional society. Birth intervals increased slightly at higher parities but were never more than 30 months. They decreased again at the highest parities, most likely because of the dominance of the highly fecund at these parities. Interval length increased at all parities

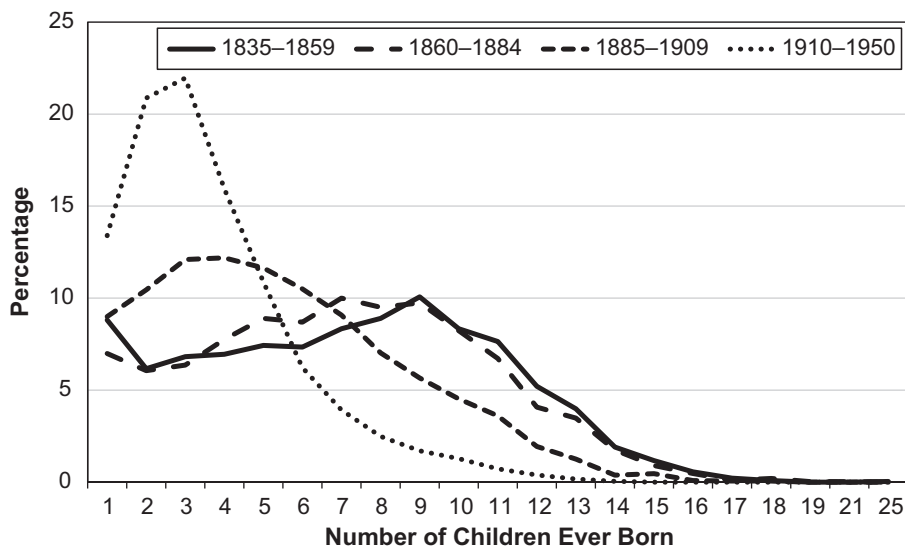


Fig. 1 Distribution of women with children ever born, by period

in the two transition periods, particularly at the lower parities. Figure 2 is strongly suggestive of some form of birth interval length changes—whether spacing or postponement changes—playing a role in the transition, especially in the later stages.

Figure 3 explores interval length in yet further detail, looking at period as well as children ever born over parity. The figure plots mean birth interval lengths (given by the symbols) and their standard deviations (given by the bars) for 1835–1859 and 1910–1950 by whether the total number of children ever born was three, five, or eight; the figure thereby captures average family sizes before the transition and at the end of the analysis period. Unsurprisingly, we find that smaller numbers of children ever born are correlated with larger birth intervals regardless of period, as must be the case if there was limited stopping.¹³ We see increases in birth interval length between periods when holding constant the number of children ever born. That is, the birth interval lengths for families with three or five children ever born were shorter in the 1835–1859 period than in the 1910–1950 period, showing an increase in spacing or postponement behavior over time and suggesting that only limited spacing behavior took place before the transition. Finally, we see very little change in the interval lengths at low parities for women with eight children between the two periods: an increase in interval length for the younger group is evident only after the fifth child. This is to be expected: there is not much time to waste on spacing or postponing if there will be eight children.

Table 1, panel B reports shares for demographic, economic, and cultural characteristics. Mother's age is her age at the conception of each child. The number of observations here is the number of birth intervals, shown in the final row of the table. Just

¹³ The intervals indicated by the solid black circles and triangles are larger than those indicated by the hollow circles and solid diamonds.

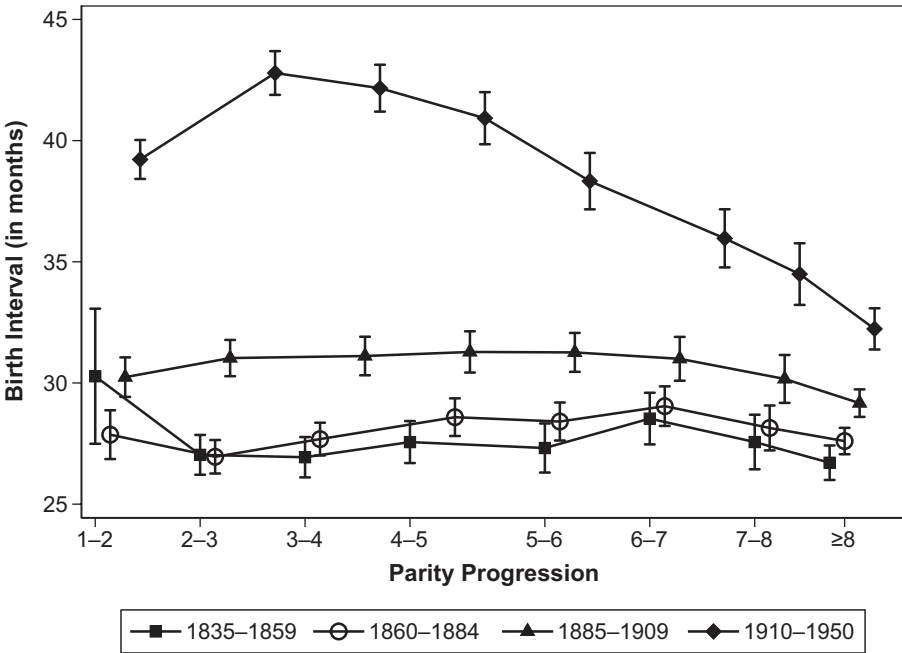


Fig. 2 Birth interval length over parity, by period. Symbols represent mean interval length for each period; bars represent standard deviations.

over 30% of all the birth intervals occurred for mothers aged 24 and younger, with this number decreasing to 14% over time. We see increases in the percentage of birth intervals for mothers aged 30–34, 35–39, and 40 or older—a finding that signifies changes in the timing of births across intervals. The decrease in share at the youngest age suggests a later start to childbearing as well as an increase in birth interval length once childbearing has started. The proportion of final birth intervals increased from 10.34% for the 1835–1859 period to 32.11 % for the post-1910 period. The increase is predominantly between the third and fourth periods.

As can be seen for each economic and cultural characteristic in the table, there is a somewhat high percentage of unknowns. Because some selection bias is likely correlated with the unknowns, we include them in the subsequent analysis (although we do not report them in the analysis). The registration documents seldom required the husband to report his occupation, and hence the occupation information available in the sample is subject to some bias, as noted earlier.

With respect to region, births in the first period were predominantly located in the Cape Colony, particularly in the rural areas, as expected, given that this was a largely rural colony. As the settlers began to migrate, the percentage in the Cape Colony dropped, and the percentage in the interior and of unknowns increased.¹⁴ We continue to see increases in the percentage of the sample from outside the Cape Colony and, in particular, an increase in the proportion of the sample in the New Rural area over time.

¹⁴ It is quite likely that as the settlers migrated, the quality of the records deteriorated. The implications of this for the purposes of analysis are discussed at length in Cilliers and Mariotti (2019).

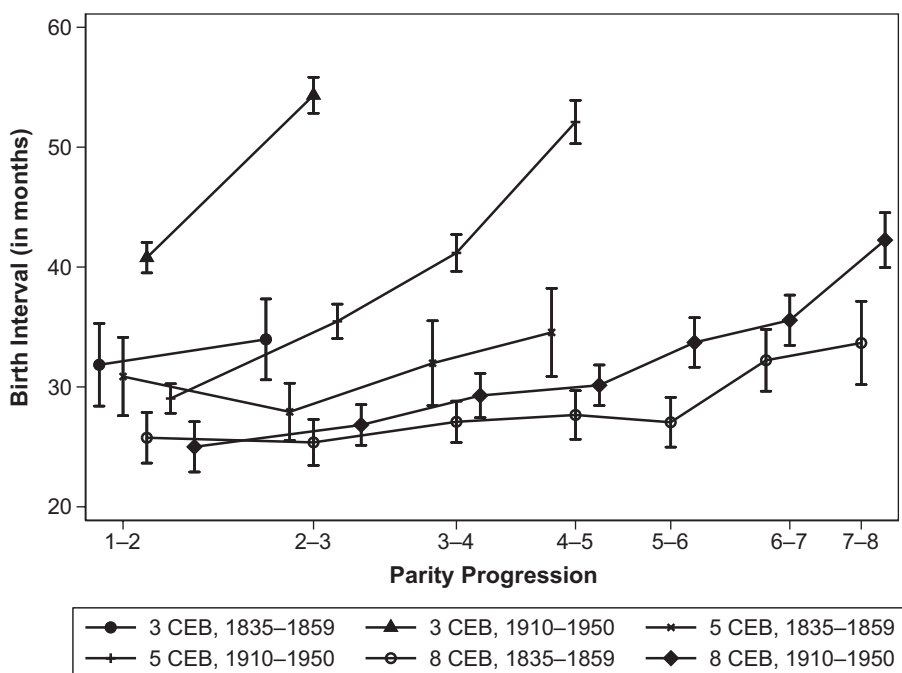


Fig. 3 Birth interval length over parity, by children ever born (CEB) and period. Symbols represent mean interval length for each period and number of children ever born; bars represent standard deviations.

The sample is dominated by women born locally but also consists of a large proportion of unknowns and is most likely skewed toward the local-born. As to be expected given the late arrival of British migrants, the dominant home language of the early cohorts is Afrikaans. This dominance abates somewhat over the century due to increasing migration from Britain.

Simultaneous Determination of Stopping and Spacing

Table 1 and the figures show substantial changes in child numbers, the distribution of children ever born, and interval lengths over time, but they cannot separate spacing, postponement, and stopping behavior during the transition and thus cannot pick up intentional family limitation *before* the transition. For this, we turn to the empirical analysis.

A typical survival model, such as a Cox proportional hazards model, might suffice to evaluate the time until marriage or time until first birth if all the observations in the data experience the event. Such a model is inadequate when evaluating time until subsequent births because not all women have additional births. The problem with a standard survival model is that it amalgamates the speed of progression to the next event with the proportion progressing; it assumes that everyone in the data is at some point subject to the event (Schmidt and Witte 1989). In fertility analysis, this simply is not the case: some women will drop out of the analysis of time to subsequent birth because they stop having children. Furthermore, factors affecting the speed to pro-

gression of a subsequent child may differ from the factors affecting whether a woman does progress to have a subsequent child. One final concern is censoring in the data, whereby some individuals are classified as not experiencing the event because they did not experience it under the period of observation. This misclassification may lead to bias in a Cox model (Li and Choe 1997).

Methodologically, split population models, also known as *cure models* in the biomedical literature, have been used to evaluate stopping and transition rates in demographic research (Alter 2019; Alter et al. 2007; Bremhorst et al. 2016; Gray et al. 2010; Li and Choe, 1997; Yamaguchi and Ferguson 1995). The advantage of these models over other event-history models is that they allow for women to leave the duration estimation if they have stopped having children.

The intuition is that we want to estimate the proportion of the population surviving until some time, t —that is, not having a subsequent birth by t . This proportion includes two groups of women: (1) women who never have another child, termed *stoppers*, with proportion denoted p ; and (2) those who have not yet had another child but will eventually, sometimes termed *movers*.

Mathematically, we write this as

$$S(y) = p(x) + (1 - p(x))S_m(t, y, z), \quad (1)$$

where $p(x)$ is the proportion of stoppers (those who never have another child) estimated from the following logistic regression:

$$L_{i,t} = \ln \frac{p_{i,t}}{1 - p_{i,t}} = \beta X_{i,t} + \varepsilon_{i,t}, \quad (2)$$

where $\ln(p_{i,t}/1 - p_{i,t})$ is the log transformation of the probability that no subsequent child is born after the observed birth, and $\mathbf{X}$ is a vector of explanatory variables.

$S_m(t, y, z)$, the duration model, is the probability that women who do have another birth have not had one by time t . Using the log normal distribution, the survivor function of movers is as follows:

$$S_m(t, y, z) = 1 - \Phi\left(\frac{\ln(t) - \mu}{\sigma}\right) = 1 - \Phi\left(\frac{\ln(t) - \beta_y Y_{i,t}}{\beta_z Z_{i,t}}\right), \quad (3)$$

where $\Phi()$ is the cumulative distribution function of the standard normal distribution, $\ln(t)$ is the log of time until the next birth, and $Y_{i,t}$ and $Z_{i,t}$ are the explanatory variables. Following Yamaguchi and Ferguson (1995), we use an accelerated failure-time regression specification for the duration model rather than a proportional hazards model.

Notice that covariates appear both in our estimate of μ , the mean of the distribution of the log of duration, as well as in the estimate of σ , the standard deviation of the log of duration. We refer to the estimate of the mean as the “scale” parameter, and the estimate of the standard deviation is the “shape” parameter in our results.

The scale parameter measures the mean of a birth interval conditional on the number of existing children. We can think of the mean as measuring the population’s socially accepted time interval between births. If the mean at a particular parity increases, then the society has adopted longer birth intervals at that parity. The scale parameter therefore measures parity-dependent birth spacing.

The shape parameter, in contrast, is an indication of non-parity-specific postponement. Econometrically, it picks up variation in the time to the next child. That is, it measures changes in individual behavior that do not impact the mean. These changes would be decisions to have or not to have another birth because of temporary personal circumstances, such as the state of the local economy or individual income and health reasons. An increase in the variance in the duration to the next child therefore shows only some people making postponement decisions and not a shift for the whole society (Timæus and Moultrie 2008).

It is perhaps elucidatory to reiterate what we mean by parity-dependent spacing and parity-independent postponement. Parity-dependent spacing may be directly related to influencing the final number of children, or it may be related to new preferences for the time between successive children without any desire to limit final numbers. Longer birth intervals due to either one of these explanations are termed *parity-dependent spacing* in that the decision about when to have a child depends on when the last child was born. If there is a population-wide shift in birth interval lengths, then we pick this up as an increase in the mean interval length.

Parity-independent postponement is the decision that at a specific moment in time, having another child is not a good idea because of circumstances unrelated to the age of the last child and with no intention to limit family size ultimately or to change the experienced birth interval length. Of course, the consequence of postponement may well be a reduction in the total number of children, given a woman's constraint on the ability to conceive as she ages. If there is no population-wide change in preferences but there is individual-level postponement, then we see a change in the variance of interval length rather than in the mean. If many in the population postpone, then we should indeed see an increase in the mean length. Because our data come from all over South Africa, there is only one uniform shock—the 1899–1902 Boer War—that may result in postponement showing up as a societal lengthening of birth intervals. In our data, external circumstances resulting in postponement should show up in the shape parameter but not the scale.

Finally, some of those who postpone, and even those who intentionally increase spacing, could quite possibly be seen as having stopped because they will have run out of time to have another child. We discuss how this influences our findings in the Results section.

Our primary measure of interest is a dummy variable for period, given that we explore the relationship between period and the birth interval structure. We include both the supply of children and the demand for them in the explanatory variables to reduce the bias arising from the relationship between these variables and the period. On the supply side, we control for fecundity by accounting for the age at conception of the current birth, given that age is highly associated with fecundity. On the demand side, we include region controls, the husband's occupation, whether the mother was born in South Africa or abroad (Europe), and whether the home language was English or Afrikaans.

Results

Table 2 provides the results of the cure regression discussed in the empirical section. The table is divided into three panels. Panel A shows log odds coefficients for the fraction of women who stop having children, called the *cure fraction*; coefficients

Table 2 Cure model of second and higher-order births, by parity progression

	Parity Progression						
	1→2	2→3	3→4	4→5	5→6	6→7	7→8+
A. Stopping							
Age <25	-3.284*** (-9.81)	-0.435*** (-3.80)	-0.473*** (-4.10)	-0.335* (-2.05)	0	0	0
Age 25-29	-1.677*** (-4.94)	-0.392*** (-3.79)	-0.412*** (-4.65)	-0.668*** (-6.98)	-0.229* (-2.09)	-0.070 (-0.50)	0.148 (0.69)
Age 35-39	0.411 (0.93)	0.382* (2.31)	0.0102 (0.06)	0.430** (3.16)	0.823*** (8.65)	0.807*** (8.24)	0.731*** (6.61)
Age ≥40	0	-13.31 (-0.04)	-14.37 (-0.04)	-15.64 (-0.03)	-16.27 (-0.03)	-15.31 (-0.03)	-15.33 (-0.03)
1860-1884	-0.658* (-2.02)	-0.053 (-0.25)	0.052 (0.28)	-0.348 (-1.76)	0.284 (1.40)	-0.184 (-0.93)	0.149 (0.64)
1885-1909	-0.824** (-2.66)	0.790*** (4.31)	0.357* (2.06)	0.214 (1.25)	0.547** (2.86)	0.331 (1.81)	0.808*** (3.73)
1910-1950	-3.086*** (-4.16)	1.824*** (10.39)	1.749*** (10.93)	1.448*** (8.96)	1.757*** (9.57)	1.301*** (7.38)	1.546*** (7.13)
White collar	-0.685 (-1.33)	0.267* (2.09)	0.353** (2.64)	0.424** (2.84)	0.291 (1.79)	-0.011 (-0.06)	0.0477 (0.22)
Skilled/semiskilled	-0.520 (-0.77)	0.136 (0.70)	0.0343 (0.17)	0.177 (0.80)	0.168 (0.73)	0.050 (0.18)	0.120 (0.39)
Unskilled	-0.222 (-0.30)	-0.569 (-1.32)	-0.061 (-0.19)	-0.057 (-0.18)	-0.446 (-1.27)	0.064 (0.22)	-0.043 (-0.12)
Old Urban	0.076 (0.13)	0.386* (2.04)	-0.224 (-0.90)	0.373 (1.68)	0.997*** (4.76)	-0.705 (-1.28)	0.208 (0.62)
New Urban	1.424** (2.89)	0.527*** (3.55)	0.225 (1.36)	0.526** (2.81)	-0.138 (-0.54)	0.076 (0.29)	0.037 (0.13)
New Rural	0.753* (2.19)	0.009 (0.09)	-0.048 (-0.50)	0.172 (1.64)	0.093 (0.84)	0.152 (1.26)	0.0711 (0.52)

Table 2 (continued)

	Parity Progression						
	1 → 2	2 → 3	3 → 4	4 → 5	5 → 6	6 → 7	7 → 8+
Born abroad	-0.266 (-0.30)	0.730*** (3.79)	0.196 (0.70)	0.639* (2.29)	0.348 (0.96)	0.724 (1.77)	1.130* (2.55)
English	-0.003 (-0.01)	0.112 (1.37)	0.219* (2.55)	0.096 (1.02)	0.179 (1.79)	0.042 (0.36)	0.125 (0.98)
Constant	-1.800*** (-4.46)	-3.329*** (-16.01)	-2.846*** (-15.49)	-2.696*** (-14.90)	-3.144*** (-15.80)	-2.799*** (-14.85)	-3.214*** (-14.06)
B. Spacing							
Age <25	0.447*** (21.29)	0.527*** (24.90)	0.414*** (23.55)	0.435*** (20.16)	0	0	0
Age 25–29	0.197*** (8.87)	0.284*** (13.31)	0.221*** (13.15)	0.202*** (15.26)	0.200*** (14.94)	0.214*** (12.49)	0.201*** (9.28)
Age 35–39	-0.145*** (-3.37)	-0.192*** (-4.11)	-0.354*** (-7.81)	-0.371*** (-9.89)	-0.237*** (-10.94)	-0.202*** (-11.87)	-0.190*** (-11.97)
Age ≥ 40	0	-0.568*** (-12.35)	-0.675*** (-17.58)	-0.737*** (-22.66)	-0.765*** (-22.98)	-0.791*** (-28.20)	-0.813*** (-31.09)
1860–1884	0.0466*** (3.40)	0.028 (1.91)	-0.002 (-0.12)	-0.010 (-0.65)	0.006 (0.35)	-0.007 (-0.34)	0.001 (0.03)
1885–1909	0.037** (2.63)	-0.008 (-0.55)	0.001 (0.05)	0.020 (1.20)	-0.004 (-0.21)	0.015 (0.71)	0.015 (0.65)
1910–1950	-0.055*** (-3.77)	-0.074*** (-4.78)	-0.085*** (-4.98)	-0.031 (-1.71)	-0.026 (-1.17)	0.000 (0.01)	-0.024 (-0.95)
White collar	-0.013 (-0.74)	0.025 (1.31)	0.027 (1.26)	0.006 (0.25)	0.054* (2.10)	0.019 (0.65)	0.008 (0.29)
Skilled/semiskilled	0.046 (1.63)	0.026 (0.96)	0.019 (0.70)	0.056 (1.57)	0.038 (1.25)	0.004 (0.11)	0.040 (1.11)
Unskilled	-0.015 (-0.46)	0.033 (0.75)	0.013 (0.37)	-0.012 (-0.37)	0.002 (0.06)	-0.058 (-1.63)	-0.018 (-0.34)

Table 2 (continued)

	Parity Progression						
	1→2	2→3	3→4	4→5	5→6	6→7	7→8+
Old Urban	0.113*** (4.96)	0.090** (3.22)	0.029 (0.75)	0.034 (0.88)	0.073* (2.07)	0.091 (1.25)	0.211*** (4.40)
New Urban	-0.020 (-0.86)	-0.035 (-1.26)	-0.036 (-1.16)	-0.004 (-0.13)	-0.086* (-2.35)	-0.060 (-1.71)	-0.030 (-0.80)
New Rural	-0.057*** (-4.76)	-0.037** (-2.83)	-0.054*** (-3.84)	-0.024 (-1.59)	-0.030 (-1.72)	-0.008 (-0.42)	-0.019 (-0.99)
Born abroad	0.029 (0.98)	0.135*** (3.79)	0.057 (1.24)	0.090 (1.85)	-0.031 (-0.55)	0.051 (0.42)	0.039 (0.40)
English	0.043*** (3.67)	0.009 (0.78)	0.042** (3.22)	0.034* (2.55)	0.028 (1.81)	0.059** (3.00)	0.039* (2.23)
Constant	-1.068*** (-42.66)	-1.154*** (-46.43)	-1.013*** (-46.02)	-0.958*** (-49.01)	-0.880*** (-40.60)	-0.815*** (-35.89)	-0.738*** (-31.80)
C. Postponement							
Age <25	0.349*** (13.63)	0.416*** (14.89)	0.335*** (12.31)	-0.093** (-2.90)	0	0	0
Age 25-29	0.119*** (4.40)	0.205*** (7.36)	0.155*** (6.30)	0.180*** (8.19)	0.121*** (5.22)	-0.109*** (-3.73)	0.081 (1.86)
Age 35-39	-0.118** (-2.58)	-0.112* (-2.27)	-0.295*** (-6.76)	-0.287*** (-6.70)	-0.154*** (-4.78)	-0.108*** (-3.78)	-0.146*** (-5.20)
Age ≥40	0	-0.195*** (-3.73)	-0.246*** (-5.39)	-0.316*** (-7.77)	-0.440*** (-11.60)	-0.417*** (-11.34)	-0.499*** (-13.95)
1860-1884	-0.009 (-0.36)	-0.044 (-1.72)	-0.035 (-1.31)	-0.068* (-2.40)	0.168*** (5.18)	0.170*** (4.81)	0.161*** (4.18)
1885-1909	-0.105*** (-4.53)	-0.0179 (-0.71)	-0.080** (-3.05)	-0.206*** (-7.18)	0.040 (1.27)	-0.048 (-1.36)	0.108*** (2.78)
1910-1950	-0.125*** (-5.43)	-0.041 (-1.58)	-0.101*** (-3.68)	-0.193*** (-6.45)	-0.061 (-1.81)	-0.064 (-1.67)	0.081 (1.95)

Table 2 (continued)

	Parity Progression						
	1 → 2	2 → 3	3 → 4	4 → 5	5 → 6	6 → 7	7 → 8+
White collar	0.027 (0.98)	-0.016 (-0.52)	-0.025 (-0.73)	-0.033 (-0.88)	-0.008 (-0.19)	0.048 (0.96)	0.104* (2.07)
Skilled/semiskilled	-0.123** (-3.02)	-0.013 (-0.28)	0.133** (2.73)	-0.107* (-1.99)	0.203*** (3.40)	0.238*** (3.62)	0.116 (1.62)
Unskilled	-0.111* (-2.20)	-0.396*** (-7.11)	-0.094 (-1.67)	0.029 (0.50)	0.058 (0.89)	0.215** (3.15)	-0.251** (-3.09)
Old Urban	0.176*** (4.30)	0.010 (0.21)	-0.249*** (-4.80)	-0.138* (-2.43)	0.116 (1.80)	-0.578*** (-6.97)	-0.174* (-2.33)
New Urban	0.056 (1.53)	-0.074 (-1.70)	-0.077 (-1.60)	0.081 (1.44)	0.032 (0.51)	0.139* (2.09)	0.090 (1.22)
New Rural	-0.001 (-0.06)	-0.082*** (-3.88)	-0.063** (-2.78)	-0.011 (-0.42)	-0.040 (-1.45)	-0.087** (-2.80)	-0.077* (-2.22)
Born abroad	0.268*** (4.90)	0.200** (3.09)	0.067 (0.91)	0.105 (1.24)	0.085 (0.87)	-0.552*** (-4.58)	-0.209 (-1.54)
English	-0.039* (-2.30)	0.076*** (3.99)	0.032 (1.53)	0.088*** (3.84)	0.054* (2.06)	0.031 (1.05)	0.076* (2.44)
Constant	0.606*** (17.64)	0.593*** (16.58)	0.751*** (20.87)	0.889*** (26.94)	0.755*** (19.99)	0.837*** (23.06)	0.890*** (21.69)
Number of Observations	13,507	13,981	11,864	9,856	7,899	6,460	5,253

Notes: Age 30–34, 1835–1859, Farmers, Old Rural, Born locally, and Afrikaans are reference categories. “Unknown” categories are estimated but not reported. The length of the final interval is the length of time between the birth of her last child and the time the woman turned 50, her husband died, or she died before age 50. Values in parentheses are *t* statistics.

p* < .05; *p* < .01; ****p* < .001

larger than 0 indicate an increased log odds of *not* having another child given the current birth interval. Panel B shows the coefficients indicating mean length of time until the next birth for the women who continue to have children, indicating *intentional spacing*. In this panel, a positive coefficient suggests an increased hazard of having another birth, which can be thought of as resulting in shorter birth intervals. A negative coefficient suggests a decreased hazard of having another birth—a lengthening of the birth interval (Li and Choe 1997). Finally, panel C shows the variation in the mean length of time until the next birth, or *postponement*; a negative coefficient suggests an increase in the variation in the time between two successive births.

Each of the seven columns in Table 2 represents a move from one parity level to the next. We begin with the age variables (panel A): how old a mother was when the current observed child was conceived. The reference age category is 30–34. The coefficients on ages younger than 30 are almost all negative and significant; women younger than 30 were less likely than women aged 30–34 to stop having children at a given parity level. Women aged 35–39 were more likely to not have another child at parity levels 4 and higher relative to younger women. We find no significant results for women older than 40, most likely because of the low number of observations in that group. These age results are as expected given the decreased likelihood of conception as women age.

We now look at the probability of stopping over time, our main variable of interest. The reference period is 1835–1859. We see no significant differences in stopping for the period 1860–1884 relative to the period before at parities 2 and higher. We begin to see an increase in stopping probabilities relative to the reference group for the 1885–1909 period, and these increase between 1910 and 1950.

Panel B of Table 2 shows mean birth interval length. Relative to those aged 30–34, younger women have shorter intervals, and older women have longer intervals, as expected given a woman's reduced physiological ability to conceive as she ages. Relative to interval lengths between 1835 and 1859, longer mean intervals are evident only beginning in the 1910–1950 period. The evidence suggests that no parity-dependent spacing occurred before the fertility transition and that this behavior began only in the later stages of the transition. In the next section, we argue further why the regressions do not show parity-dependent spacing in the pretransition group.¹⁵

Finally, in panel C, we look at changes in the variation of birth interval lengths that result from postponement. We find a higher variance for older women relative to women aged 30–34, evidence of a decline in fecundity driven by age. We see less variance for

¹⁵ Although our data do not allow a study of the role of infant mortality on birth curtailment and interval length, we offer a brief discussion of what this role might be given the importance of the decline in mortality rates in the European fertility transition (Reher et al. 2017; Reher and Sanz-Gimeno 2007). As noted, we do have many recorded births without corresponding death dates. If some of these are children who died in infancy, then our findings are consistent with longer birth intervals reflecting a decline in infant mortality rates. If children who died in infancy or childhood remain completely unrecorded, then a decrease in the infant mortality rate should be associated with a reduction in birth interval lengths. Between two surviving children, we now have a third survivor who previously would have been unrecorded, thereby reducing the length of the observed interval. Because we don't see a decline in intervals, one of two scenarios is possible: (1) all deceased children are unrecorded, the infant mortality rate is not changing, and fertility declines for other reasons; or (2) some of the children with missing death dates died in childhood, the infant mortality rate may have declined, and the latter is part of the cause of the fertility decline. We think that the second scenario is more likely.

women at parities 5 and higher in the period 1860–1884. Recall that the transition had just begun during this period and therefore that a decrease in family size implies that only the most fecund women were likely to have had children at these higher parities. By nature, their intervals will be shorter and less diverse.

It is quite possible that some who chose to postpone found themselves unintentionally stopping if they waited too long for the next conception. We would not see this behavior in the shape parameters but rather in the cure fraction parameters. In the worst-case scenario, our cure fraction picks up only unintentional stoppers. Converting the stopping coefficients in panel A of [Table 2](#) to parity progression ratios allows a deeper investigation of this problem. We plot the parity progression ratios predicted by the model in [Table 2](#) in Figure A1 (online appendix). The model predicts increased stopping behavior in the final period relative to the first three. The increasingly concave curve over successive periods, particularly the last two, suggests intentional stopping. Progression to the third and fourth births drops more rapidly than progression to higher-order births (Brass and Juarez 1983; Brass et al. 1997; Timæus and Moultrie 2020). Given age at first birth, unintentional stopping is more likely to occur at higher parities, and the stopping we see at lower parities is more likely to be made up of intentional stoppers. If stopping were dominated by endless postponers, the parity progression ratios would exhibit a more linear decline.

As noted, [Table 2](#) does not report interactions of period of birth with socioeconomic characteristics. We provide these results in Table A1 in the online appendix. The results with the interactions are weaker as a result of smaller numbers of observations in each category. We still see a higher probability of stopping for the last period, but the coefficients in the third period are insignificant (although still positive).

Looking Deeper Into Parity Dependent Control

The regression results in [Table 2](#) and the online appendix tables do not bring to light any parity-dependent control before the fertility transition. We agree with Timæus and Moultrie (2013), Cinnirella et al. (2017), and Clark and Cummins (2019) that individual- and couple-level heterogeneity may be masking parity-dependent fertility control and that we need to consider this.

It is theoretically possible that our reference group, births in the period 1835–1859, exhibits parity-dependent spacing behavior and that subsequent periods exhibit precisely the same kind of parity-dependent spacing behavior. The spacing results we find are not significant at parities higher than the transition from parity 3 to 4. Thus, if parity-dependent spacing occurred at higher parities, it would not have changed over time even though the number of children ever born decreased over time. Because we can estimate stopping-, spacing-, and postponement-induced variation simultaneously, when we look at spacing and postponement at higher parities, we are really looking at only those people who reached those parities. Over time, the higher parities will be increasingly dominated by the highly fecund as more people stopped having children earlier—and yet we see no change in spacing. Is that because the highly fecund who would have had narrower intervals in the pretransition period began increasing their spacing in a way that kept the interval length exactly the same as when there was more heterogeneity at higher parities? That would be remarkable: it would suggest a degree of spacing that would not allow women to reach high parities in the first place.

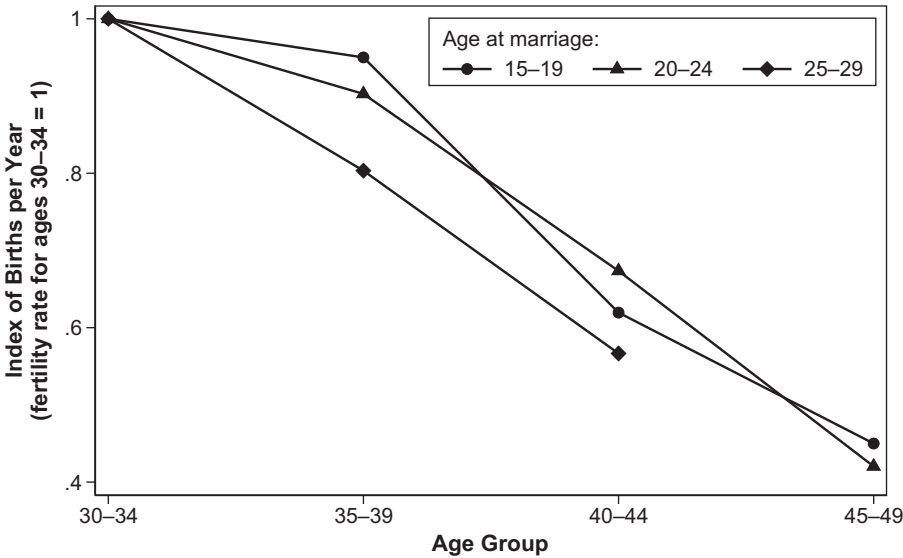


Fig. 4 Fertility by age group and age at marriage, 1835–1859

What we do see is a reduction in variance induced through postponement for the period 1860–1884 at higher parities. This reduction in variance is suggestive of more consistency in birth interval length, as one might expect at parities dominated by the highly fertile. This finding is supply-side evidence of fecundity and not demand-side evidence based on parity.

Nevertheless, we investigate this finding further. Following Clark and Cummins (2019), we first look at whether women who married younger and began having children earlier experienced faster fertility declines than those who married later, as parity-dependent control would suggest. We look at women who married at ages 15–19, 20–24, and 25–29. We set their fertility rates at ages 30–34 equal to 1 and plot their subsequent average fertility indexes at ages 35–39, 40–44, and 45–49. If there is parity-dependent fertility control, we should see a steeper decline in the fertility rates of those who married earlier. Figure 4 plots the fertility rates for the three marriage ages at four age groups for the period 1835–1859. We do not find any pattern in the fertility rate decrease. If anything, women marrying later had the fastest fertility decrease. We conduct this exercise for each of the four periods and find no evidence of parity-dependent fertility control until the last period, when the rate of fertility decline matches the hypothesis (Figures A1–A3, online appendix). This finding is as expected, given that this period is situated in the fertility transition.

We next explore the change in fertility by age group for all women who married at ages 20–24. We look at the birth rates of these women in the early stages of marriage (at ages 25–29) and split them into three groups: parity 1, parity 2, and parity 3 or higher. We assume that those with parity 1 are the least fecund and that those with parity 3 or higher are the most fecund. If there is parity-dependent control, then we would expect to see those with parity 3 or higher at ages 25–29 exhibit a faster rate of fertility decrease from ages 30 to 49. Again, we set the fertility rate at ages 30–34 equal to 1 and calculate subsequent fertility indexes relative to that. Figure 5 shows

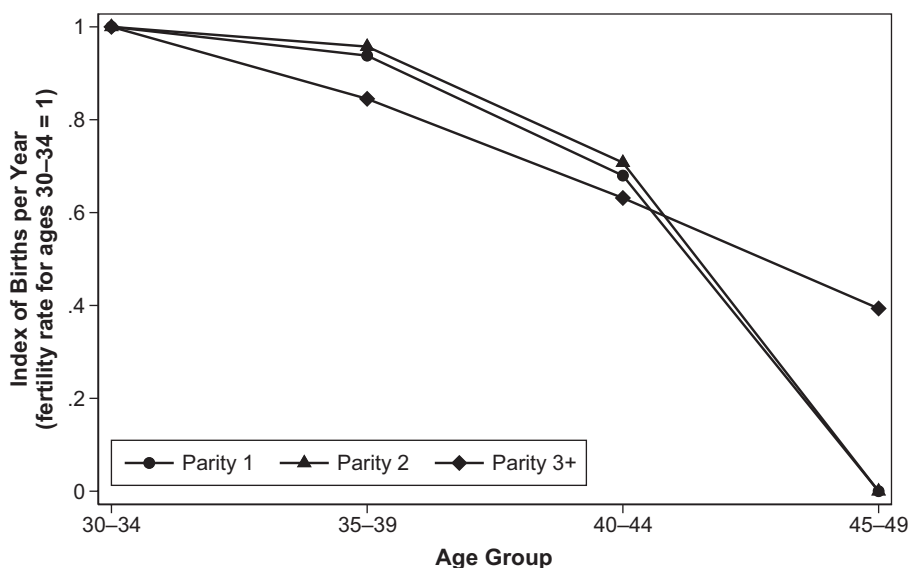


Fig. 5 Fertility by age group and parity at age 25, 1835–1859

very similar rates of fertility decrease for the three parity groups. This finding raises concern about the role of the least fecund. Within this group are women/couples who do not conceive easily. Because their fertility rate would be expected to be lower at all ages, they too would bias the finding of parity-dependent spacing. Indeed, they bias the result toward a finding of parity-dependent spacing. Ultimately, which scenario dominates is hard to say. Once the transition had begun, we again see the expected ordering of the fertility rate decline (Figures A4–A6, online appendix).

This set of evidence should assuage concerns regarding the role of individual-level heterogeneity in masking the evidence of parity-specific control. These data show no evidence of parity-specific spacing before the transition.

Conclusion

This study examines the fertility experience of settler women in nineteenth-century South Africa. The period we study, 1835–1950, covers the pretransition period, the transition period, and an overall period of extensive geographic, cultural, economic, and social change. Our data, which comprise women's complete birth histories, are well suited to event-history analysis techniques, and we use mixed population models to allow for both ongoing procreation and the cessation of childbearing. We look at the mean and variance of the length of time between consecutive births and the likelihood of stopping after a certain number of children have been born.

Our estimation shows that a woman's own physiology and fecundity are the main determinants of both when a woman stopped having children and birth interval lengths before the transition. We do not find evidence of explicit parity-dependent control for either stopping or spacing, although we do see some evidence of variation

in birth interval lengths driven by postponement. During the transition, we first see an increase in both stopping behavior and variation in birth interval length driven by postponement, followed in the last period by an increase in spacing.

Our findings show a role for both stopping and spacing in the fertility transition, finding common cause with the literature advocating for family limitation (Knodel 1987) as well as birth interval timing (Dribe et al. 2017). The results show that an empirical examination of a representative data set can shed light on reproductive behavior and compensate to some extent for the lack of direct evidence on people's reproductive intentions. Our methodology allows us to show that couples were able to limit their childbearing toward the end of the nineteenth century. That these couples did limit their childbearing shows that their society perceived the benefits of smaller families, had evolved away from the conservative large-family model, and knew how to implement its preferences. ■

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