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Legacies of loss: The intergenerational outcomes of slaveholder compensation in the British Cape Colony*

Igor Martins[†], Jeanne Cilliers[‡], and Johan Fourie[§]

Abstract

Can wealth shocks have intergenerational health consequences? We use the partial compensation slaveholders received after the 1834 slave emancipation in the British Cape Colony to measure the intergenerational effects of a wealth loss on longevity. Because the share of partial compensation received was uncorrelated to wealth, we can interpret the results as having a causal influence. We find that a greater loss of slave wealth shortened the lifespans of the generation of slaveholders that experienced the shock and those of their children, but not those of their grandchildren. We speculate on the mechanisms for this intergenerational persistence.

Keywords. intergenerational health, intergenerational persistence, wealth shock, lifespan, longevity, slave emancipation, Cape Colony

JEL code. D6, I19, J47, N37, N47, N97

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1 Introduction

Can wealth shocks have intergenerational health consequences? For a slaveholder in the Cape Colony, could the loss of his slaves after emancipation, with only partial compensation, damage his health so badly as to shorten his life and the lives of his children and perhaps even those of his grandchildren?

We know that health and wealth are positively correlated across a range of dimensions (Smith 1999; Deaton 2003; Cutler et al. 2008), but the causal mechanisms remain unclear. This is because health and wealth are inextricably bound up with each other, as Cutler et al. (2008, p. 36) cryptically note: "Some dimensions of socioeconomic status cause health, some are caused by health, and some are mutually determined with health; some fall into all three categories at once".

The question of how later-life outcomes respond to the gain or loss of wealth and how persistent these effects are over time remains open. Experimentation is understandably difficult. Randomizing the amount of wealth people receive to test its effects on health would plainly be unethical. Researchers have therefore resorted to quasi-experimental designs, most commonly involving lottery winnings (Lindahl 2005; Apouey and Clark 2015; Cesarini et al. 2016), stock or housing price fluctuations (Boen and Yang 2016; Fichera and Gathergood 2016; Engelberg and Parsons 2016; Schwandt 2018) and policy changes (Duflo 2000; Case 2004; Frijters et al. 2005; Snyder and Evans 2006; Erixson 2017). Most of this literature deals with shocks to wealth through price fluctuations or cash windfalls. Few studies deal with shocks in the form of wealth losses (González et al. 2017) and even fewer with the relationship between wealth shocks and health in historical settings (Bleakley and Ferrie 2016).

To attempt an answer, we turned to the 1834 emancipation of slaves in the Cape Colony, an event whose social and historical significance for the marginalized populations of the British Empire of course cannot be overstated. But an aspect of particular interest to economic historians is the cash compensation paid to slave owners. The records show that they received, on average, between 40 and 50% of the value of their slaves. From an economic standpoint, therefore, the emancipation represented the loss of wealth or, put differently, the loss of an asset. We were interested in identifying any health-related outcomes of this loss, and we found the means to do this by linking data from three different datasets, containing compensations claims, tax returns, and genealogical records.

We began by digitizing the claims records kept in the Cape Town Archives. These contain details of 8,452 slaves living in the Stellenbosch district of the British Cape Colony at the southern tip of Africa. The records include, most importantly for us, the value assigned to each slave. We combined this information with the compensation paid to each slaveholder (available from the UCL Online Archive), which is generally well below the aggregated value of the slaves owned. We claim that the differences in the percentages of compensation paid are random, and we support this with extensive archival evidence. We then used the average uncompensated value, i.e. shortfall, per slave, our variable of interest, to measure the shock to the former slaveholders. To do this, we linked the average shortfall per slave with the 1834 tax returns, which contain the slaveholders' details. Since our study aimed to verify the intergenerational effects of property losses, we also linked the compensation claim and tax return datasets to information on settler families between 1652 and 1910 from the South African Families database. As far as we are aware, ours is the first study to investigate the intergenerational effects of wealth loss on an individual's lifespan.

We believe that using an individual's lifespan as a proxy for health offers some advantages. Firstly, longer lifespans can be directly interpreted as improvements in health. Indeed, life expectancy is one of the key indicators in the Human Development Index. Secondly, years of life are methodologically constant over time and space, which allows for better comparability between individuals from different generations or countries.[1] If variations in wealth can causally explain variations in later-life outcomes, we speculate that the differences between the slaveholders' compensation shares – provided that they are exogenous – might be the reason for the observable differences between their lifespans.

Our estimation strategy consisted of a Poisson regression, a generalized linear model that allowed us to fit the distribution of lifespan more effectively. We estimated the effects of the average shortfall per slave on the lifespan of individuals who were slaveholders in 1834 (the first generation) and extended the analysis to their children (the second generation) and their grandchildren (the third generation). We found the shortfall to be negative and significant for the first generation, which implies that slaveholders who received a greater proportion of compensation were likely to live longer.

We found that the effects we verified in the first generation were passed on to the second generation, but only if we included infant mortality. When we excluded this from the base estimates, we could not verify any statistically significant effects of the wealth shock on the lifespans of the second generation. In other words, only the survival of infants was influenced by the shock; the lifespans of those of the second generation who survived infancy were unaffected by the shortfalls. By the third generation, the effects of the shock seemed to have completely disappeared.

The economic significance of the shortfalls was systematically smaller than the economic significance of any of the relevant demographic covariates we could control for. This is in line with the literature that suggests the effects of exogenous shocks on later-life outcomes are marginal, despite their statistical significance (Frijters et al. 2005; Cesarini et al. 2016; Erixson 2017). The same literature, however, suggests that the effects are mostly observable only in the short and medium run. Our finding of intergenerational persistence offers some challenge to that claim. This contradictory finding is perhaps the result of the nature

[1] The use of lifespan as a proxy for other later-life outcomes, such as occupational mobility, has been acknowledged in the literature. Two examples are Piraino et al. (2014) and Parman (2016).

of the shock we investigated: a wealth loss rather than a gain. We suspect that the persistent effects of this loss might point to the mechanisms at play. This paper explores that possibility.

2 Literature Review

Quasi-experimental approaches to analyzing the effects of wealth on health are of three kinds: lottery winnings, price or asset fluctuation, and policy changes. Using lottery winnings as a source of a change in wealth, Lindahl (2005) found that the gain was associated with a range of health-related outcomes, such as a reduction in self-reported illnesses, lower levels of obesity, and increased life expectancy. This suggests that health-related consumption is one of the mechanisms through which changes in wealth causally explain changes in health. However, in testing the relationship between increased income and mortality, he found that income did not provide more protection against bad health for older people. In a similar study, Apouey and Clark (2015) used lottery winners linked to the British Household Panel Survey to measure the effects of wealth gain on people's general health, mental health, physical health, and health behavior. They found significant effects on mental health but none on self-assessed overall health, which is similar to the findings of Gardner and Oswald (2007), who used a similar dataset and context. Apouey and Clark (2015) showed, however, that winners of lottery prizes bigger than £500 were more likely to engage in unhealthy behavior such as social drinking and smoking.[2] They conclude that this explains their finding that the wealth gain had an insignificant effect on overall health. In a study of the effects of wealth shocks on the consumption of healthy and unhealthy goods, Van Kippersluis and Galama (2014) found that positive wealth shocks caused people to relax their budget constraints, increasing both healthy and unhealthy consumption, but with a larger increase in the former, since unhealthy consumption has intrinsic health costs.

Cesarini et al. (2016) used a considerably larger dataset of lottery players in Sweden, covering a wide range of socioeconomic strata and with the addition of intergenerational information. They found that the effect of the wealth shock on mortality and health care utilization was close to zero. For the players' children, they estimated effects of a similar magnitude on scholastic achievement, and on cognitive and non-cognitive skills. The overall pattern, therefore, was of null results despite some exceptions.[3] They conclude that any claims of a causal relation between wealth and health should be viewed skeptically. Indeed, there is considerable literature suggesting that the causal link on the wealth-health gradient in developed countries is weak (Deaton 2002; Cutler et al. 2008; Chandra and Vogl 2010).

[2]This result holds even when controlling for general health, which the authors obtained from the General Health Questionnaire, a survey consisting of 12 questions with responses rated on a 0 to 3 scale. The result is the sum of the scores, with 36 indicating the best psychological health and 0 the worst.

[3]One of their statistically significant findings shows that wealth increases children's hospitalization risk and reduces obesity.

One of the shortcomings of using lottery winnings as a source for exogenous changes in wealth is that we can observe only positive shocks. Price fluctuations, in contrast, can cause both positive and negative shocks, so studies using housing or stock markets allow researchers to measure the effects of both. Fichera and Gathergood (2016) used large changes in the UK housing market to examine the impact of wealth gains and losses on health. The home ownership rate in the UK is around 64%, covering a large, but not necessarily representative, section of the population. They found that the effect of wealth on overall health was positive, significant and persistent over a 10-year period, but they found no significant effects on psychological health. They found that health is more responsive to price gains than to losses, even though it is unclear whether these effects were homogeneous across age cohorts since the authors do not test it. The reason why negative shocks do not have much psychological impact may be because a house is an asset whose value should increase over time despite short-run fluctuations, so homeowners tend to take a long-term view.

Schwandt (2018) findings from the stock market contradict findings from the housing market. Looking at boom and bust in the US stock market, he found that a 10% wealth loss had an effect of 2 to 3% of a standard deviation on both physical and psychological health. This support earlier studies using stock market changes as a proxy for wealth shocks (Cotti et al. 2015; Engelberg and Parsons 2016; Boen and Yang 2016). The differences between these findings and those of Fichera and Gathergood (2016) show how much the context (UK or US) and the type of shock matter. While both housing and stock markets produce positive and negative shocks, people's expectations about ownership of these assets differ substantially. The aforementioned findings support what Chandra and Vogl (2010, p. 1228) conclude on summing up the literature: "no universal rule governs the relationship between income and adult health".

We found similarly mixed results in studies that used policy changes as quasi experiments to model exogenous shocks to a person's wealth or income. A study of a change in pension policy by Snyder and Evans (2006) produced counter-intuitive results. A 1970s legislation change in the US created a 'notch' on the social security system: people born before 1 January 1917 received higher pension payments than those born after that date. Using this 'notch', i.e. a discontinuity, to examine the effect of incomes on pensioner mortality, they found that those born after the cut-off date, who received the lower pensions, had significantly lower mortality rates than those born before that date. They attribute this finding to the fact that these younger cohorts were more likely to continue working than to retire, which suggests that employment may have health benefits for the elderly. Case (2004), however, in a different context, found the opposite. She used cross-sectional data on the self-reported health status of South African pensioners. The pension, on average, more than doubled the incomes of elderly black South Africans in 1993.[4] She found that

[4] The state old-age pension in South Africa was originally designed for a small number of white South Africans who reached their retirement age without an employment-based pension. During the dismantling of apartheid, payments were equalized across all racial groups and full parity was achieved in 1993. This gave elderly blacks access to a resource previously unavailable to them.

the increased income improved the health of all members of income-pooled households where at least one person received the pension. Her findings corroborate similar findings by Duflo (2000) in the same context.

In another context, Frijters et al. (2005) investigated the effect of income changes on East and West Germans' health satisfaction after reunification, when the unanticipated fall of the Berlin Wall resulted in large wealth transfers for almost the entire population of East Germans. Comparing a treatment group of East Germans to a control group of West Germans to see if health outcomes were affected by the transfers, they found small but statistically significant effects on health satisfaction among the East Germans. Along similar lines, Erixson (2017) investigated the effect of a change in people's wealth when Swedish law exempted inheritances received after 17 December 2004 from tax. Correlating this discontinuity with hospitalization rates, the use of sick leave benefits, and mortality, he found that increased wealth had positive short and medium term, but not long term, effects on adult health.

The literature reveals a great variety of wealth shock measurements. Several studies say that findings of causal links to explain the wealth-health gradient should be viewed with skepticism (Frijters et al. 2005; Snyder and Evans 2006; Cesarini et al. 2016; Erixson 2017), while others draw conclusions that suggest these links do exist (Duflo 2000; Case 2004; Lindahl 2005; Fichera and Gathergood 2016; Schwandt 2018). The long-run, intergenerational effect of wealth shocks remains unclear, however, because measuring such effects over more than one life-time requires exceptionally rich data.

Bleakley and Ferrie (2016) is the exception. Their wealth shock example is the Cherokee Land Lottery in the US state of Georgia in 1832, in which virtually every adult male participated and large tracts of land were distributed to winners. They linked intergenerational information to the dataset of winners, but unfortunately did not measure health-related outcomes. They did, however, measure the later-life fertility of winners, and the literacy, wealth and income of their children and grandchildren, as a measure of human capital, and found that winners had slightly more children than non-winners, but were not more likely to send them to school. They found that the outcomes of the lottery had no significant effect on the two succeeding generations' literacy, wealth or income.

Because Bleakley and Ferrie (2016) used property as their wealth shock, and because people in the 19th century tended to depend heavily on this asset for their livelihood, we could expect transmission mechanisms from wealth to health to be visible and strong. Yet they found that the wealth gains produced only relatively small changes in fertility, which might be considered to be connected with health, and no significant intergenerational outcomes. Should we expect to find the same effects in studies of a *loss* of wealth?

González et al. (2017) seemed a good starting point. They analyzed the 1864 emancipation of slaves in the US, looking for possible links between the loss of slave-wealth and the likelihood of slaveholders starting a business in the post-emancipation period. They conclude that the possession of slave-wealth causally explains variations in the likelihood of starting a business, which was lower after 1864, suggesting that slave-wealth was a better collateral for credit than any additional income that wage labor could have yielded in that period. They did not investigate any later-life outcomes *per se*, but their study is the first, to our knowledge, that explores the causal links between the loss of an asset and people's subsequent behavior.

It was clear to us that there was a gap to fill. We thus chose to use the wealth loss suffered by slaveholders after the 1834 emancipation in the British Cape Colony to measure the effects on their lifespan and those of their offspring up to two generations.

3 Historical Background

Compensation paid to slave owners for the expropriation of their capital was not unique to the British Empire, and the compensation was not homogeneous across all British colonies. We therefore provide a brief history of abolitionism in the UK, followed by its ramifications in the Cape Colony.

3.1 Abolitionism in the United Kingdom

The case Somerset v. Stewart in 1772[5], often taken to signal the end of slavery in Britain, shifted the political momentum irreversibly in favor of emancipation and influenced abolitionist movements in several parts of the British Empire (Drescher 1987; Davis 1999; Carey et al. 2004). Shortly thereafter, in 1787, a group of twelve abolitionists created the Society for the Abolition of the Slave Trade, to raise public awareness of the horrific treatment of slaves by slave traders and holders and pressure the British Parliament to take a stand on the issue. The campaign was successful and the Slave Trade Act of 1807 finally outlawed slave trade within the British Empire.[6]

The emancipation of slaves was the next item on the abolitionists' agenda. But there was division in the ranks of the Anti-Slavery Society, the most prominent movement working towards this goal in the UK, as to how emancipation should take place. One group advocated a gradual process, through the ratification of amelioration laws, and the other wanted immediate action. The former approach was adopted, as evidenced by the numerous ordinances that appeared in some British overseas territories allowing slaves to get married, prohibiting the separation of married slaves by sale, preventing children under 10 from being separated from their parents, restricting corporal punishment, regulating the number of working hours, among other amelioration requirements (Dooling 2007; Spence 2014).

[5] For more information about the case, see Blumrosen and Blumrosen (2006) and Williams (2007).

[6] See Farrell (2007) for a comprehensive discussion on the parliamentary struggle behind the Act's approval.

Some authors argue that the amelioration program was in part responsible for the increase in the slave uprisings in the period after 1807, suggesting that the enslaved population saw the 1807 Act as a sign that freedom was within reach (Holt 1992; Dunkley 2012). Vernal (2011), for example, suggests that slaves in South Africa had incorporated into their own expectations and perceptions the discourse of freedom and universal rights which ultimately transformed the interaction between slaves and masters. This view is corroborated by Spence (2014, p. 238) who claims that the opportunities for slaves to organize resistance were increasing by the nineteenth century. In practice, however, very little changed for the bulk of the enslaved population. It became clear to abolitionists that a gradual emancipation process was, if anything, merely "ameliorating the circumstances of servitude" (Engerman 2008, p. 383), since slaveholders could easily avoid the enforcement of ordinances while using amelioration laws only to delay real emancipation (Lambert 2005). Coupland (1933, p. 130) notes that "virtually nothing had been done by way of 'amelioration' except in three or four of the lesser islands with small slave-populations".

The perceived inefficiency of the amelioration program prompted many moderate abolitionists who had been pushing for a gradual reform to declare support for an immediate process of emancipation. This, together with the poor economic performance of the British West Indies in the 1820s, created the political momentum that abolitionists had envisaged. It also gave free trade movements a chance to disrupt the West Indies' sugar monopoly by destabilizing its core mode of production.[7] The confluence of these political forces led to the Slavery Abolition Act of 1833, which came into effect on 1 August 1834 (Williams 2014).

3.2 Cash compensations as a wealth shock in the Cape Colony

The 1833 Act determined how emancipation would come into effect and established an 'apprenticeship' period of six years together with a financial compensation for slaveholders.[8] The compensation, as defined by Fogel and Engerman (1974, p. 401), was "philanthropy at bargain prices" since slaveholders saw slaves' freedom as "a commodity they were prepared to purchase only if it could be obtained at a very moderate cost".

For farmers in the Cape Colony, the future was uncertain. Hengherr (1953, p. 37) notes that "until the Abolition Act was published, the inhabitants were uncertain whether any amends at all would be made for the loss of capital or even what Britain's plans were for changing the status of the slaves". Cape Colony slaveholders, unlike their counterparts in the Caribbean, were mostly small landowners owning only a few

[7] According to Williams (2007), the abolition of slave trade in 1807, slavery in 1834, and the sugar monopoly in 1846 are inseparable events that constituted a systematic attack on the British West Indies operation. Engerman (1986) provides an interesting discussion on the moral, social and economic aspects of emancipation.

[8] The emancipation scheme was not identical for every British colony. In Bermuda and Antigua, for example, emancipation was granted immediately. In India, slavery was deemed a local tradition and was not abolished until 1843.

slaves. Of the more than 700,000 slaves who were emancipated in 1834, fewer than 40,000 were in the Cape Colony. Most of the slaveholders in the Colony, and certainly those in the Stellenbosch district, were of Dutch origin and had few, if any, connections in London where political developments could be observed.

Parliament decided that the slaveholders would be entitled to half of Britain's annual budget in 1835, which amounted to 20 million pounds. The money was distributed among the colonies in proportion to the value of the enslaved populations. In the Cape, the entire process was conducted by the Office of Commissioners of Compensation. On 2 April 1835 the OCC released the general rules of the compensation scheme.[9] Two forms had to be completed for each slaveholder, the Slave Returns and the Form of Claim.

The Slave Returns assessed the Colony's slave-wealth.[10] The OCC assigned appraisers to cover the colonial territory and determine the value of all the slaves. The Slave Returns classified slaves according to sex and occupation. The occupations were divided into two categories: 'predial', i.e. employed in agriculture, and 'non-predial'. Each category had sub-classifications according to the task performed by the slave.[11] The value of the slaves was reached using prices from public and private sales between 1823 and 1830 and a Slave Return was produced for each slaveholder. In total, more than 38,000 slaves were valued and the total slave-wealth in the Colony was estimated at £2,800,000 (Hengherr 1953; Meltzer 1989; Dooling 2007).

The Form of Claim was a simple form in which the slaveholder identified himself and declared the number of slaves he or she owned at the time. This form was cross-checked against the Slave Returns and if the OCC deemed the information to be correct, the claimant had the right to be compensated.

When this information was collected by the OCC, the Cape slaveholders hoped their slave-wealth would be fully compensated. Their concerns can be understood if we consider the importance of slaves as part of their total wealth. No precise estimate exists, but an analysis by Fourie (2013b) of more than 2,500 probate inventories in combination with auction rolls for the first half of the 18th century found that slaves represented 24% of the total wealth auctioned.[12] As not all the individuals whose records Fourie analyzed were slaveholders, 24% can be interpreted as a conservative estimate.

The compensation values, therefore, were a big concern for the slaveholders. In 1834 Jacob Wouter du Preez, a farmer from the Swellendam district, wrote to Benjamin D'Urban, at the time governor and commander in chief of the Cape Colony, to relate how a "succession of misfortunes" had induced him to give

[9] Cape Archives General Dispatches GH 1-105, General rules drawn up and framed by the commissioners of compensation in pursuance of the 47th & 55th clauses of the act 3 & 4 Wm.IV., Ch, 73, for the colonies of the Cape of the Good Hope and Mauritius. 2 April 1835.

[10] The sum of the value of all slaves in the Colony.

[11] Predial slaves were classified as 'Head People', 'Tradesmen', 'Inferior Tradesmen', 'Field Laborers', and 'Inferior Field Laborers'. Non-predial slaves were classified as 'Head Tradesmen', 'Inferior Tradesmen', 'Head People employed on Wharfs, Shipping, or other Avocations', 'Inferior People employed on Wharfs, Shipping, or other Avocations', 'Head Domestic Servants', and 'Inferior Domestics'.

[12] Probate inventories are detailed lists of settlers' household assets at death.

over his estate for sequestration.[13] His property consisted of "18 valuable slaves, who if disposed of under the present crisis" would not only hurt his creditors but also himself.

Added to the slaveholders' worries was uncertainty as to when the payments would be made. Du Preez hoped that the compensation money would enable him to settle some of his most pressing debts, but neither he nor his creditors knew when compensation would arrive. He begged to be informed "whether the compensation is to be paid in December next, immediately upon the enfranchisement of this slaves and if not, whether Your Excellency cannot then inform the memorialist when the payment will take place, which will entail him to make arrangements with his creditors both for their benefit and his ones". Du Preez did not elaborate in his letter about the 'succession of misfortunes' that caused him such financial distress; however, we can speculate that he had mortgaged some, or even all, of his slaves and expected to settle his debts through agricultural surpluses produced by the slaves themselves.

The practice of mortgaging slaves was well incorporated into the Cape's slave economy (Dooling 2007; Green 2014; Swanepoel 2017). In April 1834, for example, a letter signed by more than 260 former slaveholders addressed to the governor of the colony requested an advance of £400,000 worth of compensation money to settle outstanding mortgages where slaves and their labor had been used as collateral.[14] The request was denied.

It was not until 1835 that the apportionment of the compensation was completed. Britain made a provision for £1,247,000 to be paid to Cape slaveholders, less than half of the slave-wealth that had been estimated. Furthermore, the claims were calculated solely on the basis of the sex and occupation of the slaves, meaning that slaves within the same category were considered homogeneous and interchangeable (Draper 2008). This process created an arbitrary gap between individual's slave-wealth and the compensation awarded. Slaveholders who had different slave-wealth despite having the same number of slaves were eligible to the same compensation if their slaves were classified as having the same occupation.

To add to the perceived injustice, the compensation could only be claimed in London. This was directly contrary to the claimants' expectations – they had hoped the compensation would be remitted directly to the Cape Colony. The general feeling towards the compensation scheme was negative and former slaveholders used all the means available to criticize the system, saying it was the most "signally unjust, as well as offensively arbitrary, proceedings we ever heard of" and "a transaction discreditable to any government laying claim to fair and honest dealing with the public creditor".[15]

The process of repayment was also fraught with difficulty. Several payment delays – some claims were only settled as late as 1845 – added to the atmosphere of uncertainty. Around £250,000 worth of claims were

[13] Cape Archives Memorials, vol. 6, CO 3973. Letter from J.W. du Preez to Benjamin D'Urban. 6 October 1834.

[14] Cape Archives Memorials, vol. 7, CO 3974. Letter from slaveholders to Benjamin D'Urban. 7 April 1834

[15] Grahamstown Journal, January 19th, 1837 as quoted in Hengherr (1953).

later contested and, despite limited success, it suggests that the evaluation and subsequent compensation process was far from straightforward (Hengherr 1953).

Considering that slaves were at the heart of the productive activity in the British Cape Colony and their role went beyond their employment in agricultural production – for example, they were also used as collateral for loans (Swanepoel 2017), as leasing assets (Green 2014), as domestic servants (Fourie 2013b) and as semi-skilled and skilled staff on farms (Fourie 2013a; Green 2014) – it is not surprising that the period immediately following emancipation was characterized by uncertain labor relations and production activity.

We use the average shortfall per slave as our variable of interest firstly because slaveholders until the very onset of abolition were unsure about the compensation scheme and secondly because the difference between the evaluation of the their slave assets and the amount received in London was random. This is not to say, however, that either the assessment of the slave-wealth or the compensation awarded was randomly generated. They were not. But the difference between these two variables is random because considerably different criteria were used to quantify them. While the compensation was based solely on the sex and occupation of the slave, slave-wealth was based on market prices that considered a wider range of characteristics such as age, place of origin, height and weight, besides, of course, sex and occupation. Anecdotal accounts presented above support our claim; so, too, does our empirical analysis – we show that the difference between the value and the amount received was not correlated to observable characteristics.

4 Data

The data for this study came from the three sources that we linked manually to produce a unique dataset from which all our estimates derive: the valuation records matched to the compensation amounts, the tax records (also known as *opgaafrollen*), and the South African Families Database (SAF) genealogical records.

The slave valuation and compensation records are in the Cape Town Archives.[16] They contain information on 8,452 slaves who lived in the Stellenbosch district, together with their names, gender, age, place of birth, owner and, importantly for us, their value. Some basic genealogical information about the slaveholder is also available.[17] The slaves were distributed among 989 slaveholders. Because the compensation scheme was a function of slaves' characteristics and not just the total number of slaves owned, we worked with the average shortfall per slave as a measure of the magnitude of the shock to the slaveholders' wealth.

After identifying the slaveholders and their respective slave-wealth and compensation, we matched this

[16] We transcribed the lists of compensation claims for slaves from the original sources, although copies of these original lists are now available on the LDS FamilySearch website.

[17] Despite its paucity, this information was vital for us to link these individuals to the South African Families database where more complete genealogical information can be found. The compensation records contain the slaveholder's first and last name and the name of his father. In some cases, his wife's name is also provided.

information to 1,244 individuals who were registered in the tax returns of the Stellenbosch district in 1834. The tax returns, collected annually by the British colonial authorities, contain information about each resident's livestock, agricultural output, related capital and taxation. To make the matches we used the slaveholders' first and last names.[18] We classified four types of match or apparent match – perfect, semi-perfect, weak, and impossible – and used only those in the first two categories.[19] This procedure yielded 551 unique observations.

Our third source of data was the South African Families database (SAF), which contains records of all settler families in the Cape Colony between 1652 and 1910. This allowed us to append information about each slaveholder's year of birth, year of death, number of siblings, rank among siblings, gender and lifespan of parents. Each individual in this dataset has a unique identity that can be linked to the identity of his or her relatives. This, then allowed us to link slaveholders in 1834 to their children and grandchildren, referred to here as the 1st, 2nd and 3rd generations.

The 551 individuals that we linked between the compensation records and the tax returns, therefore, belong to our 1st generation. Among these 551 slaveholders, we were able to match 314 to the SAF records. This group, when linked to their offspring, yielded 1,814 children (2nd generation) and 2,458 grandchildren (3rd generation) and made up our remaining populations of interest or, put differently, the treatment group.[20]

Historical records, however, often lack complete and consistent micro-level information. Samples for analysis therefore tend to be considerably smaller than the population of interest. In our study, the large number of missing values for both birth and death year made it difficult to assess people's lifespans. Our sample size, therefore, was limited by the availability of data for this specific variable. Table 1 presents the descriptive statistics of the analytical sample for our treatment group.[21]

[18] The matching process was done manually. See Appendix F for details of our strategy.

[19] Perfect matches were those where the combination of first and last names was unique and exactly matched between the compensation records and the tax returns. Semi-perfect matches were similar, but we verified minor spelling differences in last names (e.g. Rous-Roux, Liebentrau-Liebentrouw, Bergh-Berg). Weak matches were those where the combination of first and last names was not unique and, lacking additional information, we could not match these with a reasonable degree of confidence. Impossible matches were those we did not find in the tax returns and therefore could not match.

[20] One would expect sample sizes should grow exponentially across generations, but at least four forces prevented this from happening in our sample: some of the sample had no children at all; around 8% of the 2nd generation died before the age of 16, thus were very unlikely to have produced offspring; births pertaining to the 3rd generation span between the end of the 19th and the beginning of the 20th century when the demographic transition was already underway; and migration from the Cape Colony caused some attrition in the sample. See Cilliers (2016) for more details on the SAF.

[21] See Table 8 in Appendix A for descriptive statistics for the full sample.

Table 1: Descriptive statistics - analytical sample

Variable	Obs	Mean	Std. dev.	Min	Max
1st generation					
Avg. shortfall (£)	130	60.26	25.29	5.69	163.57
Lifespan	130	64.46	15.33	30	93
Total slaves	130	10.90	10.67	1	53
Total tax (£)	130	2.92	2.90	0.30	15.75
Year of birth	130	1795.03	11.80	1761	1819
Year of death	130	1859.50	15.96	1834	1897
2nd generation					
Lifespan	577	47.68	30.72	0	105
Age father at birth	532	34.97	9.12	20	71
Age mother at birth	408	29.47	7.71	17	55
Nr. of siblings	577	9.67	3.54	0	17
Rank among siblings	577	5.87	3.91	1	21
Gender (Male=1)	577	1.38	0.48	1	2
Year of birth	577	1828.97	13.78	1787	1871
Year of death	577	1876.36	34.00	1789	1953
3rd generation					
Lifespan	907	50.03	30.71	0	110
Age father at birth	871	35.09	8.12	19	64
Age mother at birth	651	31.16	8.07	17	65
Nr. of siblings	907	8.42	3.32	0	18
Rank among siblings	903	5.22	3.32	1	18
Gender (Male=1)	907	1.39	0.48	1	2
Year of birth	907	1862.86	15.74	1810	1905
Year of death	907	1912.90	34.80	1829	1993

Because slaveholders had to be alive in 1834 to receive compensation, we have age truncation for the 1st generation.[22] This is reflected in the observed mean lifespan of the 1st generation when compared to the 2nd and 3rd generations. The result is that individuals born between 1740 and 1760 could only be observed if they had long lifespans, as shown in Figure 1.

A consequence of this is that the average lifespan for older cohorts will be systematically longer than for younger ones. Restricting our population of interest to younger cohorts, therefore, allowed us to have a wider distribution of lifespans and more intra-cohort variability. This procedure was not necessary for 2nd and 3rd generations since we were able to observe complete life cycles from infancy to old age as Figure 2 shows.

As an additional robustness check, we also added a control group of individuals we assume were living in the Stellenbosch district in 1834 and did not own slaves. To produce this group, we used the SAF to select men who were born or baptized in Stellenbosch and were alive in 1834. Since we could not link them to the claims records, they were unlikely to have been slaveholders. More than one thousand individuals met these criteria and we found their offspring in the SAF database, which meant that we could test the validity of

[22] There are a few exceptions to this rule, where the claims records report the name of the deceased slaveholder and instruct the compensation to be paid to the widow.

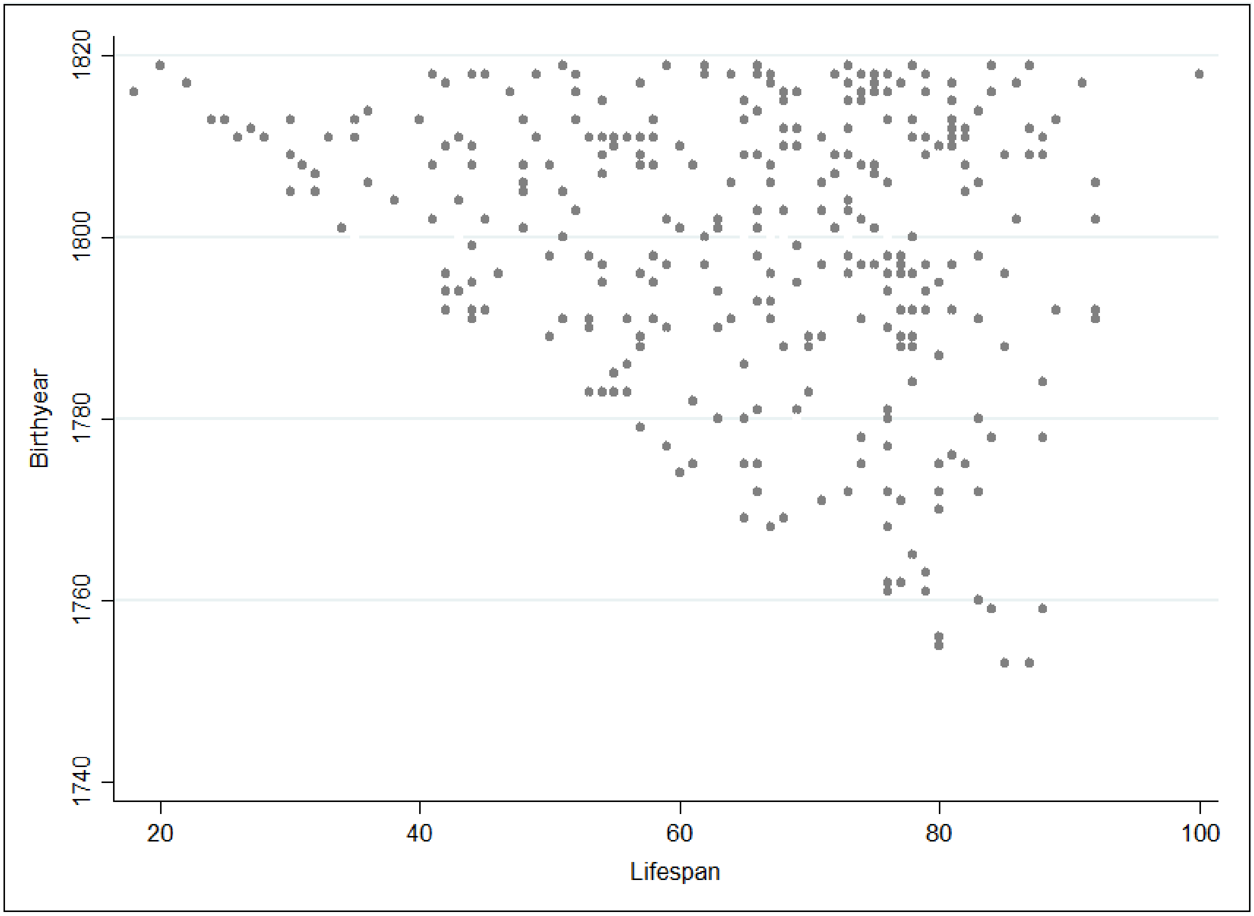

Figure 1: Lifespan in years per year of birth

our results against three generations of a group of presumed non-slaveholders.[23]

5 Methods

We start from the assumption that lifespan y – or, putting differently, the age at death – of an individual is a function of wealth. Wealth, in the context of this study, can be divided into slave (S) and non-slave (W). We write their lifespan as a function of both types of wealth and an error term μ.

$$y_i = f_i(W, S) + \mu_i \tag{1}$$

After emancipation in 1834, slaveholders were not allowed to access S and, in turn, received a compensation C. The function f, therefore, can be re-written as:

[23]See Appendix D for a detailed analysis of the presumed non-slaveholders.

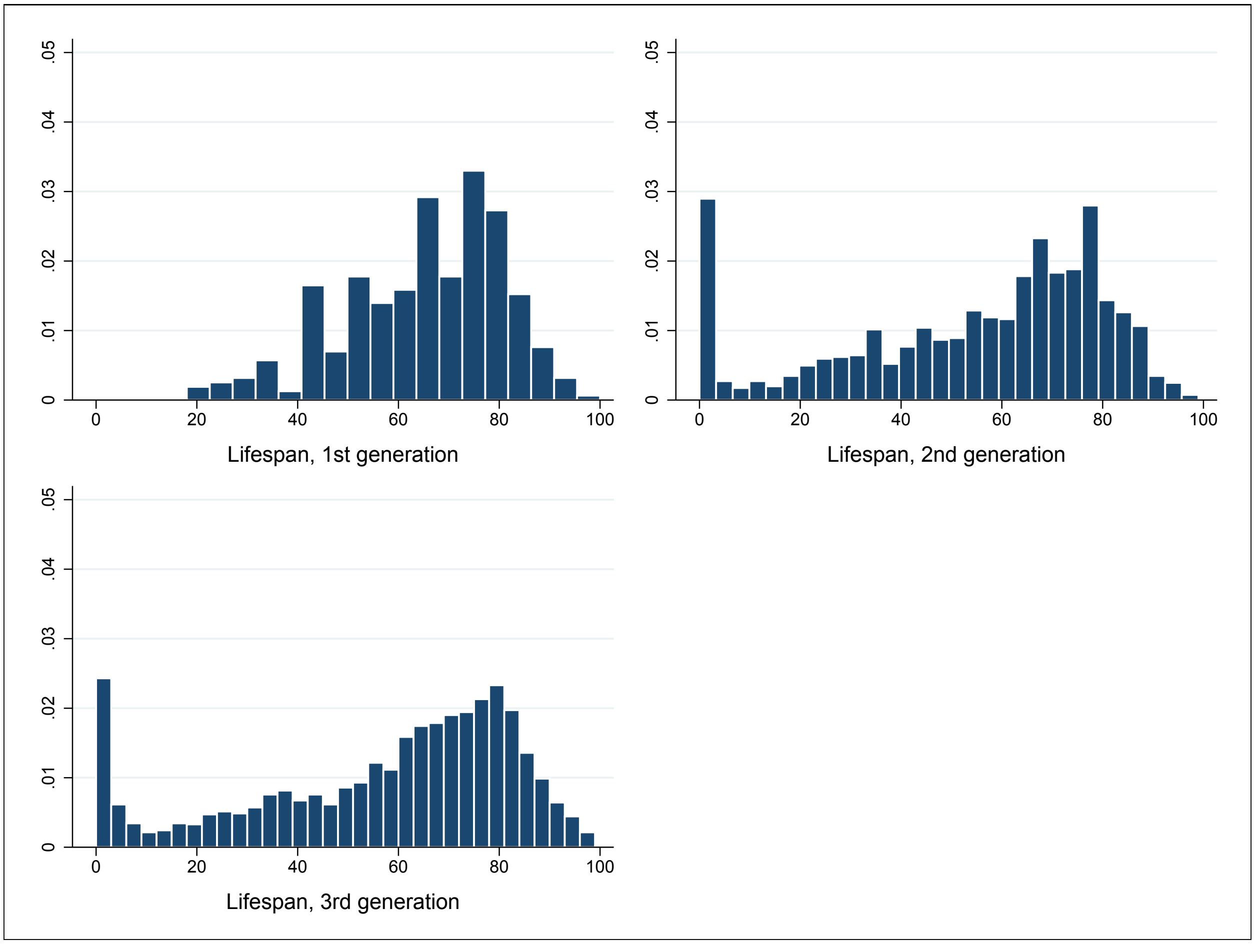

Figure 2: Distribution of lifespan in years for each population of interest

$$f = W + (pS - C) \tag{2}$$

In Equation 2, p represents the value assigned to each slave during the appraisal process. C is a direct function of the number of slaves, therefore, the more slaves owned, the bigger C will be. This would not necessarily mean, however, a better compensation deal since pS, in this case, would also be proportionally bigger. We, therefore, will consider a function g where lifespan is a function of the wealth *per slave*. In this case:

$$y_i = g_i(W, S, C) + \mu_i \tag{3}$$

And g, therefore, is:

$$g = \frac{W}{S} + \frac{(pS - C)}{S} \tag{4}$$

The component $\frac{(pS-C)}{S}$ in Equation 4 captures the average shortfall per slave holding. We use this as our shock measurement and we expect it, *a priori* to have a negative effect on a slaveholders' lifespan.

To estimate Equation 3, aside from slaveholder's average shortfall per slave and their wealth, we will also add a range of genealogical covariates.[24] Because our population of interest is dispersed across a long period of time, we also add five-year birth cohorts interacted with the average shortfall to capture cohort-specific effects. Finally, the basic functional form can be written as:

$$y_i = \beta_1 U_i + \beta_2 B_i + \beta_3 U_i B_i + \beta_4 \frac{W_i}{S_i} + \sum_z \beta_5 X_{zi} + \mu_i \quad (5)$$

The subscript i represents each slaveholder. U_i and B_i represent each individual's average shortfall per slave and birth cohort respectively. $U_i B_i$ represents the interaction between those terms. The term $\frac{W_i}{S_i}$ indicates the individual's wealth per slave through the amount of taxation paid in 1834. X_{zi} represents the range z of genealogical covariates for each individual i. Finally, μ is the error term. We derive the lifespan of slaveholders and their offspring by subtracting the year of birth from the year of death. All lifespans are thus integers and, by definition, non-negative values. Given these characteristics, linear regressions will produce unreliable results. We instead opt for a Poisson regression. Equation 5 is thus altered and takes an exponentiated form to ensure positive outcomes:

$$y_i = e^{\beta_1 U_i} + e^{\beta_2 B_i} + e^{\beta_3 U_i B_i} + e^{\beta_4 \frac{W_i}{S_i}} + e^{\sum_z \beta_5 X_{zi}} + e^{\mu_i} \quad (6)$$

Equation 6 is estimated for each generation separately. A visual inspection of Figure 3 does not enable us to draw any *a priori* conclusions as lifespan seems to behave quite independently of the average shortfall across generations. An assumption in equation 6, however, is that the individual's average shortfall is uncorrelated with wealth. Even though historical records do not suggest that the compensation scheme was biased towards richer slaveholders, an assessment of such a relationship is imperative for our empirical strategy. We estimate, therefore, the average shortfall as a function of the slaveholder's wealth alongside the characteristics of the slaves in his or her possession, such as slave's place of origin, sex and age cohort, as shown below:

[24] As shown in Table 1, these controls are age of mother and father at birth, number of siblings, rank among siblings, lifespan of mother and father and individual's gender.

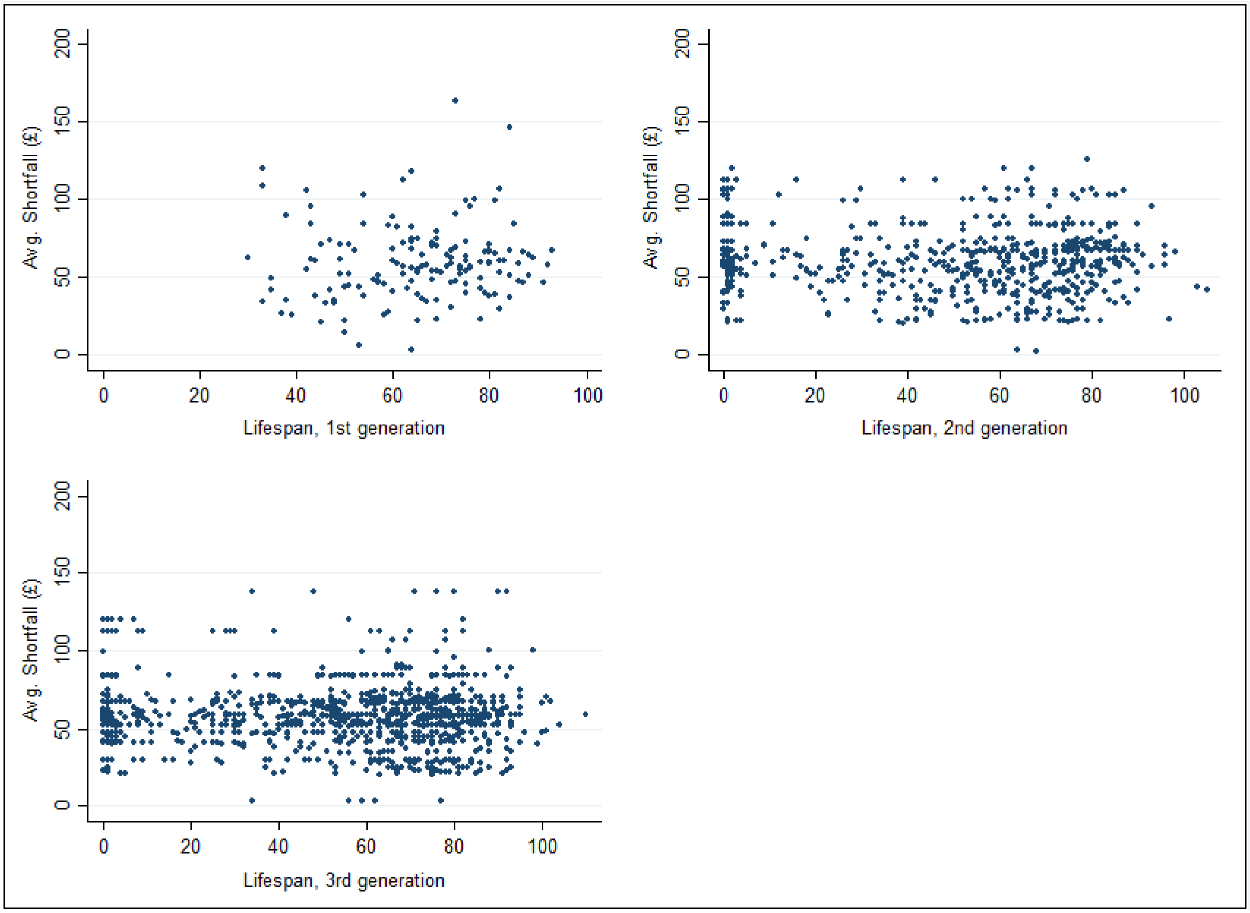

Figure 3: Relationship between the average shortfall and lifespan across generations.

$$U_i = \beta_4 W_i + \sum_k \beta_k X_{ki} + \xi_i \tag{7}$$

The covariate X_{ki} represents the range k of slaves' characteristics in owned by each individual i. ξ is the error term. Our findings for Equation 2 can be seen in Table 9 in Appendix B. They suggest that the average shortfall is uncorrelated with slaveholders' wealth regardless the functional form of both variables.[25] These results are in line with the plotted average shortfall and total tax in Figure 4. While dispersion is greater at the lower end of the distribution of total tax, the average values do not seem to differ substantially.

[25]Level-level, level-log, log-level and log-log.

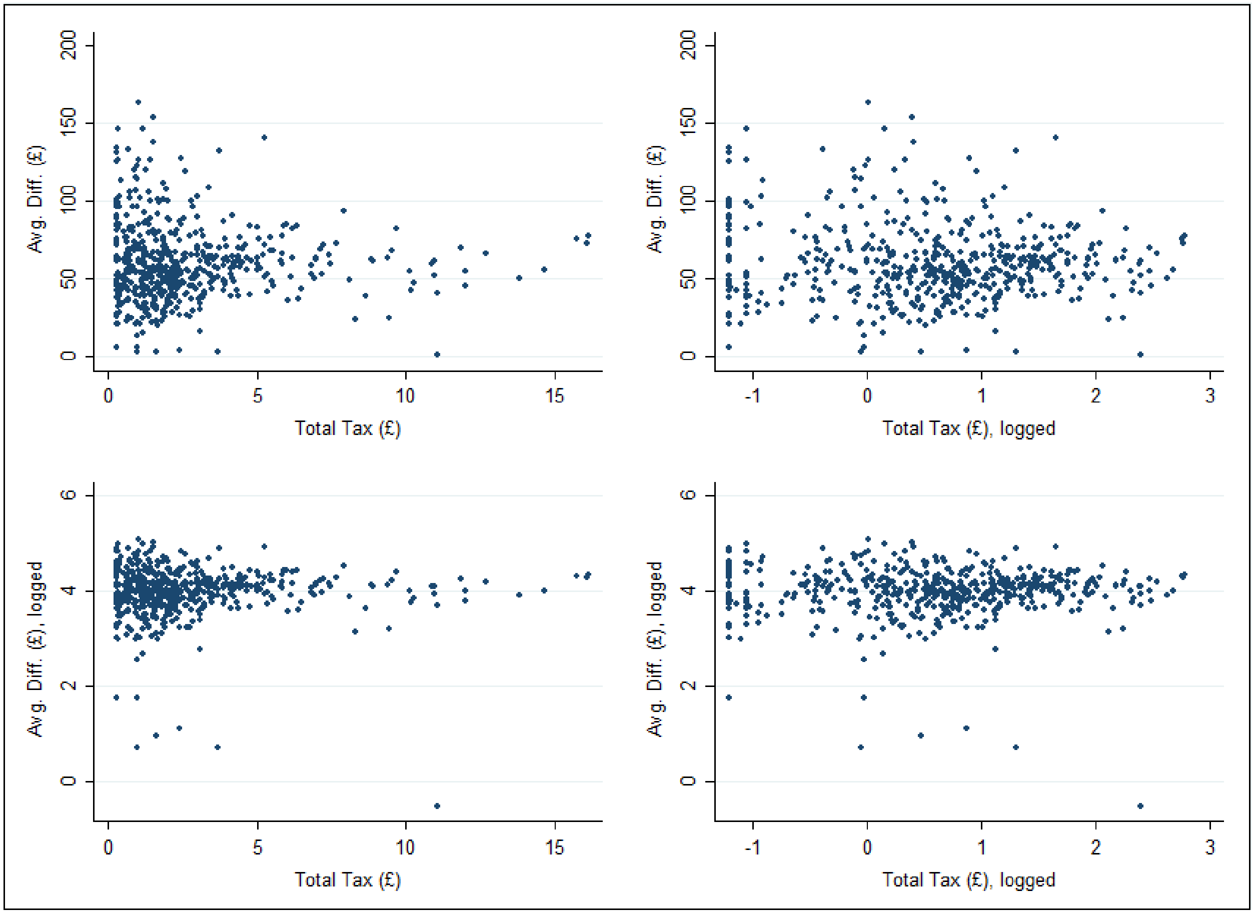

Figure 4: Relationship between the average shortfall and total in several functional forms.

These findings allow us to draw two different conclusions. Firstly, we rule out the possibility of endogenous effects arising from the relationship between the compensated values and slaveholders' wealth. Characteristics that are usually correlated with wealth, such as political savvy, are therefore unlikely to be a source of endogeneity. Secondly, the independence through several functional forms allows us to choose the estimates from which the economic significance of the coefficients can be more easily assessed. In a Poisson regression, for a one unit change in X, y is expected to change by β_i log-points since equation 7 can be re-written, as:

$$\log y_i = \beta_1 U_i + \beta_2 B_i + \beta_3 U_i B_i + \beta_4 \frac{W_i}{S_i} + \sum_z \beta_5 X_{zi} + \mu_i \tag{8}$$

Because the logarithmic function can be approximated to a percentage change, the results can be interpreted as the percentage change in y after an unit change in X. All coefficients estimated in the next sections should be understood, therefore, as a the percentage change in slaveholders' lifespan for a one unit change in any given covariate.

6 Results

To make it easier to visualize our results, we present a simplified version of our estimates in Table 2, 3, 4 and 5. These tables contain only information about the average shortfall without other relevant covariates or statistics.[26]

Table 2 shows results for the 1st generation. It offers a simple functional form where lifespan, for the 1st generation, is regressed on an individual's average shortfall, wealth and year-of-birth as a continuous variable. In equation (1), year-of-birth is negative and significant, suggesting that older cohorts would live longer on average. Since we are not able to observe mortality among older cohorts, we have little reason to trust this estimate as it is presented. We therefore divide our sample into five-year birth cohorts and, from equations (2) to (8), we successively restrict our sample to cohorts where the variability of lifespan is greater. The closer a cohort is to 1834, the wider the distribution of lifespan of slaveholders.[27] By doing this, we minimize the effects deriving from the bias produced by older cohorts and at the same time verify that the main effect of the average shortfall becomes negative, significant and progressively bigger as we increase the variability of lifespans through cohort restriction.

Table 2: Estimates of the average shortfall on the 1st generation's lifespan

y=Lifespan	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Shortfall (AS)	0.001***	0.003	0.003	0.002	−0.001	−0.006***	−0.007***	−0.008***
Total Tax/Slave	−0.027*	−0.034*	−0.033*	−0.011	0.040	−0.008	−0.005	−0.008
<1780 x AS		ref	–	–	–	–	–	–
1780-1784 x AS		0.001	ref	–	–	–	–	–
1785-1789 x AS		−0.001	−0.002	–	–	–	–	–
1790-1794 x AS		−0.000	−0.001	ref	–	–	–	–
1795-1799 x AS		−0.002	−0.002	−0.001	ref	ref	ref	ref
1800-1804 x AS		−0.001	−0.001	0.000	0.003**	0.008***	0.009***	0.010***
1805-1809 x AS		−0.005*	−0.005*	−0.004*	−0.000	0.004**	0.005***	0.007**
>1809 x AS		−0.000	−0.001	0.001	0.005**	0.009***	0.010***	0.011***
Birth Year, continuous	−0.007***							
Observations	130	130	117	91	67	64	61	55
Pseudo-R^2	0.045	0.055	0.039	0.019	0.017	0.042	0.048	0.041

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.

*p<0.10; **p<0.05; ***p<0.01

By taking equation (8) as our benchmark, we verify that a £10 increase in the average shortfall reflects in a 0.08% decrease in the expected lifespan. An average shortfall of £60, therefore, implies a mean reduction of 0.46% in the former slaveholders' years of life. The average lifespan of these farmers was 64 years – as shown in Table 1. The estimates therefore suggest that the shortfalls had an average impact of 0.3 years

[26]See Appendix C, Tables 10, 11, 12 and 13, for the complete regression table.

[27]As already explained in Section 5 referring to Figures 1 and 2.

of life if the slaveholder was subjected to losses equivalent to the mean. The results are robust to different estimation strategies such as OLS and also to different functional forms. Treating lifespan as a continuous variable did not significantly affect either the direction or the size of the coefficients. Our conclusions are similar after logging the average shortfall despite the consequent rescaling of the log function. The validity of our results can be further assessed by using a different variable-of-interest, such as compensation ratios.[28]

The conclusions derived from the estimates presented in Table 10 for the 1st generation are robust to the inclusion of our control group in the analytical sample.[29]

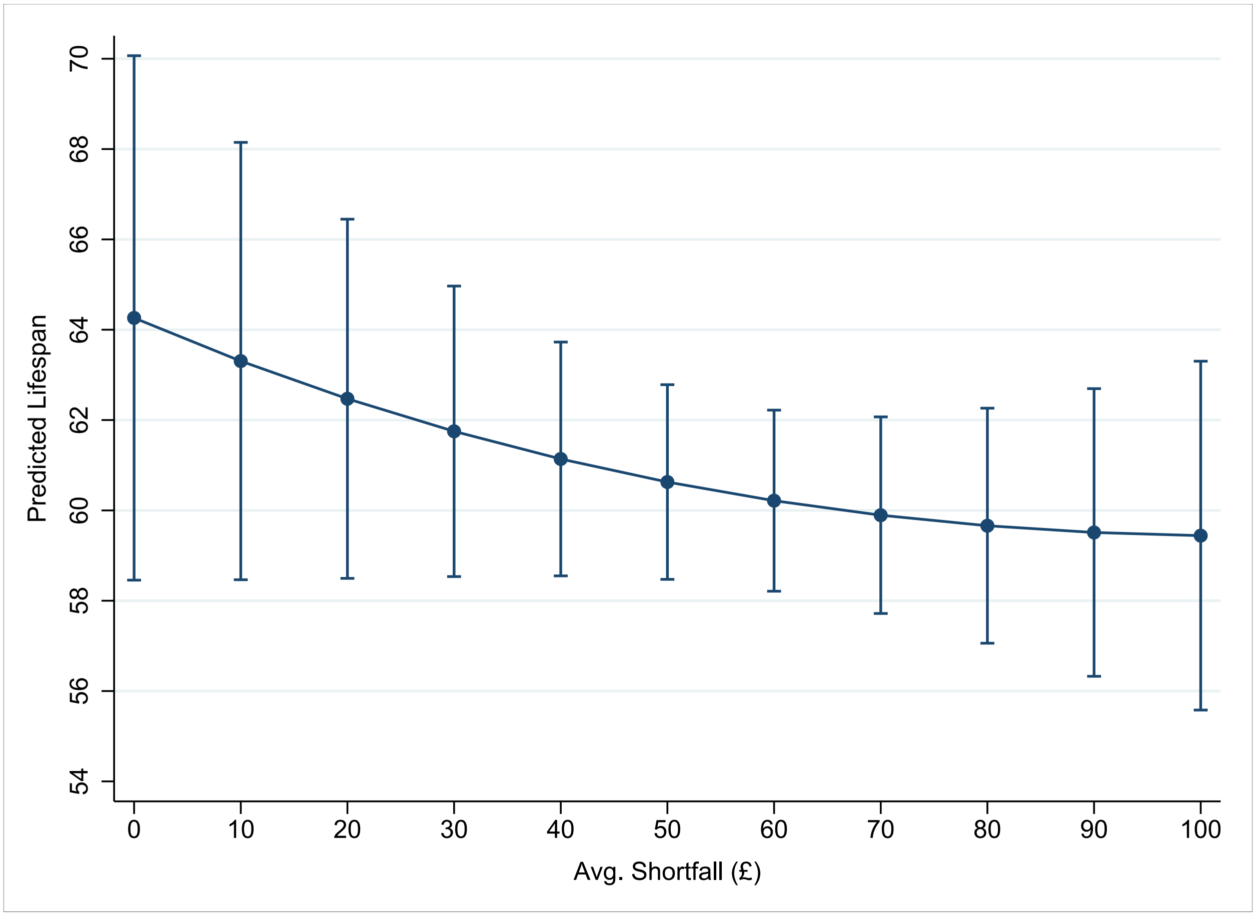

Figure 5: Marginal effects of the average shortfall, 1st generation

Table 3 serves as a starting point for discovering the effects of the average shortfall on the lifespan of the 2nd generation. Here the average shortfall is significant in most of the relevant specifications. While equations (9) and (10) consider the full sample of matched individuals in the 2nd generation, equations (11) and beyond only consider individuals born after 1816. This control is important, as this subpopulation would be older than 18 in 1834, making them more likely to have their own farms and, in some cases, slaves. If

[28] Ratio of compensation to assessed slave-wealth.

[29] See Appendix D for the results of our estimates when the control group is included in our analytical sample.

these conditions are satisfied, then this particular subpopulation – from an economic perspective – resemble their parents more than their younger siblings. Estimating equation (11) and beyond using only individuals born after 1816 allows us, consequently, to control for individuals who were more likely to live in the same household as their parents in 1834.

Table 3: Estimates of the average shortfall on the 2nd generation's lifespan

y=Lifespan	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**
Avg. Shortfall (AS)	−0.002	−0.007	−0.007*	−0.017***	−0.017***	−0.017***	−0.017***
Total Tax/Slave	0.081	0.079	0.083	−0.006	−0.074	−0.081	−0.079
<1816 x AS		ref	–	–	–	–	–
1816-1820 x AS		0.000	ref	ref	ref	ref	ref
1821-1825 x AS		0.010	0.010*	0.011	0.011	0.012	0.012
1826-1830 x AS		0.002	0.002	0.011	0.011	0.012	0.012
1831-1835 x AS		0.004	0.004	0.017**	0.016**	0.017***	0.016**
1836-1840 x AS		0.003	0.003	0.015*	0.014*	0.015**	0.014*
>1840 x AS		0.005	0.005	0.013*	0.013*	0.013*	0.012*
Birth Year, continuous	0.001						
Observations	577	577	488	239	239	239	239
Birth Restriction	no	no	yes	yes	yes	yes	yes
Pseudo-R^2	0.003	0.015	0.014	0.046	0.060	0.070	0.071

[Notes] On Equation (9), lifespan is regressed on avg. shortfall, wealth per slave and year of birth as a continuous variable. Equation (1) presents the same variables, but with 5-year birth cohorts. Equation (11) consider only individuals born after 1816. From (12) to (15), the same sample of (11) is considered and the respective covariates are added in the following order: (12) lifespan of father and mother, (13) age of father and age of mother at birth, (14) number of siblings and rank among siblings and (15) gender. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

After implementing the considerations mentioned above, in Equations (10) and (11) we do not verify any significant effects of the wealth shock in the 2nd generation. As more covariates are progressively added to the estimates, however, the coefficient represented the wealth shock increases, together with all the interaction terms. Interestingly, however, cohort-specific effects largely offset the main effect. Individuals who were born before 1834 have bigger net effects than those born after that date. These findings suggest that better compensation schemes had a greater impact on older cohorts in the 2nd generation.

The results of the average shortfall, together with other covariates (Table 11, Appendix C), also show that the lifespan of both mother and father is a significant determinant of children's lifespan. The genealogical covariates, despite their statistical significance, are usually bigger than the net effect produced by the average shortfall. These results are in line with the findings of Frijters et al. (2005) and Erixson (2017), who say that although they found significant effects of wealth shocks on later-life outcomes, the size of the coefficient was small enough for the effect to be labeled marginal.

Table 3 tells us little, however, about the transmission mechanisms. While resource allocation within the household and changing patterns of consumption of the 1st generation may have indirectly affected their offspring, it is important to note that infant mortality strongly influences the average lifespan of the 2nd generation as Figure 2 in Section 4 has already suggested. We need, therefore, to establish the effects of the average shortfall when we isolate the population of interest from the effects of infant mortality. We do this in Table 4:

Table 4: Estimates of the average shortfall on the 2nd generation's lifespan conditional on infant mortality

y=Lifespan	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**
Avg. Shortfall (AS)	-0.019^{***}	-0.008^{*}	-0.006	-0.006	-0.004	-0.004	-0.003
Total Tax/Slave	-0.241	-0.166	-0.142	-0.133	-0.161	-0.148	-0.174
<1816 x AS	–	–	–	–	–	–	–
1816-1820 x AS	ref	ref	ref	ref	ref	ref	ref
1821-1825 x AS	0.016	0.003	0.009	0.009	0.006	0.006	0.006
1826-1830 x AS	0.017^{**}	0.006	0.004	0.004	0.002	0.001	0.001
1831-1835 x AS	0.016^{**}	0.010^{**}	0.006	0.007	0.004	0.004	0.004
1836-1840 x AS	0.014	0.006	0.005	0.005	0.002	0.004	0.001
>1840 x AS	0.014^{**}	0.005	0.005	0.005	0.004	0.004	0.003
Observations	223	193	178	177	174	170	166
Birth Restriction	yes	yes	yes	yes	yes	yes	yes
Pseudo-R^2	0.075	0.071	0.028	0.032	0.025	0.021	0.025

[Notes] The estimates refer to individuals who survived past the age of 0, 1, 5, 10, 15, 20 and 25 years old respectively. All estimates contain the same covariates as Equation (15) on Table 3. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

Equation (16) excludes individuals whose lifespan was shorter than one year while equation (17) also excludes individuals who did not live past their second year. As we continue to limit the minimum lifespan to 5, 10, 15, 20 and 25 years of age, the significance of the economic shock disappears as the size of the coefficients diminish. The estimates in Table 4 suggest that the observed effect of the average shortfall is channeled through infant mortality but, once individuals survive infancy, they are not likely to be affected by the economic shock undergone by their parents. While our dataset does not allow us to establish what was the driver behind infant mortality in the post-emancipation period, we can demonstrate that infants were more vulnerable than non-infants during the economic duress imposed by the loss of capital. Figure 6 shows the marginal effects from equations (16) to (21). These effects start unusually large in the first two graphs but quickly normalize into a pattern similar to the one verified for the 1st generation. The strong downward slope at the beginning can be fully attributed to the effects of infant mortality.

The inclusion of the control group does not cause considerable changes to the above analysis. As with the 1st generation, these results can be found in Appendix D.

From an intergenerational perspective, the effects of the shortfalls, therefore, are limited to infant mortality. With that considered, the effects of the economic shock on the 3rd generation are unlikely to have any significant effect from a statistical standpoint. This is exactly what we see in Table 5.

The effects of the size of the average shortfall on the 3rd generation are systematically smaller than they are for the previous generations. While this suggests that the effects dissipate over time, none of coefficients suggest that they were different than zero. We conclude that any direct effects that differences in compensation would have on the lifespan of our populations of interest have completely worn off and are not felt by the 3rd generation.

Table 5: Estimates of the average shortfall on the 3rd generation's lifespan

y=Lifespan	**(23)**	**(24)**	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**
Avg. Shortfall (AS)	−0.002	−0.003	−0.003	−0.009	−0.013	−0.012	−0.012	−0.003
Total Tax/Slave	0.116**	0.110**	0.133**	−0.139	−0.167	−0.160	−0.160	−0.003
<1846 x AS		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AS		0.000	0.003	0.012	0.015	0.014	0.014	0.005
1851-1855 x AS		−0.005	−0.005	−0.000	0.004	0.002	0.002	−0.004
1856-1860 x AS		−0.000	0.002	0.008	0.011	0.010	0.010	0.001
1861-1865 x AS		0.000	0.001	0.011	0.015	0.013	0.013	0.004
1866-1870 x AS		0.002	0.003	0.008	0.013	0.012	0.012	0.002
1871-1875 x AS		−0.001	−0.001	0.004	0.009	0.009	0.009	−0.004
>1875 x AS		0.001	0.002	0.010	0.015	0.013	0.013	0.006
Birth Year, continuous	0.001							
Observations	907	907	768	406	405	404	404	387
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Pseudo-R^2	0.003	0.016	0.018	0.039	0.049	0.054	0.054	0.051

[Notes] On Equation (23), lifespan is regressed on avg. shortfall, wealth per slave and year of birth as a continuous variable. Equation (24) presents the same variables, but with 5-year birth cohorts. Equation (25) consider only individuals born after 1816. From (26) to (29), the same sample of (25) is considered and the respective covariates are added in the following order: (26) lifespan of father and mother, (27) age of father and age of mother at birth, (28) number of siblings and rank among siblings and (29) gender. All estimates use clustered standard errors at the individuals' father level. Equation (30) considers only individuals whose father born after 1816 and excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

It is not entirely true, though, that the shortfall has no effect on the 3rd generation. As Tables 12 and 13 show, the lifespan of the mother and father affect the lifespan of the children, both for the 2nd and the 3rd generation. One way to interpret these results is that the shortfall only affects the 3rd generation through the lifespan of the parents and grandparents; if a grandfather lived shorter because of a larger shortfall, his grandchild may be affected through this channel. There is thus no additional effect of the shortfall above and beyond the intergenerational persistence of lifespan.[30]

[30] Other household and demographic variables also matter in explaining lifespan for individuals belonging to the 3rd generation, like the total number of siblings and the individual's gender. Considering that births in the 3rd generations are mostly within the period commonly defined as the South African fertility transition, slightly different patterns would be expected between generations 2 and 3. See, for example, Cilliers and Mariotti (2018).

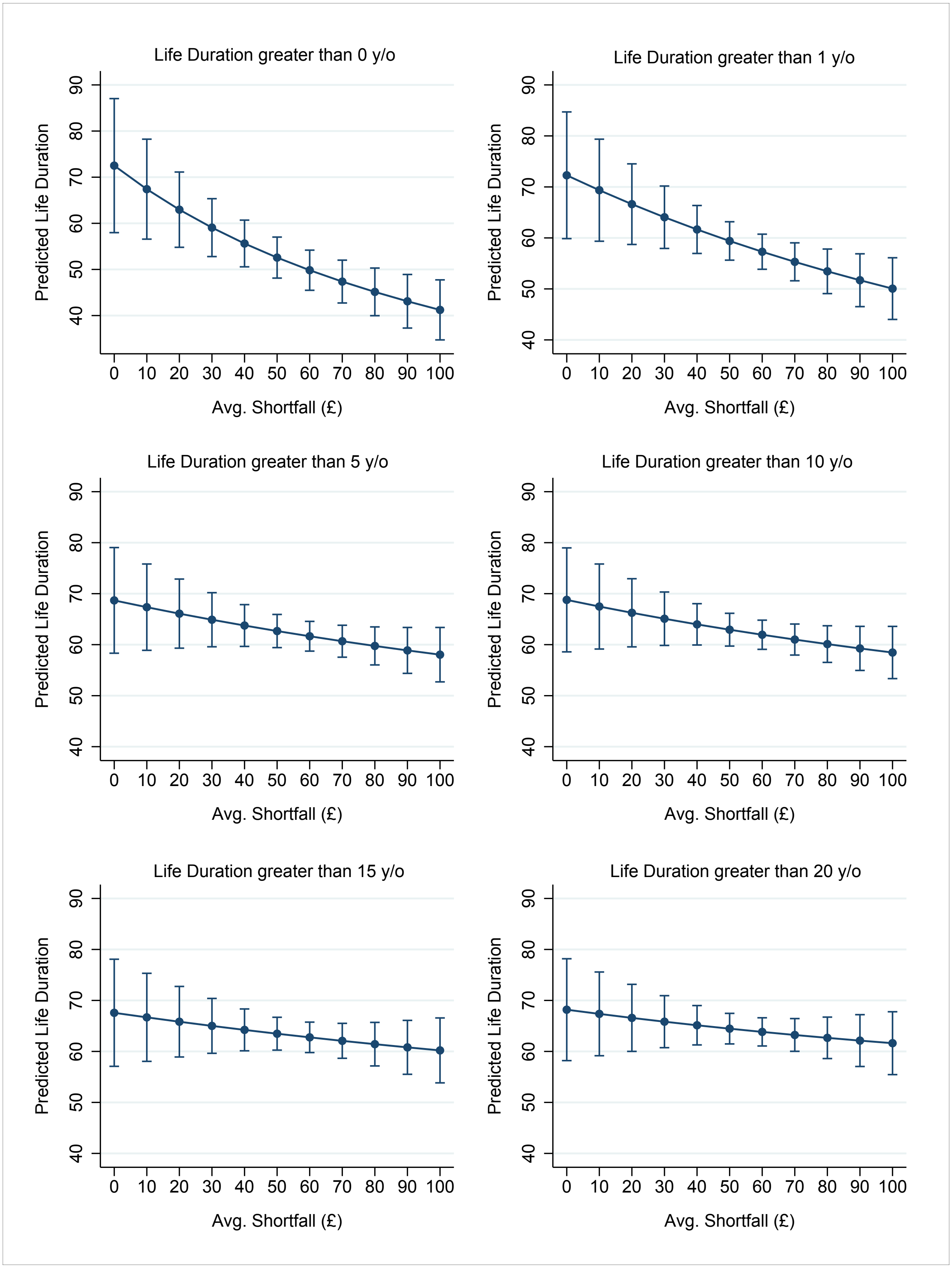

Figure 6: Marginal effects of the average shortfall between conditional to a given minimum lifespan, 2$^{\text{nd}}$ generation

7 Transmission Mechanisms

Our analysis shows that the shortfalls directly affected two groups of individuals: the first generation slaveholders and their infants. Regarding the latter, however, we were not able to verify any significant effect when individuals lived beyond the second year of life. In this section we look at possible transmission mechanisms that might have caused these effects.

The loss of slaves meant the loss of an asset. Slaves generated incomes for slaveholders mostly, but not exclusively, through their employment in agriculture. The tax composition of our sample from the *opgaafrollen* (tax records) showed that more than 84% of the farmers did not declare any income from non-farming activities. This means that a large majority of our sample was made up of individuals who derived their income only from farming. Since slaves were a major component of the workforce on the farms, the loss of labor was a heavy blow.

The Abolition Act ruled that slaves had to serve their former masters for six years after 1834 in what was labeled an 'apprenticeship' period. At the Cape, however, this period was shortened to four years because the local government was unable to enforce the legislation. And the farmers were unable to compel the slaves to continue working. Isaac van der Merwe from Worcester, for example, claimed that by already in 1834 his former slaves "were all in disorder" and "in open resistance to lawful commands".[31] In fact, many slaves left their former masters as soon as the Abolition Act came into effect, believing they were illegally held in bondage. Some slaves were captured and returned for the completion of their 'apprenticeship' but many others were still at large up to 1838 (Dooling 2007).

Dooling (2007, p. 116) notes that when the 'apprenticeship' period was over, "the freed slaves left their masters *en masse*". He says that in the days immediately after the emancipation there was a "large-scale withdrawal of labor from the wheat and wine estates of the Western Cape". While many slaves undoubtedly did leave their masters for good, their freedom was still limited. Giliomee (2003) notes that the colonial government did not make any land available for small-scale farming and most of the slaves had very few options other than to remain farm laborers. He quotes H. Calderwood, an eye-witness to the emancipation day, who commented that it was "ridiculous to talk of [the emancipated slaves] refusing to work when they know very well they must either work or starve".

Despite the conflicting views as to whether freed slaves would make a reliable form of labor, it is a fact that in the short term the Cape's agricultural output fell dramatically. In Stellenbosch alone, between 1828 and 1834, annual wine output declined by roughly 50%. From 1834 to 1842, barley and wheat production dropped by a third. It was only by the mid-1840s that agricultural output was back at its pre-emancipation

[31] As quoted in Worden (2017).

levels (Giliomee 2003; Dooling 2006).

Could this drop in output for a short period explain the intergenerational effects we find? To some extent this is plausible, especially when one considers the literature exploring the causes of infant mortality. Theoretical frameworks constructed to synthesize the determinants of infant mortality, such as those by Mosley and Chen (1984) and Norren and Vianen (1986), emphasize the malnutrition-infection syndrome. Infants whose immune systems do not develop properly because breastfeeding is inadequate due to maternal nutritional deprivation are more susceptible to infection.

It is, unfortunately, impossible to determine the exact consequences of the Cape's output decline between 1834 and 1842 for the residents' nutritional status with the available data. What we do know is that in societies where people mostly derive their income from farming a shock in output is likely to have direct consequences on economic and demographic variables such as lifespan, fertility and mortality. As Hedefalk et al. (2017, p. 1041) point out, "common factors that affected nutritional status [in preindustrial societies] were income and wealth, which [...] were mostly determined by the ability of individuals to support themselves from the land they owned or worked on", or had slaves working on. Output levels, if low enough, could affect societies across generations.

Labor, however, is not the only mechanism by which the shock could affect current and future generations. The Cape Colonial economic system strongly relied in what Dooling (2007, p. 128) classifies as "networks of indebtedness and patronage". More recent research on the role of slaves in the Cape Colony shows that their owners perceived them as capital investments (Fourie 2013a; Fourie 2013b; Green 2014; Du Plessis et al. 2015; Swanepoel 2017). They were an integral part of a credit market, serving as collateral for loans and as means for settling long distance payments. Slaves were also mortgaged and many slaveholders were still paying for their slaves by the onset of emancipation. In fact, many farmers declared insolvency in the years following emancipation and blamed the shortfalls, together with difficulties of hiring labor, as major causes of their financial situation (Shell 1994; Dooling 2007). Social networks and moral obligations provided many former slaveholders with a safety net, yet, many faced years of economic hardship after emancipation, especially the highly indebted ones. Anecdotal evidence shows that some farmers were mortgaged up to 160% of the value of their estate (Theal 1891; Hengherr 1953; Ross 1993; Dooling 2007). Worden (2017) suggests that the compensation money alleviated the short-term economic difficulties caused by the new mode of production, paid labor, but clearly the effect was short-lived. Dooling (2007, p. 138) notes that between 1841 and 1843 more than 60 farmers in the Cape declared bankruptcy.

Economic hardship and debt are triggers for psychological stress (Gallo and Matthews 2003; Drentea and Reynolds 2015). A clear relationship between psychological stress and physical health has been demonstrated through enzymatic (Hajat et al. 2010; Cohen et al. 2012; Boen and Yang 2016) and consumption patterns

(Catalano et al. 2011; Black et al. 2015), both of which cause poor health.[32] Because the declines in output experienced by the Cape Colony were of short duration and the effects of the shortfalls in compensation did not affect second generation individuals who lived past the second year, we speculate that another transmission mechanism must be at play through the role of slaves as capital investments, since output declines would not explain such selectivity of generations affected by the shortfalls. Here, we are capable to substantiate our claims with quantitative evidence.

Farmers were dependent on their slaves at different levels and for different purposes. Large-scale wine farmers, for example, were more likely to draw their incomes through the employment of slaves as agricultural workers, while small-scale grain farmers could hardly profit from enslavement if they did not employ slaves in other activities aside from farm labor alone (Du Plessis et al. 2015). To understand how the wealth shock affected different slaveholders, we divided the first generation of slaveholders according to agricultural specialization, reported in the *opgaafrollen* of 1834, in four different categories: strictly grain producer, strictly wine producer, producer of both crops and non-producer. Some descriptive statistics concerning this division are presented in Table 6.

Table 6: Mean taxation, output, livestock and slaves per slaveholders' specialization, 1st generation.

Specialization	Total tax (£)	Grain	Wine	Cattle	Sheep	Slaves
Grains only	2.52	217.50	0	25.44	63.10	7.34
Wine only	2.37	0	24.22	18.78	11.45	10.05
Grains and wine	4.34	119.48	35.39	47.84	59.61	18.41
None	1.33	0	0	3.81	4.91	5.29

[Notes] Grains are reported in muids, a South African dry measure of capacity equivalent to about 109 liters. Wine is reported in leggers, equivalent to 516 liters.

Table 6 shows that farmers that invested in grains and wine simultaneously were also the ones with the biggest number of slaves and livestock on top of being the group subjected to the highest taxation. They are the wealthiest strata among slaveholders. This group is followed by the farmers who specialized in either grains or wine and, at the bottom of the wealth distribution among slaveholders, we find the ones who were producing neither wine nor grains. This is also the group who possess the least cattle and sheep, suggesting that the absence of crop farming was not translated into livestock farming. The group of non-producers, surprisingly, despite the absence of agricultural output, averages more than 5 slaves per farm, in line with the evidence that slaves were integrated into the Cape's economy through a wide range of activities aside from agricultural work (Fourie 2013a; Green 2014; Swanepoel 2017).

The analysis of Table 6 suggests that farming diversification is a proxy for farm wealth. Wealthy farmers operated larger farming units and were capable of profiting from diverse crops through economies of scale

[32] Kalwij (2018), using a natural experiment to investigate the effects of competition results on the health of US Olympic medalists, offers an interesting example of psychological effects on later-life outcomes. He found that the outcomes for bronze and gold medalists did not differ significantly, but silver medalists lived 2.4 years less than the bronze and 3.9 years less than the gold.

and scope (Fourie 2013a). Poorer farmers, on the other hand, did not have this option and resorted to specialization. It is also possible to infer that slaves met wealthy farmers' demands for labor while slaves among non-producers were likely to meet their demands for capital through their employment as collateral.

To understand the effects of the economic shock on these very distinct groups of slaveholders, we interacted the average shortfall with each farmer's agricultural specialization. Table 7 contains the regression output.[33]

Table 7: Estimates of the average shortfall on the 1st generation's lifespan including crop type.

y=Lifespan	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Shortfall (AS)	0.001***	0.004*	0.005*	0.002	−0.002	−0.004*	−0.005***	−0.010***
Total Tax/Slave	−0.021	−0.034*	−0.035*	−0.010	0.084*	0.041	0.050	0.101
None x AS		ref	ref	ref	ref	ref	ref	ref
Grains only x AS		0.002	0.001	0.002	0.003	0.004*	0.003*	0.004*
Wine only x AS		0.003**	0.003**	0.003*	0.005***	0.003	0.003	0.004**
Grains & Wine x AS		−0.004**	−0.004**	−0.004**	−0.004**	−0.004**	−0.004**	−0.005***
Birth Year, continuous	−0.007***							
Constant	15.872***	4.176***	4.074***	4.148***	4.311***	4.463***	4.582***	4.906***
Observations	130	130	117	91	67	64	61	55
Pseudo-R^2	0.050	0.073	0.057	0.041	0.088	0.093	0.103	0.108

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.
*p<0.10; **p<0.05; ***p<0.01

Our results suggest that farmers who produced both grain and wine in their units were subjected to the most negative effects of the shock on their lifespans. On the other hand, specialized producers seem to have been affected less in comparison to non-producers. The implications from Table 7, therefore, are clear: conditional on the same shortfall, non-producers and diversified producers were the most vulnerable groups of slaveholders after the economic shock. Transmission mechanisms acted at both ends of wealth distribution. Wealthier farmers were affected through the loss of labor while poorer farmers were more likely to be affected through the loss of collateral since the number of slaves in their units is not compatible with their livestock or agricultural produce, meaning that slaves, in this case, supplied their demand for capital, not labor.

In closing, we must emphasize that our database did not allow for a conclusive investigation of all possible transmission mechanisms or the magnitude of the influence of each source of economic and psychological distress in 1834. It is, nevertheless, clear that understanding slaves as both labor and assets allow us to infer mechanisms through which economic shocks may affect longevity across more than one generation.

[33]See Appendix E, Table 21 for the complete regression table.

8 Conclusions

We contribute to the literature that explores the effects of exogenous shocks on later-life outcomes by presenting a novel strategy that accounts for the loss of property and wealth while investigating its intergenerational effects in a historical setting. To do so, we exploit exogenous variations in differential compensation schemes of former slaveholders in the Cape Colony to test if the partial compensation received after the emancipation of slaves in 1834 had any significant role on explaining variation on these individuals' lifespans.

Our empirical strategy reveals that the slaveholders who suffered the biggest shortfalls lived shorter lives. We believe that the loss of slave-wealth and the high level of debt verified in the Stellenbosch district were major causes of the economic duress experienced by the former slaveholders in the post-emancipation years. Debt and economic duress are particularly relevant elements in stress process models, suggesting that the verifiable effects on slaveholders' lifespans were channeled through their psychological ill-health. Shortly after emancipation, the Cape's agricultural output suffered a short-term decline. While it is impossible to determine, with the data currently available to us, how this affected the lifespan of these former slaveholders, we recognize that living standards in farming societies are directly dependent on agricultural output.

These direct effects, however, were mostly overcome by the second generation, the slaveholders' children. All the significant results we found among these individuals were driven by infant mortality. Lifespan conditional on infancy survival was largely unaffected by the wealth shock. For the third generation we found no direct effects on either lifespan or infant mortality. Our investigation of fertility for all generations produced a series of null results. The only effect of the shortfall on grandchildren could be through the indirect channel of affecting the lifespan of the grandparents and, because of intergenerational persistence of lifespans, therefore also the lifespans of their children and grandchildren.

Our findings are broadly in line with the literature suggesting that the effects of wealth shocks on later-life outcomes are marginal, despite their statistical significance (Frijters et al. 2005; Cesarini et al. 2016; Erixson 2017). The same literature, however, finds that the effects are mostly observable in the short and medium run. Our intergenerational analysis is in partial disagreement with this claim since we observe effects beyond a single generation. This could be due to the nature of the shock we are studying: we are not looking at an exogenous wealth *gain*, but rather at the *loss* of an asset that was the most important source of wealth for our population of interest. In that context our findings agree with those of González et al. (2017) who demonstrate that the absence of slaves limited the former slaveholders' access to the credit market, hindering their ability to start a business.

Our study benefits from a historical setting, because individuals had fewer coping mechanisms to deal with wealth losses. Studies of present-day settings find that extensive social security nets reduce the effects of wealth on wealth-mortality gradients (Deaton 2003; Cesarini et al. 2016). While anecdotal evidence suggests that the population of slaveholders at the Cape were able to access social networks and moral obligations in times of economic duress (Shell 1994; Dooling 2007), this is hardly comparable to social security systems in the developed countries of the 21st century. This increases our ability to capture the effects of shocks.

This paper also makes a contribution to research on the history of slavery and the emancipation. While our population of interest in this study consisted of slaveholders and their offspring – which certainly limits the scope of our findings – it does offer some food for thought. As we noted in Section 3, the emancipation process can appear somewhat counter-intuitive to modern readers. Slaveholders, and not the enslaved, were the ones who received reparations. This shows that the British government recognized that the loss of their slaves would have a severe effect on slaveholders' livelihoods. The greater the loss, the greater the drop would be in their living standards. For this reason, it is probably true that no emancipation would have occurred had it not been for compensation payments to former slaveholders. Our evidence suggests that while this benefit did not completely offset the negative effects of the wealth loss suffered by the Cape Colony slaveholders, it certainly made the transition away from slave economy less traumatic – and, consequently, without serious political resistance.

Appendix A: Descriptive Statistics - Full Sample

Table 8 presents the complete matched sample. It differs from Table 1 since the latter presents the descriptive statistics of our analytical sample (i.e. the sample used to obtain our estimates).

It is possible to observe that Table 8 has some minor inconsistencies such as the minimum age of father and age of mother at birth concerning the 2$^{\text{nd}}$ generation – which is too low by any standards – alongside the negative figures of the average shortfall for the 1$^{\text{st}}$ generation.

We believe that these unexpected values derive from digitization problems since none of these caveats can be observed in a systematic manner. The minimum of 11 years old for the age of father at birth refers to only one observation. If excluded from the sample, the minimum becomes 16 years. Similarly, the minimum for age of mother at birth is affected by a single observation that causes it to be 11. If excluded, the minimum becomes 17 years old. Concerning the negative figures for the average shortfall, it is important to note that they represent less than 5% of the sample size (22 observations out of 551).

While the exclusion of the inconsistent observations concerning age of father and age of mother at birth and the average shortfall would be justified on demographic and historical grounds respectively, that was not needed since lifespan, due to its several missing values, acts as a natural filter to the dataset. Consequently, these inconsistencies were not carried on to the estimates, with the exception of 6 observations that had a negative average shortfall. Those were, then, dropped from the analytical sample.

Table 8: Descriptive statistics - full matched sample

Variable	Obs	Mean	Std. dev	Min	Max
1st generation					
Avg. shortfall	551	55.09	32.68	-187.28	163.57
Lifespan	154	65.35	15.05	30	93
Total slaves	551	10.00	9.83	1	83
Total tax (£)	551	2.54	2.55	0.30	16.14
Year of birth	259	1793.99	12.97	1748	1819
Year of death	176	1859.39	16.50	1834	1908
2nd generation					
Lifespan	654	47.97	30.18	0	105
Age father at birth	1378	34.83	9.00	11	71
Age mother at birth	584	29.48	7.37	13	55
Nr. of siblings	1744	7.91	4.09	0	17
Rank among siblings	1733	5.28	3.61	1	23
Gender (Male=1)	1744	1.49	0.50	1	2
Year of birth	1526	1827.56	14.48	1760	1873
Year of death	705	1876.70	33.70	1789	1959
3rd Generation					
Lifespan	1034	51.09	30.17	0	110
Age father at birth	1910	35.12	8.43	19	68
Age mother at birth	1140	31.19	8.06	17	65
Nr. of siblings	2458	7.08	4.08	0	18
Rank among siblings	2422	5.04	3.35	1	23
Gender (Male=1)	2458	1.48	0.49	1	2
Year of birth	2055	1861.26	18.42	1782	1907
Year of death	1126	1912.50	35.24	1819	1993

Appendix B: Endogeneity Check

Table 9: Endogeneity Check

	y=Avg. Shortfall		y=log(Avg. Shortfall)			
Variable	**(A)**	**(B)**	**(C)**	**(D)**	**(E)**	**(F)**
Total Tax	0.395		0.043^{**}	0.018		
Total Tax, squared	-0.048		-0.004^{**}	-0.001		
Total Tax, logged		0.138			0.034^{*}	0.018
Ratio of Males	-22.008^{***}	-21.960^{***}	-0.320^{***}	-0.277^{***}	-0.318^{***}	-0.275^{***}
Ratio of Child (0-5 y/o)	-69.231^{***}	-68.848^{***}	-1.438^{***}	-1.240^{***}	-1.424^{***}	-1.238^{***}
Ratio of Child (5-10 y/o)	-66.798^{***}	-66.381^{***}	1.243^{***}	-1.191^{***}	-1.224^{***}	-1.187^{***}
Ratio of Adult (10-20 y/o)	13.446^{**}	13.751^{**}	0.132	0.139	0.148	0.144
Ratio of Adult (20-40 y/o)	25.464^{***}	25.723^{***}	0.319^{**}	0.290^{***}	0.322^{**}	0.293^{***}
Ratio of Adult (40-60 y/o)	-13.636^{***}	-13.377^{***}	-0.477^{***}	-0.157^{**}	-0.437^{***}	-0.155^{**}
Ratio of Elder (60+ y/o)	-80.120^{***}	-79.509^{***}	-1.944^{***}	-1.503^{***}	-1.917^{***}	-1.495^{***}
Ratio of Origin at Cape	8.849^{**}	8.704^{***}	0.348^{***}	0.163^{***}	0.340^{***}	0.161^{***}
Constant	73.908^{***}	74.006^{***}	4.161^{***}	4.260^{***}	4.197^{***}	4.278^{***}
Observations	522	522	522	506	522	506
R^2	0.567	0.567	0.410	0.601	0.407	0.600

[Notes] Estimates (D) to (F) exclude 16 outliers whose average shortfall per slave holding (avg. shortfall) was lower than £21.

*p<0.10; **p<0.05; ***p<0.01

Appendix C: Full regression tables, main results.

Table 10: Regression output for individuals belonging to the 1st generation

y=Lifespan	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Shortfall (AS)	0.001***	0.003	0.003	0.002	−0.001	−0.006***	−0.007***	−0.008***
Total Tax/Slave	−0.027*	−0.034*	−0.033*	−0.011	0.040	−0.008	−0.005	−0.008
<1780		ref	–	–	–	–	–	–
1780-1784		−0.080	ref	–	–	–	–	–
1785-1789		−0.025	0.050	–	–	–	–	–
1790-1794		−0.135	−0.061	ref	–	–	–	–
1795-1799		−0.092	−0.018	0.016	ref	ref	ref	ref
1800-1804		−0.219	−0.146	−0.111	−0.220**	−0.465***	−0.585***	−0.709***
1805-1809		0.084	0.157	0.179	0.042	−0.176	−0.298**	−0.420**
>1809		−0.217	−0.114	−0.118	−0.244*	−0.473***	−0.594***	−0.716***
<1780 x AS		ref	–	–	–	–	–	–
1780-1784 x AS		0.001	ref	–	–	–	–	–
1785-1789 x AS		−0.001	−0.002	–	–	–	–	–
1790-1794 x AS		−0.000	−0.001	ref	–	–	–	–
1795-1799 x AS		−0.002	−0.002	−0.001	ref	ref	ref	ref
1800-1804 x AS		−0.001	−0.001	0.000	0.003**	0.008***	0.009***	0.010***
1805-1809 x AS		−0.005*	−0.005*	−0.004*	−0.000	0.004**	0.005***	0.007**
>1809 x AS		−0.000	−0.001	0.001	0.005**	0.009***	0.010***	0.011***
Birth Year, continuous	−0.007***							
Constant	16.268***	4.172***	4.099***	4.066***	4.178***	4.420***	4.540***	4.663***
Observations	130	130	117	91	67	64	61	55
Pseudo-R^2	0.045	0.055	0.039	0.019	0.017	0.042	0.048	0.041

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.

*p<0.10; **p<0.05; ***p<0.01

Table 11: Regression output for individuals belonging to the 2[nd] generation

y=Lifespan	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**
Avg. Shortfall (AS)	−0.002	−0.007	−0.007*	−0.017***	−0.017***	−0.017***	−0.017***
Total Tax/Slave	0.081	0.079	0.083	−0.006	−0.074	−0.081	−0.079
<1816		ref	–	–	–	–	–
1816-1820		−0.081	ref	–	–	–	–
1821-1825		−0.749*	−0.667**	−0.685	−0.686	−0.765	−0.734
1826-1830		−0.115	−0.033	−0.401	−0.364	−0.511	−0.496
1831-1835		−0.350	−0.269	−0.874**	−0.837**	−0.947***	0.905**
1836-1840		−0.240	−0.159	−0.642	−0.655	−0.732	−0.681
>1840		−0.264	−0.185	−0.503	−0.549	−0.588*	−0.571*
<1816 x AS		ref	–	–	–	–	–
1816-1820 x AS		0.000	ref	ref	ref	ref	ref
1821-1825 x AS		0.010	0.010*	0.011	0.011	0.012	0.012
1826-1830 x AS		0.002	0.002	0.011	0.011	0.012	0.012
1831-1835 x AS		0.004	0.004	0.017**	0.016**	0.017***	0.016**
1836-1840 x AS		0.003	0.003	0.015*	0.014*	0.015**	0.014*
>1840 x AS		0.005	0.005	0.013*	0.013*	0.013*	0.012*
Birth Year, continuous	0.001						
Lifespan, father				0.008***	0.007**	0.008**	0.008**
Lifespan, mother				0.004	0.003	0.004	0.004
Age of father at birth					−0.019	−0.019	−0.019
Age of mother at birth					0.025*	0.023	0.024
Number of siblings						−0.035*	−0.036*
Rank among siblings						0.013	0.014
Gender (ref=Male)							−0.048
Constant	1.617	4.261***	4.179***	3.887***	3.970***	4.233***	4.203***
Observations	577	577	488	239	239	239	239
Birth Restriction	no	no	yes	yes	yes	yes	yes
Pseudo-R^2	0.003	0.015	0.014	0.046	0.060	0.070	0.071

[Notes] Equations (11) to (15) consider only individuals born after 1816. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

Table 12: Regression output for individuals belonging to the 2nd generation conditional on infant mortality

y=Lifespan	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**
Avg. Shortfall (AS)	−0.019***	−0.008*	−0.006	−0.006	−0.004	−0.004	−0.003
Total Tax/Slave	−0.241	−0.166	−0.142	−0.133	−0.161	−0.148	−0.174
<1816	–	–	–	–	–	–	–
1816-1820	ref	ref	ref	ref	ref	ref	ref
1821-1825	−1.035	−0.280	−0.564*	−0.561*	−0.403	−0.357	−0.380
1826-1830	−0.838*	−0.228	−0.106	−0.108	−0.007	0.028	0.038
1831-1835	−0.981**	−0.615*	−0.283	−0.281	−0.170	−0.193	−0.172
1836-1840	−0.733	−0.318	−0.261	−0.256	−0.150	−0.272	−0.037
>1840	−0.752*	−0.253	−0.201	−0.201	−0.134	−0.174	0.129
<1816 x AS	–	–	–	–	–	–	–
1816-1820 x AS	ref	ref	ref	ref	ref	ref	ref
1821-1825 x AS	0.016	0.003	0.009	0.009	0.006	0.006	0.006
1826-1830 x AS	0.017**	0.006	0.004	0.004	0.002	0.001	0.001
1831-1835 x AS	0.016**	0.010**	0.006	0.007	0.004	0.004	0.004
1836-1840 x AS	0.014	0.006	0.005	0.005	0.002	0.004	0.001
>1840 x AS	0.014**	0.005	0.005	0.005	0.004	0.004	0.003
Lifespan, father	0.009**	0.006***	0.002	0.003	0.002	0.001	0.000
Lifespan, mother	0.000	−0.002	−0.002	−0.002	−0.002	−0.001	−0.000
Age of father at birth	−0.016	−0.002	−0.001	−0.002	−0.000	−0.001	−0.002
Age of mother at birth	0.021	0.011	0.003	0.002	0.004	0.005	0.004
Number of siblings	−0.029	−0.019	−0.001	−0.003	0.003	−0.002	−0.008
Rank among siblings	0.007	−0.001	−0.004	−0.003	−0.011	−0.005	−0.002
Gender (ref=Male)	−0.076	−0.096	−0.011	−0.019	−0.021	−0.024	−0.005
Constant	4.639***	4.247***	4.448***	4.459***	4.316***	4.337***	4.416***
Observations	223	193	178	177	174	170	166
Birth Restriction	yes	yes	yes	yes	yes	yes	yes
Pseudo-R^2	0.075	0.071	0.028	0.032	0.025	0.021	0.025

[Notes] The estimates refer to individuals who survived past the age of 0, 1, 5, 10, 15, 20 and 25 years old respectively. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

Table 13: Regression output for individuals belonging to the 3rd generation

y=Lifespan	**(23)**	**(24)**	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**
Avg. Shortfall (AS)	−0.002	−0.003	−0.003	−0.009	−0.013	−0.012	−0.012	−0.003
Total Tax/Slave	0.116**	0.110**	0.133**	−0.139	−0.167	−0.160	−0.160	−0.003
<1846		ref	ref	ref	ref	ref	ref	ref
1846-1850		−0.094	−0.240	−0.906	−1.003	−0.905	−0.907	−0.439
1851-1855		0.145	0.171	−0.130	−0.254	−0.157	−0.160	0.173
1856-1860		−0.190	−0.241	−0.633	−0.678	−0.602	−0.604	−0.098
1861-1865		0.002	0.060	−0.596	−0.596	−0.487	−0.490	0.010
1866-1870		−0.014	−0.014	−0.330	−0.356	0.265	−0.267	0.289
1871-1875		0.160	0.180	−0.126	−0.194	−0.125	−0.127	0.565
>1875		−0.136	−0.117	−0.634	−0.678	−0.594	−0.596	−0.225
<1846 x AS		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AS		0.000	0.003	0.012	0.015	0.014	0.014	0.005
1851-1855 x AS		−0.005	−0.005	−0.000	0.004	0.002	0.002	−0.004
1856-1860 x AS		−0.000	0.002	0.008	0.011	0.010	0.010	0.001
1861-1865 x AS		0.000	0.001	0.011	0.015	0.013	0.013	0.004
1866-1870 x AS		0.002	0.003	0.008	0.013	0.012	0.012	0.002
1871-1875 x AS		−0.001	−0.001	0.004	0.009	0.009	0.009	−0.004
>1875 x AS		0.001	0.002	0.010	0.015	0.013	0.013	0.006
Birth Year, continuous	0.001							
Lifespan, father				0.003	0.003	0.003	0.003	0.000
Lifespan, mother				0.005**	0.005*	0.005**	0.005**	0.005**
Age of father at birth					−0.015	−0.015	−0.015	−0.012
Age of mother at birth					−0.001	−0.002	−0.002	−0.003
Number of siblings						−0.019	−0.018	−0.015
Rank among siblings						0.006	0.006	−0.003
Gender (ref=Male)							−0.003	−0.004
Constant	3.003	4.080***	4.055***	3.932***	4.529***	4.655***	4.658***	4.258***
Observations	907	907	768	406	405	404	404	387
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Pseudo-R^2	0.003	0.016	0.018	0.039	0.049	0.054	0.054	0.051

[Notes] Equations (25) to (29) consider only individuals whose father was born after 1816. Equation (30) considers only individuals whose father born after 1816 and excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level.

*p<0.10; **p<0.05; ***p<0.01

Appendix D: Placing the results into a broader scope

Historical perceptions on post-emancipation

The main results of this paper are focused in the slaveholders and the effects of compensation money in their living standards. We established that compensation values were usually smaller than the appraised slave-wealth and that slaveholders who earned a bigger share lived, on average, longer. Little was said, however, about the aforementioned effects when compared to the considerable fraction of society who did not own slaves. While the loss of assets is intuitively thought as damaging to the ones who lose it, this feeling was not necessarily unanimous within the Cape society, as this fragment from the South African Commercial Advertiser – the first privately owned newspaper in Cape Colony and the leading English newspaper in the Western Cape at the time – suggests:

"One million sterling (...) is to be added to the Capital of the Colony at once, (...) Passing over immediate and temporary effects, such as the advance of prices and the decline in the rate of the interest, the ultimate effect will be a great improvement in the style of living throughout the Country Districts. We do not refer merely to the Farmers. The numerous class of Laborers will lay out their gains on food, clothes, and furniture to an extent far beyond their present accommodation There will be a great increase of buildings, both in town and country."[34]

The Commercial Advertiser had its editorial based on the humanistic views of its owner – John Fairburn – and it is not surprising that from the very onset of emancipation talks the newspaper took a positive stance towards the freedom of slaves. The *De Zuid-Afrikaan*, on the other hand, more fiercely advocated in favor of slaveholders. During the 1820's when several amelioration laws came into effect, for example, the newspaper fiercely served as a spokesman and apologist for the Afrikaans speaking community of slaveholders. Yet, by the early 1830's the *De Zuid-Afrikaan* shifted its editorial and began to "talk of a general desire of owners for the abolition of slavery" (Giliomee 2003, p. 113).

Indeed, many historians have described the post-emancipation Cape Colony as a dynamic economy a lot due to the compensation money that provided many former slaveholders the much needed liquidity to invest in the most varied sectors ranging from overseas trade to the newly formed Joint Stock Companies. (Hengherr 1953; Liebenberg 1959; Meltzer 1989; Ross 1993; Dooling 2007). Few scholars, however, provide a clear distinction between urban Cape Colony – mostly centered in Cape Town – and the farmlands. Since slavery was also an urban phenomenon in the colony, this distinction is important.[35] Dooling (2007, p. 135) is one of the exceptions. Even though he claims that "it is (...) no longer possible to uphold an older

[34] South African Commercial Advertiser edition of September 11th, 1833 (as cited in Meltzer (1989, pp. 46–47)).

[35] For a thorough description of the urban character of slavery at the Cape and its decline, see Bank (1991).

conservative historiography that saw emancipation as an economic disaster and compensation payments as hopelessly deficient", his claims pertain Cape Town specifically. In the farming districts it is pointed that "the consequences of emancipation on the rural economy and individual slave-owners are harder to ascertain".

Dooling's work allow us to hypothesize that the effects of emancipation and compensation differed considerably between urban and rural areas. To what extent, then, living standards of former rural slaveholders were affected in comparison to their counterparts who did not possess any slave by 1834? Dooling (2007) brings conflicting anecdotal evidence. Some farmers found themselves in a situation of insolvency after 1834 but others benefited from the compensation awarded and managed expand their business even further. A systematic analysis of this phenomenon using quantitative data, however, can be profitable to explore the aforementioned question. We present short quantitative evidence in the subsections below.

Producing the 'control group'

To analyze the extent in which rural slaveholders' living standards changed in comparison to non slaveholders, we append to the dataset already presented on Section 4 a control group who is assumed not to possess any slaves.

To produce the control group, we use the SAF to filter males who were born or baptized in Stellenbosch and that were alive in 1834 but, differently than individuals belonging to the 1st generation, are not present – or were not successfully liked – in the Claims' Records. The control group, therefore, is formed by individuals who are assumed not to possess slaves. Applying these conditions to the SAF yields 1,114 individuals who, in turn produce 3,893 children and 8,669 grandchildren.[36]

Since the assessment of the lifespan for individuals belonging to the control group is the same as the treatment group, we suffer from the same caveat where the analytical sample is considerably smaller than the full sample of our populations of interest. The descriptive statistics of the control group are presented below on Table 14:

[36] It is important to note that the control group is not perfect since unlink-ability is likely non-random (Güell et al. 2014; Rijpma et al. 2018). Moreover, we selected individuals who were born and/or baptized in Stellenbosch but have no way to verify if they still remained in the district in 1834.

Table 14: Descriptive statistics - analytical sample, control group

Variable	Obs	Mean	Std. dev.	Min	Max
1st generation					
Lifespan	223	67.06	14.77	22	100
Year of birth	223	1800.54	14.99	1753	1819
Year of death	223	1867.60	19.26	1834	1918
2nd generation					
Lifespan	1216	54.39	26.39	0	136
Age father at birth	1204	35.97	8.98	17	73
Age mother at birth	960	30.61	7.70	16	67
Nr. of siblings	1216	9.32	3.53	0	18
Rank among siblings	1216	5.75	3.64	1	19
Gender (Male=1)	1216	1.37	0.48	1	2
Year of birth	1216	1830.99	22.25	1741	1894
Year of death	1216	1885.39	34.45	1779	1989
3rd generation					
Lifespan	2321	56.39	26.61	0	129
Age father at birth	2267	34.92	8.51	17	68
Age mother at birth	1478	30.82	8.22	16	67
Nr. of siblings	2321	9.04	3.51	0	18
Rank among siblings	2309	5.37	3.57	1	26
Gender (Male=1)	2321	1.35	0.47	1	2
Year of birth	2321	1861.46	24.28	1782	1922
Year of death	2321	1917.85	36.27	1805	2002

When comparing Table 14 with Table 1 we verify that the mean lifespan of individuals belonging to the 2$^{\text{nd}}$ and 3$^{\text{rd}}$ generations of the control group is bigger than the treatment group's mean. This is mostly because infant mortality between these two populations is different, as evidenced by Figure 7 where the distributional differences of lifespan for all populations of interest are shown.

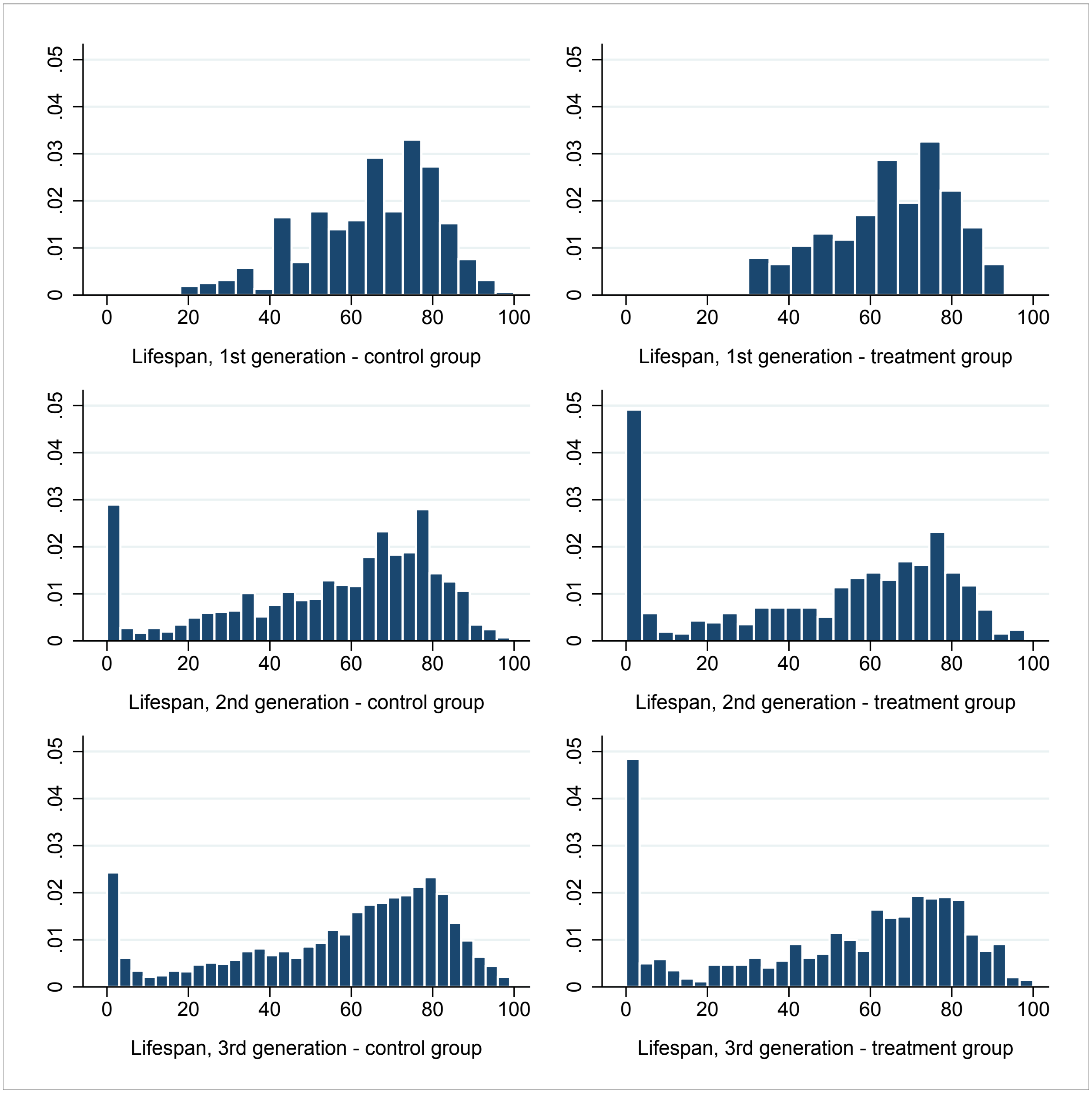

Figure 7: Distribution of lifespan in years for each population of interest

We are not capable of determining the specific reasons behind this phenomenon. We can, however, control for child mortality in our estimates.[37] It is, nonetheless, clear that distributional patterns are similar between groups.

Having this control group, however, despite important in a quasi-experimental design, does not solve the problem of survivorship bias. In fact, both groups – treatment and control – suffer from this issue within the 1st generation. To address such problem, – similarly to what was done with the main results – we divide both

[37]By excluding individuals who had a lifespan smaller than 1 year, the averages between both groups become roughly the same.

groups into 5-year birth cohorts and estimate several models where we impose restrictions on individuals' year of birth.

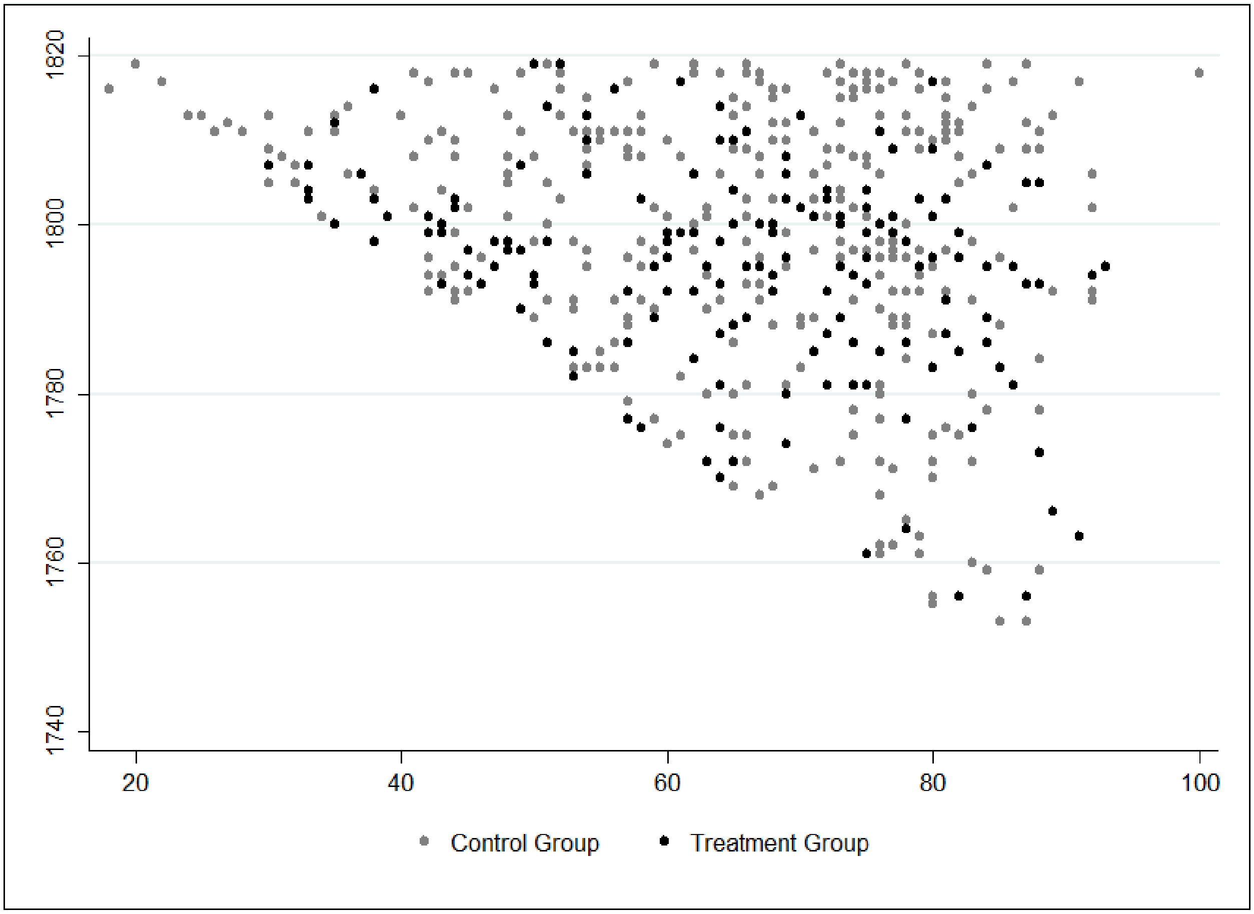

Figure 8: Lifespan in years per year of birth

Finally, it is important to note that we are not able to control for these individuals' wealth since no matching against the tax censuses – i.e. *opgaafrollen* – was possible. The results presented on next section, therefore, control for the average difference[38], birth cohorts and the whole range of genealogical covariates that were included in the previous estimates.

Results

The results are presented below on their reduced forms, similarly to Section 6. The full regression tables can be found on Appendix E. Our findings show that, indeed, the average shortfall had statistically significant effects on the lifespan of former slaveholders and their offspring even when compared to a group of individuals who are assumed not to possess slaves.

[38]Individuals who are assumed not to possess slaves are assigned 0 as their average shortfall per slave.

Table 15: Estimates of the average shortfall on the 1st generation including non-slaveholders

y=Lifespan	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Avg. Shortfall (AS)	−0.001**	0.001	0.002	−0.001*	−0.002***	−0.004***	−0.004***	−0.009***
<1780 x AS		ref	–	–	–	–	–	–
1780-1784 x AS		0.000	ref	–	–	–	–	–
1785-1789 x AS		0.000	−0.001	–	–	–	–	–
1790-1794 x AS		−0.002*	−0.003*	ref	–	–	–	–
1795-1799 x AS		−0.001	−0.002	0.001	ref	ref	ref	ref
1800-1804 x AS		−0.001	−0.002	0.001	0.002***	0.004***	0.003***	0.008***
1805-1809 x AS		−0.002**	−0.003***	−0.000	0.000	0.002**	0.002*	0.007***
>1809 x AS		−0.002	−0.003**	0.000	0.001	0.003***	0.002**	0.007***
Birth Year, continuous	−0.003***							
Observations	342	342	305	256	211	200	194	183
Pseudo-R^2	0.016	0.028	0.016	0.010	0.013	0.022	0.018	0.017

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.The complete regression output concerning these estimates is found on Table 18.

*p<0.10; **p<0.05; ***p<0.01

Among individuals belonging to the 1st generation. The overall effects of the average shortfall are significant across different equations. Similarly to the results without considering non slaveholders, the effects seem to increase in size as we restrict the estimates to younger cohorts. Yet, the size is small. In equation (8), for a £10 increase in the average shortfall, Lifespan is expected to change in 0.09%, with cohort-specific effects being even smaller. This pattern of significance but small results are also verified when analyzing the 2nd generation on Table 16 below:

Table 16: Estimates of the average shortfall on the 2nd generation including non-slaveholders

y=Lifespan	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Avg. Shortfall (AS)	−0.002***	−0.003	−0.002	−0.006**	−0.006**	−0.006**	−0.006**	−0.004*
<1816 x AS		ref	–	–	–	–	–	–
1816-1820 x AS		0.001	ref	ref	ref	ref	ref	ref
1821-1825 x AS		0.001	0.000	0.002	0.002	0.002	0.002	0.001
1826-1830 x AS		0.002	0.001	0.005*	0.005*	0.005*	0.005*	0.005*
1831-1835 x AS		0.000	−0.001	0.004	0.004	0.004	0.004	0.002
1836-1840 x AS		0.001	0.000	0.006*	0.006*	0.006*	0.006*	0.004
>1840 x AS		0.002	0.001	0.004	0.004	0.004	0.004	0.002
Birth Year, continuous	0.000							
Observations	1803	1803	1421	701	700	700	700	670
Birth Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.008	0.015	0.013	0.034	0.034	0.036	0.036	0.030

[Notes] On Equation (9), lifespan is regressed on avg. shortfall, wealth and year of birth as a continuous variable. Equation (10) presents the same variables, but with 5-year birth cohorts. Equation (11) consider only individuals born after 1816. From (12) to (15), the same sample of (11) is considered and the respective covariates are added in the following order: (12) lifespan of father and mother (13) age of father and age of mother at birth, (14) number of siblings and rank among siblings and (15) gender. Equation (16) excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level. The complete regression output is found on Table 19.

*p<0.10; **p<0.05; ***p<0.01

The simple regression of equation (9), already shows a negative and significant effect of the average shortfall on lifespan. This significance however is lost when including cohorts of birth year and their respective interactions on equation (10). The same pattern of (10) is verified on equation (11) even after restricting the sample to individuals who were born after 1816. The significance of the results is retaken from equation (11) onwards, specially after the addition of the lifespan of both the father and the mother as a control. The coefficient remains stable despite the inclusion of several other different genealogical covariates only to suffer a small reduction on equation (16) when infant mortality – i.e. individuals whose lifespan was smaller than 1 year – are excluded from the database.[39]

Among individuals belonging to the 3rd generation very little is to be said except that we do not observe any statistically significant effects of the average shortfall upon their lifespans and that the coefficients – despite not being statistically different than zero – are smaller when compared to both 1st and 2nd generations.

Table 17: Estimates of the average shortfall on the 3rd generation including non-slaveholders

y=Lifespan	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
Avg. Shortfall (AS)	−0.002***	−0.001	−0.002	−0.003	−0.003	−0.003	−0.003	−0.001
<1846 x AS		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AS		−0.003**	−0.002	−0.006	−0.005	−0.005	−0.005	−0.005
1851-1855 x AS		−0.003*	−0.003	−0.002	−0.002	−0.002	−0.003	−0.002
1856-1860 x AS		−0.003*	−0.002	−0.001	−0.001	−0.001	−0.002	0.000
1861-1865 x AS		−0.001	0.001	0.002	0.002	0.002	0.002	0.000
1866-1870 x AS		0.001	0.002	0.003	0.004	0.003	0.003	−0.000
1871-1875 x AS		0.002	0.002	0.004	0.004	0.004	0.004	0.000
>1875 x AS		−0.001	−0.000	−0.000	0.000	0.000	−0.000	−0.002
Birth Year, continuous	0.000							
Observations	3242	3242	2413	1205	1195	1194	1194	1079
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.007	0.013	0.014	0.041	0.046	0.047	0.050	0.033

[Notes] On Equation (17), lifespan is regressed on avg. shortfall, wealth and year of birth as a continuous variable. Equation (18) presents the same variables, but with 5-year birth cohorts. Equation (19) consider only individuals whose father was born after 1816. From (20) to (23), the same sample of (19) is considered and the respective covariates are added in the following order: (20) lifespan of father and mother, (21) age of father and age of mother at birth, (22) number of siblings and rank among siblings and (23) gender. Equation (24) excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level. The complete regression output is found on Table 20.

*p<0.10; **p<0.05; ***p<0.01

We conclude from this brief analysis that in Stellenbosch, aside from some anecdotal successful cases cited on Dooling (2007), evidence suggests that the overall effect of the average shortfall remained negative even when adding a control group who, in theory, would not be directly affected by the economic shock.

[39] By conditioning lifespan to survivorship through infancy – similar to Tables 4 and 12 – we reach a pattern of null results. The implications are the same as already discussed in previous sessions: aside from the effects transmitted through infant mortality, no significant effects of the economic shock can be found on individuals' lifespan after those belonging to the 2nd generation survive their first years of life.

Appendix E: Full regression tables including a control group & transmission mechanisms

Table 18: Regression output for individuals belonging to the 1st generation including non-slaveholders

y=Lifespan	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Shortfall (AS)	-0.001^{**}	0.001	0.002	-0.001^{*}	-0.002^{***}	-0.004^{***}	-0.004^{***}	-0.009^{***}
<1780		ref	–	–	–	–	–	–
1780-1784		-0.088^{**}	ref	–	–	–	–	–
1785-1789		-0.110^{***}	0.004	–	–	–	–	–
1790-1794		-0.113^{***}	0.000	ref	–	–	–	–
1795-1799		-0.120^{***}	-0.007	-0.021	ref	ref	ref	ref
1800-1804		-0.209^{***}	-0.095^{*}	-0.110^{***}	-0.122^{***}	-0.183^{***}	-0.197^{***}	-0.558^{***}
1805-1809		-0.126^{***}	-0.012	-0.026	-0.039	-0.100^{**}	-0.114^{**}	-0.475^{***}
>1809		-0.134^{***}	-0.020	-0.034	-0.047	-0.108^{***}	-0.122^{***}	-0.483^{***}
<1780 x AS		ref	–	–	–	–	–	–
1780-1784 x AS		0.000	ref	–	–	–	–	–
1785-1789 x AS		0.000	-0.001	–	–	–	–	–
1790-1794 x AS		-0.002^{*}	-0.003^{*}	ref	–	–	–	–
1795-1799 x AS		-0.001	-0.002	0.001	ref	ref	ref	ref
1800-1804 x AS		-0.001	-0.002	0.001	0.002^{***}	0.004^{***}	0.003^{***}	0.008^{***}
1805-1809 x AS		-0.002^{**}	-0.003^{***}	-0.000	0.000	0.002^{**}	0.002^{*}	0.007^{***}
>1809 x AS		-0.002	-0.003^{**}	0.000	0.001	0.003^{***}	0.002^{**}	0.007^{***}
Birth Year, continuous	-0.003^{***}							
Constant	10.071^{***}	4.316^{***}	4.202^{***}	4.216^{***}	4.229^{***}	4.290^{***}	4.304^{***}	4.665^{***}
Observations	342	342	305	256	211	200	194	183
Pseudo-R^2	0.016	0.028	0.016	0.010	0.013	0.022	0.018	0.017

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.

*p<0.10; **p<0.05; ***p<0.01

Table 19: Regression output for individuals belonging to the 2^{nd} generation including non-slaveholders

y=Lifespan	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Avg. Shortfall (AS)	-0.002^{***}	-0.003	-0.002	-0.006^{**}	-0.006^{**}	-0.006^{**}	-0.006^{**}	-0.004^{*}
<1816		ref	–	–	–	–	–	–
1816-1820		-0.134^{**}	ref	–	–	–	–	–
1821-1825		-0.174^{**}	-0.040	-0.122	-0.123	-0.126	-0.126	-0.091
1826-1830		-0.098^{*}	0.036	-0.081	-0.083	-0.101	-0.102	-0.123
1831-1835		-0.080	0.053	-0.103	-0.104	-0.117	0.113	-0.054
1836-1840		-0.112^{*}	0.021	-0.150	-0.155	-0.161	-0.155	-0.122
>1840		-0.026	0.108	-0.011	-0.010	-0.022	-0.019	-0.017
<1816 x AS		ref	–	–	–	–	–	–
1816-1820 x AS		0.001	ref	ref	ref	ref	ref	ref
1821-1825 x AS		0.001	0.000	0.002	0.002	0.002	0.002	0.001
1826-1830 x AS		0.002	0.001	0.005^{*}	0.005^{*}	0.005^{*}	0.005^{*}	0.005^{*}
1831-1835 x AS		0.000	-0.001	0.004	0.004	0.004	0.004	0.002
1836-1840 x AS		0.001	0.000	0.006^{*}	0.006^{*}	0.006^{*}	0.006^{*}	0.004
>1840 x AS		0.002	0.001	0.004	0.004	0.004	0.004	0.002
Birth Year, continuous	0.000							
Lifespan, father				0.004^{**}	0.004^{**}	0.005^{**}	0.005^{**}	0.004
Lifespan, mother				0.004^{**}	0.004^{**}	0.004^{**}	0.005^{**}	0.003^{**}
Age of father at birth					-0.001	-0.002	-0.002	0.000
Age of mother at birth					0.002	0.001	0.001	0.000
Number of siblings						-0.010	-0.010	-0.007
Rank among siblings						0.004	0.004	0.003
Gender (ref=Male)							-0.042	-0.055
Constant	3.823^{***}	4.054^{***}	3.920^{***}	3.472^{***}	3.471^{***}	3.559^{***}	3.570^{***}	3.648^{***}
Observations	1803	1803	1421	701	700	700	700	670
Birth Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.008	0.015	0.013	0.034	0.034	0.036	0.036	0.030

[Notes] Equations (11) to (15) consider only individuals born after 1816. Equation (16) considers only individuals born after 1816 and excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the 1^{st} generation level.

*p<0.10; **p<0.05; ***p<0.01

Table 20: Regression output for individuals belonging to the 3rd generation including non-slaveholders

y=Lifespan	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
Avg. Shortfall (AS)	−0.002***	−0.001	−0.002	−0.003	−0.003	−0.003	−0.003	−0.001
<1846		ref	ref	ref	ref	ref	ref	ref
1846-1850		0.114***	0.090	0.142	0.159	0.162	0.169	0.088
1851-1855		0.050	0.054	−0.025	0.001	0.007	0.021	−0.044
1856-1860		0.018	0.017	−0.101	−0.062	−0.055	−0.042	−0.079
1861-1865		0.053	0.043	−0.040	−0.001	0.010	0.019	−0.012
1866-1870		0.057	0.033	−0.040	0.021	0.031	0.047	0.070
1871-1875		−0.035	−0.047	−0.171	−0.108	−0.098	−0.084	−0.039
>1875		0.036	0.022	−0.075	0.004	0.004	0.012	−0.004
<1846 x AS		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AS		−0.003**	−0.002	−0.006	−0.005	−0.005	−0.005	−0.005
1851-1855 x AS		−0.003*	−0.003	−0.002	−0.002	−0.002	−0.003	−0.002
1856-1860 x AS		−0.003*	−0.002	−0.001	−0.001	−0.001	−0.002	0.000
1861-1865 x AS		−0.001	0.001	0.002	0.002	0.002	0.002	0.000
1866-1870 x AS		0.001	0.002	0.003	0.004	0.003	0.003	−0.000
1871-1875 x AS		0.002	0.002	0.004	0.004	0.004	0.004	0.000
>1875 x AS		−0.001	−0.000	−0.000	0.000	0.000	−0.000	−0.002
Birth Year, continuous	0.000							
Lifespan, father				0.004**	0.004**	0.005***	0.004***	0.003*
Lifespan, mother				0.004***	0.003**	0.003***	0.003***	0.003***
Age of father at birth					−0.008*	−0.009*	−0.009*	−0.007
Age of mother at birth					0.002	−0.000	0.000	−0.001
Number of siblings						−0.011	−0.010	−0.010
Rank among siblings						0.009	0.009	0.010
Gender (ref=Male)							−0.079**	−0.047*
Constant	3.486***	4.007***	4.019***	3.590***	3.751***	3.861***	3.889***	4.078***
Observations	3242	3242	2413	1205	1195	1194	1194	1079
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.007	0.013	0.014	0.041	0.046	0.047	0.050	0.033

[Notes] Equations (19) to (23) consider only individuals whose father was born after 1816. Equation (24) considers only individuals whose father born after 1816 and excludes individuals whose lifespan was smaller than 1 year. All estimates use clustered standard errors at the 2nd generation level.

*p<0.10; **p<0.05; ***p<0.01

Table 21: Regression output for individuals belonging to the 1st including crop type

y=Lifespan	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Shortfall (AS)	0.001***	0.004*	0.005*	0.002	−0.002	−0.004*	−0.005***	−0.010***
Total Tax/Slave	−0.021	−0.034*	−0.035*	−0.010	0.084*	0.041	0.050	0.101
<1780		ref	–	–	–	–	–	–
1780-1784		−0.119	ref	–	–	–	–	–
1785-1789		0.036	0.074	–	–	–	–	–
1790-1794		−0.079	0.024	ref	–	–	–	–
1795-1799		−0.033	0.080	0.010	ref	ref	ref	ref
1800-1804		−0.236	−0.126	−0.179	−0.326**	−0.474***	−0.588***	−0.921***
1805-1809		0.107	0.211	0.160	0.063	−0.084	−0.199	−0.553***
>1809		−0.219	−0.106	−0.194	−0.384**	−0.548***	−0.669***	−1.004***
<1780 x AS		ref	–	–	–	–	–	–
1780-1784 x AS		0.001	ref	–	–	–	–	–
1785-1789 x AS		−0.001	−0.003	–	–	–	–	–
1790-1794 x AS		−0.002	−0.003	ref	–	–	–	–
1795-1799 x AS		−0.003	−0.004	−0.000	ref	ref	ref	ref
1800-1804 x AS		−0.000	−0.002	0.001	0.007**	0.010***	0.011***	0.015***
1805-1809 x AS		−0.005**	−0.006**	−0.003	0.001	0.004*	0.005*	0.010***
>1809 x AS		−0.000	−0.002	0.002	0.008***	0.011***	0.012***	0.017***
None	ref	ref	ref	ref	ref	ref	ref	ref
Grain only	−0.044	−0.124	−0.113	−0.188*	−0.238*	−0.257*	−0.269**	−0.298**
Wine only	0.035	−0.135	−0.164*	−0.142	−0.187	−0.086	−0.089	−0.134
Grain & Wine	0.021	0.222**	0.212**	0.212**	0.176	0.134	0.140	0.224*
None x AS		ref	ref	ref	ref	ref	ref	ref
Grain only x AS		0.002	0.001	0.002	0.003	0.004*	0.003*	0.004*
Wine only x AS		0.003**	0.003**	0.003*	0.005***	0.003	0.003	0.004**
Grain & Wine x AS		−0.004**	−0.004**	−0.004**	−0.004**	−0.004**	−0.004**	−0.005***
Birth Year, continuous	−0.007***							
Constant	15.872***	4.176***	4.074***	4.148***	4.311***	4.463***	4.582***	4.906***
Observations	130	130	117	91	67	64	61	55
Pseudo-R^2	0.050	0.073	0.057	0.041	0.088	0.093	0.103	0.108

[Notes] Estimates (3) to (8) refer to individuals born after 1780, 1790, 1795, 1796, 1797 and 1798 respectively.

*p<0.10; **p<0.05; ***p<0.01

Appendix F: Matching rule

The matching process was dividing in two stages. In the first stage, we matched individuals between the Claims' Records and the *Opgaafrollen* using last names and names. We named the resulting dataset CR-OGR. The second stage consisted on matching the CR-OGR to the South African Families Database (SAF). The procedure adopted to match the CR-OGR to the SAF is described below.

a) If the name and last name of the individual matched perfectly between the CR-OGR and the SAF and this observation is unique in both datasets, it is a direct match.

b) If the name and last name of the individual matched perfectly between the CR-OGR and the SAF but this observation is not unique in the SAF, check for the genealogical information provided in the CR;

b.1) If the genealogical information can be inferred in the CR and cross-checked successfully with the SAF, it is a direct match;

b.2) If the genealogical information cannot be inferred or cannot be cross-checked successfully, seek for the farm's name in the CR;

b.2.1) If the name of the farm (defined as *'woonplek'* in the CR) can be inferred and cross-checked successfully, it is a semi-direct match;

b.2.2) If the name of the farm can be inferred in the CR but cannot be cross-checked successfully in the SAF, seek for the farm's location;

b.2.2.1) If the farm's location in the CR matches with either the place of birth or place of death of the individual in the SAF, it is a weak match;

b.2.2.2) If the farm's location in the CR does not match with either the place of birth or place of death of the individual in the SAF, it is an impossible match. The observation will not be carried further in the process.

b.2.3) If the name of the farm cannot be inferred in the CR, it is an impossible match. The observation will not be carried further in the process.

c) If the individual's name matched perfectly between the CR-OGR and the SAF, but this is not true for his/her last name, this is an impossible match;

c.1) Exceptions are made for last names that clearly refer to the same family but contain spelling differences that can be attributed to language differences between English and Afrikaans (e.g. Berg/Bergh, Bernhardi/Bernhardie, Liebentrouw/Liebertrau, Roux/Rous). These cases were treated as direct matches.

d) If the individual's last name matched perfectly between the CR-OGR and the SAF, but this is not true for his/her name, this is an impossible match;

d.1) Exceptions are made for names that contain spelling differences that can attributed to style (e.g. Jan/Johan/Johannes). If the observation is unique, it is treated as a semi-direct match. If this observations is not unique, then the procedure described in **b)** is followed.

e) If the name or last name of the individual cannot be found in the SAF, it is an impossible match. The observation will not be carried further in the process.

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