Expropriation with partial compensation: Slaveholders' reparations and intergenerational outcomes*

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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 had a negative effect on the longevity of both the generation of slaveholders that experienced the shock and their children, but not for grandchildren. We speculate on the mechanisms for this intergenerational persistence.

Keywords. intergenerational, wealth shock, longevity, persistence, Cape Colony

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1 Introduction

Can wealth shocks have intergenerational health consequences? We now 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 tied together; as Cutler et al. (2008, p. 36) conclude: 'Some dimensions of SES cause health, some are caused by health, and some are mutually determined with health; some fall into all three categories at once'.

Experimentation is understandably difficult. Randomizing the amount of wealth individuals receive to test its causal effects on health raise an ethical dilemma. Researchers have, in response, resorted to several quasi-experimental designs. The most common designs involve 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 forthcoming) and policy changes (Duflo 2000; Case 2004; Frijters et al. 2005; Snyder and Evans 2006; Erixson 2017). Most of this literature deals with exogenous shocks to wealth through price fluctuations or cash windfalls. Few studies deal with exogenous shocks in the form of property losses (González et al. 2017) and even fewer deal with the relationship between health and wealth shocks in historical settings (Bleakley and Ferrie 2016). Consequently, the question of how later-life outcomes respond to the loss of an income-generating asset and how persistent these effects are over time, remains open.

To cast light on this question, we turn to the 1834 emancipation of slaves in the British Cape Colony. The emancipation represents a point of paramount significance in the history of the marginalized populations of the British Empire – including the Cape Colony – in their fight for freedom. Its social and historical significance cannot be overstated. On the fringes of this process, however, an event of interest to economic historians arises. Slaveholders were awarded cash compensations for the slaves they had in their possession. Historical records show that they received, on average, a compensation between 40-50% of the value of their slaves. From an economic standpoint, therefore, the emancipation process represents the loss of property or, put differently, the loss of an income-generating asset. The potential to verify any health-related outcomes generated by the uncompensated values arises from the linkage of three different datasets that enable us to consolidate compensations claims together with tax returns and genealogical

records for our population of interest.

We begin by digitizing the claim duplicates kept in the Cape Town Archives. These documents contain the names of 8,452 slaves residing in the Stellenbosch district, as well as information about their gender, age, place of birth, owner, value, and, importantly for us, the value of each slave. We combine this information with the compensation paid to each slaveholder, available from the UCL Online Archive. The compensation received by the owner is generally well below the aggregated value of the slaves owned. We claim this difference is random, and we support this with extensive archival support. We then use the average uncompensated value per slave to measure the shock undergone by the former slaveholders. This is our variable of interest. To measure the shock, we link the average uncompensated value per slave with tax returns of 1834. The tax returns were collected by the colonial government and consisted of individual-level information regarding each slaveholders' livestock, agricultural output, related capital, and taxation. Since our study also aims to verify the intergenerational effects of property losses, we also link the aforementioned datasets to the South African Families database (SAF). The SAF registers are genealogical records of all settler families in South Africa between 1652-1910 (Cilliers and Fourie 2018). Through the SAF, we can append intergenerational information regarding the settler's year of birth, death, number of siblings, rank among siblings, gender, and the age of the parents at birth. As far as we are aware, this is the first study that investigates the effects of property losses on individual's life duration in an intergenerational context.

Our estimation strategy consists of a Poisson regression, which is a generalized linear model that allows us to fit the distribution of life duration more effectively. We estimate the effects of the average uncompensated value per slave holding on the life duration of individuals who were slaveholders in 1834 (1^{st} generation) and extend the analysis to their children (2^{nd} generation) and grandchildren (3^{rd} generation). We find the uncompensated value to be negative and significant among individuals belonging to the 1^{st} generation, meaning that slaveholders that received a greater proportion of compensation from their expropriated property, were more likely to live longer.

The effects verified within the 1^{st} generation are passed upon the 2^{nd} generation, however, this is only true as long as infant mortality is not isolated from the analysis. When the effects of infant mortality are excluded from the base estimates, we are not able to verify any statistical significant effects of the economic shocks upon the life duration of the 1^{st} generation's offspring.

In other words, only survivorship of infants was influenced by the economic shock, but as long as an individual survives infancy, their life duration is independent from the effects of the uncompensated values. By the 3rd generation, the effects of the exogenous shock seem to have completely dissipated.

The economic significance of the uncompensated values is systematically smaller than any of the relevant demographic covariates we can control for. This is in line with the literature suggesting that 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 in the short and medium run. Our intergenerational analysis seems to be in disagreement with this claim. This could be due to the nature of the shock we are studying: we are not looking at an exogenous wealth *gain*, but rather, the *loss* of an income-generating asset that was the most important source of wealth for our population of interest. The intergenerational persistence of a wealth loss, in comparison to a gain, may also point to the mechanism at play. We explore these ideas below.

2 Literature Review

Quasi-experimental approaches help to analyze the effects of wealth on health. They can be divided into three categories: (1) lottery winnings, (2) price/asset fluctuation and (3) policy changes. Lindahl (2005) uses lottery prizes as a source of exogenous variation in wealth. He finds that increased income is associated with a range of health-related outcomes, such as a reduction in self-reported illnesses, lower levels of obesity, and increased life expectancy. The results suggest that health-related consumption is one of the transmission mechanisms through which variations in wealth causally explain variations in health. However, when testing the relationship between increased income and mortality, Lindahl (2005, p. 162) finds that 'income has a statistically insignificant smaller impact on health, as compared to the estimate for all ages. Hence, income is not more protective against bad health for older people'.

Apouey and Clark (2015) conduct a similar study while exploring lottery winners linked to the British Household Panel Survey. They measure the effects of increased income on general health status, mental health, physical health problems, and health behaviors of individuals. They find significant effects on mental health but fail to detect any effects of the increased income on

self-assessed overall health, which is similar to the results found by Gardner and Oswald (2007), who use a similar dataset and context. Apouey and Clark (2015), however, show that winners of lottery prizes bigger than £500 were more likely to engage in unhealthy behaviors such as social drinking and smoking.[1] They conclude that the verified unhealthy consumption – which is detrimental to individual's health – explains the insignificant effect of increased incomes found on overall health status. Van Kippersluis and Galama (2014) expands on the effects of wealth shocks in the consumption of healthy and unhealthy goods. They suggest that individuals' budget constraints are relaxed under positive wealth shocks, which allows for an increase of both healthy and unhealthy consumption. Since unhealthy consumption imposes a health toll – modeled as a health cost intrinsic to unhealthy consumption – the consumption of healthy goods tends to increase relatively more.

Cesarini et al. (2016) use a considerably larger dataset of lottery players in Sweden covering a wide range of socioeconomic strata. They are able to append intergenerational information to the panel of lottery players. They find that among players the effect of the wealth shock on mortality and health care utilization is close to zero. For the players' children, they estimate effects of a similar magnitude on scholastic achievement, cognitive and non-cognitive skills. The overall pattern, therefore, is of null results despite some exceptions.[2] Cesarini et al. (2016) conclude that any causal relation between health and wealth 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).

One of the inherent shortcomings of using lottery winnings as a source for exogenous variations in wealth is that one can only observe *positive* shocks. Exogenous price fluctuations, in contrast, can yield both positive and negative shocks. Studies utilizing housing or stock markets allow researchers to measure to the macro-effects of shocks on individual's health from both positive and negative wealth variations. Fichera and Gathergood (2016), for example, exploit large exogenous changes in the housing market in the United Kingdom to examine the impact of wealth on health. The home ownership rate in the UK is around 64%, covering a large – but not necessarily representative – part

[1] This result holds even when controlling for general health, which is obtained from the General Health Questionnaire, a survey that consists of 12 questions where the responses range from a 0 to 3 scale. The result is the sum of the scores. A result of 36 indicates the best psychological health while 0 indicates the worst.

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

of the population. The authors distinguish overall health from psychological health and find that the effect of wealth on the former is positive, significant and persistent over a 10-year period. On psychological health, however, the authors find no significant effects that can be traced to wealth variations. The authors find that individual health is more responsive to price gains compared to losses, even though it is unclear if these effects are homogeneous across different age-cohorts. Still, given that real estate is rarely a source of monthly income, it seems reasonable that negative shocks do not have a profound psychological impact since housing-wealth is usually perceived as an asset that will increase its value over time despite short-run fluctuations. This scenario might create an incentive for individuals to adopt a long-run perspective.

Using stock market changes, Schwandt (forthcoming) finds results that contradict the results of the housing market. The author exploits the booms and busts of the stock market in the USA, and finds that a 10% wealth loss has an effect of 2 to 3% of a standard deviation both in physical and mental health. This seems to be support earlier studies that use stock market changes as proxy for wealth shocks (Cotti et al. 2015; Engelberg and Parsons 2016; Boen and Yang 2016). The differences between these results and the ones produced by Fichera and Gathergood (2016) show how much distinct contexts (UK vs. USA) and the type of shock used, matters. While both housing and stock markets have positive and negative shocks, individuals' expectations towards ownership of these assets can differ substantially. In fact, these results support what Chandra and Vogl (2010, p. 1228) concluded when summarizing the literature: 'no universal rule governs the relationship between income and adult health'.

Similar ambiguity is found in studies that use policy changes as quasi-natural experiments to model exogenous shocks on individuals' wealth or income. Snyder and Evans (2006) is one example that offers counter-intuitive results. They use a 1970s legislation change in the United States that created a 'notch' on the Social Security system. Individuals born before January 1, 1917 benefited from higher pension payments compared with individuals who were born afterwards. The authors exploit this 'notch' – i.e. a discontinuity – to examine the impacts of increased old-age incomes on mortality among elders. Snyder and Evans (2006), however, find that individuals born after January 1st, 1917 – who received lower pension payments – had significantly lower mortality rates compared to their counterparts. The authors attribute the results to the increased post-retirement work effort engaged by younger cohorts. This suggests, therefore, that employment among the elderly might have beneficial health effects.

In a different context, Case (2004) finds the opposite. She uses cross-sectional data on self-reported health status of South Africans who became beneficiaries of the old-age pension. The benefit, on average, more than doubled the incomes of elderly black South Africans in 1993.[3] The results indicate that increased incomes in the form of old age pensions improved the health status of all members of income-pooled households where at least one beneficiary of the old age pension lived. Case's findings corroborate earlier evidence by Duflo (2000), who had found similar effects of old-age pensions in South Africa.

It is not only the elderly that are affected by wealth shocks. Frijters et al. (2005) investigate the causal effect of income changes on health satisfaction among East and West Germans following reunification. The fall of the Berlin Wall was completely unanticipated and 'resulted in large income transfers to virtually all of the population of East Germans', and as such, health-related outcomes among the treatment group of East Germans could be attributed to capital transfers when comparing it to the control group formed by West Germans (Frijters et al. 2005, p. 999). The authors find statistically significant – but small – effects on health satisfaction deriving from the increased income experienced by East Germans. Along similar lines, Erixson (2017) uses the repeal of the Swedish inheritance tax to instrument an exogenous variation on individual's wealth. The new legislation ruled that inheritances received after December 17$^{\text{th}}$, 2004 were exempt from taxation, and then correlated this discontinuity to hospitalization rates, the use of sick leave benefits and mortality. While increased wealth had positive short and medium run impacts on adult health, the effects had limited in its duration.

A brief analysis of the literature reveals a great variety of wealth shock measurements. Several studies indicate that causal relations on the wealth-health gradient should be taken with skepticism (Frijters et al. 2005; Snyder and Evans 2006; Cesarini et al. 2016; Erixson 2017), while others draw conclusions that imply more direct causation (Duflo 2000; Case 2004; Lindahl 2005; Fichera and Gathergood 2016; Schwandt forthcoming). What remains unclear, though, is the long-run, intergenerational effect of these wealth shock. There are good reasons why this question remains largely unanswered: measuring intergenerational effects over more than one life-time requires exceptionally rich data.

Bleakley and Ferrie (2016) is the exception. They investigate the Cherokee Land Lottery in

[3]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. Among elderly blacks, therefore, this represents the access to a resource previously unavailable to them.

the US state of Georgia in 1832. In this lottery – in which virtually every adult male participated – large tracts of land were distributed to winners. The authors are able to append intergenerational information to the dataset of winners. Unfortunately, however, no health-related outcomes are measured. Instead, the later-life outcome analyzed is the fertility of winners and, for the offspring of winners, the authors measure human capital through literacy rates alongside wealth and income. The findings suggest that winners had slightly more children compared to non-winners, but were not more likely to send them to school. In fact, among children and grandchildren of winners, the outcomes of the lottery had no significant impact on their literacy rates, wealth or income.

Because Bleakley and Ferrie use property as an exogenous shock, and because people tend to be more dependent on this asset for their livelihood, at least during the 19^{th} century, one could expect transmission mechanisms from wealth to health to be visible and strong. Yet, this is not completely consistent with their findings since only relatively small changes in fertility are associated with the gain of an income-generating asset and no intergeneration outcomes (at least in education) can be observed deriving from the shock. Should we expect this behavior to be similar when analyzing the *loss* of an income-generating asset?

González et al. (2017) might be a good starting point. The authors analyze the 1864 emancipation of slaves in the United States and potential links between the loss of slave-wealth for collateral versus the likelihood of slaveholders to open business in the post-emancipation period. They conclude that the existence of slave wealth causally explain variations in the likelihood of opening business, which was lower post 1864, suggesting that slave-wealth was a better and more decisive collateral for credit compared to any additional income that wage labor could have yielded in the post-emancipation period. It is true, however, that González et al. (2017) are not looking into any later-life outcome *per se*, but their study is the first one to our knowledge that attempts to model the loss of an income-generating asset while exploring causal links derived from this phenomenon.

We fill this gap. We use the 1834 emancipation of slaves in the British Cape Colony to measure the impact of partial compensation on the life duration of former slaveholders and their offspring up to two generations after the shock. We believe that using an individual's life duration as a proxy for health offer some advantages. Firstly, longer life durations can be directly interpreted as improvements in health and, consequently, wealth. 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 allowing better comparability between individuals belonging to different

generations and/or countries.[4] If variations in wealth can causally explain variations in later-life outcomes, we speculate that the differences between compensation shares – provided that they are exogenous – might be behind observable differences between slaveholders' life durations.

Compensation for the expropriation of capital are not unique to the British Empire, and they were not homogeneous across all British colonies. In the next section we, therefore, provide a brief history of abolitionism in the United Kingdom, followed by its ramifications in the Cape Colony of South Africa.

3 Historical Background

This section begins with a overall description of the abolitionist movement in the United Kingdom and its practical implications. Following this general historical background, we will explain the nuances in the cash compensations and explore their suitability as a source of exogenous variation.

3.1 Abolitionism in the United Kingdom

The case Somerset v. Stewart[5] in 1772 is oftentimes interpreted as jurisprudence signaling the emancipation of all slaves in Britain. Its implications, indeed, shifted the political momentum in favor of emancipation in an irreversible manner. This case – sometimes through misinterpretations of the original decision – influenced abolitionist movements in several parts of the British Empire (Drescher 1987; Davis 1999; Carey et al. 2004).

One of the movements that came into existence shortly thereafter was the Society for the Abolition of the Slave Trade. It was created in 1787 by a group of 12 abolitionists who sought to raise public awareness of the horrific treatment of slaves by slave traders and holders. The aim was to pressure representatives in the British Parliament to take a definitive stance on the issue. The campaign was successful and, in 1807, celebrated a political victory when the Parliament voted in favor of the Slave Trade Act 1807 that outlawed slave trade within the British Empire.[6]

After the suppression of the trade, the emancipation of slaves became the primary item on the abolitionists' agenda. The Anti-Slavery Society arose as the most prominent movement working

[4] The use of life duration as a proxy for other later-life outcomes – e.g. occupational mobility – is already acknowledged in the literature. Some examples are Piraino et al. (2014) and Parman (2016).

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

[6] A comprehensive discussion on the parliamentary struggle behind the Act's approval can be found in Farrell (2007).

towards this goal in the United Kingdom. However, the Anti-Slavery Society had dissent among its ranks as to how emancipation should take place. Two competing groups emerged from this debate, one advocating in favor of a gradual process through the ratification of amelioration laws leading towards emancipation and another in favor of immediate action. Ultimately a gradual approach was adopted as evidenced by the numerous ordinances that appeared in some British overseas territories allowing slaves to get married, prohibiting married slaves to be separated by sale, demanding children under 10 years of age to be kept together with their parents, restricting corporal punishment, regulating the number of working hours, among other amelioration requirements (Dooling 2007; Spence 2014).

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 perceived 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 emancipation properly (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 once envisaged. This momentum was captured not only for the abolitionists themselves, but also for other groups who saw an opportunity to push for an agenda of free trade by disrupting the monopoly the West Indies enjoyed while destabilizing the core of its mode of production.[7] The confluence of these political forces culminated on the

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

Slave Emancipation Act of 1833, with its effects starting on August 1st, 1834 (Williams 2014).

3.2 Cash compensations as an exogenous shock in Cape Colony

The Slave Abolition Act of 1833 determined how emancipation would come into effect and established an 'apprenticeship' period of 6 years together with a financial compensation for slaveholders.[8] The financial 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'.

When placing the aforementioned events in chronological order, one might believe that any slaveholder within the British Empire was capable of anticipating the steps towards emancipation and adapt accordingly. For farmers in the Cape Colony, however, the future was uncertain. Hengherr (1953, p. 37) suggests 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 – different from their counterparts in the Caribbean – were mostly small landowners that possessed considerably fewer slaves. More than 700,000 slaves were emancipated in 1834 but fewer than 40,000 were located 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.

The emancipation scheme only became clearer after 1834, but the exact compensation slaveholders would receive was unknown. It was later 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, OCC henceforth. In April 2nd, 1835 the OCC released the general rules of the compensation scheme[9]. The procedure consisted on the fulfillment of two documents: (1) Slave Returns and (2) Form of Claim.

aspects of emancipation.

[8]The emancipation scheme was not identical in 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.

[9]Cape Archives (CA, henceforth) 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. April 2nd, 1835.

The first document aimed at assessing the Colony's slave-wealth.[10] To ensure the completion of the task, the OCC assigned several appraisers who covered the colonial territory determining the value of all enslaved population. The Slave Returns classified slaves according to sex and occupation. The occupation of a slave was divided between 'predial' – i.e. related to the employment in agriculture – which was subsequently divided between 'attached'/'unattached' and 'non-predial'. Within each category, sub-classifications were made in accordance 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, during the process, a version of the Slave Returns would be produced for each slaveholder. In total, more than 38,000 slaves were valued and total slave-wealth in the Colony was estimated to be £2,800,000 (Hengherr 1953; Meltzer 1989; Dooling 2007).

The second document – the Form of Claim – was fulfilled by the slaveholder and consisted of a simple form where the claimant identified himself and declared the number of slaves he/she possessed at the time. This form was cross-checked with the Slave Returns and if the OCC deemed the information to be correct, the claimant had the right to be compensated.

By the time this information was laid out by the OCC, Cape's slaveholders had hopes that their slave-wealth would be fully compensated. Still, many slaveholders were unsure as to *when* the payments would be made. Jacob Wouter du Preez, a farmer from the Swellendam district was among this group. In a letter[12] sent in 1834 to Benjamin D'Urban – at the time the governor and commander in chief of the Cape Colony – he explains how a 'succession of misfortunes' has induced him to give over his estate for sequestration. His property consisted of '18 valuable slaves, who if disposed of under the present crisis would not only hurt his creditors' but also himself.

The claimant was hoping that the compensation money would enable him to settle some of his most pressing debts, however, neither him nor the creditors knew when compensation would arrive: '(the) memorialist had therefore to beg that Your Excellency will be pleased to inform him 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

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

[11] For predial slaves, the classification consisted in 'Head People', 'Tradesmen', 'Inferior Tradesmen', 'Field Labourers' and 'Inferior Field Labourers'. For non-predial slaves, the occupations could be classified as 'Head Tradesmen', 'Inferior Tradesmen', 'Head People employed on Wharfs, Shipping, or other Avocations', 'Inferior People employed on Wharfs, Shipping, of other Avocations', 'Head Domestic Servants' and 'Inferior Domestics'.

[12] CA Memorials, vol 6, CO 3973. Letter from J.W. du Preez to Benjamin D'Urban. October 6th, 1834.

their benefit and his ones'. Mr. du Preez does not elaborate in his letter about the 'succession of misfortunes' that caused him such financial distress; however, it is possible to speculate that he had mortgaged some – or even all – of his slaves and expected to settle his debts through surpluses produced by the slaves themselves.

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 advancement of £400,000 worth of compensation money to settle outstanding mortgages where slaves and their labor were used as collateral.[13] The request was later 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 allegedly possessed by them. Furthermore, the claims were calculated based solely on the sex and occupation of the slaves, meaning that slaves within the same category were considered homogeneous and interchangeable (Draper 2008). This process generated 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.

In addition to the aforementioned scheme, the compensation could only be claimed in London, which imposed a further toll. This was directly against the claimants' expectations who hoped the compensation to 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 as the most 'signally unjust, as well as *offensively arbitrary*, proceedings we ever heard of, and is a transaction discreditable to any government laying claim to fair and honest dealing with the public creditor.'[14]

The process of repayment was also fraught with difficulty. Several payment delays – some claims were only settled as late as 1845 – contributed to an environment of uncertainty. Around £250,000 worth of claims were later contested and, despite limited success, it suggests that the evaluation and subsequent compensation processes was far from straightforward (Hengherr 1953). As Dooling (2007, p. 149) puts, '(...) post-emancipation settlement was born of negotiation'.

Considering that slaves were at the heart of the productive activity in the British Cape Colony

[13] CA Memorials, vol 7, CO 3974. Letter from slaveholders to Benjamin D'Urban. April 7th, 1834

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

and their role went beyond their employment in agricultural production – for example, slaves in the Cape Colony were also used as collateral for loans (Swanepoel 2017), as leasing assets (Green 2014) as domestic servants (Fourie 2013b) and were also employed in semi-skilled and skilled occupations on farms (Fourie 2013a; Green 2014) – it is not surprising that the period immediately following emancipation was characterised by uncertain labour relations and production activity. Our use of the average uncompensated value per slave as variable-of-interest comes from the understanding that (1) slaveholders until the very onset of abolition were unsure about the emancipation scheme and (2) the difference between the evaluation of the slaveholders' slave assets and the amount received in London is random. While the former is certain, we also find no evidence to falsify the latter. This is not to say, however, that the compensations awarded or the slave-wealth were randomly generated. They were not. But the difference between these two variables is random since the criteria that quantified of each variable was considerably different. While the compensation was based solely on sex and occupation of the slave, slave-wealth was based on market prices that considered a wider range of characteristics on its valuation such as age, place of origin, height and weight alongside, of course, sex and occupation. Anecdotal accounts presented above support our claim; so, too, does the empirical analysis – we show that the difference between the value and the amount received is uncorrelated to observable characteristics. To do this, we first discuss our sources of data.

4 Data

The data for this study comes from three different sources that were manually linked to produce a unique dataset from where all our estimates derive: (1) duplicates of the valuation records matched to the compensation amounts, (2) tax returns (also known as *opgaafrollen*) and (3) South African Families Database (SAF).

The duplicates of the slave valuation and compensation records are found in the Cape Town Archives.[15] They contain information on 8,452 slaves who resided in the Stellenbosch district together with their names, gender, age, place of birth, holder and values. Some basic genealogical information about the slaveholder is also available.[16] The slaves were distributed among 989

[15] See H.F. Heese, *Amsterdam tot Zeeland. Slawestand tot Middestand?, 'n Stellenbosse slawegeskiedenis, 1679-1834*, Stellenbosch, 2016. The lists of compensation claims for slaves were transcribed from the original sources, although copies of these original lists are now available on the LDS FamilySearch website.

[16] Despite the paucity thereof, this information is key for the linkage between these individuals and

slaveholders. Because the compensation scheme was a function of slaves' characteristics – and not purely the total number of slaves – we work with the average uncompensated value 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 information to 1,244 individuals who were registered in the tax returns of the Stellenbosch district in 1834. The tax returns were collected annually by the British colonial authorities and contained information regarding each resident's stock, agricultural output, related capital and taxation (Fourie and Green 2018).

The matching process was made using individual's first and last names.[17] We classified four different match types: perfect matches, semi-perfect matches, weak matches and impossible matches. We only considered matches falling in the first two categories to avoid using weak linkages in our estimates.[18] This procedure yielded 551 unique observations.

The final source used was the South African Families database (SAF). This dataset registers the genealogical records of all settler families in the Cape Colony between 1652-1910. The SAF allows us to append information regarding the settlers' year of birth, death, number of siblings, rank among siblings, gender and life duration of parents. Each individual in this dataset possess a unique ID that can be linked to the ID of his/her relatives. This, then, allows the linkage of slaveholders in 1834 to their children and grandchildren. For brevity, we will refer to the slaveholders in 1834 as the 1st generation, while their children and grandchildren will be the 2nd and 3rd generations respectively.

The 551 individuals who were linked between the compensation records and the tax returns, therefore, belong to our 1st generation. Among these 551 slaveholders, 314 were matched to the SAF. This group, when linked to their offspring, yields 1,814 children (2nd generation) and 2,458 grandchildren (3rd generation) and make up our remaining populations of interest or, put

the South African Families database where more complete genealogical information is found. In the compensation records we can find the slaveholder's first and last name together with the name of their father. In some cases, wife's name is also provided.

[17] The matching process was conducted by hand. The strategy employed is be found in the Appendix F.

[18] Perfect matches refer to individual's whose first and last names were unique and perfectly matched between the compensation records and the tax returns. Semi-perfect matches followed a similar understanding but, in this case, we verified minor spelling differences in individual's last names (e.g. Rous-Roux, Liebentrau-Liebentrouw, Bergh-Berg). Weak matches refer to individual's whose combination of name and last name was not unique and, given the lack of additional information, the matching of these individuals could not be made to a reasonable degree of confidence. Impossible matches refer to individuals who were not found in the tax returns and, therefore, cannot be matched.

differently, the treatment group.[19]

Historical records, however, often lack complete and consistent micro-level information. Analytical samples therefore tend to be considerably smaller compared to the population of interest. In our study, the large number of missing values for both birth and death year of individuals produce a constraint in our assessment of individual's life durations. Our sample size, therefore, is limited by the availability of data for this specific variable. Below, Table 1 presents the descriptive statistics of the analytical sample for our treatment group[20].

Table 1: Descriptive Statistics - Analytical Sample

Variable	**Obs**	**Mean**	**Std. dev**	**Min**	**Max**
1st Generation					
Avg. Unc. Value (£)	130	60.26	25.29	5.69	163.57
Life Duration	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					
Life Duration	577	47.68	30.72	0	105
Age Father @ birth	532	34.97	9.12	20	71
Age Mother @ 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					
Life Duration	907	50.03	30.71	0	110
Age Father @ birth	871	35.09	8.12	19	64
Age Mother @ 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

[19]While it seems intuitive to imagine that sample sizes should grow exponentially across generations, it is important to note that there are at least 4 forces that prevent this from happening in our sample. Firstly, around 8% of individuals belonging to the 2nd generation died before the age of 16, rendering them unlikely to produce offspring. Secondly, not all individuals recorded in the dataset produce any offspring at all. Thirdly, 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. Lastly, migratory movements outside the Cape Colony produce some level of attrition in the sample. See Cilliers (2016) for more details on the SAF.

[20]The descriptive statistics for the full sample can be found on Table 8 in Appendix A.

for the 1st generation.[21] This is reflected in the observed mean life duration of the 1st generation when compared to the 2nd and 3rd generations. The result is that individuals born between 1740-1760 can only be observed if they had long life durations, which is reflected in Figure 1.

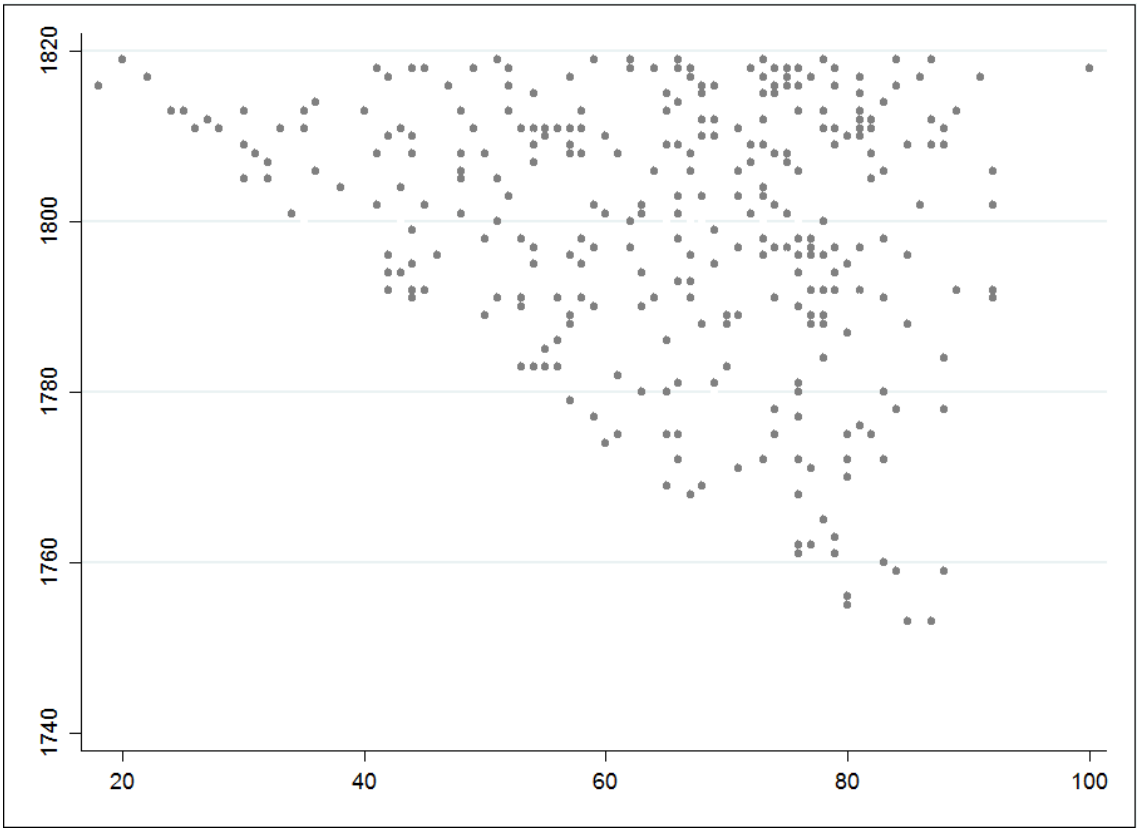

Figure 1: Life Duration in years per year of birth

A consequence of this is that the average life duration for older cohorts will be systematically bigger when compared to younger ones. Restricting our population of interest into younger cohorts, therefore, allow us to have a wider distribution of life durations and more intra-cohort variability. This procedure is not necessary for 2nd and 3rd generations since we are able to observe complete life cycles from infancy to elderhood as Figure 2 illustrates. In the next section, we discuss our model specification and estimation strategies.

[21] There are 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.

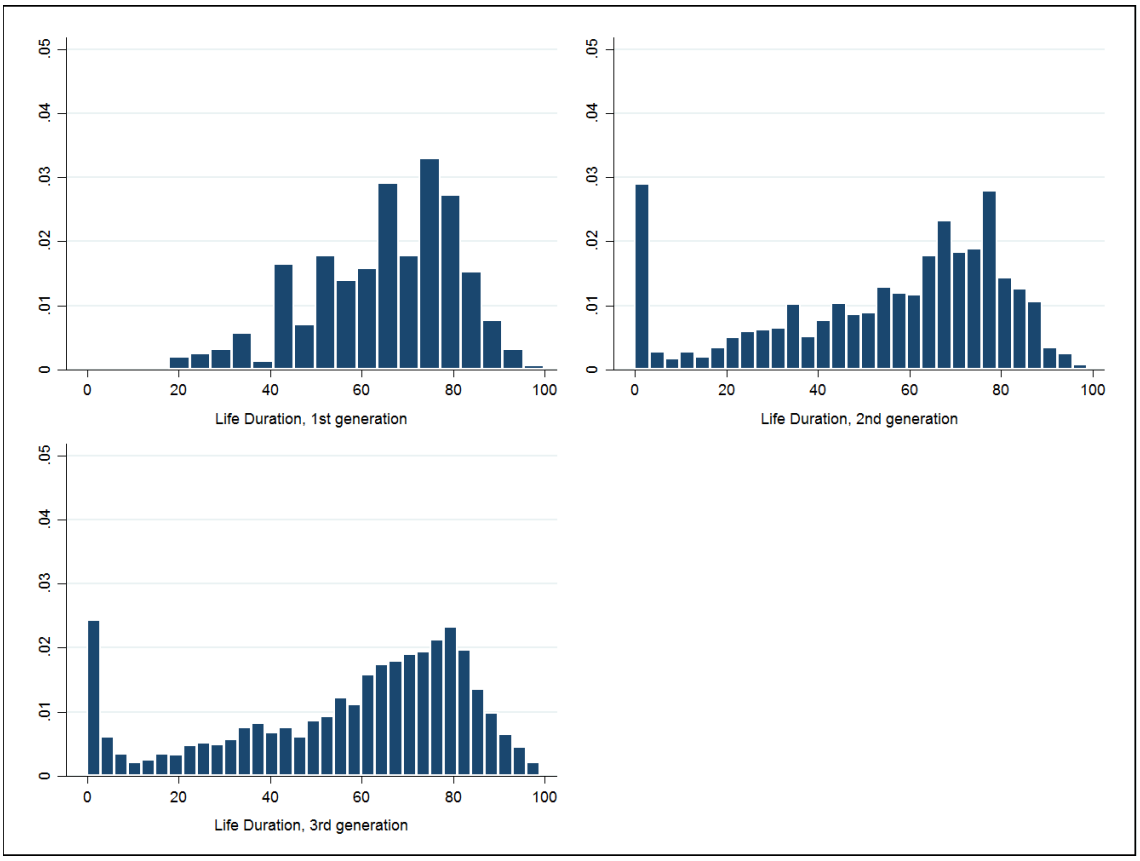

Figure 2: Distribution of Life Duration in years for each population of interest

5 Methods

To assess how the difference between the value of the slaves and the compensation slaveholders received explains variations in the life duration of slaveholders, we use the slaveholder's average uncompensated value per slave[22], wealth and range of genealogical covariates.[23] Because our population of interest is dispersed across a long period of time, we also add 5-year birth cohorts interacted with the average difference to capture cohort-specific effects. The basic functional form can be written as:

$$y_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{1i} X_{2i} + \beta_4 X_{4i} + \sum_z \beta_5 X_{zi} + \mu_i \tag{1}$$

[22] From now on, average difference, for brevity.

[23] As presented on Table 1, the controls are: Age of mother and father at birth, number of siblings, rank among siblings, life duration of mother and father and individual's gender.

The subscript i represents each slaveholder. X_{1i} and X_{2i} represent each individual's average uncompensated value per slave and birth cohort respectively. $X_{1i}X_{2i}$ represents the interaction between the aforementioned terms. X_{4i} indicates individual's wealth 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 life duration of slaveholders and their offspring by subtracting the year of birth from the year of death. All life durations 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. By doing so, Equation 1 is altered and takes an exponentiated form to ensure positive outcomes:

$$y_i = e^{\beta_1 X_{1i}} + e^{\beta_2 X_{2i}} + e^{\beta_3 X_{1i} X_{2i}} + e^{\beta_4 X_{4i}} + e^{\sum_z \beta_5 X_{zi}} + e^{\mu_i} \tag{2}$$

Equation 2 will be estimated for each generation separately. A visual inspection of Figure 3 prevents us from drawing any *a priori* expectations as life duration seems to behave quite independently from the average difference across generations. An assumption in Equation 2, however, is that individual's average difference is uncorrelated with wealth. Even though historical records do not suggest that the compensation scheme was biased towards richer slaveholders, an assessment of such relationship is imperative for our empirical strategy. We estimate, therefore, the average difference as a function of the slaveholders' wealth alongside the characteristics of the slaves in his/her possession such as slave's place of origin, sex and age cohort as shown below:

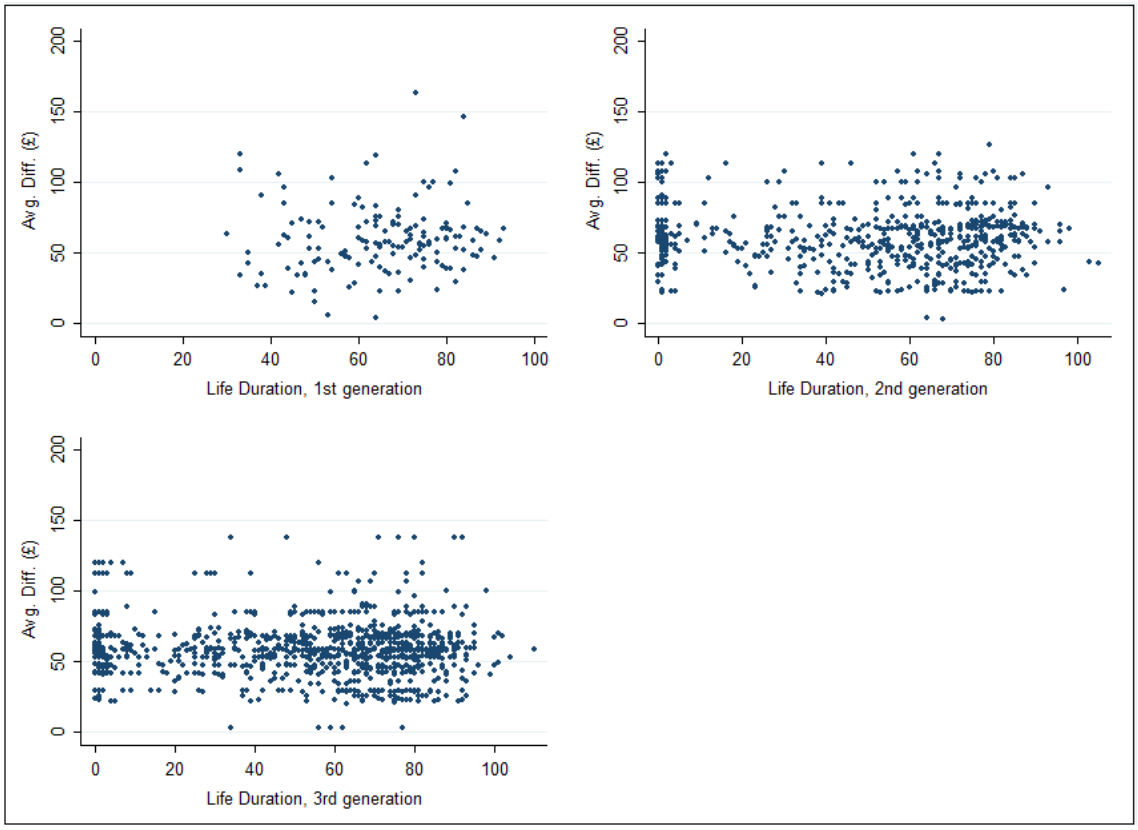

Figure 3: Relationship between the Average Uncompensated Values and Life Duration across generations.

$$X_{1i} = \beta_4 X_{4i} + \sum_k \beta_k X_{ki} + \xi_i \tag{3}$$

The covariate X_{ki} represents the range k of slaves' characteristics in possession of each individual i. ξ is the error term. Our findings for Equation 2 can be found in Table 7 in Appendix B. They suggest that the Average Difference is uncorrelated with slaveholders' wealth regardless the functional form of both variables.[24] These results are in line with the plotted average difference and total tax in Figure 4. While dispersion is greater at the lower end of total tax's distribution, the average values do not seem to differ substantially.

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

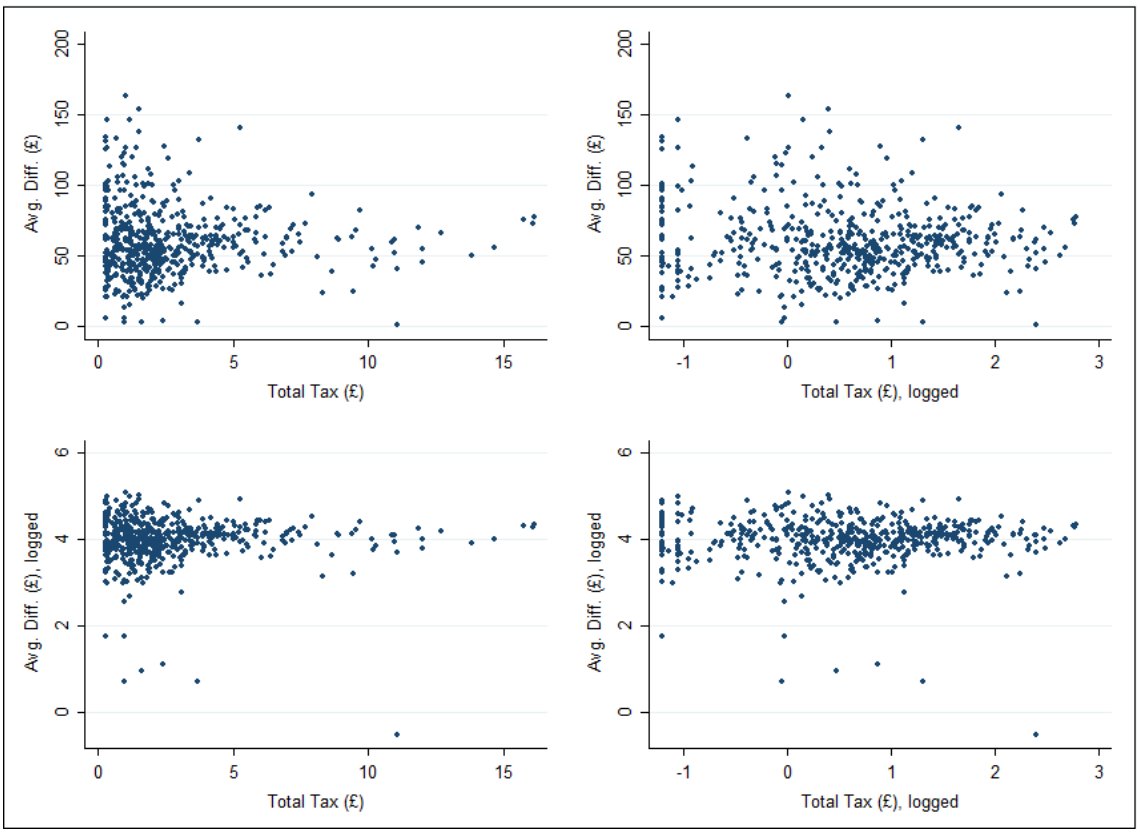

Figure 4: Relationship between the Average Uncompensated Values and Total Tax in several functional forms.

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. 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 2 can be re-written, as:

$$\log y_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{1i} X_{2i} + \beta_4 X_{4i} + \sum_z \beta_5 X_{zi} + \mu_i \tag{4}$$

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 coefficient estimated in the next sections should be understood, therefore, as a the percentage change on slaveholders' life duration for a one unit change in any given covariate.

6 Results

To facilitate the visualization of our results, we present a simplified version of our estimates through Tables 2, 3, 4 and 5. These tables only contain information regarding the average difference without other relevant covariates or statistics.[25]

Table 2 offers a simple functional form where life duration is regressed on individual's average difference, 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, divided our sample in 5-year birth cohorts and, from equations (2) to (8) we successively restrict our sample to cohorts where the variability of life duration is greater. The closer a cohort is to 1834, the wider the distribution of life duration of slaveholders.[26] By doing so, we minimize the effects deriving the from bias produced by older cohorts and at the same time verify that the main effect of average difference becomes negative, significant and progressively bigger as we increase life duration's variability through cohort restriction. Figure 5 provides a visual representation of the results while considering the marginal effects derived from Equation (5) to (8).

Table 2: Estimates of the Average Uncompensated Value on the 1st generation.

y=Life Duration	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Diff. (AD)	0.001**	0.003	0.003	0.002	−0.001	−0.006***	−0.007***	−0.009***
<1780 x AD		ref	–	–	–	–	–	–
1780-1784 x AD		0.000	ref	–	–	–	–	–
1785-1789 x AD		−0.002	−0.002	–	–	–	–	–
1790-1794 x AD		−0.001	−0.002	ref	–	–	–	–
1795-1799 x AD		−0.002	−0.002	−0.001	ref	ref	ref	ref
1800-1804 x AD		−0.001	−0.001	0.000	0.003**	0.008***	0.009***	0.010***
1805-1809 x AD		−0.005*	−0.005*	−0.004*	−0.000	0.004**	0.005***	0.007**
>1809 x AD		−0.000	−0.000	0.001	0.004*	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.The complete regression output concerning these estimates is found on Table 8.

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

[25] The complete regression table can be found on Appendix X, Tables 8, 9, 10 and 11.

[26] As already explained in Section 5 through Figures 2 and 1.

By taking Equation (8) as benchmark, we verify that a £10 increase in the average uncompensated value reflects in a 0.9% decrease in the expected life duration. An average difference of £60, therefore, implies a mean reduction of 0.54% on the former slaveholders' years of life. The average life duration of these farmers was 64 years – as presented on Table 1 – the estimates, therefore, suggest that the uncompensated values had an average impact of 0.3 years 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 robust to different functional forms. Treating life duration as a continuous variable did not significantly affect neither the direction nor the size of the coefficients. The conclusions are similar after logging the average difference 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.[27]

As an additional robustness check, we also add to our analysis a group of individuals who are assumed to reside in Stellenbosch district in 1834 and did not to possess slaves. To produce this group, we use the SAF to select males who were born or baptized in Stellenbosch and that were alive in 1834. These individuals, in turn, were not successfully linked to the Claims' Records, meaning that the likelihood of them being slaveholders is considerably diminished. More than one thousand individuals were met the proposed criteria and, through the SAF we were capable of determining their offspring, meaning that for every generation we are capable of testing the validity of our results against a group of presumed non-slaveholders.[28] The conclusions derived from the estimates presented on Table 8 concerning the 1st generation are robust to the inclusion of this group into the analytical sample.

The robustness of the results presented on Table 2 help us answer the long-term effects of negative economic shocks. We are, however, also interested in the intergenerational impact of such shocks. Table 3 serves as a starting point as we look into the effects of the average uncompensated value on the life duration of the 2st generation. There, the average difference is significant in most of the relevant specifications. While Equations (9) and (10) consider the full sample of matched individuals belonging to the 2nd generation, Equation (11) and beyond only considers individuals born after 1816. This control is important as this subpopulation would be older than 18 years in 1834, making them more likely to have their own farms and, in some cases, slaves. If

[27] Ratio between the value received as compensation and the assessed slave-wealth.

[28] A detailed analysis of the presumed non-slaveholders is made on Appendix D. There, we also present the results of our estimates when this group is included into our analytical sample.

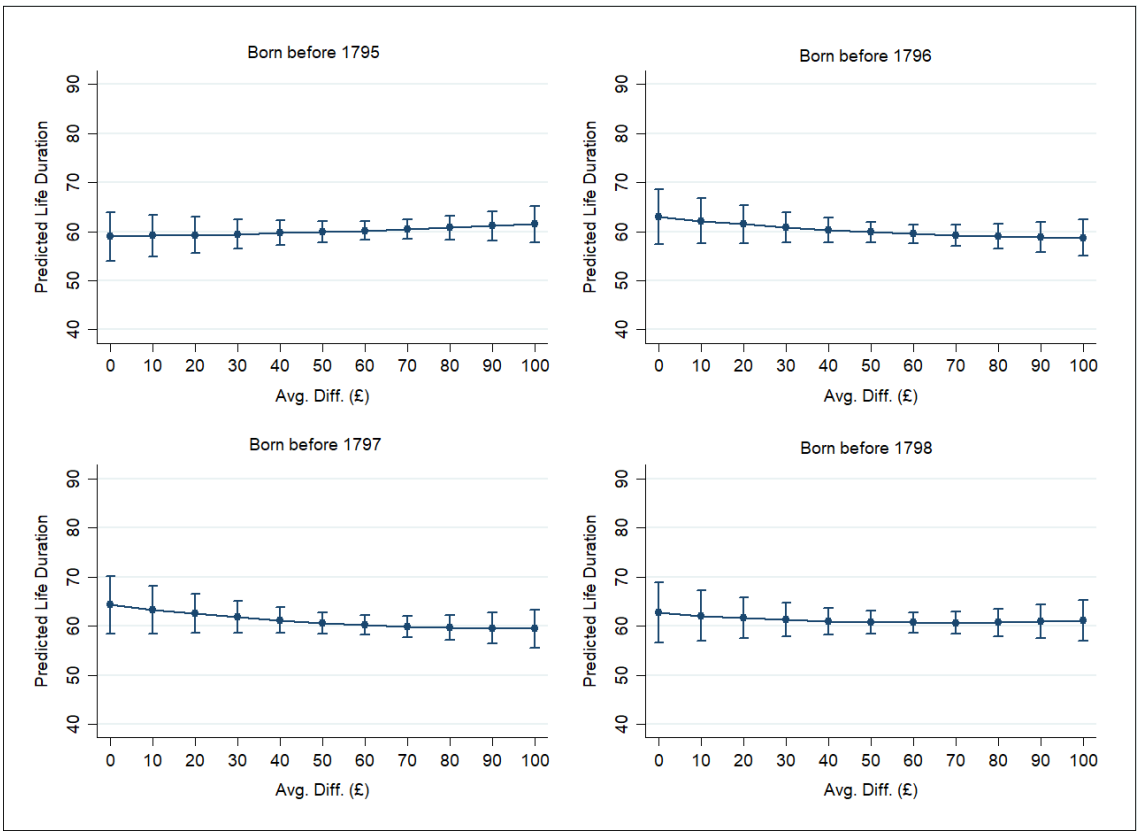

Figure 5: Marginal Effects of the Average Uncompensated Value between different cohorts, 1st generation

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

Immediately 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 offset the main effect to a great extent. Individuals who were born before the emancipation have bigger net effects when compared to individuals who were born after 1834. These findings suggest that better compensation schemes had a greater impact among older cohorts within the 2nd generation.

When analyzing the results of average difference together with other covariates (Table 17,

Table 3: Estimates of the Average Uncompensated Value on the 2nd generation.

y=Life Duration	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**
Avg. Diff. (AD)	−0.002	−0.007	−0.007	−0.017***	−0.017***	−0.018***	−0.017***
<1816 x AD		ref	–	–	–	–	–
1816-1820 x AD		0.000	ref	ref	ref	ref	ref
1821-1825 x AD		0.011	0.010*	0.011	0.011	0.012	0.012
1826-1830 x AD		0.003	0.003	0.011	0.011	0.013*	0.012
1831-1835 x AD		0.005	0.005	0.017**	0.016**	0.017***	0.016**
1836-1840 x AD		0.004	0.004	0.015*	0.015*	0.015**	0.014*
>1840 x AD		0.005	0.005	0.013*	0.014*	0.014**	0.013**
Birth Year, continuous	0.001						
Observations	577	577	488	239	239	239	239
Father 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), life duration is regressed on avg. diff, 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) life duration of father and mother (13) age of father and age of mother at birth, (14) number of siblings and rank among siblings and (15) slaveholder's gender. All estimates use clustered standard errors at the individuals' fathers level. The complete regression output is found on Table 9.

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

Appendix C), we can also observe that the life duration of both mother and father appear as significant determinants of life duration. The genealogical covariates – despite statistical significance – are usually bigger than the net effect produced by the average difference. These results are in line with Frijters et al. (2005) and Erixson (2017) who suggest that despite significant effect of wealth shocks on individual's later life outcomes, the size of the coefficient was small enough for the effect to be labeled as marginal.

Table 3, however, says little about the transmission mechanisms. While resource allocation within the household and changing patterns of consumption of the 1st generation might indirectly affect their offspring, it is important to note that infant mortality strongly influences the average life duration of the 2nd generation as Figure 2 in Section 4 already suggested. It is imperative, therefore, to establish the effects of the average uncompensated value when isolating the population of interest from the effects of infant mortality. We do so in Table 4 below:

Equation (16) excludes individuals whose life duration was smaller than one year while Equation (17) also excludes individuals who did not live past the second year. As we continue to limit the minimum life duration to 5, 10, 15, 20 and 25 years of age, the significance of the economic shock disappears at the same time that the size of the coefficients diminish. The estimates in Table 4 suggest that the observed effect of the average difference is channeled through infant mor-

Table 4: Regression output for individuals belonging to the 2nd generation

y=Life Duration	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**
Avg. Diff. (AD)	-0.020^{***}	-0.010^{**}	-0.007	-0.007	-0.005	-0.005	-0.005
<1816 x AD	–	–	–	–	–	–	–
1816-1820 x AD	ref	ref	ref	ref	ref	ref	ref
1821-1825 x AD	0.016	0.003	0.009^{*}	0.009	0.006	0.006	0.006
1826-1830 x AD	0.018^{**}	0.007^{*}	0.005	0.005	0.002	0.002	0.002
1831-1835 x AD	0.018^{**}	0.011^{**}	0.007	0.008	0.005	0.005	0.005
1836-1840 x AD	0.015^{*}	0.007	0.006	0.006	0.004	0.005	0.002
>1840 x AD	0.017^{**}	0.007	0.007	0.007	0.006	0.006	0.005
Observations	223	193	178	177	174	170	166
Father 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

tality but, once individuals survive infancy, they are not likely to be affected by the economic shock undergone by their fathers. While our dataset does not allow us to establish what was the driver behind infant mortality in the period post-emancipation, we can demonstrate that infants were more vulnerable than non-infants during the economic duress imposed by the loss of capital. Figure 6 provides a visual representation of the marginal effects from Equation (16) to (21). The marginal effects start unusually big in the first two graphs but it quickly normalizes into a pattern similarly to the one verified in the 1st generation. The strong downward slope at the beginning can be fully attributed to the effects of infant mortality. It is worth noticing that the inclusion of a group of individuals assumed not to be slaveholders do not promote considerable changes into the analysis aforementioned. Similarly to the 1st generation, these results can be found on Appendix D.

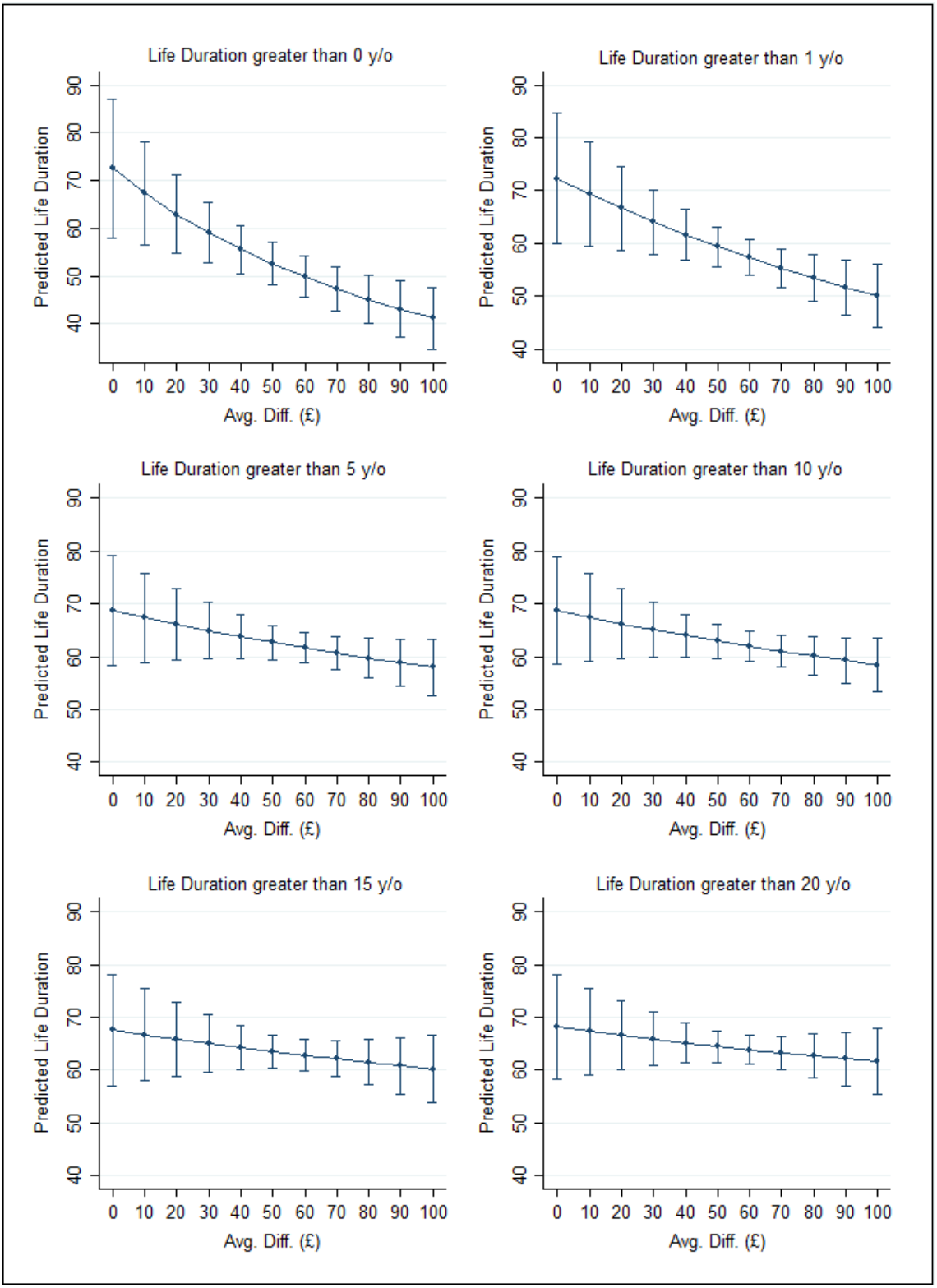

Figure 6: Marginal Effects of the Average Uncompensated Value between conditional to a given minimum life duration, 2st generation

From an intergenerational perspective, the effects of the uncompensated values, 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 has been verified through Table 5.

Table 5: Estimates of the Average Uncompensated Value on the 3rd generation.

y=Life Duration	**(23)**	**(24)**	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**
Avg. Diff. (AD)	−0.002	−0.002	−0.003	−0.010	−0.014	−0.012	−0.012	−0.001
<1846 x AD		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AD		0.001	0.005	0.013	0.015	0.014	0.014	0.006
1851-1855 x AD		−0.006	−0.005	0.001	0.005	0.003	0.003	−0.004
1856-1860 x AD		−0.000	0.002	0.008	0.012	0.011	0.011	0.005
1861-1865 x AD		−0.001	0.001	0.012	0.015	0.014	0.014	0.001
1866-1870 x AD		0.001	0.003	0.009	0.013	0.012	0.012	−0.000
1871-1875 x AD		−0.002	−0.000	0.004	0.009	0.008	0.008	−0.007
>1875 x AD		0.001	0.002	0.009	0.014	0.013	0.013	0.003
Birth Year, continuous	0.001							
Observations	907	907	768	406	405	404	404	342
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.003	0.016	0.018	0.039	0.049	0.054	0.054	0.051

[Notes] On Equation (23), life duration is regressed on avg. diff, wealth 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 whose father was 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) life duration 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. Equation (30) excludes individuals whose life duration was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level. The complete regression output is found on Table 11.

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

The size of average difference for the 3rd generation is systematically smaller when compared to 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 effects that different compensation schemes would have on the life duration of our populations of interest are completely worn off and not felt among the 3rd generation.

Some determinants of life duration, however, seem to persist between generations such as the life duration of mother and father that is significant for both generations 2 and 3. Alongside the genetic component, total number of siblings and individual's gender also appear as determinants of life duration for individuals belonging to the 3rd generation. Considering that births within the 3rd generations are mostly within the period commonly defined as the South African fertility transition,[29] slightly different patterns are justified between generations 2 and 3.

[29]See Cilliers and Mariotti (2018)

7 Transmission Mechanisms

Our analysis shows that the effects of the uncompensated values affected two groups of individuals: the 1st 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 will explore potential transmission mechanisms behind such findings.

The loss of slaves meant the loss of an income-generating asset. Slaves generated incomes for slaveholders mostly – but not exclusively – through their employment in agriculture. By examining the tax composition of our analytical sample through the *opgaafrollen* it is possible to verify that more than 84% of the farmers did not declare any income derived from non-farming activities. It means that the vast majority of our sample is composed of individuals who strictly derived their income from farming. The loss of labor in such context is significant since slaves were a major component of the workforce in the farms.

The Emancipation Act ruled that slaves had to serve their former masters for 6 additional years after 1834 in what was labeled an 'apprenticeship' period. At the Cape, however, this period was shortened to four years given the inability of the local government to enforce such legislation. Isaac van der Merwe from Worcester, for example, claimed that by 1834 his former slaves 'were all in disorder' and 'in open resistance to lawful commands'.[30] In fact, many slaves left their former masters as early as the Emancipation 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).

By the time the 'apprenticeship' period was over, Dooling (2007, p. 116) observes that 'the freed slaves left their masters *en masse*' and proceeds to describe the days immediately following the emancipation as a 'large-scale withdrawal of labour from the wheat and wine estates of the Western Cape'. While it is plausible that many slaves left their masters for good, their freedom was still limited. Giliomee (2003) points out that the colonial government did not make any land available for small-scale farming and the majority of the slaves had very few options but to remain farm labourers. H. Calderwood, an eye-witness to the emancipation day, commented how it was 'ridiculous to talk of them (the emancipated slaves) refusing to work when they know very well they must either work or starve'.[31]

[30] As quoted in Worden (1994).
[31] As quoted in Giliomee (2003).

Despite the conflicting views as to whether freed slaves would make a reliable form of labour or not, 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 roughly 50%. From 1834 up to 1842, barley and wheat production dropped by a third. It was only by the mid-1840's that the agricultural output was back at its pre-emancipation levels (Giliomee 2003; Dooling 2006).

Can this drop in output for a short period explain the effects we observe over the uncompensated values on former slaveholders life durations? To some extent this is plausible, especially when considering the literature exploring the causes of infant mortality. Theoretical frameworks constructed to synthesize the determinants of infant mortality such as Mosley and Chen (1984) and Norren and Vianen (1986) emphasize the malnutrition-infection syndrome. It happens when infants are incapable of developing their immunologic system due to scarce breastfeeding – which is, in turn, caused by the mother's nutritional deprivation – and, consequently, are more susceptible to be infected by diseases.

While impossible to determine the exact consequences of the Cape's output decline between 1834 and 1842 on the residents nutritional status with the data structure in our possession, it is evident that in societies where individuals mostly derive their income from farming-related activities a shock in output is likely to have direct consequences on economic and demographic variables such fertility and mortality. As Hedefalk et al. (2017, p. 1041) points 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 scarce enough, could affect societies across generations. Interestingly, however, our results suggest that only the 1^{st} generation was affected by the uncompensated values while individuals who survived the first two years of age among the 2^{nd} generation escaped seemingly unscathed. Here, it is important to bring the concept of slaves as assets to look for further insights.

As already explored, slaves were income-generating assets. The Cape Colonial economic system strongly relied in what Dooling (2007, p. 128) classifies as 'networks of indebtedness and patronage'. Modern research on the role of slaves in the Cape Colony strongly suggests that slaves were perceived by their former owners as capital investment (Fourie 2013a; Fourie 2013b; Green 2014; Du Plessis et al. 2015; Swanepoel 2017). As capital investments, slaves 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 uncompensated values – together with difficulties to hire labor – as important causes for their financial situation (Shell 1994; Dooling 2007). Social and moral networks provided many former slaveholders with a safety net, yet, many faced years of economic duress following emancipation, specially the highly indebted ones. Anecdotal evidence shows that some farmers could be mortgaged up to 160% the price of their estate (Theal 1891; Hengherr 1953; Ross 1993; Dooling 2007). Worden (2017) suggests that the compensation money alleviated the short-term economic duress imposed by the new mode of production – i.e. paid labor – but clearly these effects were of limited duration. This view is corroborated by Dooling (2007, p. 138) when showing that between 1841 and 1843, for example, more than 60 farmers in the Western Cape declared bankruptcy.

Economic duress and debt are considered two important triggers for psychological stress as demonstrated by the literature exploring stress process models (Gallo and Matthews 2003; Drentea and Reynolds 2015). Complementing these findings, other studies find a clear relationship between psychological stress and physical health through different enzymatic and (Hajat et al. 2010; Cohen et al. 2012; Boen and Yang 2016) consumption patterns (Catalano et al. 2011; Black et al. 2015) both leading to worse overall health status. Because the declines in output experienced by the Cape Colony only happened in the short-term and the effects of the uncompensated value do not affect 2nd generation individuals who lived past the second year, we speculate that another transmission mechanism must be at play since output declines would not explain such selectivity of generations affected by the uncompensated values.

In closing this section, it is important to emphasize that our database does not allow for a conclusive investigation on all potential transmission mechanisms or how big was the influence of each source of economic and psychological distress in 1834. It is, nevertheless, clear that the understanding of slaves as both labour and assets allow us to infer mechanisms through which farming and finance shocks may affect longevity (intergenerationally).

8 Conclusions

We contribute to the literature that explores the effects of exogenous shocks on later-life out-

comes 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' life durations.

Our empirical strategy reveals that slaveholders who were subjected to worse compensation schemes lived shorter lives if they belonged to the 1st generation. We believe that the loss of slave-wealth and the high level of debt verified in the Stellenbosch district are important components behind the economic duress experienced by the former slaveholders in the years post-emancipation. Debt and economic duress are particularly relevant elements on stress process models, suggesting that the verifiable effects on slaveholders life duration were channeled through their psychological health. It is important to note, however, that shortly after emancipation the Cape's agricultural output suffered a short-term decline. While impossible to determine the impact of this decline on the life duration of former slaveholders with the current data availability, we recognize that living standards in farming societies are directly linked with agricultural output.

These effects, however, were mostly overcome by the 2nd generation. All significant results found among these individuals were driven by infant mortality. Life duration conditional on infancy survival was largely unaffected by the economic shock. Among individuals belonging to the 3rd generation, no effects could be verified neither on life duration or infant mortality. Results concerning fertility for all generations revealed a series of null results.

Cohort-specific analysis reveal some differences on how the shock was absorbed within our populations of interest, however, these differences are rather small in absolute terms, preventing us from conclusively stating that certain cohorts had a clear and sizable advantage against others.

Another contribution of this paper can be placed within the history of slavery and its emancipation. While our population of interest in this study consisted of slaveholders and their offspring – which certainly limits the overall scope of our findings – we can, in the minimum, derive some important reflections from this analysis.

As already discussed in Section 3, the emancipation process looks somewhat counterintuitive to modern readers. Slaveholders, and not the enslaved, were the ones who received reparations as a result of the emancipation process. With that in mind, it is possible to argue that the emancipation of slaves affected the slaveholders severely. The greater the loss, the poorer 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 suggest that while this benefit did not prevent slaveholders based at the Cape Colony to completely offset the negative effects of the wealth loss, it certainly allowed the transition away from slave economy to be less traumatic – and, consequently, without serious political resistance.

Appendix A: Descriptive Statistics - Full Sample

Table 6 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 6 has some minor inconsistencies such as the minimum Age Father and Age Mother @ birth concerning the 2nd generation – which is too low by any standards – alongside the negative figures of the Average Uncompensated Value for the 1st 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 Father @ birth refers to only one observation. If excluded from the sample, the minimum becomes 16 years. Similarly, the minimum for Age Mother @ 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 uncompensated Value, 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 Father and Age Mother @ at birth and the Average Uncompensated Values would be justified on demographic and historical grounds respectively, that was not needed since life duration, 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 Uncompensated Value. Those were, then, dropped from the analytical sample.

Table 6: Descriptive Statistics - Full Matched Sample

Variable	**Obs**	**Mean**	**Std. dev**	**Min**	**Max**
1st Generation					
Avg. Unc. Value	551	55.09	32.68	-187.28	163.57
Life Duration	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					
Life Duration	654	47.97	30.18	0	105
Age Father @ birth	1378	34.83	9.00	11	71
Age Mother @ 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					
Life Duration	1034	51.09	30.17	0	110
Age Father @ birth	1910	35.12	8.43	19	68
Age Mother @ 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 7: Endogeneity Check

	y=Avg. Diff.		y=log(Avg. Diff.)			
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 @ 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 Uncompensated Value per slave holding (Avg. Diff.) was lower than £21.

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

Appendix C: Full regression tables, main results.

Table 8: Regression output for individuals belonging to the 1st generation

y=Life Duration	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Diff. (AD)	0.001^{**}	0.003	0.003	0.002	−0.001	-0.006^{***}	-0.007^{***}	-0.009^{***}
<1780		ref	–	–	–	–	–	–
1780-1784		−0.058	ref	–	–	–	–	–
1785-1789		0.027	0.079	–	–	–	–	–
1790-1794		−0.093	−0.041	ref	–	–	–	–
1795-1799		−0.069	−0.017	0.010	ref	ref	ref	ref
1800-1804		−0.195	−0.114	−0.117	-0.229^{**}	-0.462^{***}	-0.583^{***}	-0.710^{***}
1805-1809		0.089	0.141	0.168	0.056	−0.177	-0.298^{**}	-0.425^{**}
>1809		−0.206	−0.154	−0.127	-0.239^{*}	-0.472^{***}	-0.593^{***}	-0.720^{***}
<1780 x AD		ref	–	–	–	–	–	–
1780-1784 x AD		0.000	ref	–	–	–	–	–
1785-1789 x AD		−0.002	−0.002	–	–	–	–	–
1790-1794 x AD		−0.001	−0.002	ref	–	–	–	–
1795-1799 x AD		−0.002	−0.002	−0.001	ref	ref	ref	ref
1800-1804 x AD		−0.001	−0.001	0.000	0.003^{**}	0.008^{***}	0.009^{***}	0.010^{***}
1805-1809 x AD		-0.005^{*}	-0.005^{*}	-0.004^{*}	−0.000	0.004^{**}	0.005^{***}	0.007^{**}
>1809 x AD		−0.000	−0.000	0.001	0.004^{*}	0.009^{***}	0.010^{***}	0.011^{***}
Birth Year, continuous	-0.007^{***}							
Constant	16.220^{***}	4.151^{***}	4.099^{***}	4.072^{***}	4.184^{***}	4.417^{***}	4.538^{***}	4.665^{***}
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 9: Regression output for individuals belonging to the 2nd generation

y=Life Duration	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**
Avg. Diff. (AD)	−0.002	−0.007	−0.007	−0.017***	−0.017***	−0.018***	−0.017***
<1816		ref	–	–	–	–	–
1816-1820		−0.103**	ref	–	–	–	–
1821-1825		−0.777**	−0.675**	−0.684	−0.679	−0.759	−0.727
1826-1830		−0.141	−0.038	−0.402	−0.368	−0.517	−0.502
1831-1835		−0.369	−0.266	−0.875**	−0.858**	−0.970***	0.926**
1836-1840		−0.265	−0.162	−0.644	−0.672	−0.753	−0.700
>1840		−0.263	−0.160	−0.506	−0.588	−0.635*	−0.617*
<1816 x AD		ref	–	–	–	–	–
1816-1820 x AD		0.000	ref	ref	ref	ref	ref
1821-1825 x AD		0.011	0.010*	0.011	0.011	0.012	0.012
1826-1830 x AD		0.003	0.003	0.011	0.011	0.013*	0.012
1831-1835 x AD		0.005	0.005	0.017**	0.016**	0.017***	0.016**
1836-1840 x AD		0.004	0.004	0.015*	0.015*	0.015**	0.014*
>1840 x AD		0.005	0.005	0.013*	0.014*	0.014**	0.013**
Birth Year, continuous	0.001						
Life duration, father				0.008***	0.007**	0.008***	0.008***
Life duration, mother				0.004	0.004	0.004	0.005
Age of father @ birth					−0.019	−0.019	−0.019
Age of mother @ birth					0.025*	0.022	0.023
Number of siblings						−0.035*	−0.036*
Rank among siblings						0.015	0.016
Gender (ref=Male)							−0.048
Constant	1.312	4.300***	4.197***	3.885***	3.942***	4.228***	4.198***
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 10: Regression output for individuals belonging to the 2nd generation

y=Life Duration	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**
Avg. Diff. (AD)	-0.020^{***}	-0.010^{**}	-0.007	-0.007	-0.005	-0.005	-0.005
<1816	–	–	–	–	–	–	–
1816-1820	ref	ref	ref	ref	ref	ref	ref
1821-1825	-1.016	-0.284	-0.556^{*}	-0.554^{*}	-0.401	-0.351	0.373
1826-1830	-0.861^{*}	-0.263	-0.144	-0.144	-0.057	-0.016	-0.016
1831-1835	-1.056^{**}	-0.679^{**}	-0.339	-0.333	-0.238	0.254	0.246
1836-1840	-0.800	-0.162	-0.333	-0.323	-0.236	-0.346	-0.055
>1840	-0.894^{**}	-0.377	-0.311	-0.304	-0.261	-0.291	0.269
<1816 x AD	–	–	–	–	–	–	–
1816-1820 x AD	ref	ref	ref	ref	ref	ref	ref
1821-1825 x AD	0.016	0.003	0.009^{*}	0.009	0.006	0.006	0.006
1826-1830 x AD	0.018^{**}	0.007^{*}	0.005	0.005	0.002	0.002	0.002
1831-1835 x AD	0.018^{**}	0.011^{**}	0.007	0.008	0.005	0.005	0.005
1836-1840 x AD	0.015^{*}	0.007	0.006	0.006	0.004	0.005	0.002
>1840 x AD	0.017^{**}	0.007	0.007	0.007	0.006	0.006	0.005
Life duration, father	0.009^{***}	0.007^{**}	0.003	0.003	0.002	0.001	0.001
Life duration, mother	0.001	-0.001	-0.002	-0.001	-0.001	-0.001	0.000
Age of father @ birth	-0.016	-0.003	-0.002	-0.002	-0.001	-0.001	-0.003
Age of mother @ birth	0.018	0.009	0.001	0.001	0.002	0.003	0.002
Number of siblings	-0.031	-0.020	-0.002	-0.004	0.002	-0.003	-0.010
Rank among siblings	0.014	0.003	0.000	0.002	-0.006	-0.001	0.003
Gender (ref=Male)	-0.079	-0.101	-0.015	-0.023	-0.025	-0.029	-0.012
Constant	4.623^{***}	4.256^{***}	4.459^{***}	4.470^{***}	4.334^{***}	4.353^{***}	4.434^{***}
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 11: Regression output for individuals belonging to the 3rd generation

y=Life Duration	**(23)**	**(24)**	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**
Avg. Diff. (AD)	−0.002	−0.002	−0.003	−0.010	−0.014	−0.012	−0.012	−0.001
<1846		ref	ref	ref	ref	ref	ref	ref
1846-1850		−0.111	−0.332	−0.909	−1.005	−0.887	−0.890	−0.613
1851-1855		0.203	0.178	−0.191	−0.321	−0.213	−0.217	0.179
1856-1860		−0.141	−0.249	−0.674	−0.726	−0.642	−0.645	−0.283
1861-1865		0.055	0.056	−0.617	−0.617	−0.509	−0.512	0.062
1866-1870		0.031	−0.022	−0.364	−0.391	0.301	−0.304	0.223
1871-1875		0.200	0.165	−0.145	−0.212	−0.145	−0.147	0.650
>1875		−0.091	−0.127	−0.632	−0.672	−0.590	−0.593	−0.130
<1846 x AD		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AD		0.001	0.005	0.013	0.015	0.014	0.014	0.006
1851-1855 x AD		−0.006	−0.005	0.001	0.005	0.003	0.003	−0.004
1856-1860 x AD		−0.000	0.002	0.008	0.012	0.011	0.011	0.005
1861-1865 x AD		−0.001	0.001	0.012	0.015	0.014	0.014	0.001
1866-1870 x AD		0.001	0.003	0.009	0.013	0.012	0.012	−0.000
1871-1875 x AD		−0.002	−0.000	0.004	0.009	0.008	0.008	−0.007
>1875 x AD		0.001	0.002	0.009	0.014	0.013	0.013	0.003
Birth Year, continuous	0.001							
Life duration, father				0.003	0.003	0.003	0.003	−0.002
Life duration, mother				0.004*	0.004*	0.005*	0.005*	0.003
Age of father @ birth					−0.015	−0.016	−0.016	−0.012
Age of mother @ birth					−0.000	−0.003	−0.003	−0.002
Number of siblings						−0.020	−0.020	−0.008
Rank among siblings						0.011	0.011	0.004
Gender (ref=Male)							−0.003	−0.065
Constant	3.486***	4.007***	4.019***	3.590***	3.751***	3.861***	3.889***	4.078***
Observations	907	907	768	406	405	404	404	342
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	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 life duration 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.'[32]

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

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

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[33]. Dooling (2007) is one of the exceptions. Even though he claims that 'it is (...) no longer possible to uphold an older 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' (p. 135).

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.[34]

Since the assessment of the life duration for individuals belonging to the control group is

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

[34]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.

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 12:

Table 12: Descriptive Statistics - Analytical Sample, control group

Variable	**Obs**	**Mean**	**Std. dev**	**Min**	**Max**
1st Generation					
Life Duration	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					
Life Duration	1216	54.39	26.39	0	136
Age Father @ birth	1204	35.97	8.98	17	73
Age Mother @ 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					
Life Duration	2321	56.39	26.61	0	129
Age Father @ birth	2267	34.92	8.51	17	68
Age Mother @ 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 12 with Table 1 we verify that the mean life duration of individuals belonging to the 2nd and 3rd 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 life duration for all populations of interest are shown.

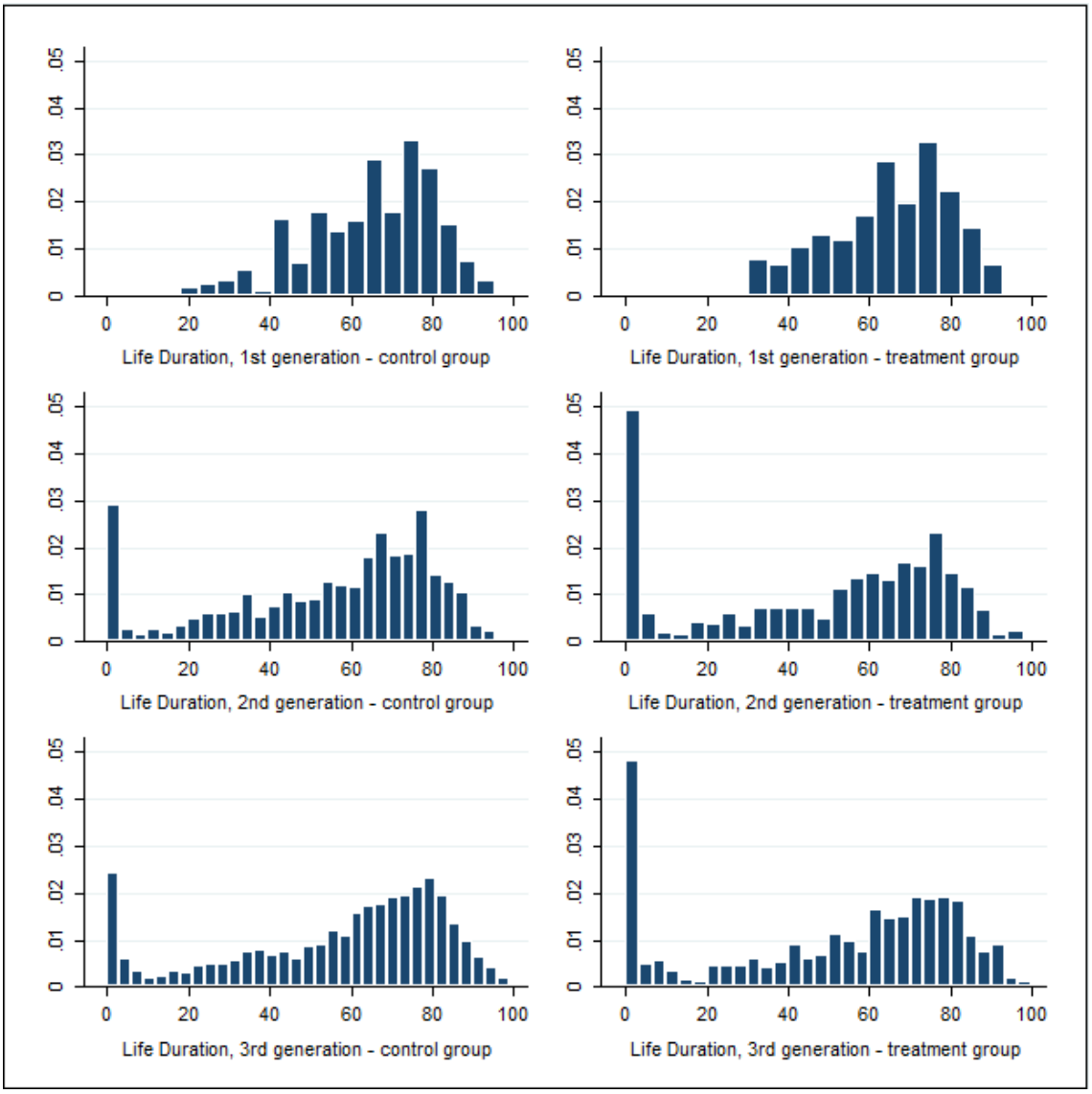

Figure 7: Distribution of Life Duration in years for each population of interest

We are not capable of determining the specific reasons behind this phenomenon. We can, however, control for child mortality in our estimates.[35] 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

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

the main results – we divide both groups into 5-year birth cohorts and estimate several models where we impose restrictions on individuals' year of birth.

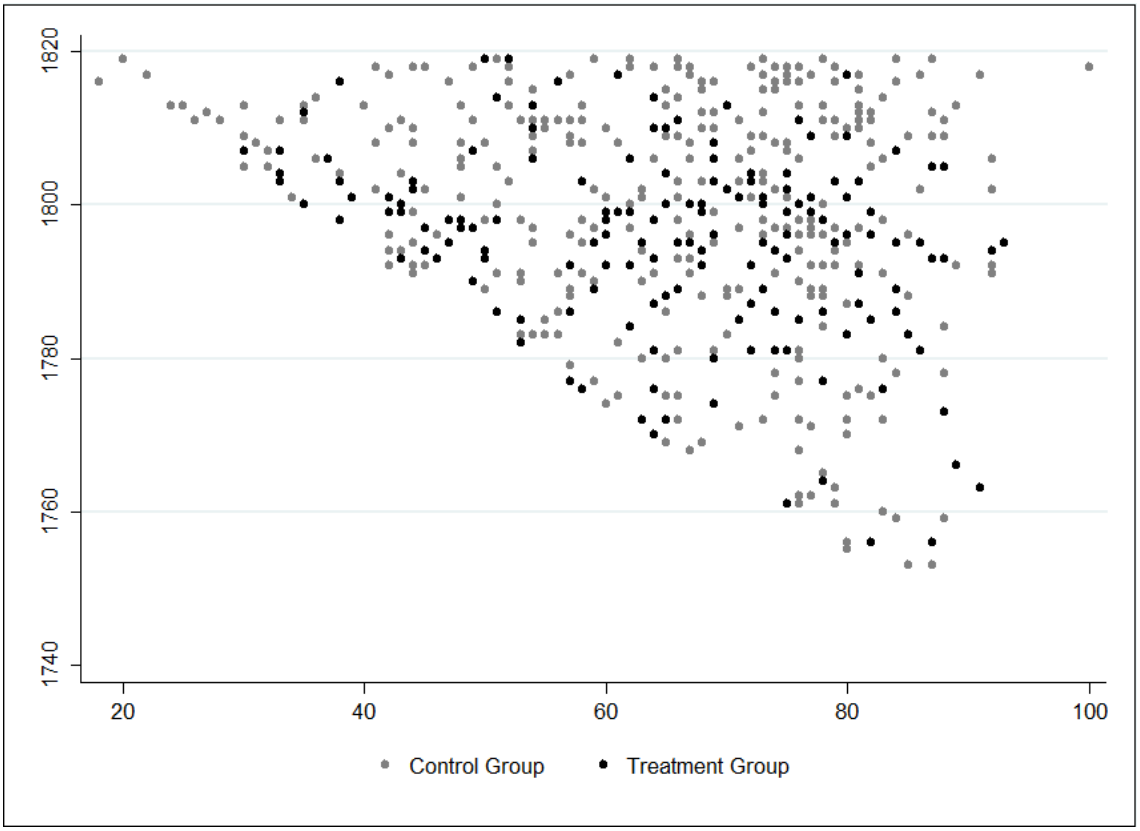

Figure 8: Life Duration in years per year of birth

Results

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

Table 13: Estimates of the Average Uncompensated Value on the 1st generation.

y=Life Duration	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Diff. (AD)	−0.001**	0.001	0.002	−0.001*	−0.002***	−0.004***	−0.004***	−0.009***
<1780 x AD		ref	–	–	–	–	–	–
1780-1784 x AD		0.000	ref	–	–	–	–	–
1785-1789 x AD		0.000	−0.001	–	–	–	–	–
1790-1794 x AD		−0.002*	−0.003*	ref	–	–	–	–
1795-1799 x AD		−0.001	−0.002	0.001	ref	ref	ref	ref
1800-1804 x AD		−0.001	−0.002	0.001	0.002***	0.004***	0.003***	0.008***
1805-1809 x AD		−0.002**	−0.003***	−0.000	0.000	0.002**	0.002*	0.007***
>1809 x AD		−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 16.

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

Among individuals belonging to the 1st generation. The overall effects of the average uncompensated value 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 uncompensated value, life duration 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 14 below:

Table 14: Estimates of the Average Uncompensated Value on the 2nd generation.

y=Life Duration	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Avg. Diff. (AD)	−0.002***	−0.003	−0.002	−0.006**	−0.006**	−0.006**	−0.006**	−0.004*
<1816 x AD		ref	–	–	–	–	–	–
1816-1820 x AD		0.001	ref	ref	ref	ref	ref	ref
1821-1825 x AD		0.001	0.000	0.002	0.002	0.002	0.002	0.001
1826-1830 x AD		0.002	0.001	0.005*	0.005*	0.005*	0.005*	0.005*
1831-1835 x AD		0.000	−0.001	0.004	0.004	0.004	0.004	0.002
1836-1840 x AD		0.001	0.000	0.006*	0.006*	0.006*	0.006*	0.004
>1840 x AD		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), life duration is regressed on Avg. Diff, 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) life durationg of father and mother (13) age of father and age of mother @ birth, (14) number of siblings and rank among siblings and (15) gender. Equation (16) excludes individuals whose life duration was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level. The complete regression output is found on Table 17.

*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 uncompensated value on life duration. This significance however is lost when including cohorts of birth year and their respective interaction 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 life duration of both the father and the mother as a control. The coefficient remains stable despite the inclusion of several other difference genealogical covariates only to suffer a small reduction on equation (16) when infant mortality – i.e. individuals whose life duration was smaller than 1 year – are excluded from the dataset.

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

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 uncompensated values remained negative even when adding a control group who, in theory, would not be directly affected

Table 15: Estimates of the Average Uncompensated Value on the 3rd generation.

y=Life Duration	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
Avg. Diff. (AD)	−0.002***	−0.001	−0.002	−0.003	−0.003	−0.003	−0.003	−0.001
<1846 x AD		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AD		−0.003**	−0.002	−0.006	−0.005	−0.005	−0.005	−0.005
1851-1855 x AD		−0.003*	−0.003	−0.002	−0.002	−0.002	−0.003	−0.002
1856-1860 x AD		−0.003*	−0.002	−0.001	−0.001	−0.001	−0.002	0.000
1861-1865 x AD		−0.001	0.001	0.002	0.002	0.002	0.002	0.000
1866-1870 x AD		0.001	0.002	0.003	0.004	0.003	0.003	−0.000
1871-1875 x AD		0.002	0.002	0.004	0.004	0.004	0.004	0.000
>1875 x AD		−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), life duration is regressed on Avg. Diff, 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) life duration of father and mother, (21) age of father and age of mother @ birth, (22) number of siblings and rank among siblings and (23) gender. Equation (24) excludes individuals whose life duration was smaller than 1 year. All estimates use clustered standard errors at the individuals' father level. The complete regression output is found on Table 18.

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

by the economic shock.

Appendix F: Full regression tables including a control group

Table 16: Regression output for individuals belonging to the 1st generation

y=Life Duration	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Diff. (AD)	-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 AD		ref	–	–	–	–	–	–
1780-1784 x AD		0.000	ref	–	–	–	–	–
1785-1789 x AD		0.000	−0.001	–	–	–	–	–
1790-1794 x AD		-0.002^{*}	-0.003^{*}	ref	–	–	–	–
1795-1799 x AD		−0.001	−0.002	0.001	ref	ref	ref	ref
1800-1804 x AD		−0.001	−0.002	0.001	0.002^{***}	0.004^{***}	0.003^{***}	0.008^{***}
1805-1809 x AD		-0.002^{**}	-0.003^{***}	−0.000	0.000	0.002^{**}	0.002^{*}	0.007^{***}
>1809 x AD		−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 17: Regression output for individuals belonging to the 2nd generation

y=Life Duration	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Avg. Diff. (AD)	-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 AD		ref	–	–	–	–	–	–
1816-1820 x AD		0.001	ref	ref	ref	ref	ref	ref
1821-1825 x AD		0.001	0.000	0.002	0.002	0.002	0.002	0.001
1826-1830 x AD		0.002	0.001	0.005^{*}	0.005^{*}	0.005^{*}	0.005^{*}	0.005^{*}
1831-1835 x AD		0.000	-0.001	0.004	0.004	0.004	0.004	0.002
1836-1840 x AD		0.001	0.000	0.006^{*}	0.006^{*}	0.006^{*}	0.006^{*}	0.004
>1840 x AD		0.002	0.001	0.004	0.004	0.004	0.004	0.002
Birth Year, continuous	0.000							
Life duration, father				0.004^{**}	0.004^{**}	0.005^{**}	0.005^{**}	0.004
Life duration, mother				0.004^{**}	0.004^{**}	0.004^{**}	0.005^{**}	0.003^{**}
Age of father @ birth					-0.001	-0.002	-0.002	0.000
Age of mother @ 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 life duration was smaller than 1 year. All estimates use clustered standard errors at the 1st generation level.

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

Table 18: Regression output for individuals belonging to the 3rd generation

y=Life Duration	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
Avg. Diff. (AD)	-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 AD		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AD		-0.003^{**}	-0.002	-0.006	-0.005	-0.005	-0.005	-0.005
1851-1855 x AD		-0.003^{*}	-0.003	-0.002	-0.002	-0.002	-0.003	-0.002
1856-1860 x AD		-0.003^{*}	-0.002	-0.001	-0.001	-0.001	-0.002	0.000
1861-1865 x AD		-0.001	0.001	0.002	0.002	0.002	0.002	0.000
1866-1870 x AD		0.001	0.002	0.003	0.004	0.003	0.003	-0.000
1871-1875 x AD		0.002	0.002	0.004	0.004	0.004	0.004	0.000
>1875 x AD		-0.001	-0.000	-0.000	0.000	0.000	-0.000	-0.002
Birth Year, continuous	0.000							
Life duration, father				0.004^{**}	0.004^{**}	0.005^{***}	0.004^{***}	0.003^{*}
Life duration, mother				0.004^{***}	0.003^{**}	0.003^{***}	0.003^{***}	0.003^{***}
Age of father @ birth					-0.008^{*}	-0.009^{*}	-0.009^{*}	-0.007
Age of mother @ 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 life duration 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

Appendix G: Other functional forms

Table 19: Regression output for individuals belonging to the 1st generation, logged functional form

y=Life Duration	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Avg. Diff. (AD), logged	0.085^{***}	0.184^{*}	0.138	0.135^{*}	-0.098	-0.309^{***}	-0.375^{***}	-0.525^{***}
<1780		ref	–	–	–	–	–	–
1780-1784		0.137	ref	–	–	–	–	–
1785-1789		0.353	0.2104	–	–	–	–	–
1790-1794		0.036	-0.113	ref	–	–	–	–
1795-1799		0.374	0.258	0.314	ref	ref	ref	ref
1800-1804		-0.297	-0.433	-0.340	-1.201^{***}	-2.008^{***}	-2.326^{***}	-2.993^{***}
1805-1809		0.833	0.704	0.785^{*}	-0.068	-0.858^{*}	-1.169^{**}	-1.847^{***}
>1809		0.022	-0.121	-0.015	-0.885^{*}	-1.711^{***}	-2.035^{***}	-2.691^{***}
<1780 x AD		ref	–	–	–	–	–	–
1780-1784 x AD		-0.049	ref	–	–	–	–	–
1785-1789 x AD		-0.109	-0.060	–	–	–	–	–
1790-1794 x AD		-0.051	-0.000	ref	–	–	–	–
1795-1799 x AD		-0.140	-0.098	0.087	ref	ref	ref	ref
1800-1804 x AD		0.011	0.057	0.059	0.293^{***}	0.505^{***}	0.572^{***}	0.721^{***}
1805-1809 x AD		-0.259^{*}	-0.214	-0.208^{**}	0.024	0.231^{**}	0.296^{**}	0.448^{**}
>1809 x AD		-0.058	-0.010	-0.010	0.226^{*}	0.441^{***}	0.510^{***}	0.656^{***}
Total Tax, logged	0.021^{*}	0.017	0.013	0.021	0.015	0.003	-0.002	0.006
Birth Year, continuous	-0.006^{***}							
Constant	15.160^{***}	3.577^{***}	3.712^{***}	3.620^{***}	4.481^{***}	5.288^{***}	5.606^{***}	6.273^{***}
Observations	130	130	117	91	67	64	61	55
Pseudo-R^2	0.053	0.063	0.047	0.028	0.024	0.044	0.049	0.044

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

Table 20: Regression output for individuals belonging to the 2nd generation, logged functional form

y=Life Duration	**(17)**	**(18)**	**(19)**	**(20)**	**(21)**	**(22)**	**(23)**	**(24)**
Avg. Diff. (AD)	−0.086	−0.205*	−0.319*	−0.462**	−0.441**	−0.828***	−0.799***	−0.459**
<1816		ref	–	–	–	–	–	–
1816-1820		0.379	ref	–	–	–	–	–
1821-1825		−1.824*	−2.230*	−2.751*	−2.926**	−2.304	−2.169	−0.597
1826-1830		−0.239	−0.179	−0.423	−0.600	−1.729	−1.676	−0.920
1831-1835		−0.605	−1.027	−1.979*	−2.016*	−3.188***	−3.010**	−2.165*
1836-1840		−0.144	−0.567	−1.469	−1.236	−2.535*	−2.294	−1.134
>1840		−0.724	−1.154	−1.397	−1.207	−2.457**	−2.366**	−1.290
<1816 x AD		ref	–	–	–	–	–	–
1816-1820 x AD		−0.105	ref	ref	ref	ref	ref	ref
1821-1825 x AD		0.428	0.541*	0.683*	0.721*	0.559	0.524	0.113
1826-1830 x AD		−0.038	0.078	0.168	0.197	0.483	0.469	0.250
1831-1835 x AD		0.141	0.258	0.504**	0.498*	0.795***	0.750**	0.525*
1836-1840 x AD		0.045	0.162	0.403	0.328	0.661*	0.601	0.276
>1840 x AD		0.213	0.332	0.407	0.341	0.643**	0.619**	0.304
Total Tax, logged	0.080**	0.086**	0.095**	0.055	0.038	0.004	0.007	−0.041
Birth Year, continuous	0.003							
Age of father @ birth				−0.011	−0.004	−0.018	−0.018	−0.003
Age of mother @ birth				0.010	0.014	0.021	0.022	0.011
Number of siblings					−0.024	−0.038*	−0.039*	−0.025*
Rank among siblings					−0.012	0.017	0.017	0.006
Life duration, father						0.009***	0.009***	0.008***
Life duration, mother						0.004	0.005	−0.001
Gender (ref=Male)							−0.056	−0.104
Constant	−1.257	4.604***	5.008***	5.630***	5.563***	6.502***	6.384***	5.492***
Observations	577	577	488	325	325	239	239	193
Birth Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.012	0.023	0.027	0.027	0.036	0.067	0.068	0.069

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

Table 21: Regression output for individuals belonging to the 3rd generation, logged functional form

y=Life Duration	**(25)**	**(26)**	**(27)**	**(28)**	**(29)**	**(30)**	**(31)**	**(32)**
Avg. Diff. (AD)	−0.142*	−0.088	−0.150	0.593	0.808	−0.951	−0.950	−0.066
<1846		ref	ref	ref	ref	ref	ref	ref
1846-1850		0.036	−1.027	2.428	2.937	−4.980	−4.976	−2.568
1851-1855		1.303	1.184	3.963	4.473	−2.142	−2.138	0.710
1856-1860		0.241	−0.202	2.142	2.768	−3.652	−3.649	−1.165
1861-1865		0.295	0.199	2.390	3.143	−3.543	−3.539	0.070
1866-1870		0.159	−0.112	3.065	3.875	−3.461	−3.458	0.213
1871-1875		0.435	0.204	3.081	3.775	−3.441	−3.439	1.371
>1875		0.060	−0.136	2.788	3.700	−3.730	−3.728	−0.322
<1846 x AD		ref	ref	ref	ref	ref	ref	ref
1846-1850 x AD		−0.024	0.246	−0.610	−0.721	1.215	1.214	0.581
1851-1855 x AD		−0.359	−0.323	−0.996	−1.177	0.531	0.530	−0.187
1856-1860 x AD		−0.103	0.021	−0.545	−0.684	0.908	0.907	0.300
1861-1865 x AD		−0.071	−0.028	−0.541	−0.711	0.955	0.954	0.025
1866-1870 x AD		−0.015	0.063	−0.687	−0.875	0.967	0.966	0.009
1871-1875 x AD		−0.087	−0.018	−0.688	−0.852	0.948	0.947	−0.281
>1875 x AD		−0.030	0.030	−0.657	−0.875	0.977	0.977	0.102
Total Tax, logged	−0.014	−0.010	0.002	−0.010	0.012	0.064	0.064	0.053
Birth Year, continuous	0.000							
Age of father @ birth				−0.008	−0.007	−0.018	−0.018	−0.014
Age of mother @ birth				0.004	0.010	−0.002	−0.002	0.004
Number of siblings					−0.020	−0.024	−0.024	−0.011
Rank among siblings					−0.019	0.013	0.013	0.005
Life duration, father						0.003	0.003	−0.001
Life duration, mother						0.005**	0.005**	0.003
Gender (ref=Male)							0.001	0.063
Constant	3.833	4.310***	4.503***	1.543	0.646	7.698	7.694	4.373
Observations	907	907	768	520	519	404	404	342
Father Restriction	no	no	yes	yes	yes	yes	yes	yes
Infant Mortality	no	no	no	no	no	no	no	yes
Pseudo-R^2	0.004	0.017	0.019	0.027	0.036	0.056	0.056	0.051

[Notes] Equations (27) to (31) consider only individuals whose father was born after 1816. Equation (32) considers only individuals whose father born after 1816 and excludes individuals whose life duration was smaller than 1 year. All estimates use clustered standard errors at the 2nd generation level.

Appendix H: 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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