Estimating the effect of racial classification on labour market outcomes: A case study from Apartheid South Africa

Miquel Pellicer and Vimal Ranchhod

Working Paper Series
Number 259, Version 1

About the Author(s)

Miquel Pellicer: Maynooth University and SALDRU, University of Cape Town
Vimal Ranchhod: School of Economics and SALDRU, University of Cape Town

About the Author(s)

We are grateful to Keith Breckenridge, seminar participants at the University of Michigan, Middlebury College, the University of Cape Town, the African Economic History conference, University College Dublin and Maynooth University. Special thanks go to Catherine Kannemeyer and her family for explaining how racial classification operated in practice and the personal impact it could have.

All errors and omissions remain the full responsibility of the authors. We recognize that the use of the term "non-White" is problematic. We apologize for this, but it has proven to be impossible to write this paper without using Apartheid era terminology and racial groups.

Recommended citation

Pellicer, M., Ranchhod, V. (2020). Estimating the effect of racial classification on labour market outcomes: A case study from Apartheid South Africa. Cape Town: SALDRU, UCT. (SALDRU Working Paper Number 259).

ISBN: 978-1-928516-20-0

Estimating the effect of racial classification on labour market outcomes: A case study from Apartheid South Africa*

Miquel Pellicer
Maynooth University and SALDRU, University of Cape Town
Vimal Ranchhod
School of Economics and SALDRU, University of Cape Town

February 23, 2020

Abstract

What were the effects of being officially classified as White on labour market outcomes during *apartheid* in South Africa? South Africa's apartheid government implemented a comprehensive system of discrimination against "non-Whites"[1] that covered every major facet of life. Discrimination in educational opportunities, healthcare, and neighbourhood quality were designed to create productivity differentials across race groups; and these effects would not be included in most estimates of labour market discrimination. We quantify the cumulative effect of all of these forms of discrimination by estimating the causal effect of being classified as White on education, employment and income.

Our identification strategy is based on a policy change that privileged ancestry over appearance in the process of racial classification for those born after the 1951 Census. We use census data from 1980, 1991, and 1996, and restrict our sample to Whites and Coloureds. The data exhibits a discontinuity as well as a trend change in racial shares for cohorts born after 1951. Combined, these imply a 6 percentage point lower likelihood of being classified as White for people born 10 years after 1951.

Our preferred estimates indicate that being classified as White instead of Coloured resulted in a more than threefold increase in income for men. This corresponds to approximately 65% of the difference in mean incomes between the two population groups. Our findings for women are inconclusive.

*We are grateful to Keith Breckenridge, seminar participants at the University of Michigan, Middlebury College, the University of Cape Town, the African Economic History conference, University College Dublin and Maynooth University. Special thanks go to Catherine Kannemeyer and her family for explaining how racial classification operated in practice and the personal impact it could have. All errors and omissions remain the full responsibility of the authors.

[1]We recognize that the use of the term "non-White" is problematic. We apologize for this, but it has proven to be impossible to write this paper without using Apartheid era terminology and racial groups.

1 Introduction

What were the effects of officially being classified as White on labour market outcomes during *apartheid* in South Africa? South Africa's *apartheid* government implemented a comprehensive system of discrimination against "non-Whites" that covered every major facet of life. Discrimination in educational opportunities, healthcare, and neighbourhood quality were designed to create productivity differentials across race groups; and these effects would not be included in most estimates of labour market discrimination. We quantify the cumulative effect of all of these forms of discrimination by estimating the causal effect of being classified as White on education, employment and income.

Most empirical studies in economics that focus on discrimination restrict their attention to labour market discrimination, defined as the differential treatment in the labour market of people of equal productivity but with different group membership.[2] It has long been acknowledged that both labour market discrimination and "pre-market" discrimination (for instance, due to differences in neighborhood and school quality) can account for differences in labor market outcomes across population groups (Altonji & Blank, 1999). However, most focus in the empirical labour literature has been on labour market discrimination narrowly defined; i.e. ignoring the effects of pre-labour market discrimination.

Earlier discrimination studies used wage regressions and controlled for productivity related characteristics such as age and years of education. They then decomposed the observed wage gap into explained and unexplained components, and interpreted the unexplained components as evidence of discrimination. (See for example Blinder (1973); Oaxaca (1973); Juhn, Murphy, and Pierce (1993)). Estimates using these methodologies are biased if productivity-related characteristics that are not observable to the researcher are correlated with group membership. As a response to this, the empirical literature on discrimination evolved to attempt to control for unobservable characteristics.

In "audit studies", comparably matched people from different groups were sent into the field to try to successfully complete a task such as obtaining a job offer.[3] In correspondence studies, CVs with *a priori* identical information are prepared and then randomly assigned to a group with some characteristics that identify the sender as likely belonging to one demographic group or another. Here, researchers have investigated whether *perceptions* of race may affect the senders' labour market outcomes. The most famous of these is probably the work by Bertrand and Mullainathan (2004) who sent out thousands of CVs using different sounding ethnic names and measured the callback rates in Boston and Chicago. Since then, many more studies of this type have been conducted (for recent reviews, see Neumark (2018) and Bertrand and Duflo (2017)).

A direct consequence of this evolution in the literature has been that the research focus has become even narrower, as people investigate the effects of discrimination on

[2]Groups can be defined by many factors, but most commonly we find studies on race, gender, or ethnicity.

[3]For an illustrative example see Neumark, Bank, and Van Nort (1996). For a discussion on the methodological underpinnings and a critique of audit studies, see Siegelman and Heckman (1993) and Heckman (1998).

specific events where the *ceteris paribus* assumption could plausibly hold.[4] Thus, both audit studies and correspondence studies that investigate labour market discrimination cannot measure the magnitude of labour market discrimination overall.

In addition, and perhaps more importantly, there has been to our knowledge no attempt to estimate the combined effect of different types of discrimination on labour market outcomes, what we call the "*cumulative effect of racial discrimination*". These include all the ways in which being discriminated against over the life cycle might directly or indirectly impact on labour market outcomes. For example, the cumulative effect of discrimination would include differences in neighbourhoods, school quality, and healthcare, amongst other things.

In addition, discrimination in one aspect of society can have complementary interactions with other types of discrimination. For example, the 'stereotype threat' hypothesis (Steele & Aronson, 1995) from social psychology has received overwhelming empirical support for its effects on test performances, and there is evidence that stereotype threat can also affect productivity directly.[5] Under these conditions, discrimination in social interactions, educational opportunities, access to healthcare, neighbourhood environments and the labour market can have profound implications on overall outcomes, especially when potential feedback loops are taken into consideration.

We argue that the cumulative effect of discrimination can be approximated by the causal effect of race or, to be more precise, of racial classification. The thought experiment of comparing the life outcomes of two identical children that differ only in terms of their race would deliver an estimate of the cumulative effect of racial discrimination. However, obtaining a plausible causal estimate of the effects of race has proven to be challenging, if not impossible. Holland (1986) argues that empirically estimating the causal effects of immutable characteristics, including race, is a nonsensical endeavor.[6] The premise here is that 'race' is fully determined by genetics, and as such, one cannot possibly imagine manipulating a person's race without actually replacing the person with an altogether different person.

This critique softens if we were to conceptualize race as the outcome of both genetic endowments as well as a social recognition process. The thought experiment of manipulating how a person is socially recognized in terms of their race is a sensible one. It is possible to imagine two identical children who are given different 'tags' at birth. These tags, in turn, will determine how they will be racially categorized throughout their lives. We thus propose to estimate the cumulative effect of racial discrimination via an estimate of the causal effect of racial classification.

We make use of census data from the 1980s and 1990s to estimate the effect of racial classification on economic outcomes in apartheid South Africa. Apartheid South Africa was a context with extremely large raw differences in educational attainment and income between population groups. In our sample, White people obtained on average about 5 more years of education than Coloured people, and White men earned approximately 4.5 times more income than Coloured men.[7]

[4]See Guryan and Charles (2013) for a more thorough discussion.

[5]See Hoff and Pandey (2006, 2014).

[6]A more recent discussion on estimating the causal effects of immutable characteristics is presented by Greiner and Rubin (2011).

[7]We use upper case names for race groups corresponding to those used in South Africa. The four mutually exclusive and exhaustive groups are African, Coloured, Asian/Indian and White. At various points in time the South African legislature referred to 'Natives' or 'Bantu', these are substantively

We estimate the causal effect of racial classification by exploiting a relative change in the racial classification process that applied to children born after 1951. At the onset of apartheid, an individual's racial classification was determined by the criteria set out in the Population Registration Act of 1950, and the 1951 Census was the key source of information used for implementation. Racial classification involved three criteria; appearance, social acceptability, and ancestry or descent. However, for South Africans born before 1951, it was not practical to use ancestry as a criterion, due to incomplete records relating to the race of the parents. For these people, the main criteria used for classification were appearance and social acceptability. For people born after the 1951 Census, the official race of the individual's parents became the main criterion for classification.[8] In places where there had been a long history of inter-racial marriage or coupling, this change in policy generated an exogenous change in the population shares of various race groups. We use this change as an instrument to identify the effects of being White, relative to being Coloured, on education, employment and income.

Our data exhibits a clear discontinuity and trend change in racial shares for cohorts born after 1951. These changes combined imply a 6 percentage point lower likelihood of being classified as White for cohorts born 10 years after 1951. These discontinuities are present even though no discontinuity or trend change is detected for the overall size of cohorts over the same time period. Our primary findings are that being classified as White as opposed to Coloured conferred an extremely large advantage in terms of education for both men and women, and in income for men. Our preferred estimates imply that being classified as White led to 3.5 to 4 more years of education and a 1.1 unit increase in log income for men. The estimate for income corresponds to approximately 65% of the very large disparity in mean incomes between the two population groups.[9]

Our estimates more closely resemble the cumulative effects of racial discrimination than estimates of labour market discrimination or educational discrimination measured in isolation. Apartheid policy involved several statutes that involved racially segregated neighbourhoods, education, healthcare, public transport, and labour market opportunities; with the clear and stated objective of maintaining White supremacy. It is quite impossible to imagine that discrimination in these and other markets, including financial market access and the rights to own businesses, would not have substantial interaction effects on employment and income.

Our paper illustrates a novel way to identify the causal effects of racial classification on economic outcomes, which can hopefully be useful to other researchers as well. In this specific context an individual's officially designated race was definitely something that could be manipulated, as illustrated by the fact that a very small pro-

the same as African.

[8]This change was formalized by an Amendment to the Population Registrations Act in 1967, which was applied retroactively to 1950, whereby one's racial classification was determined by the racial classification of one's parents, regardless of one's own appearance.

[9]The estimate of 65% applies to men with strictly positive income levels. Moreover, our causal estimates applies only to the 'compliers' of the policy, whereas the raw differences applies to the whole population. Thus, strictly speaking, we cannot conclude that racial classification accounted for 65% of the observed income differentials between White and Coloured men. This being said, it seems likely that the raw differences between Whites and Coloureds among the complier sub-population would be smaller than they would be for the overall population. In this case, the 65% would be a lower bound for the complier sub-population.

portion of the population were re-classified on appeal. Our analysis involves a specific application of a general estimation strategy that may have considerably broader applicability. In essence, we are estimating the economic effects of group membership, where membership guarantees differential rights and privileges, and is determined by some formal classification system. Any time that the classification system changes exogenously yields an opportunity to estimate the effect of membership, for the subset of people affected by the change in the classification system. Some groups and places where this might be useful would include Scheduled Castes in India, Indigenous people in Brazil, Native Americans in the USA, First Nations people in Canada, and Malays in Malaysia.

2 South African History and Apartheid

South Africa as a country with its current borders was formed in 1910 when the South African Union was granted independence from Britain and became a self-governing dominion of the British Empire. This followed two and a half centuries of European settlement, expansion, and conquest in South Africa, beginning with the first Dutch settlement in 1652 in what is now the city of Cape Town.

Apartheid as official state policy was introduced by the newly elected National Party in 1948, and ended with the first fully democratic election in South Africa in 1994. The system was based on the belief in scientific racism and White supremacy, and emphasized racial 'Separateness' as its core principle. Practically, the policy involved the statutory classification of people into mutually exclusive racial groups. These groups had varying degrees of state support, legal rights, access to healthcare and education, and geographic mobility; with White people always receiving vastly better opportunities, facilities, subsidies, and welfare transfers. Implementation was achieved by passing into law several statutes that were complete in their coverage and effectively generated different legal systems for people of different races This led to the creation and stabilization of a social and economic class structure that was fully congruent with the apartheid racial hierarchy.

A thorough discussion of what apartheid was and how it operated is beyond the scope of this article. At the same time, it is important for our purposes to note a few salient points. The following discussion draws on the books by Clark and Worger (2016), Dubow (2014) and Thompson (2001).

First, while Apartheid was ostensibly motivated by beliefs about racial purity and White supremacy, it was also very much about economic rent seeking by a political group that had kept the majority of the population disenfranchised. Such rent seeking involved explicit affirmative action and job reservation for working class Whites. In addition, White owned businesses benefitted from an over-supply of cheap and unskilled black labour, while also being protected from competition by heavily regulating and stifling black owned businesses.[10]

Moreover, South Africa prior to 1948 was already a heavily racialized and stratified society, much like all of the British colonies in Africa. Indeed, one of the most destructive laws for African people was the Natives Land Act of 1913, which prevented Africans from owning or renting land in designated areas that accounted for 93% of

[10]We use the term 'black' here to refer to all of the 'non-White' groups in South Africa

the total land area.[11] This, in turn, strongly encouraged Africans to seek wage labour on White-owned farms and in the White-owned mines in Johannesburg and Kimberly. Where South Africa's history does diverge from that of its neighbours is in the period following World War II. While the rest of Africa went through a sustained period of decolonization and withdrawal of the settler communities, the South African experience involved a sharp increase in the subjugation and racial exploitation of non-White people by White people.

While the primary discrimination cleavage was between Whites and non-Whites, there were nevertheless important differences in the treatment of different non-White groups. African blacks, in particular, were especially discriminated against. They faced important additional restrictions in terms of residence, geographic mobility, labour market opportunities and access to educational institutions.

It is difficult is to convey the breadth and depth to which race circumscribed an individual's life experience under *apartheid*. Which healthcare facility one was born in (if any), where one lived, what schools one attended, what subjects and to what level one was allowed to study, one's employment prospects and socio-economic mobility, who one could marry and have a family with, which churches one could attend, the ability to own property or start a business, and which graveyard one would eventually be buried in: these were all strongly constrained by one's racial classification. Thus race affected one's life experiences 'from the cradle to the grave', quite literally, without a moment's respite.

This excessively racialized society was attained by means of several statutes that legally entrenched discrimination as part of national policy. Of these, some of the more far reaching were:

- The Prohibition of Mixed Marriages Act, 1949. This made it illegal for White people and people from non-White race groups to be legally married. This law was followed by the Immorality Amendment Act, 1950 - which prohibited sexual relationships between Whites and members of the other race groups.

- The Population Registration Act, 1950. This required that every individual be classified and provided with an official racial category.[12]

- The Group Areas Act, 1950. This provided for separate residential areas for members of different races, which allowed the state to invest heavily in infrastructure in White areas while providing minimal levels of services in black areas. It also facilitated the geographic exclusion of black communities by forcing them to take residence in townships on the periphery of urban centres.

- The Bantu Authorities Act, 1951. This law enabled the development of 'independent' homelands that were effectively rural reservations where Africans of different linguistic groups were forcibly removed to. These were ruled by tribal chiefs, and were used to strip Africans of citizenship rights in the 'White' South Africa.

[11] An amendment to the 1913 Natives Land Act was passed in 1936 that increased the percent of total land area where Africans could own property from 7% to 13%. Whites, who made up less than 20% of the population, were allotted over 80% of the land.

[12] We discuss this law in detail in the next section.

- The Bantu Education Act, 1953. This was used to intentionally limit the type and quality of education that Africans were provided with, so that they would only be adequately trained for subservient and menial tasks in the employment of Whites.

- The Separate Amenities Act, 1953. This governed which public goods people could access, depending on their race. This ranged from relatively petty levels such a parks, beaches, and restrooms; to more important facilities such as public transport networks and hospitals.

Once one understands the breadth and complexity of the legal framework that was used to implement the apartheid system, it becomes clear that it was necessary for every individual to have an official racial classification. This would be required so that people understood which laws applied to them and which did not, so that officers of the law could quickly and consistently determine an individual's race in order to determine whether a transgression had occurred, and so that different members of the judiciary or bureaucracy would be able to adjudicate or process violations consistently across time and space. In the next section we discuss the process by which racial classification occurred, which in turn enables us to explain our identification strategy.

3 Racial classification

3.1 Racial classification during Apartheid

One of the challenges facing the National Party was that there were many people who were either racially ambiguous or who held different racial identities in different social settings. The need for clarity on what constituted a race and how to determine an individual's racial category was addressed from a legal perspective by the Population Registration Act of 1950. This act required that every individual be classified and needed to obtain a South African ID card that would state the person's official racial category.

In terms of definitions, the Act provided for the creation of three categories[13]:

- A "White person" means a person who in **appearance** obviously is, or who is **generally accepted** as a White person; but does not include a person who, although in appearance obviously a White person, is **generally accepted** as a Coloured person.

- A "Native" means a person who in fact is or is generally accepted as a member of any aboriginal race or tribe of Africa;

- A "Coloured person" means a person who is **not a White person or a Native**;

There is a vagueness and self-referential structure in the definition of "White" and it appears that this was both intentional and pragmatic. The architects of Apartheid had in their minds the ideal of racial purity, with racially pure Europeans being White

[13]The Indian/Asian race group was initially part of Coloured group, but later became an official separate category through a different piece of legislature in 1959.

and superior to all other race groups. At the same time, a large part of the National Party's voter base were Afrikaners, i.e. South Africans of primarily Dutch descent who had originally settled in the Cape. Over the course of the preceding centuries, some degree of racial mingling had occurred, such that no Afrikaner could be absolutely certain about their ancestry. A practical issue thus arose whereby the people designing the law, while also considering the challenges of implementation, wrote the law while not mentioning "European" as the basis for being White. Related to these concerns was the existence of a sizable number of "marginal Whites", estimated at close to 100 000, who represented a threat to the process of racial classification if they were forced to become officially Coloured (James, 1992).

The main administrative device used for the initial classification was the South African Census of May 1951. In this Census, the enumerators would fill in the name and address of the respondent, as well as the enumerator's belief about the respondent's race. The actual process of assigning people with a classification was done by "Race Classification Boards". Officials who worked for these boards would use the data from the Census, as well as a photograph that was submitted by the applicant, as the basis for an initial classification. This initial classification would then be that individual's official racial category unless they appealed the decision. There were many levels of appeal but the first one involved sending the person to get an official photograph from a group of state vetted official photographers who would submit a new photograph. The process could continue on appeal if no resolution was achieved, but there was an eventual endpoint where a state official would use any number of physical 'tests' to determine a person's race.

Implementation of the Act was achieved fairly quickly and completely, and by 1958 95% of Whites, Coloureds and Indians had been issued with IDs that included a racial category (Breckenridge, 2014). Also worth noting is the relatively low levels of resistance to the classification process. Historically, less than 0.1% of people were ever re-classified.

In terms of implementation, we see that throughout the existence of the Population Registration Act[14] there were three different criteria being articulated as the basis for racial classification. These were appearance, social acceptance, and ancestry or descent. This being said, at least for the stock of people who were born before 1951, there was no official way to verify ancestry. Birth certificates existed but were not standardised, and it was also not clear whether the race on the birth certificate captured the race of the father or the mother, nor whether both parents themselves were of the same race. Thus, for the people born before the 1951 Census, the main criteria used for classification were just the two that were observable; appearance and social acceptance.

As time progressed, more and more people obtained an official racial classification. For children born after the 1951 Census, there was an increased ability to use the official racial classification of the two parents to classify the child. Thus, two officially White parents would have children who would also be classified as White, two officially Native parents would have children who would also be classified as Native, and all other children would be classified as Coloured. The idea was that, at some point, the appearance of the child would cease to be relevant and the ancestry of the child would be sufficiently well known from official records to generate a classification. This was

[14]The Act was officially repealed in 1991, along with a number of other Apartheid statutes.

formalized by an Amendment to the Population Registration Act (1967) [15] which was enacted on the 19th of May 1967 and applied retroactively to 7th July 1950.[16]

3.2 Racial classification and year of birth

The objective of our research is to quantify the magnitude of economic advantage that racial classification conferred on an individual in South Africa during Apartheid. Data limitations notwithstanding, the correlation between an individual's racial classification and their parents' racial classification, which in turn would imply a correlation between an individual's classification and their parents' socioeconomic standing, would lead to an endogeneity problem that confounds an OLS regression analysis.

For a specific subset of people born around the 1951 Census, however, we have variation in classification that would occur purely due to the relative change in the salience of appearance and social acceptability for classification on the one hand, as compared to the salience of ancestry on the other. In a society with a long history of genetic mixing, children's appearances will not be fully determined by their parents' appearances, and this was relevant for the classification process. Thus, it is possible that two White parents had a dark-skinned child, or that two Coloured parents had a very fair-skinned child.[17] The distribution of appearance amongst children of mixed race couples would probably have an even wider variance.

The extent of ambiguity in the classification process is well documented by reports by the "Survey of Race Relations", an annual publication that monitored government policies. In 1955, while the classification of the population born before 1951 was still in process, the report states that "there were already 90,000 border-line cases. There might be many more in the long run" (Horrell, 1956, p. 35). In the 1966/67 issue more detail is provided: "The Minister said that in some cases investigations had proved necessary because there was a measure of doubt: of those classified, 48,000 Whites were involved, almost 179,000 Coloureds, 14,000 Malays, 27 Indians, 14 Chinese, and 26,500 Africans." (Horrell, 1967, p. 19)

[15] The relevant section is the new section 5(5) which states:
(5) In the application of this section— (a) a person shall be classified as a white person if his natural parents have both been classified as white persons;
(b) a person shall be classified as a coloured person if his natural parents have both been classified as coloured persons or one of his natural parents has been classified as a white person and the other natural parent has been classified as a coloured person or a Bantu;
(c) a coloured person whose natural parents have both been classified as members of the same ethnic or other group, shall be classified as a member of that group;
(d) a person shall be classified as a Bantu if his natural parents have both been classified as Bantus.

[16] It is not coincidental that the shift in criteria for implementation occurred in 1967. South Africans are eligible to apply for an adult ID number at age 16. The passage of this Amendment was stimulated by the first cohort of post-1951 Census babies reaching 16 years of age in 1967.

[17] This paper was partly inspired by a story shared by our friend and former colleague Catherine Kannemeyer. Her maternal grandmother, Mabel Canterbury née Slatem, was the relatively dark-skinned child in an otherwise all-White immediate family. Her grandmother attended a different school to that of her siblings, was officially classified as Coloured, and married a Coloured man. She and her husband were forced to move out of Goodwood when it was declared 'Whites Only'. As an adult, she became estranged from all members of her immediate biological family, even though some members lived less than 10km away, and only reconnected with some of them much later in life. She died in 2001 without reconciling with some of her siblings. Catherine's mother, Jennifer, only saw her maternal grandmother once, and has only ever met one of her numerous cousins.

We expect that individuals born immediately after 1951 would be less likely to be classified as White than individuals born just before 1951, and that this differential would increase over time. The ancestry based criterion for classification required both parents to be classified as White in order for the child to be classified as White. Children of inter-racial couples could have been classified as White if those children were fair-skinned and were born before 1951, but those same fair-skinned children would have been classified as Coloured if they were born after 1951. Children of "racially ambiguous" people might have been classified as White if they were born before 1951, but would have been classified as Coloured if they were born after 1951 and at least one of the parents had already been classified as Coloured on the basis of the Census.

As an illustrative example, consider a couple where one partner was classified as White and the other classified as Coloured, who were married in 1945. This couple had four children, 2 born before 1951 and two born after 1951. Of the two older children, one had darker skin and was classified as Coloured and the other one had lighter skin and was classified as White, based on their appearances.[18] The two younger children had similar variation in appearances as their two older siblings, but since they are classified based on their parents' classifications, which is multi-racial, they are both classified as Coloured. The existence of people whose appearances are racially ambiguous thus enables us to identify the causal effect of being assigned to a different racial category, for the subset of people who might have been affected by this change in classification systems.

In principle, changes in racial classification due to this policy change could have applied to any margin: Coloured / White, African / Coloured, or African / White. In practice, most changes were likely to involve Coloureds. The reason is that the change would have mainly affected those people with a degree of racial ambiguity in terms of their appearance. These were particularly likely to involve Coloureds, as is clear from the 1966/67 Survey of Race Relations quote provided above.[19]

The two most relevant margins affected by the change in the racial classification process are thus likely to be the Coloured/ White margin, and the African / Coloured margin. Our analysis will focus on the Coloured/ White margin. This is mainly due to data limitations, as we explain in the next section. However, the Coloured / White margin is also likely to have been particularly relevant in practice. From 1983 to 1990 there were 5622 applications for reclassification from Coloured to White. This was close to double the number of applications for reclassification from African to Coloured (3,207), and there were no applications for reclassification from African to White (Du Pré, 1994).

[18] There are several documented cases in which families and siblings were indeed classified differently, with devastating implications for families and social cohesion.

[19] Indeed, there is evidence that the current Coloured population is highly genetically diverse, including a substantial share of European ancestry: "the CMA [Cape Mixed Ancestry] population shows the highest levels of intercontinental admixture of any global population, with nearly equal high levels of SAK [Southern African Khoesan] ancestry, Niger-Kordofanian ancestry, Indian ancestry, and European ancestry." (Tishkoff et al., 2009, p. 1043).

4 Data

The data for our analysis comes from the 1980, 1991 and 1996 Censuses.[20] For 1980 and 1991 the publicly available data includes the whole population whereas for 1996 only a 10% sample is available. We restrict our sample to Coloured and White South African citizens who were born in South Africa, and who, at the time of the relevant Census, were residing in areas that currently constitute the Western Cape or Northern Cape provinces.

The restriction to only consider the White and Coloured race groups is a substantial limitation in our study, but it is unavoidable. This is mainly because the 1980 and 1991 Censuses excluded certain former "Homeland" areas, and thus did not cover the entire South African population. The exclusion of Homelands is especially problematic for us because a large proportion of the African population were forcibly removed to the former Homelands (Abel, 2019). In addition, influx control in areas outside of the Homelands was such that employment was a condition for most Africans to be granted a Pass to reside in 'White' South Africa. This restriction on mobility would generate a sample selection issue if we were to include the Africans who were residing in the Northern or Western Cape at the time of the relevant Census.[21]

The geographic restriction to the Western Cape and Northern Cape provinces is imposed because these are the areas where the overwhelming majority of the Coloured population lived. In 1996, these provinces accounted for only 12% of the total population in South Africa, but accounted for 72% of the total Coloured population. Since our instrument mainly affects the classification of Coloureds, using data from areas with no Coloureds would result in a weak instruments problem.

The geographical restriction is based on place of residence rather than the place of birth because place of birth is only available for one census year (1980). One potential disadvantage of using place of residence to select our sample is that migration decisions may be endogenous to an individual's racial classification. This is particularly true in the context of South Africa under apartheid, where African and Coloured people were subject to "forced removals" because of their racial classification. However, unlike the case of Africans, Coloureds were always relocated within the same city or region. This implies that most Coloureds who were forced to migrate are likely to remain in our sample. Because our identification relies on cohort variation rather than spatial variation, as long as migrants remain in our sample, this type of migration should not substantially affect our estimates.

Finally, we further restrict our sample to cohorts born in a window around 1951, and focus primarily on those born between 1931 and 1961.

The variables that we use are *year of birth*, *race*, *education*, *employment* and *income*. The processing of these variables is straightforward. Education is processed into a numerical variable of school grades completed. The only noteworthy issue is that there is a small proportion of people in the 1996 Census with missing values for the race variable (3%). The income variable for 1991 and 1996 is reported in brackets;

[20] These data are publicly available at https://www.datafirst.uct.ac.za/.

[21] The interaction between residency in these regions and employment was further complicated by the Coloured Labour Preference Policy, which reserved certain types of jobs in the Cape province for Coloureds. This, combined with the pass laws, meant that most Africans could not legally live in the Cape Province.

we impute a value using the midpoint of the bracket.[22] We adjust incomes for inflation using the Statistics South Africa's historical CPI series, with 2010 as base year. Approximately 2% of respondents have extremely low income levels (less that 0.1 rand per month in 2010 rands); we code these as missing.

Table 1 contains the descriptive statistics from our sample, reported separately by year and by sex. Around 30% of the sample is classified as White and this number is similar across the three waves. Years of education are also fairly constant across waves, although they are slightly lower in 1980. Women are much less likely to be employed than men. Even the income variable, which is typically measured with the most noise, shows a fairly consistent pattern across waves. Among those with positive income values, women earn less than men, but for both groups income rises slightly across waves. Overall, the patterns in the data seem plausible and this provides us with confidence about the quality of the data.

To illustrate just how large the differences in economic outcomes across racial categories are, Figure 1 shows the densities of log income in 1980, 1991 and 1996. These are presented separately for men and women aged between 20 and 65. The income distribution for Whites lies clearly to the right of the corresponding distribution for Coloureds. In fact, there is only limited overlap between the two distributions, particularly for men. This indicates that relatively low income White men still earned more than relatively highly paid Coloured men. These differences become slightly smaller over time and are somewhat less pronounced for women, although they remain large.

5 Empirical Approach

We want to know by how much a particular racial classification benefited one group relative to another. In our case, however, there is a dual causality between racial classification and subsequent economic outcomes. As argued by Posel (2001), the 'social acceptance' criterion described in the Population Registration Act of 1950 involved a judgement about a person's social status as much as it involved a judgement about their physical appearance. Indeed, relevant information in difficult cases included an individual's place of work, occupation and wages earned. Add to this the high levels of inter-generational transmission of both racial classification and socioeconomic status, and it becomes clear how the raw difference in average economic outcomes is probably an over-estimate of the true causal effect of a particular classification.

The race classification law that we use changed the official racial composition of the population for cohorts born after 1951, relative to earlier cohorts. We use this change to identify the effect of one's official race on labor market outcomes. This approach essentially attributes departures from trend in outcomes for cohorts born after 1951 to the change in the racial classification process.

There are two clear *a priori* threats to our identification strategy. First, outcomes such as employment and income have a strong non-linear life-cycle profile. For a given census year, different birth cohorts are observed at different moments in their life-cycle.

[22] The top bracket is open ended. We thus use as the endpoint of the topmost bracket three times the lower limit of that bracket. This choice is arbitrary but is unlikely to have a strong effect on our estimates since our analysis uses income in logs and there are relatively few observations in those categories (0.01% in 1996 and less than 0.001% in 1991).

Non-linear life-cycle patterns translate into non-linear patterns in outcomes across cohorts. These non-linearities in the dependent variable can threaten the validity of our instrument, thus potentially rendering it invalid. To deal with this problem one could introduce higher-order polynomials, but this is not recommended (Gelman & Imbens, 2018). Instead, we take advantage of the three census years that we have and control for life-cycle patterns using a full set of age dummies. This accounts for the non-linearities in outcomes across cohorts induced by life-cycle considerations.

Second, there is the problem of age heaping and year of birth heaping where an improbably large share of people use round figures for these variables (see Table A1 in the online appendix).[23] This can be problematic because such heaping is correlated with race and education: Those more likely to report being aged and born in a round year tend to be Coloured and have fewer years of education. Moreover, the threshold that we use, 1951, falls just after a round number. Since people reporting to be born in 1950 are less likely to be White, and are more likely to display relatively low levels of education, this can generate an artificial discontinuity in outcomes after 1950 that confounds our approach. We address these problems in the following way. The problem of age heaping is partially addressed by the age dummies that we use to deal with the life-cycle problems. These dummies absorb all age effects, and therefore also absorb those effects that arise due to age heaping. We deal with the problem of year of birth heaping using the "donut" estimator suggested in Barreca, Lindo, and Waddell (2016). This estimator simply removes the observations with years of birth where most heaping occurs. In our case, these relate to those years of birth ending in zero. Below we check the robustness of our results to including the years of birth ending in zero.

We aggregate our data by census year and year of birth and conduct the analysis separately for men and women. Thus, our data points are averages of census-year x year-of-birth x sex cells. In our analyses below we use weights, where the weights are the number of observations in each cell. Since the 1996 data is a 10% sample, the cells corresponding to this year are 10 times smaller and weight ten times less in the analysis. In our benchmark specification we leave these weights, but we also show the robustness of results to two ways of dealing with the issue: we inflate the 1996 cell-based weights tenfold; and we use individual level data with weights equal to 1 for 1980 and 1991, and to 10 for 1996, clustering standard errors by year of birth. Overall, the results from these specifications yield similar estimates to those obtained from the benchmark specification.

The first stage is:

$$White_{yac} = \alpha_{1y} + \alpha_{2a} + \alpha_3 yob_c + \alpha_4 D51_c + \alpha_5 yob_c D51_{1c} + u_{yac}$$

where $White_{yac}$ is the fraction of Whites in each census year, age, and cohort cell, α_{1y} and α_{2a} are census year and age fixed effects, yob_{1c} is year of birth (centered in 1951), and $D51_c$ is an indicator variable for cohorts born during or after 1951. The variables $D51_c$ and $yob_c D51_c$ are our instruments. They capture the change in the level and trend in official race classification for cohorts born after 1951. The second stage is:

[23]The reason why there is both age heaping and year of birth heaping is that in the 1991 and the 1996 censuses respondents were asked for both their age and their year of birth. These two variables were then combined by the census agency to report a single age variable.

$$Y_{yac} = \theta_{1y} + \theta_{2a} + \theta_3 yob_{1c} + \rho White_{yac} + v_{yac}$$

We conduct the analysis separately for men and women because labour market outcomes differ markedly between them. We estimate these regressions using 2SLS with heteroskedasiticity robust standard errors. The coefficient of interest is ρ. The second stage controls for all of the fixed effects in the first stage and for year of birth. The two omitted instruments, $D51_c$ and $yob_c D51_c$, identify the effect of being classified as White.

We restrict the sample to cohorts affected by the policy change in 1951. Given our focus on labour market outcomes, it makes sense to restrict the sample to individuals who are at least 20 years old. Since our earliest census is from 1980, this puts an upper bound on the window to the right of the threshold of 10 years. For the boundary to the left of 1951, we do not have this problem and we use 20 years. Our benchmark cohorts are thus those born between 1931 and 1961. Throughout the paper we also show the results for a shorter window that is half as large as the window used in our benchmark case, i.e. 5 years on the right side and 10 years on the left of 1951. We also check whether our main results are robust to using other windows and specifications.

5.1 Validity

The validity of our approach rests on the assumption that, except for the effect of racial classification, background variables relevant for economic outcomes did not experience a jump or a kink for cohorts born after 1951. The conventional way to check this assumption is to check whether pre-classification variables exhibit a kink or a jump at the specified threshold. In our case, this is difficult to implement. Racial classification under Apartheid impacted on all aspects of life so strongly, that there is probably no individual level variable that would be unaffected by one's classification. This would include health outcomes, mortality risks, educational outcomes, marital status, linguistic background, and even probably religious affiliations. An alternative would be to use family background variables. Unfortunately, members of the same family cannot be linked in the relevant census unless they live together, which would not generally be the case for the ages of "children" considered.

The one variable that we can use to check the validity of our approach is the overall size of the different birth cohorts. If the change in racial classification that we exploit did not lead to migration, then the size of the birth cohorts in our sample should be exogenous to racial classification. If we find no change in the overall size of the population, then any jump and kink that we find in racial shares would be a strong indication that people were indeed classified differently to earlier birth cohorts. This would then be fully consistent with our interpretation of the estimates as the effects of the racial classification policy changes.

Table 2 shows the results of this validity test. The table shows estimates from reduced form equations such as the first stage regression model described above, where the outcome variable is the log of the size of the cohort in each year of birth / census year cell. The table shows coefficients for the dummy variable identifying cohorts born after 1951 and the interaction of this with year-of-birth, thus capturing the jump and kink in population size for the cohort born in 1951. Each column corresponds to a different specification. The first specification corresponds to the benchmark specification

with a relatively large window. The second specification corresponds to the robustness specification with the smaller window mentioned above.

The coefficients in Table 2 are generally small and statistically insignificant, implying that there is no substantial change in the population level or trend after 1951. Nevertheless, the coefficient for the born-after-1951 dummy is marginally statistically significant in the benchmark specification, and this could be considered a threat to the validity of our approach. It turns out that this small effect is mainly driven by the particularly small size of cohorts born during World War II. The left panel in Figure 2 shows the residuals from regressions of the log of cohort size on census year and age dummies, as a function of the year of birth. The figure also adds the fitted values from a regression of these residuals on the jump and kink variables. This is the counterpart of the reduced form estimate in column 1 of Table 2. The dots do not show a particularly strong jump at 0, which corresponds to the 1951 cohort. Instead, there is quite a clear trough in the years that correspond to 1942 and 1943. These are the World War II years, where many men where abroad, and fewer children were born.

To address the extent to which these unusual years are responsible for the positive jump coefficient in Table 2, column 3 re-estimates the benchmark specification with a dummy variable with a value of one for birth-years 1942 and 1943. The jump coefficient becomes almost zero. The F-statistic for the jump and trend change coefficient being zero is small, at 0.96. This is confirmed by the right panel in Figure 2, which shows the same residuals as the left panel, but including the 1942 and 1943 birth-year dummy in the regression: no clear change in 1951 is observed. It appears that, once we account for the war years, there is neither a jump nor a kink in the size of the population in our sample born after 1951. This provides some assurances regarding the validity of our approach. For the rest of the analysis, all of our analyses include the war years dummy variable.

6 Results

6.1 First stage

Table 3 shows the first stage results for men and for women separately. The structure of the table is the same as in Table 2, showing the coefficients for the jump and kink at 1951 with the war years dummy. There are now two panels, the top one for men and the bottom one for women. The outcome variable is now the proportion of people classified as White (as compared to being classified as Coloured).

All coefficients in the table are negative, implying that a change in racial shares occurred in 1951, in terms of both level and trends. Our benchmark estimates indicate a drop in the share of Whites in 1951 of about 3 percentage points, and this share falls further by approximately 0.3 percentage points for each subsequent year of birth. This implies that being born 10 years after 1951 reduced the probability of being classified as White by approximately 6 percentage points, relative to what would have been predicted by the pre-1951 trends in racial shares. Since these regressions include dummies for being born in 1942-43, this implies that once the effect of the war is taken into account, racial shares changed substantially after 1951 even though the aggregate population size did not.

In the benchmark specifications, all of the coefficients are statistically significant at conventional levels for both men and women. The F-statistic used to gauge the strength of the instrument is large, greater than 20, thus indicating that the instruments satisfy the conventional test for weak instruments. When using the estimation sample based on the small window, the coefficients become less precisely estimated, as expected. All of the coefficients remain negative and of similar magnitude, although most are not statistically significant at conventional levels.

Figure 3 depicts the graphical illustration of the first stage, separately for men and women. It shows the residuals of a regression of the share of Whites on census year, age dummies, and the war birth-years dummy, as a function of year of birth. The figure shows quite clearly that after 1951, the share of Whites drops and the slope of the trend line decreases.

6.2 IV results

Table 4 shows the IV results. Each row corresponds to an outcome variable and each column to a different specification. Column 1 shows the OLS estimate for comparison. The OLS specification is a regression of the respective outcome on the variable White, controlling for age dummies and census year dummies. The IV regressions identify the effect of interest locally for cohorts around the 1951 threshold. We thus restrict the sample for the OLS regression to cohorts born between 1948 and 1953. The results are shown separately for men (above) and women (below).

Our instrument identifies effects for those affected by the change in racial classification: people who, if they had been born before 1951 would have been classified as White, but if they had been born after 1951 would have been classified as Coloured. The treatment effect for this population is unlikely to be the same as the Average Treatment Effect. Therefore, the IV estimates cannot be directly compared to the OLS estimates. It is likely that the 'compliers' of our instrument are people with a relatively light skin tone and with more homogeneous characteristics in general than the whole population of Whites and Coloureds combined. For this reason, if this group could be identified and racial categories could be attributed to each individual, we would probably observe smaller raw racial differences for this sub-population than those shown in the OLS estimates for the whole population. Nonetheless, we provide the OLS estimates as a reference point to anchor the magnitudes of the IV results.

The OLS results show very large racial differences in socioeconomic outcomes. White men and women have approximately 5 more years of education than their Coloured counterparts. White men have over 1.5 more log points in income, while White women have over 1.2 more log points in income, relative to Coloured men and Coloured women respectively. For men, this is approximately equal to the difference in log income between White men at the 70th income percentile and those at the 30th income percentile. Differences in employment rates are high for men at 10 percentage points, but are negligible for women.

Columns 2 and 3 show our IV estimates with the benchmark window and the short window. The benchmark specification with years of education as the outcome shows a coefficient of 3.7 for men and 3.9 for women. This implies that being classified as Coloured as opposed to White implied a loss in educational attainment of almost 4 years on average, for the compliers of our instrument. This corresponds to around

three quarters of the raw gap in educational attainment between the Coloured and White population groups. Estimates using the shorter window show somewhat larger estimates for men and smaller estimates for women.

The results for employment are less clear. For men, the estimates are between -0.2 and 0.01, but the standard errors too large to draw definite conclusions. For women, in contrast, the estimates are very large and negative, in the order of -0.8. This large negative value is surprising, particularly given that the OLS estimate is approximately zero. We believe the reason for this result lies in the changing life-cycle patterns of labour force participation amongst women during the period under consideration.

Figure 4 shows the employment rates of females and males separately, as a function of age in 1980, 1991 and 1996 (Coloureds and Whites combined). While employment for men decreases over time for all ages during working age, the reverse is true for women. This suggests an increase in female labour force participation during the period under consideration. However, for women, not only levels of employment change over time; the age profile of employment also changes. For 1980, and only for 1980, we observe a steep decline from age 20 until age 30, which becomes less steep in subsequent censuses. Presumably this profile is related to marriage and motherhood. This profile generates a kink in female employment around age 30 in 1980 that is not present in other years. This kink can confound the effect of our instruments and can explain our very large IV estimates for female employment. People aged 30 in 1980 were born in 1950. The kink around age 30 in 1980 corresponds to an upward kink in year of birth for women born around 1950. [24] This is close to the year of birth threshold that we use for our instruments. Thus, it is likely that the (upward) kink in employment observed in 1980 for women born after 1951 appears in our estimates as being driven by the correspondingly lower share of Whites, and hence manifests as a negative effect of being White on female employment. Because this kink only occurs only in 1980, it is not picked up by the full set of age dummies that we include in our regressions, and thus confounds the effect of the instruments.

Our most striking and statistically significant results are those relating to income for men. The IV coefficient is around 1.1 (or 1.4 using the short window). This implies that the effect of racial classification on income is very large. On average, being classified as a White man as opposed to a Coloured man would have *more than tripled* a person's income. Relative to the OLS coefficient of 1.5, it corresponds to about 65% of the observed income differences between White and Coloured men.

The coefficients for female incomes are also large, but they are imprecise and unstable across specifications. The coefficients in the benchmark specification mirrors the results for employment and is probably confounded by the same changes in labour force participation rates discussed above. This, together with the large standard errors, makes it impossible to draw clear conclusions about the effects of racial classification on female income.

Figure 5 shows the graphical representations of the reduced form results corresponding to those presented in Table 4. The jumps in years of education for both males and females are evident from the figure. For male income, both the jump downwards and the downward change in trend is also apparent. This pattern closely mirrors the one observed for the proportion of Whites in Figure 3. Consistent with the incon-

[24] See Figure A1 in the online appendix, which highlights employment changes by birth cohort, showing the same data as Figure 4, but as a function of year of birth instead of age.

clusive results in Table 4, the graphs for male employment and female income do not deliver very clear jumps or changes in trend after 1951.

6.3 Robustness

Our most remarkable results correspond to the effect of racial classification on income for men. We discuss the robustness of this particular result to different specifications. We also consider briefly the robustness of results regarding education.

Table 5 presents the coefficients of the first stage (top panel), the reduced form coefficients (middle panel), and the IV coefficient (bottom panel), under different specifications. Columns 1 and 2 show the two specifications shown already in Table 4: with the benchmark window, and the short window. The two specifications show a negative jump and kink in the White racial classification and the income variable, and a corresponding positive IV coefficient.

Columns 3 to 4 consider robustness to different windows, using the Imbens and Kalyanaraman (2012) (IK) and the Calonico, Cattaneo, and Titiunik (2014) (CCT) procedures, as implemented in Calonico, Cattaneo, Farrell, and Titiunik (2017).[25] Our results are fairly robust to using these windows. In fact, the IV point estimates using these windows are larger than the benchmark estimates, although they are also more imprecisely estimated. These differences are mainly due the fact that the first stage coefficients using these optimal windows are smaller than they are in the benchmark case, particularly the CCT one. Overall, the reduced form coefficients with optimal windows are comparable to those in the benchmark specification.

Column 5 considers the implications of ignoring the potential year of birth heaping problem unaddressed, by leaving the years of birth ending in zero in the analysis. Relative to the benchmark specification, the coefficient for the 1951 jump becomes smaller in both the first stage and in the reduced form. This was as expected because people reporting to have been born in a zero-ending year of birth (including 1950) are more likely to be Coloured and have lower incomes; and this makes the reduction in Whites and the reduction in income for those born after 1951 less marked. Nevertheless, the IV estimate remains similar to the benchmark.

Columns 6 and 7 check the robustness of our results to different ways of handling the fact that the 1996 data includes only a 10% sample. Column 6 uses aggregate data but inflates our cell-size weights in 1996 observations by a factor of 10. Column 7 uses individual data weighting 1996 individuals by ten. Both procedures generate very similar point estimates. Coefficients remain strongly significant although somewhat smaller than in the benchmark specification. This is perhaps not surprising given that by 1996 Apartheid had already ended.

Column 8 uses a quadratic specification instead of a linear one. The results remain qualitatively unchanged, although coefficients (including the IV one) become larger in absolute value.

Overall our main result appears to be fairly robust. In all specifications, a decrease in Whites after 1951 is accompanied by a decrease in income, leading to a positive IV coefficient. The reduced form coefficients are mostly fairly similar to those obtained

[25] We compute the optimal bandwidth using as outcome variable in the procedure the residual of a regression of income on a full set of age dummies, census year dummies, and the war dummy. We do this because of the marked age profile of income.

from the benchmark specification. The size of the IV coefficient varies by specification, from around 0.8 in the specifications that inflate the 1996 data, to 1.8 in the quadratic and the IK window specification (not considering the CCT window specification that has small first stage coefficients). The benchmark specification of 1.1 lies between these two sets of estimates.

Tables A2 and A3 in the online appendix conduct the same robustness tests for the results concerning education for men and women. As with income, the IV coefficients for education are all positive regardless of the specification, but there is variation in the size of coefficients; more so than for the case of income. Again there are some very large coefficients mainly driven by the small first stage estimates and some small coefficients (in this case, mostly those including the zero-ending years of birth). And again the benchmark coefficients of 3.7 (for men) and 3.9 (for women) lie in between these extremes.

7 Concluding remarks

In this research, we illustrated an empirical approach that can be used to estimate the effects of cumulative discrimination on economic outcomes. We applied this method to obtain the first causal estimate of the effect of racial classification on labour market outcomes in South Africa during apartheid. We used a change in the way that people were classified to address the endogeneity problems common in most empirical studies on discrimination, thus identifying the effects of being White for a group of marginal people on the Coloured-White margin.

Being White had a large and statistically significantly positive effect on educational attainment, regardless of sex. For employment, our IV results did not allow us to reach any clear conclusions for the sample being analysed. For earnings, being a White male resulted in an exceptionally large racial earnings premium. In total, differences in income due to racial classification for the population of compliers corresponds to about 65% of the raw income gap between White and Coloured men. In addition, we argue that 65% is probably a conservative estimate because the complying population is likely to have been been more homogenous than the overall population.

The main limitation in our study is that we have no clear way to estimate the effect of racial classification for Africans. Since this group is the largest demographic group in the country, and since this group was clearly the most disadvantaged in terms of violence, neglect, and restrictions; it would be desirable to have a similar measure for the African-White racial premium. This was not possible due to data limitations, the requirement that there be a sufficiently large marginal group, and the imposition of much stricter geographic residency restrictions for Africans.

Overall, the approach that we used highlights a novel method that may prove to be useful in other contexts where discrimination or affirmative action need to be evaluated. The requirements for using this method for econometric identification include the need to classify people into mutually exclusive categories for administrative purposes, combined with an exogenous change in the rules that determine an individual's classification. If there exists a set of people whose classification would be altered by this change in the rules, then there exists the opportunity for the empirical estimation of a more holistic concept of discrimination than has previously been estimated.

References

Abel, M. (2019). Long-run effects of forced resettlement: evidence from Apartheid South Africa. *The Journal of Economic History*, *79*(4), 915–953.

Altonji, J. G., & Blank, R. M. (1999). Race and gender in the labor market. *Handbook of labor economics*, *3*, 3143–3259.

Barreca, A. I., Lindo, J. M., & Waddell, G. R. (2016). Heaping-induced bias in regression-discontinuity designs. *Economic Inquiry*, *54*(1), 268–293.

Bertrand, M., & Duflo, E. (2017). Field experiments on discrimination. In *Handbook of economic field experiments* (Vol. 1, pp. 309–393). Elsevier.

Bertrand, M., & Mullainathan, S. (2004). Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination. *American economic review*, *94*(4), 991–1013.

Blinder, A. S. (1973). Wage discrimination: reduced form and structural estimates. *Journal of Human resources*, 436–455.

Breckenridge, K. (2014). The Book of Life: The South African population register and the invention of racial descent, 1950-1980. *Kronos*, *40*(1), 225–240.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2017). rdrobust: Software for regression-discontinuity designs. *The Stata Journal*, *17*(2), 372–404.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295–2326.

Clark, N. L., & Worger, W. H. (2016). *South Africa: The rise and fall of apartheid*. Routledge.

Dubow, S. (2014). *Apartheid, 1948-1994*. Oxford University Press.

Du Pré, R. H. (1994). *Separate but unequal: the coloured people of South Africa, a political history*. Jonathan Ball Publishers.

Gelman, A., & Imbens, G. (2018). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, 1–10.

Greiner, D. J., & Rubin, D. B. (2011). Causal effects of perceived immutable characteristics. *Review of Economics and Statistics*, *93*(3), 775–785.

Guryan, J., & Charles, K. K. (2013). Taste-based or statistical discrimination: The economics of discrimination returns to its roots. *The Economic Journal*, *123*(572), F417–F432.

Heckman, J. J. (1998). Detecting discrimination. *Journal of Economic Perspectives*, *12*(2), 101–116.

Hoff, K., & Pandey, P. (2006). Discrimination, social identity, and durable inequalities. *American Economic Review*, *96*(2), 206–211.

Hoff, K., & Pandey, P. (2014). Making up people—The effect of identity on performance in a modernizing society. *Journal of Development Economics*, *106*, 118–131.

Holland, P. W. (1986). Statistics and causal inference. *Journal of the American Statistical Association*, *81*(396), 945–960.

Horrell, M. (1956). *A survey of race relations in South Africa, 1955*. South African Institute of Race Relations Johannesburg.

Horrell, M. (1967). *A survey of race relations in South Africa, 1966*. South African Institute of Race Relations Johannesburg.

Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of economic studies*, *79*(3), 933–959.

James, W. G. (1992). TE Donges and the Group Areas Act.

Juhn, C., Murphy, K. M., & Pierce, B. (1993). Wage inequality and the rise in returns to skill. *Journal of political Economy*, *101*(3), 410–442.

Neumark, D. (2018). Experimental research on labor market discrimination. *Journal of Economic Literature*, *56*(3), 799–866.

Neumark, D., Bank, R. J., & Van Nort, K. D. (1996). Sex discrimination in restaurant hiring: An audit study. *The Quarterly journal of economics*, *111*(3), 915–941.

Oaxaca, R. (1973). Male-female wage differentials in urban labor markets. *International economic review*, 693–709.

Posel, D. (2001). What's in a name? racial categorisations under apartheid and their afterlife. *Transformation*, 50–74.

Siegelman, P., & Heckman, J. (1993). The Urban Institute audit studies: Their methods and findings. *Clear and Convincing Evidence: Measurement of Discrimination in America, Washington*, *187*, 258.

Steele, C. M., & Aronson, J. (1995). Stereotype threat and the intellectual test performance of African Americans. *Journal of personality and social psychology*, *69*(5), 797.

Thompson, L. M. (2001). *A history of South Africa*. Yale University Press.

Tishkoff, S. A., Reed, F. A., Friedlaender, F. R., Ehret, C., Ranciaro, A., Froment, A., ... others (2009). The genetic structure and history of Africans and African Americans. *Science*, *324*(5930), 1035–1044.

Tables

Table 1: Descriptive statistics of cohorts born in 1931-1961

	1980			1991			1996		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Male									
White	0.32	0.46	510961	0.31	0.46	414779	0.32	0.47	38514
Years of education	7.77	4.09	506724	8.24	4.14	408723	8.34	4.21	36708
Employed	0.88	0.33	510961	0.84	0.36	414779	0.73	0.44	38514
Log income	3.65	1.04	446576	3.72	1.24	367654	3.71	1.19	32169
Female									
White	0.31	0.46	519234	0.30	0.46	454676	0.31	0.46	43942
Years of education	7.51	3.89	514645	8.00	3.88	447928	8.05	3.92	42363
Employed	0.49	0.50	519234	0.49	0.50	454676	0.45	0.50	43942
Log income	2.97	1.05	265861	3.12	1.14	245243	3.16	1.10	25154

Means, standard deviations and number of observations of the variables used in the analysis by census year and gender. The sample is restricted to cohorts used in the benchmark analysis (born between 1931 and 1961). Statistics are based on the individual level data. The 1996 census has considerably lower sample size because it is based on the publicly available 10% sample.

Table 2: Placebo test: Effect of instruments on total population

	1	2	3
Born after 1951	0.059*	-0.003	0.016
	(0.031)	(0.052)	(0.029)
Year of Birth x Born after 1951	0	0.006	0.003
	(0.005)	(0.011)	(0.005)
Fstat	3.36	0.15	0.95
Subset	Benchmark	Short window	War birth-years dummy
Cohorts	1931-61	1941-56	1931-61
N	168	90	168

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Coefficients from regressions of log population size on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Birth-years ending in zero are removed to partially deal with heaping. Fstat is the value of the F-statistic testing whether both coefficients are equal to zero. Data are aggregated by year or birth, census year and gender; regressions use cell size weights. The first column is the benchmark specification. The second column uses a shorter windwow arond the 1951 cohort. The third column controls also for war birth-years with a dummy for 1942 and 1943, where cohort sizes were unusually low.

Table 3: First stage: Effect of instruments on being classified as White

	1	2
Male		
Born after 1951	-0.031***	-0.035
	(0.009)	(0.027)
Year of Birth x Born after 1951	-0.003**	-0.008*
	(0.002)	(0.004)
Fstat	24	4.57
Female		
Born after 1951	-0.031***	-0.023
	(0.008)	(0.027)
Year of Birth x Born after 1951	-0.003*	-0.006
	(0.002)	(0.005)
Fstat	20.96	1.52
Subset	Benchmark	Short window
Cohorts	1931-61	1941-56
N	84	45

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Coefficients from regressions of share of Whites on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Birth-years ending in zero are removed to partially deal with heaping. Fstat is the value of the F-statistic testing whether both coefficients are equal to zero. Separate analysis for men and women. Data are aggregated by year or birth, census year and gender; regressions use cell size weights. Column 1 is the benchmark specification. Column 2 uses a shorter windwow arond the 1951 cohort. Both specifications control for war birth-years with a dummy for 1942 and 1943,where cohort sizes were unusually low.

Table 4: OLS and IV results: The effect of being classified White on economic outcomes

	1	2	3
Male			
Years of education	5.213***	3.676***	5.152**
	(0.012)	(0.886)	(2.299)
Employed	0.096***	0.015	-0.248
	(0.001)	(0.107)	(0.335)
Log income	1.537***	1.098***	1.407***
	(0.003)	(0.158)	(0.416)
Female			
Years of education	5.114***	3.906***	1.6
	(0.011)	(0.837)	(3.436)
Employed	-0.003	-0.887***	-0.613**
	(0.002)	(0.164)	(0.256)
Log income	1.245***	-0.771*	0.578
	(0.005)	(0.432)	(1.061)
Estimation	OLS	IV	IV
Subset	Born close to 1951	Benchmark	Short window
Cohorts	1948-54	1931-61	1941-56
N	150852	84	45

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Coefficients from OLS and 2SLS regressions of education, employment and log income on being classified as White, controlling for year of birth, age and census year dummies. The instruments for being classified as white are a dummy for cohorts born after 1951, and the interaction of this with year of birth. Data are aggregated by year or birth, census year and gender; regressions use cell size weights. Birth-years ending in zero are removed to partially deal with heaping. Each column correspond to a different specification. Column 1 uses the OLS estimator for comparison. This is estimated using cohorts born close to the year 1951. Columns 2 to 3 use the IV estimator. Column 2 is the benchmark specification. Column 3 uses a shorter windwow arond the 1951 cohort. Both IV specifications control for war birth-years with a dummy for 1942 and 1943, where cohort sizes were unusually low.

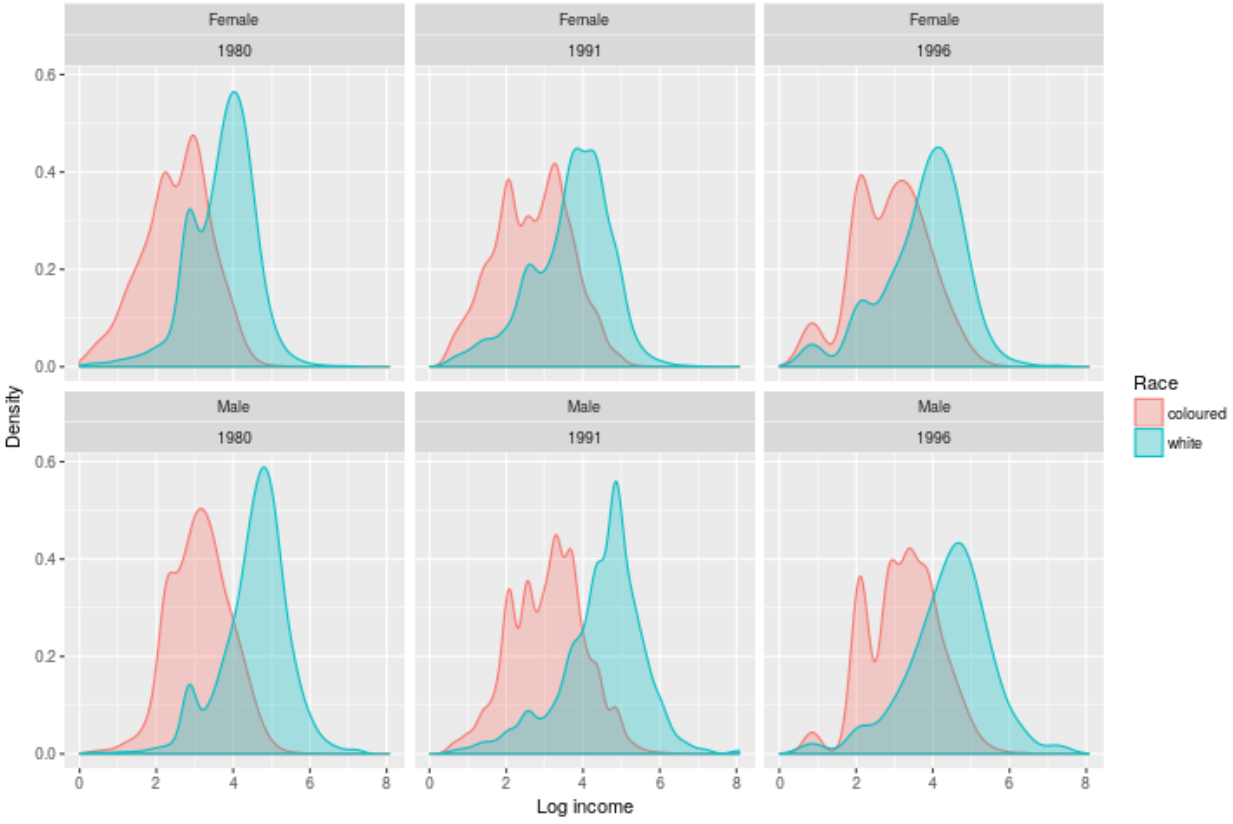
Table 5: Robustness checks for male log income results: First stage, reduced form, and IV estimates for different specifications

	1	2	3	4	5	6	7	8
White(First Stage)								
Born after 1951	-0.031***	-0.035	-0.029	-0.02	-0.018*	-0.026***	-0.026***	-0.04***
	(0.009)	(0.027)	(0.017)	(0.021)	(0.01)	(0.009)	(0.005)	(0.011)
Year of Birth x Born after 1951	-0.003**	-0.008*	-0.004	-0.001	-0.004***	-0.004***	-0.004***	-0.007**
	(0.002)	(0.004)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.003)
Fstat	24	4.57	2.45	0.52	25.67	18.78	66.94	7.93
Log income (Reduced Form)								
Born after 1951	-0.045*	-0.077	-0.067	-0.059	-0.018	-0.043*	-0.044***	-0.075***
	(0.023)	(0.067)	(0.04)	(0.059)	(0.017)	(0.021)	(0.012)	(0.025)
Year of Birth x Born after 1951	-0.002	-0.008	-0.005	-0.001	-0.006**	0	0	-0.014*
	(0.004)	(0.01)	(0.004)	(0.009)	(0.002)	(0.003)	(0.002)	(0.008)
Fstat	8.72	1.5	2.24	0.51	8.41	3.77	8.04	5.21
Log income(IV)								
White	1.098***	1.407***	1.848***	3.07	1.201***	0.831***	0.778***	1.917***
	(0.158)	(0.416)	(0.473)	(1.973)	(0.17)	(0.292)	(0.202)	(0.365)
Subset	Benchmark	Short Window	IK window	CCT window	With zero-ending birth-years	Weights 1996x10	Individual data	Quadratic
Cohorts	1931-61	1941-56	1941-61	1944-58	1931-61	1931-61	1931-61	1931-61
Bandwidth left	20	10	10	7	20	20	20	20
Bandwidth right	10	5	10	7	10	10	10	10
N	84	45	57	42	93	84	807995	84

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Top Panel: Coefficients from OLS regressions of share of Whites on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Middle Panel: Coefficients from OLS regressions of male log income on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Fstat is the value of the F-statistic testing whether both coefficients are equal to zero. Bottom Panel: Coefficients from 2SLS regressions of male log income on being classified as white, controling for year of birth, age and census year dummies. The instruments for being classified as white are a dummy for cohorts born after 1951, and the interaction of this with year of birth. Unless otherwise stated, birth-years ending in zero are removed to partially deal with heaping. Unless otherwise stated, data is aggregated by year or birth, census year and gender, and regressions use cell size weights. Column 1 is the benchmark specification. Column 2 uses a shorter windwow around the 1951 cohort. Columns 3 and 4 use an optimal window: the Calonico, Cattaneo, and Titiunik (CCT) procedure in column 3 and the Imbens and Kalyanaraman (IK) procedure in column 4. In both procedures male log income is used as outcome variable in the procedure. Column 5 leaves the birth years ending in zero in the analysis. Columns 6 and 7 address the issue that the 1996 data includes only a 10% sample. Column 6 uses aggregate data but inflates cell-size weights in 1996 observations by a factor of 10. Column 7 uses individual data weighting 1996 individuals tenfold and clustering standard errors at the year of birth level. Column 8 uses a quadratic specification in year-of-birth. All specifications control for war birth-years with a dummy for 1942 and 1943, where cohort sizes were unusually low.

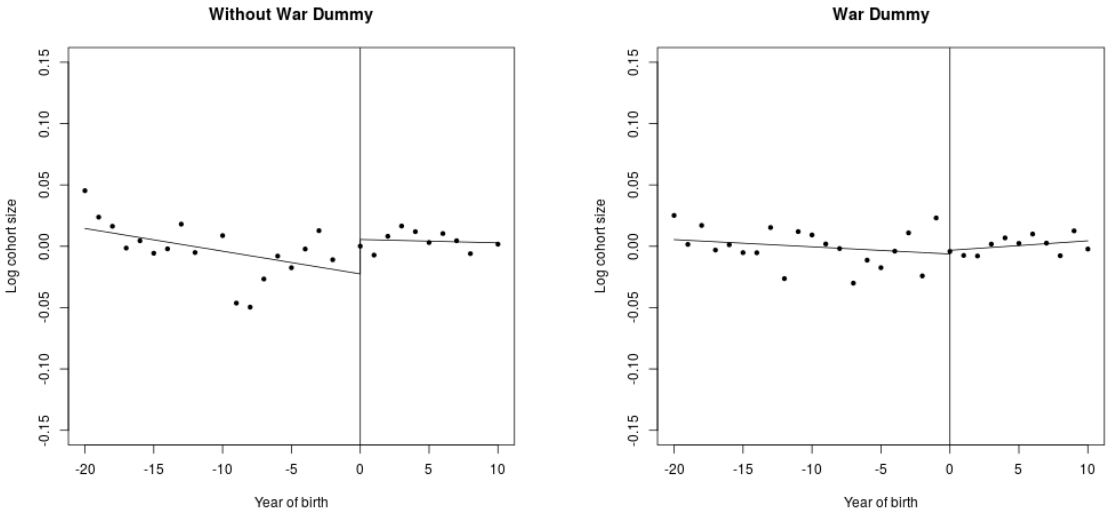
Figures

Figure 1: Distribution of log income of Coloureds and Whites, by census year and gender



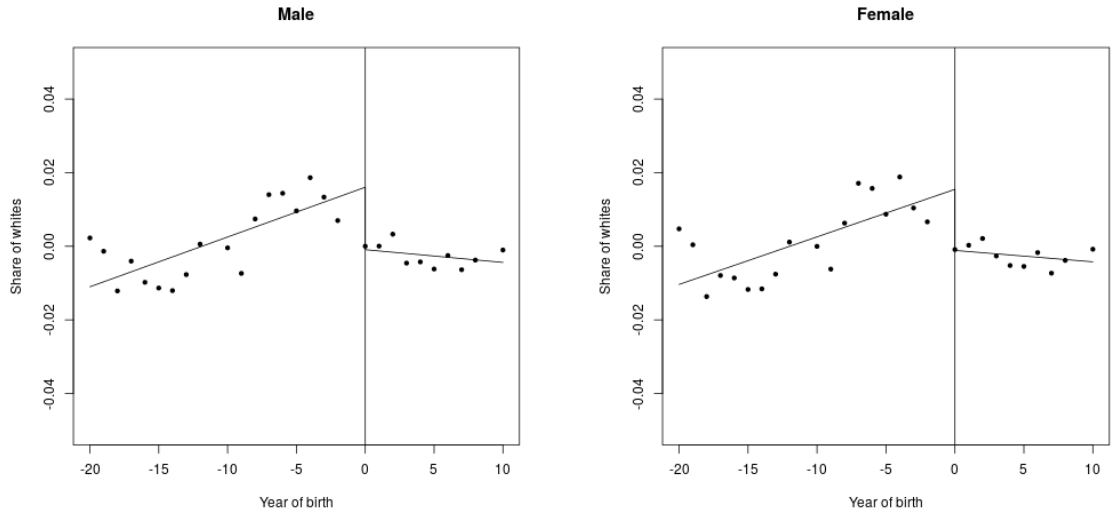
Income is monthly and has been adjusted for CPI using 2010 as base year. Sample includes individuals between 20 and 65 years of age.

Figure 2: Placebo test: Jump and trend break in overall population size for cohorts born after 1951



Year of birth centered at year 1951. The dots are residuals from regressions of share of log size of a year of birth cohort on age and census year dummies as a function of year of birth. The line is the fitted value of a regression of these residuals on year of birth and our two instruments: a dummy for cohorts born after 1951, and the interaction of this with year of birth. The right panel adds to the regressions a war birth-year dummy: a dummy with value one for cohorts born in 1942 or 1943.

Figure 3: First stage: Jump and trend break in share of Whites for cohorts born after 1951



Year of birth centered at year 1951. The dots are residuals from regressions of share of Whites on census year, age dummies, and the war birth-years dummy, as a function of year of birth. The line is the fitted value of a regression of these residuals on year of birth and our two instruments: a dummy for cohorts born after 1951, and the interaction of this with year of birth.

Figure 4: Age profile of employment, by census year and gender

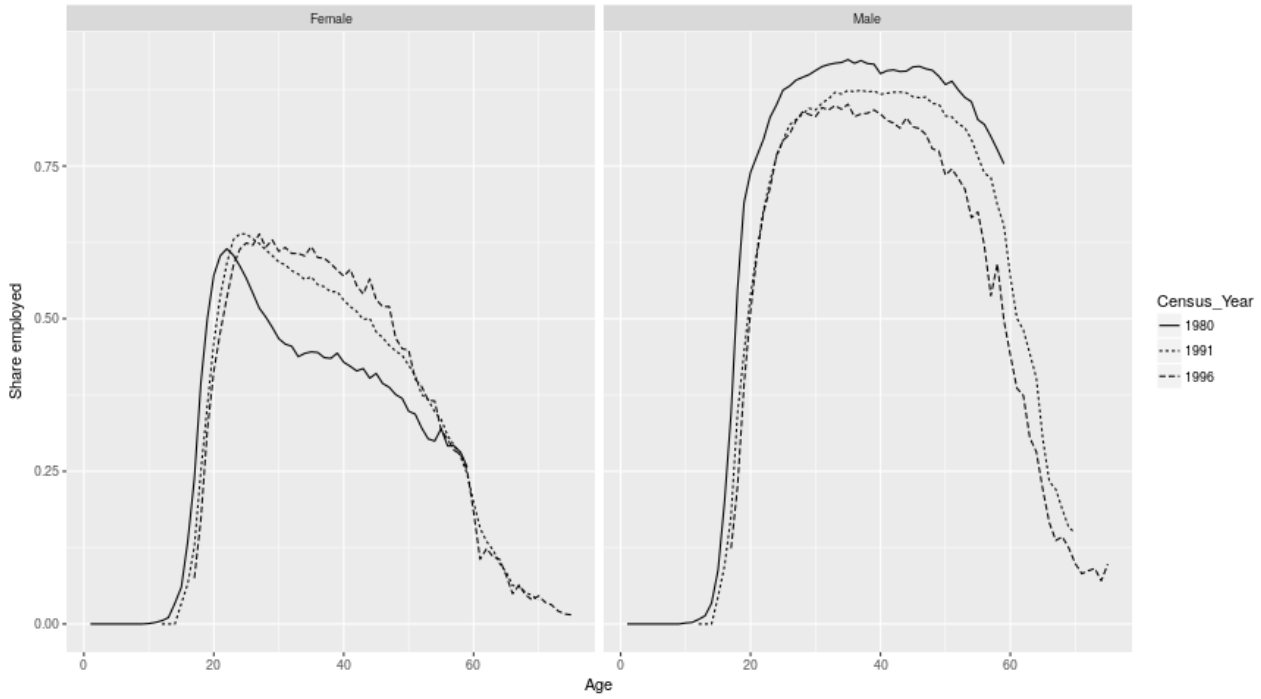
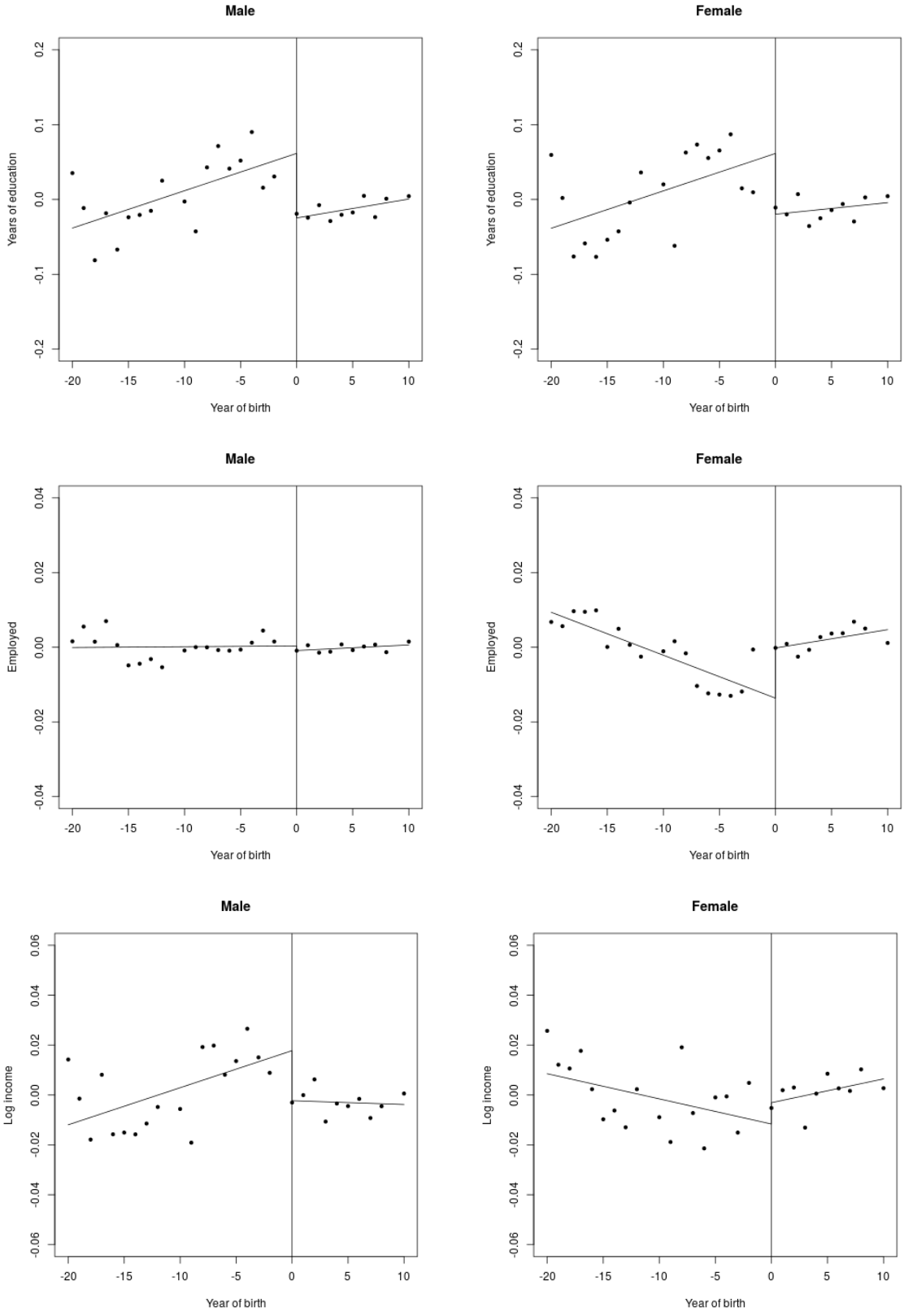


Figure 5: Reduced form results: Jump and trend break in education, employment, and log income for cohorts born after 1951



Year of birth centered at year 1951. The dots are residuals from regressions of share of the corresponding outcome variable on census year, age dummies, and the war birth-years dummy as a function of year of birth. The line is the fitted value of a regression of these residuals on year of birth and our two instruments: a dummy for cohorts born after 1951, and the interaction of this with year of birth.

Online Appendix

Table A1: Age and year of birth heaping

	Log cohort size
Last digit age	
0	0.095**
	(0.04)
1	-0.069**
	(0.033)
2	0.027
	(0.031)
3	0.006
	(0.03)
4	0.026
	(0.032)
5	0.047
	(0.034)
6	0.014
	(0.029)
8	0.061**
	(0.027)
9	0.038
	(0.031)
Last digit birth-year	
0	0.075**
	(0.036)
1	0.021
	(0.031)
2	-0.002
	(0.023)
3	-0.006
	(0.029)
4	-0.006
	(0.038)
5	0.021
	(0.035)
6	-0.022
	(0.031)
8	0.024
	(0.027)
9	0.011
	(0.034)
N	354

Robust standard errors in parenthesis. Signif. codes: 0.01 *** 0.05 ** 0.1 *. Coefficients of an OLS regression of the (log) size of a year of birth cohort on census year, last digit of age, and last digit of year of birth. Sample includes individuals between 20 and 65 years of age. Observations are aggregated at the census year x birth-year level.

Figure A1: Birth-year profile of employment, by census year and gender

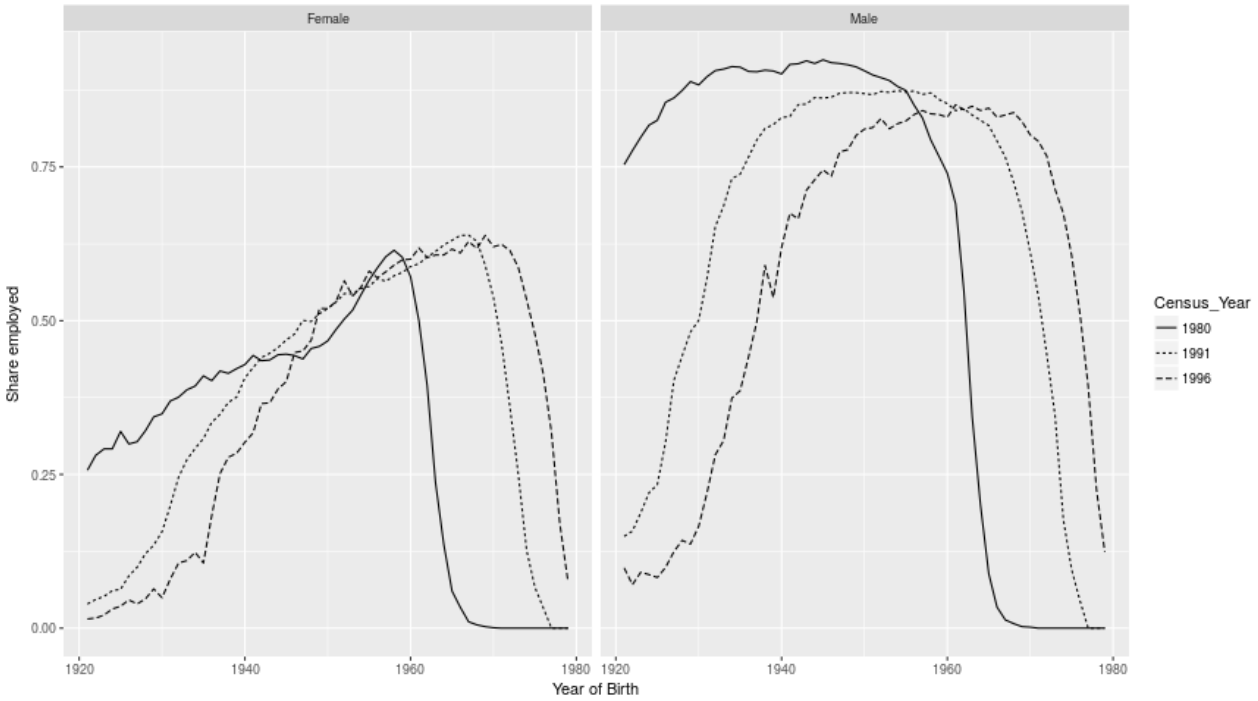


Table A2: Robustness checks for male education results: First stage, reduced form, and IV estimates for different specifications

	1	2	3	4	5	6	7	8
White(First Stage)								
Born after 1951	-0.031***	-0.035	-0.028**	-0.02	-0.018*	-0.026***	-0.026***	-0.04***
	(0.009)	(0.027)	(0.012)	(0.021)	(0.01)	(0.009)	(0.005)	(0.011)
Year of Birth x Born after 1951	-0.003**	-0.008*	-0.004*	-0.001	-0.004***	-0.004***	-0.004***	-0.007**
	(0.002)	(0.004)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.003)
Fstat	24	4.57	2.85	0.52	25.67	18.78	66.94	7.93
Education (Reduced Form)								
Born after 1951	-0.29***	-0.47*	-0.322***	-0.401*	-0.181**	-0.324***	-0.324***	-0.313***
	(0.084)	(0.208)	(0.097)	(0.208)	(0.087)	(0.08)	(0.039)	(0.084)
Year of Birth x Born after 1951	0.017	-0.007	-0.002	0.009	0.012	0.019	0.019***	0.008
	(0.013)	(0.027)	(0.013)	(0.035)	(0.013)	(0.012)	(0.005)	(0.026)
Fstat	9.32	3.08	14.38	2.07	2.38	10.28	26.78	6.92
Education(IV)								
White	3.676***	5.152**	7.644	21.444	1.158	3.672**	3.638***	5.897***
	(0.886)	(2.299)	(5.184)	(16.355)	(1.642)	(1.42)	(0.807)	(1.812)
Subset	Benchmark	Short Window	IK window	CCT window	With zero-ending birth-years	Weights 1996x10	Individual data	Quadratic
Cohorts	1931-61	1941-56	1941-62	1944-58	1931-61	1931-61	1931-61	1931-61
Bandwidth left	20	10	11	7	20	20	20	20
Bandwidth right	10	5	11	7	10	10	10	10
N	84	45	60	42	93	84	911795	84

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Top Panel: Coefficients from OLS regressions of share of Whites on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Middle Panel: Coefficients from OLS regressions of male education on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Fstat is the value of the F-statistic testing whether both coefficients are equal to zero. Bottom Panel: Coefficients from 2SLS regressions of male education on being classified as white, controling for year of birth, age and census year dummies. The instruments for being classified as white are a dummy for cohorts born after 1951, and the interaction of this with year of birth. Unless otherwise stated, birth-years ending in zero are removed to partially deal with heaping. Unless otherwise stated, data is aggregated by year or birth, census year and gender, and regressions use cell size weights. Column 1 is the benchmark specification. Column 2 uses a shorter windwow around the 1951 cohort. Columns 3 and 4 use an optimal window: the Calonico, Cattaneo, and Titiunik (CCT) procedure in column 3 and the Imbens and Kalyanaraman (IK) procedure in column 4. In both procedures male education is used as outcome variable in the procedure. Column 5 leaves the birth years ending in zero in the analysis. Columns 6 and 7 address the issue that the 1996 data includes only a 10% sample. Column 6 uses aggregate data but inflates cell-size weights in 1996 observations by a factor of 10. Column 7 uses individual data weighting 1996 individuals tenfold and clustering standard errors at the year of birth level. Column 8 uses a quadratic specification in year-of-birth. All specifications control for war birth-years with a dummy for 1942 and 1943, where cohort sizes were unusually low.

Table A3: Robustness checks for female education results: First stage, reduced form, and IV estimates for different specifications

	1	2	3	4	5	6	7	8
White(First Stage)								
Born after 1951	-0.031***	-0.023	-0.02	0.014	-0.018*	-0.021***	-0.021***	-0.038***
	(0.008)	(0.027)	(0.013)	(0.071)	(0.01)	(0.007)	(0.004)	(0.009)
Year of Birth x Born after 1951	-0.003*	-0.006	-0.003	0.01	-0.003**	-0.003**	-0.003***	-0.006
	(0.002)	(0.005)	(0.003)	(0.018)	(0.001)	(0.001)	(0.001)	(0.003)
Fstat	20.96	1.52	1.32	0.26	11.91	12.19	48.78	8.34
Education (Reduced Form)								
Born after 1951	-0.241***	-0.162	-0.163	-0.053	-0.152*	-0.174***	-0.175***	-0.252***
	(0.057)	(0.178)	(0.102)	(0.273)	(0.09)	(0.056)	(0.033)	(0.086)
Year of Birth x Born after 1951	0.008	0.005	0.008	0.051	0.014	0.009	0.009*	0.004
	(0.01)	(0.027)	(0.014)	(0.051)	(0.015)	(0.009)	(0.005)	(0.026)
Fstat	11.4	0.44	2.19	0.78	1.47	4.9	10.15	6.2
Education(IV)								
White	3.906***	1.6	1.427	7.856	0.709	2.587*	2.169***	5.505***
	(0.837)	(3.436)	(4.567)	(5.398)	(2.302)	(1.442)	(0.834)	(1.8)
Subset	Benchmark	Short Window	IK window	CCT window	With zero-ending birth-years	Weights 1996x10	Individual data	Quadratic
Cohorts	1931-61	1941-56	1941-61	1945-57	1931-61	1931-61	1931-61	1931-61
Bandwidth left	20	10	10	6	20	20	20	20
Bandwidth right	10	5	10	6	10	10	10	10
N	84	45	57	36	93	84	961136	84

Robust standard errors in parenthesis. Signif. codes: 0.1 *** 0.01 ** 0.05 *. Top Panel: Coefficients from OLS regressions of share of Whites on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Middle Panel: Coefficients from OLS regressions of female education on a dummy for cohorts born after 1951, and the interaction of this with year of birth, controlling for year of birth, age and census year dummies. Fstat is the value of the F-statistic testing whether both coefficients are equal to zero. Bottom Panel: Coefficients from 2SLS regressions of female education on being classified as white, controling for year of birth, age and census year dummies. The instruments for being classified as white are a dummy for cohorts born after 1951, and the interaction of this with year of birth. Unless otherwise stated, birth-years ending in zero are removed to partially deal with heaping. Unless otherwise stated, data is aggregated by year or birth, census year and gender, and regressions use cell size weights. Column 1 is the benchmark specification. Column 2 uses a shorter windwow around the 1951 cohort. Columns 3 and 4 use an optimal window: the Calonico, Cattaneo, and Titiunik (CCT) procedure in column 3 and the Imbens and Kalyanaraman (IK) procedure in column 4. In both procedures female education is used as outcome variable in the procedure. Column 5 leaves the birth years ending in zero in the analysis. Columns 6 and 7 address the issue that the 1996 data includes only a 10% sample. Column 6 uses aggregate data but inflates cell-size weights in 1996 observations by a factor of 10. Column 7 uses individual data weighting 1996 individuals tenfold and clustering standard errors at the year of birth level. Column 8 uses a quadratic specification in year-of-birth. All specifications control for war birth-years with a dummy for 1942 and 1943, where cohort sizes were unusually low.

SALDRU

Southern Africa Labour and Development Research Unit

The Southern Africa Labour and Development Research Unit (SALDRU) conducts research directed at improving the well-being of South Africa's poor. It was established in 1975. Over the next two decades the unit's research played a central role in documenting the human costs of apartheid. Key projects from this period included the Farm Labour Conference (1976), the Economics of Health Care Conference (1978), and the Second Carnegie Enquiry into Poverty and Development in South Africa (1983-86). At the urging of the African National Congress, from 1992-1994 SALDRU and the World Bank coordinated the Project for Statistics on Living Standards and Development (PSLSD). This project provide baseline data for the implementation of post-apartheid socio-economic policies through South Africa's first non-racial national sample survey.

In the post-apartheid period, SALDRU has continued to gather data and conduct research directed at informing and assessing anti-poverty policy. In line with its historical contribution, SALDRU's researchers continue to conduct research detailing changing patterns of well-being in South Africa and assessing the impact of government policy on the poor. Current research work falls into the following research themes: post-apartheid poverty; employment and migration dynamics; family support structures in an era of rapid social change; public works and public infrastructure programmes, financial strategies of the poor; common property resources and the poor. Key survey projects include the Langeberg Integrated Family Survey (1999), the Khayelitsha/Mitchell's Plain Survey (2000), the ongoing Cape Area Panel Study (2001-) and the Financial Diaries Project.

www.saldru.uct.ac.za
Level 3, School of Economics Building, Middle Campus, University of Cape Town
Private Bag, Rondebosch 7701, Cape Town, South Africa
Tel: +27 (0)21 650 5696
Fax: +27 (0) 21 650 5797
Web: www.saldru.uct.ac.za

University of Cape Town • iYunivesithi yaseKapa • Universiteit van Kaapstad

SPES BONA