Does Education Increase Interethnic and Interreligious Tolerance? Evidence from a Natural Experiment

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21 May 2015

Toward Pro-Poor Policy through Research

WORKING PAPER

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ACKNOWLEDGEMENTS

ABSTRACT

Does Education Increase Interethnic and Interreligious Tolerance? Evidence from a Natural Experiment

Christopher Roth[i] and Sudarno Sumarto[ii]

Can the government increase tolerance among its citizens by providing them with more schooling? We exploit a large school building program from Indonesia to examine the causal relationship between education and attitudes towards people from a different ethnicity and religion. Our generalized difference-in-differences estimates suggest that receiving more education causes an increase in interethnic and interreligious tolerance. Specifically, a one standard deviation increase in education results in a .4 standard deviation increase in tolerance. We demonstrate robustness of results and show that the common trend assumption is satisfied. Subsequently, we shed light on several mechanisms: first, treated individuals are more likely to migrate, to live in cities and to work in occupations outside of agriculture. This in turn, increases their incomes and the religious and ethnic diversity of their social environments. Second, we employ an additional identification strategy to show that the educational content under Suharto emphasizing the national unity of Indonesia is an important mechanism underlying the estimated treatment effects.

Key words: Racism, Tolerance, Education, Natural Experiment

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I. INTRODUCTION

Religious and ethnic conflict have tremendous economic and non-economic costs and are still widespread today [26]. This is particularly true in the developing world [23], where conflict is a major obstacle for development [51, 22, 15, 27]. Little is understood about policies that governments can use to avoid religious and ethnic conflict and how to lower prejudice among citizens. Attitudes and tolerance are shaped by a variety of factors, such as socialization early in life, social divisions and inequalities. Both a theoretical and empirical literature identify education as a key determinant of attitudes, tolerance and identity [3, 42, 34].

However, no evidence on the causal relationship between schooling and tolerance towards people from a different ethnicity and religion exists. Moreover, little is known on how the effort of the state to positively affect cohesion through moral education at schools affects attitudes. While a variety of studies have documented positive correlations between tolerance and schooling [42], there are potentially confounding factors underlying these correlations. Therefore, evidence on the causal effect of education on tolerance and racism is needed.

In this paper, we study how an exogenous increase in education and the moral education under Suharto affect attitudes towards people from a different religion and ethnicity. We use several data sources: first, we employ a large Indonesian cross-sectional dataset from 2012 with attitudinal questions. Specifically, it contains questions on attitudes towards individuals from a different ethnicity and religion. Second, we link this with data on the INPRES[1] school building program [25] which is one of the largest school building programs in the world. In particular, over the period from 1973 to 1979 more than 60,000 primary schools were built in Indonesia. This was a substantial shock to the availability of primary schools in Indonesia. The program resulted in an increase in enrollment rates from 69 percent in 1973 to 83 percent in 1978.

As in [25], we apply a generalized difference-in-differences methodology to evaluate the effect of the INPRES program on schooling as well as attitudes. Specifically, we control for district of birth and cohort fixed effects. We define individuals as treated if they are born after 1965 to make sure that they necessarily were affected by the school building program. We interact this treatment indicator with the intensity of the INPRES program[2] in a given district of birth. In other words, we combine differences in cohorts in exposure to the program with differences in the spatial intensity of the program. We find that individuals whose educational level was exogenously increased, display higher levels of interethnic and interreligious tolerance. Specifically, a one standard deviation increase in education results in a .4 standard deviation increase in tolerance.

Identifying the causal impact of education on attitudes is challenging due to potential confounding factors such as selection biases. For example, more tolerant individuals may choose to receive more education [37]. To circumvent these methodological problems, we revert to a generalized difference-in-differences methodology by exploiting the INPRES school building program. Crucially, the data allow us to test one of the key identifying assumptions. Specifically, there may be differential district-specific trends in educational achievement and attitudes.

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[1]INPRES means "Presidential instructions".

[2]Given by the number of schools built.

To test for such differential trends, we conduct two placebo exercises for cohorts that were not differentially affected by INPRES. First, we use individuals born between 1950 and 1960. We define individuals born between 1956 and 1960 as "pseudo-treated". Second, we compare cohorts born between 1970 and 1985 and treat those born after 1977 as "pseudo-treated". We estimate a generalized difference-in-differences strategy interacting a "pseudo-treatment" indicator with an indicator for high treatment intensity with INPRES. We find no significant pseudo-treatment effects. This evidence is comforting as it shows that the results are not driven by unwarranted identifying assumptions.

Then, we contemplate a variety of mechanisms that could be underlying the results: First, we employ a strategy to provide evidence on the relevance of moral education on the state ideology at schools. The main target of this state ideology taught at school was to increase cohesion among Indonesian citizens. Specifically, we use the fact that intensity of exposure to the state ideology at school drastically decreased with the fall of Suharto. Therefore, individuals born after 1988 should not be affected as much by moral education on the state ideology. We interact the years of schooling with an indicator for whether an individual could be affected by moral education conditional on both cohort and district of birth fixed effects. We find that exposure to moral education under Suharto results in higher levels of tolerance. Moreover, we conduct a variety of falsification exercises to rule out time-varying unobservables as a driver of the results on moral education.

In addition, we provide evidence on the relevance of labor market mechanisms: We show that INPRES caused increases in migration to cities, occupational choice into trade and industry and increases in income. This migration into cities in turn implies higher exposure to individuals from a different ethnicity as cities are much more ethnically and religiously heterogeneous and much less ethnically and religiously segregated than villages. Also, the increases in income may also result in more pro-social attitudes towards individuals from a different ethnicity [36, 24].

Finally, we rule out a variety of alternative explanations: We show that there is no evidence in favor of the contact hypotheses, i.e. we find no heterogeneous treatment effects by ethnic segregation [6] at the district of birth. Moreover, we find no evidence that those treated display higher levels of well-being or are more sociable.

This paper makes several contributions: First, it provides novel causal evidence on how receiving more education affects attitudes towards individuals from a different ethnicity and religion later in life. It is thus the first to shed light on the effects of education early in life and ethnic and religious harmony. Our paper is the first to provide evidence that these effects are long-lasting, i.e. individuals aged between 40 and 50 in 2012 have significantly different levels of tolerance depending on their exogenously changed school achievement. Second, we shed light on the mechanisms underlying the effects: we show educational content *per se* seems to matter for the treatment effects. Moreover, we provide evidence on the relevance of a labor market mechanism: migration from rural areas into cities as well as different occupational choices and income increases are likely mechanisms driving the treatment effects.

We add to several strands in the literature: first, we contribute to the literature on the question whether policymakers can affect children's attitudes through education [17]. Our paper is most closely related to [21], who study the effect of school currriculum on political attitudes. In particular, they exploit the roll-out of a textbook reform in China to identify the

effect of changes in educational content on political attitudes, such as attitudes towards democracy and markets. They find strong evidence that indoctrination worked. In a related manner, [56] show that indoctrination under the Nazi regime resulted in higher levels of anti-semitism among individuals more heavily exposed to the Nazi ideology.

Moreover, our analysis adds to the literature that identifies the effect of education on social preferences [39, 30], political and social attitudes [31], and the effects of education and schooling on religiosity [38, 35], secularization [13] and female empowerment [35]. These latter papers document that increases in education cause decreases in religiosity and increases in female empowerment. This paper is also related to recent work by [50] who shows that mixing wealthy and poor students in schools affects social preferences. Further, this paper contributes to the literature on integration policies and inter-group attitudes [18, 32]. Several papers establish that interactions with randomly assigned peers from a different race lower prejudices [18, 16]

Finally, our analysis contributes to the literature on the determinants of racism, tolerance [47, 42] and prejudice [7]. There is a large literature based on observational data investigating the determinants of racism, right-voting behavior and mistrust. One exception is [24] who provides causal evidence on right-voting behavior in Germany and economic shocks by changes in import competition. In addition, we add to the literature on human capital externalities [43]: Growing evidence suggests positive externalities of education on citizenship [41] as well as lowered delinquencies [40].

Indonesia is a very well-suited country to study the effects of education on religious and ethnic harmony. First, the country was plagued by major civil wars in Aceh and East Timor in the past decades. Second, local conflict that is ethnically and religiously motivated is still quite prevalent in Indonesia today [12]. Third, the Indonesian government under Suharto made substantial efforts to inculcate its citizens with an ideology emphasizing the unity of Indonesia. Fourth, Indonesia is one of the most ethnically diverse countries in the world with more than 300 ethnic groups as documented in the 2010 census.

This paper proceeds as follows: In Section 2, we describe the data, the context and the identification strategy. Section 3 presents the results. In Section 4, we rule out alternative explanations of the results through robustness as well as placebo tests. In Section 5, we discuss and test for several mechanisms that could drive the results. Finally, Section 6 concludes.

II. DATA, CONTEXT AND IDENTIFICATION

In this section we describe the attitudinal data as well as the context. Specifically, we describe the INPRES program, the conceptual framework and our hypotheses. Then, we outline the identification strategy to estimate the causal effect of education on interethnic and interreligious attitudes.

2.1 Data

We use data from the socio-cultural module from the social economic survey (SUSENAS) from 2012. It contains information on about 70,000 households in total. The dataset is representative at the province level. This is the first and only dataset from the social economic survey with attitudinal questions that contains information on the district of birth of respondents as well as the age of respondents - the two key critical ingredients for our identification strategy. The dataset not only contains attitudinal questions, but also information on demographics, and education. The two key outcome variables of interest for this paper are:

What is your opinion on an activity done in your neighborhood by a group of people which are from a different:
   (a) ethnicity and
   (b) religion.

People can respond on a four point scale ranging from (4) "very happy" to (1) "quite unhappy".[3] Moreover, we create an index of tolerance based on the two questions through principal component analysis. It is crucial to note that this variables could capture two factors: it could be that individuals are more tolerant, but it could also mean that individuals identify less with their own religion and with their own ethnicity.

In our main specifications we use data from 31,616 individuals born between 1951 and 1972. Using data on the district of birth of each individual, we match[4] the data on education and attitudes with administrative data on the number of schools planned to be built under the INPRES program.[5] Moreover, we use data from the 2000 population census to create measures of ethnic segregation. Our segregation measure takes values between 0 and .9, where higher values indicate that the area is highly segregated i.e. there are many different groups in the district , but each group lives in its own sub-district. 0 indicates that the composition of each subdistrict mirrors the composition of the district as a whole.[6] We create the segregation measure by comparing subdistrict-level segregation with district level segregation. We match this data with individuals' district of birth to proxy the likelihood of interacting with non-co-ethnics at school.

To shed light on potential mechanisms and confounds underlying the results we also use data on social club memberships, i.e. how active individuals are in religious and social organizations.[7] Moreover, we use data on whether households migrated over the course of their lifetime, defined as households reporting not to live in their district of birth. We also

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[3]The four answer categories are given as follows: (4) very happy, (3) happy, (2), not happy and (1) quite unhappy.

[4]Due to the splittings of districts in Indonesia over time, we use a district-level crosswalk linking district border in the 1970s with the district borders in 2012.

[5]According to the central planning agency (BAPPENAS) the actual number of school built coincides with the scheduled number.

[6]If there is only one group in a given district then I re-code the value to one. A completely homogeneous district is conceptually very similar to a completely segregated one to test for the contact hypothesis as we are interested in an individual's likelihood of interacting with someone from a different ethnic group.

[7]The question is given as follows: Do you usually participate in social activities in your neighborhood in (a) religious organizations, (b) social organizations. Answers are given on a four-point scale ranging from (4) always to (1) never.

make use of data on occupational choice, such as working in agriculture vs. working in trade and industry and whether an individual lives in rural or urban areas. Further, we use data on general levels of pro-sociality through the willingness to help neighbors measured on a four point Likert scale ranging from (4) very willing to (1) not willing.[8] Finally, we also use data on a measure of happiness on a four-point Likert scale ranging from (4) very happy to (1) not very happy.

In what follows, we describe the characteristics of the sample used in our main specifications. The average number of schools built in a given district as a part of the INPRES program is 265. The mean district population size in 1971 is 518,000. Eight percent of our sample have no schooling, about 47 percent are male and the average age of individuals is about 49. On average the individuals in our sample exhibit moderately positive attitudes toward individuals from a different ethnicity and religion. Further summary statistics of key variables can be found in table 1.

Table 1. Summary statistics for the main sample of the main specification

Variable	Mean	Std. ev.	Min.	Max.	N
Tolerance: Ethnicity	2.846	0.519	1	4	28709
Tolerance: Religion	2.722	0.622	1	4	28147
Index of Tolerance	0	1.27	-4.496	3.047	27603
Age	48.775	6.089	40	61	32035
Number of Schools Built in District (73-79)	265.04	188.433	16	824	31750
Proportion of villages with "Karang Taruna" in 1983	0.468	0.301	0.01	1	30628
Proportion of villages with "Karang Taruna" in 1990	0.769	0.257	0.13	1	30628
No Schooling	0.078	0.268	0	1	32035
Log Expenditure	14.538	0.728	11.897	18.779	32035
Male	0.47	0.499	0	1	32035
Enrollment in 1971	0.182	0.112	0.031	0.993	31491
Population Size in 1971	518169	406319	12427	1974614	31616
Ethnic Fractionalization	0.313	0.315	0	0.977	31067
Religious Organizations	2.814	0.739	1	4	31511
Social Organizations	2.433	0.879	1	4	29646
Willingness to help	3.004	0.511	1	4	32035
Subjective Well-Being (SWB)	2.97	0.511	1	4	32035

2.2 The INPRES School Building Program

Following an oil-boom in the 1970s the Indonesian government started to increase expenditures on development programs. In particular, the government implemented a variety of "development programs" to increase equality across different provinces. The

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[8]The exact question is as follows: Are you ready to help others who are powerless (need help) in your neighborhood?

INPRES school building program, was one of the first and – at the time – the largest government program [25]. Between 1973 and 1978 more than 60,000 new primary schools were built.[9] It constituted a substantial shock to the availability of primary schools in Indonesia. The program resulted in an increase in enrollment rates from 69 percent in 1973 to 83 percent in 1978.

The aim of the program was to increase enrollment for children not previously enrolled in school at all. Therefore, the allocation rule of the program prescribed that the number of new schools to be constructed should be proportional to the number of children not enrolled in school in 1972. While the initial allocation plan from the government was more or less followed, [25] shows that the program was less re-distributive as originally intended.

2.3 Conceptual Framework and Hypotheses

In what follows, we highlight several general mechanisms through which education may increase tolerance and lower prejudice.

2.3.1 Contact hypothesis and labor market mechanisms

The relationship between education and tolerance, hatred and xenophobia has received some scholarly attention. Most prominently, [34] derives a political economy model of hatred. His model predicts that education lowers hatred through two distinct channels: first, more educated individuals are more likely to differentiate between lies made by politicians about minorities and actual facts. One potential mechanism is thus that becoming more educated actually makes you more tolerant. Second, he argues that social interactions with minorities may increase the incentives to acquire more information about the minorities.

The latter explanation based on the potential benefits of social interactions with members from minorities has received a great deal of attention in the social sciences [54, 48, 50]. According to the contact hypothesis [9, 48] interactions with individuals from an outgroup lower prejudice under a given set of conditions.

Moreover, interactions with members from an outgroup may lower prejudice through an indirect channel based on cognitive dissonance [29]. Cognitive dissonance theory claims that individuals are likely to change their attitudes on a topic if their actions contradict their attitudes. For example, if individuals interact with people from a different ethnicity in a peaceful manner, then they may change their attitudes and increase their tolerance. On the other hand, it is also possible to imagine mechanisms by which schooling would increase prejudice: if individuals go to school in an all one-ethnicity school, group identification might increase.

More generally, it may be that social interactions at school affect an individual's social skills and their social capital, which in turn might affect their levels of socialization. Similarly, explanations based on identity formation [2, 4] may be a useful guide to understand the relationship between education and attitudes. For example, it may be that more educated individuals identify less with their own religion and ethnicity, but more with individuals with

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[9]According to the World Bank, INPRES was the fastest school construction program ever undertaken in the world [25]. For more information see [25].

the same education or from the same social class. This could decrease the general salience of ethnic and religious markers for defining out-group members [55]. If there is a general tendency of ingroup-outgroup biases, this could lower prejudice and increase tolerance.

Moreover, there may be a labor market mechanism through which higher education may affect tolerance: more educated individuals are more likely to work in a city and thus in a more ethnically and religiously diverse environment. Higher levels of education are also likely to affect occupational choice, which in turn not only affects incomes but also the social environment.

Finally, more educated individuals are wealthier and have higher incomes. This could directly affect their levels of tolerance. In addition, higher income may give individuals access to different market opportunities, goods and services (for example travel) that may affect their interethnic and interreligious tolerance.

2.3.2 The Indonesian State Ideology

In what follows, I describe one further mechanism through which schooling may affect interethnic and interreligous attitudes: the Indonesian state ideology which was taught in primary schools. Pancasila was the official ideology of Indonesia under the Suharto regime. It started to gain dramatic importance beginning in 1975, culminating in 1985 [45].[10] This ideology was nationalistic emphasizing the unity of Indonesia.[11] This ideology tried to bridge interethnic and interreligious cleavages. The first principle of Pancasila encouraged every citizen to respect each otherâ€™s faiths for the sake of the harmony and peace of mankind. In other words, this principle advocates religious tolerance and freedom of all to adhere to the religion or faith of his or her choice [44]. Moreover, Pancasila emphasized the unity of Indonesia and was a substantial attempt of social engineering: the program was supposed to increase social cohesion among Indonesian citizens [44, 45].

Between 1978 and 1998, Indonesians citizens spent many hours studying the principles of Pancasila. As Pancasila was part of the official school curriculum, students were not permitted to progress to the next grade, unless they had mastered its principles. In particular, beginning in 1975[12] moral education on Pancasila became part of the school curriculum [45].

2.3.3 Hypotheses

Almost all of the listed theoretical channels through which education could affect tolerance suggest a positive relationship. Education could increase income as well as the probability of migrating. This in turn would affect the diversity of the social environment that an individual is exposed to. Moreover, there is a channel based on the educational content that is particular to primary schooling in Indonsia in the 1980s: moral education on the state ideology. As individuals attending more schooling will be exposed to moral education, we hypothesize that they will embrace some of the principles of this ideology. Therefore, we predict that exposure to the Indonesian state ideology will result in increases of interreligious and interethnic tolerance.

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[10][45] explains that Pancasila not influential in the first few years of the Suharto regime until 1975.

[11]It encompasses five components: (i) A belief in the one and only God; (ii) Just and civilized humanity; (iii) The unity of Indonesia; (iv) democracy as well as (v) social justice [44]

[12]The intensity of indoctrination and the efforts of the government to inculcate citizens with the Pancasila ideology substantially increased in 1978.

2.4 Identification Strategy

As in [25], our identification strategy exploits both spatial variation in treatment intensity of INPRES and temporal variation in terms of the exposure to INPRES. Crucially, different regions and different cohorts benefited differentially from the program. Children usually attend primary school when they are aged between 7 and 12. This means that children born in or before 1960[13] could not benefit from the INPRES program.[14] The intensity of the treatment is increasing in the year of birth until 1971, when the treatment intensity reaches its peak and remains constant.

In this paper, we use data on cohorts born between 1951 and 1972[15], i.e. about 11 years before and after the first cohort that could have potentially benefited from the INPRES program. In our main specification, we use a dummy variable $Treat_{idt}$ taking value one if individuals were fully treated by INPRES, i.e. individuals were born between 1966 and 1972; and another dummy variable $Parttreat_{idt}$, taking value one if individuals were partially treated by INPRES, i.e. individuals born between 1961 and 1965. We use individuals born between 1951 and 1960 as controls.

Our second source of variation comes from regional intensity of the school building program. There were substantial differences in exposure of different regions with the program. Importantly, [25] shows that region of birth is highly correlated with region of education. This is important since region of education may be endogenous to the INPRES program [52, 53], while region of birth is not endogenous as all individuals from our sample were born before the first INPRES schools opened. We use the treatment intensity in two different ways: first, we differentiate between high intensity and low intensity treatments by bifurcating the data by the median treatment intensity. In other words, we create a dummy variable, H_d, taking value one if an individual was born in a district with high intensity of the INPRES program. In our main specification,[16] we interact the indicator for high treatment intensity with a dummy variable indicating that an individual could benefit from the INPRES program. Second, we also use the intensity of INPRES.

In the simple specification, we include five variables: an indicator for whether an individual was born in a high intensity INPRES district, H_d; an indicator for "treatment" and "partial treatment"; and interaction terms of partial treatment with the "high intensity district indicator". The outcome variables of interest y_{idt} denotes the schooling and tolerance of individual i in district of birth d part of cohort t.

$$y_{idt} = \alpha_0 + \alpha_1 Treat_{idt} + \alpha_2 Parttreat_{idt} + \alpha_3 H_d + \alpha_4 (Treat_{idt} \times H_d) + \alpha_5 (Parttreat_{idt} \times H_d) + \varepsilon_{idt}$$

(1)

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[13]In the robustness section, we demonstrate that changing cutoff to 1961 does not affect our results.

[14]Duflo uses the Indonesian Family Life Survey (IFLS) from 1993 to show that only 3.5 percent of children older than 12 are still in primary school.

[15]In our main specification, we do not use data from 1973 or after as there are concerns that people might have migrated to districts with higher treatment of INPRES schools. In the robustness section we show that our results hold if we use observations from 73, 74 and 75 in our treatment group.

[16]We choose this as our main specification as it provides a more intuitive interpretation of the coefficients of interest than when we use the treatment intensity. All of our results are robust to using the treatment intensity rather than the dummy variable.

In our generalized difference-in-differences strategy we control for cohort fixed effects, β_t, and district of birth fixed effects, α_d. This is pivotal given the possibility of age effects in tolerance and preferences [28, 8] as well as regional cultural differences in prosociality and conflict in Indonesia [12]. Moreover, we control for population size of the district of birth in 1971, X_d, interacted with cohort fixed effects. In other words, we control for differential trends by population size as it may be that districts with different initial population sizes may be on different cohort-trends in terms of enrollment and tolerance.

$$y_{idt} = \alpha_d + \beta_t + \gamma_1(Treat_{idt} \times H_d) + \gamma_2(Parttreat_{idt} \times H_d) + \sum_{t=2}^{22}(X_d \times \beta_t)\delta_{1t} + \varepsilon_{idt} \quad (2)$$

We prefer this specification with a binary indicator as it provides us with higher power and as it facilitates the interpretation of our coefficient estimates. In an alternative specification, we use the treatment intensity of INPRES in the district of birth, Int_d, rather than an indicator variable for high intensity. We interact the treatment intensity with an indicator for cohorts that could benefit from the INPRES program, $Treat_{idt}$ and that could partially benefit from the program, $Parttreat_{idt}$. The most parsimonious specification using the treatment intensity is given as follows:

$$y_{idt} = \alpha_0 + \alpha_1 Treat_{idt} + \alpha_2 Parttreat_{idt} + \alpha_3 Int_d + \alpha_4(Treat_{idt} \times Int_d) + \alpha_5(Parttreat_{idt} \times Int_d) + \varepsilon_{idt} \quad (3)$$

Moreover, we employ a generalized difference-in-differences strategy using the same control variables as in equation (2), i.e. controlling for cohort and district of birth fixed effects:

$$y_{idt} = \alpha_d + \beta_t + \gamma_1(Treat_{idt} \times Int_d) + \gamma_2(Parttreat_{idt} \times Int_d) + \sum_{t=2}^{22}(X_d \times \beta_t)\delta_{1t} + \varepsilon_{idt} \quad (4)$$

In both of our specifications we cluster the standard errors by the district of birth to allow for arbitrary correlations in error terms at the level of our main variable of interest, intensity of INPRES by district of birth [11]. In the robustness section, we show that clustering standard errors at different levels and allowing for two-way clustering does not affect our results.

We will test for five outcome variables: level of schooling[17], an indicator for "no schooling", tolerance towards people from a different ethnicity; tolerance towards people from a different religion as well as an index of tolerance. As we will conduct multiple hypothesis tests for these different variables that are all conceptually related, we conduct a correction for multiple hypothesis testing in our main specification that is evidenced in Table 2. We follow the "sharpened q value" approach [14]. The q value controls for the False Discovery Rate, i.e. "the expected proportion of rejections that are type I errors" [10].

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[17]Measured on an 8-point scale The eight categories are given by: (1) No schooling to (8) graduated from high school.

III. RESULTS

In this section, we first present the reduced form results from our main identification strategy. Then, we employ an instrumental variables strategy to provide estimates on the causal effect of education on tolerance.

3.1 Reduced Form

Our identification strategy shows that cohorts old enough to benefit from INPRES from high-intensity areas increased their schooling significantly. Moreover, our estimates show that treated cohorts from high-intensity regions have more positive attitudes towards individuals from a different religion and ethnicity. The results from our simple difference-in-differences specification in Panel A and the more restrictive generalized difference-in-differences from Panel B are quite similar.

This result holds similarly for our main alternative specification making use of treatment intensity of the INPRES school building program. As evidenced in Panels C and D, we find that individuals that could potentially benefit from the program in high treatment intensity districts display more positive attitudes. We find that the "treated" individuals display approximately one tenth of a standard deviation more positive attitudes towards people from a different ethnicity and religion. This suggests a large effect of receiving additional schooling on tolerance. Importantly, our results are robust to accounting for multiple hypothesis testing. Specifically, even after taking account of multiple hypothesis testing our estimates are still significant at the five percent level in Panels A and B; and significant at the ten percent level in Panels C and D.

Table 2. Main Results: Reduced Form

	Schooling	No Schooling	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A:					
Treatment ×	0.715***	-0.055***	0.133***	0.056***	0.049***
High Intensity	(0.124)	(0.015)	(0.039)	(0.015)	(0.019)
	[.001]	[.001]	[.001]	[.001]	[.002]
N	31854	31854	27429	28528	27973
R^2	0.034	0.011	0.003	0.003	0.001
Panel B:					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.136)	(0.015)	(0.044)	(0.018)	(0.020)
	[.001]	[.003]	[.023]	[.019]	[.038]
N	31616	31616	27203	28301	27741
R^2	0.014	0.008	0.001	0.001	0.001
Panel C:					
Treatment ×	0.0017***	-0.0001***	0.0004***	0.0001***	0.0001**
Intensity	(0.0004)	(0.0000)	(0.0001)	(0.0000)	(0.0001)
	[.001]	[.003]	[.003]	[.003]	[.004]
N	31750	31750	27335	28434	27879
R^2	0.034	0.010	0.002	0.002	0.002
Panel D:					
Treatment ×	0.0023***	-0.0002***	0.0004*	0.0001*	0.0002*
Intensity	(0.0005)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
	[.001]	[.001]	[.089]	[.095]	[.095]
N	31533	31533	27125	28223	27663
R^2	0.014	0.008	0.001	0.001	0.001

Standard errors clustered at the district of dirth in (). Sharpened q values taking account of multiple hypothesis testing through the family-wise error rate adjustment are in []. In Panels A and B we interact treatment defined as as an individual born in a high treatment area between 1966 and 1972 with an indicator for high treatment intensity with INPRES. The control group are cohorts born between 1951 and 1960. In Panels C and D, we use the same timing definitions for treatment and control, but interact the treatment indicator with the treatment intensity. In Panels A and C we control for a high intensity indicator and a treatment intensity variable respectively as well as a "treatment" and a "partial treatment" indicator. In Panels B and D we control for cohort fixed effects, district of birth fixed effects as well as cohort-fixed effects interacted with population size in 1971. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table 3 results on partially treated individuals are displayed. In line with our hypotheses we find weaker effects on partially treated individuals both in terms of educational achievement as well as in terms of interethnic and interreligious tolerance. Yet, we still find some significant increases in tolerance for some variables.

Table 3. Partially Treated: Reduced Form

	Schooling	No Schooling	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A:					
Partial Treatment ×	0.457***	-0.046***	0.091**	0.036**	0.031
High Intensity	(0.105)	(0.011)	(0.042)	(0.016)	(0.021)
N	31854	31854	27429	28528	27973
R^2	0.034	0.011	0.003	0.003	0.001
Panel B:					
Partial Treatment ×	0.570***	-0.053***	0.095*	0.035	0.041*
High Intensity	(0.137)	(0.016)	(0.054)	(0.021)	(0.025)
N	31616	31616	27203	28301	27741
R^2	0.014	0.008	0.001	0.001	0.001
Panel C:					
Partial Treatment ×	0.0009***	-0.0001***	0.0002	0.0001	0.0001
Intensity	(0.0003)	(0.0000)	(0.0001)	(0.0000)	(0.0001)
N	31750	31750	27335	28434	27879
R^2	0.034	0.010	0.002	0.002	0.002
Panel D:					
Partial Treatment ×	0.0015***	-0.0002***	0.0002	0.0000	0.0002*
Intensity	(0.0006)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
N	31533	31533	27125	28223	27663
R^2	0.014	0.008	0.001	0.001	0.001

Standard errors clustered at the district of dirth in (). Partial treatment is defined as as an individual born in a high treatment area between 1961 and 1965. For Panels A and B, we interact the partial treatment with an indicator taking value one for individuals born in a district with high intensity of the INPRES program. The control group are cohorts born between 1951 and 1960. In Panels C and D, we use the same timing definitions for partial treatment and control, but interact the treatment indicator with the treatment intensity. In Panels A and C we control for a high intensity indicator and a treatment intensity variable respectively as well as a "treatment" and a "partial treatment" indicator. In Panels B and D we control for cohort fixed effects as well as district of birth fixed effects. Moreover, we control for cohort-fixed effects interacted with population size in 1971. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Moreover, we interact the cohort dummies with the treatment-intensity conditional on region of birth and cohort fixed effects. This allows us to analyze the data as an event study.

$$y_{ilt} = \theta_0 + \alpha_d + \beta_t + \sum_{t=2}^{22} (H_d \times d_{it})\pi_{1t} + \varepsilon_{lt} \tag{5}$$

We plot the coefficients, π_{1t}, both for schooling, tolerance towards people from a different religion and ethnicity as well as for the index of tolerance with and without confidence intervals in figures 1 to 8. We illustrate the results in two different ways: First, we plot the three-year moving average of treatment effects. Second, we employ local polynomial regressions with confidence intervals to illustrate the relationship between year-specific treatment effects and birth year.[18] As illustrated in figures 1 and 2, we find a substantial increase in schooling beginning in the 1960s, i.e. for cohorts that could potentially benefit

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[18]We choose a bandwidth of two years and employ an epanechnikov kernel.

from the INPRES program. We find increases in education for cohorts beginning in 1960. In line with the assumption of a common trend we find no increases in schooling in the 1950s.
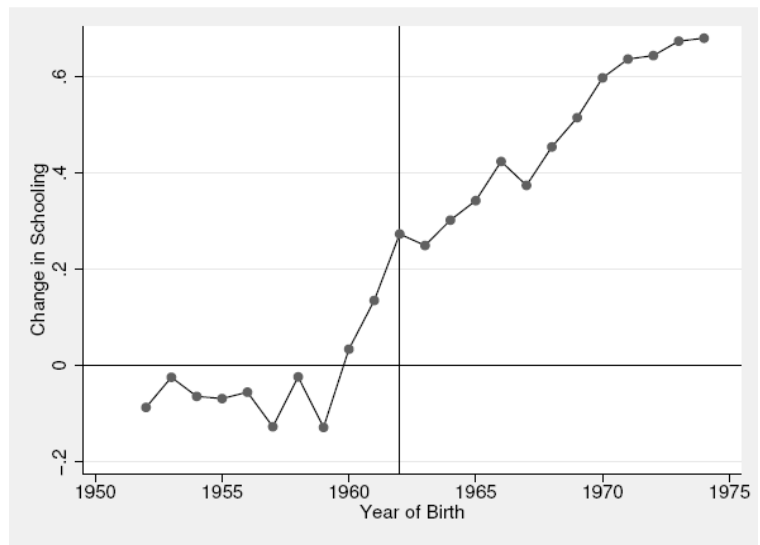
Figure 1. Education: MA(3) of the coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.
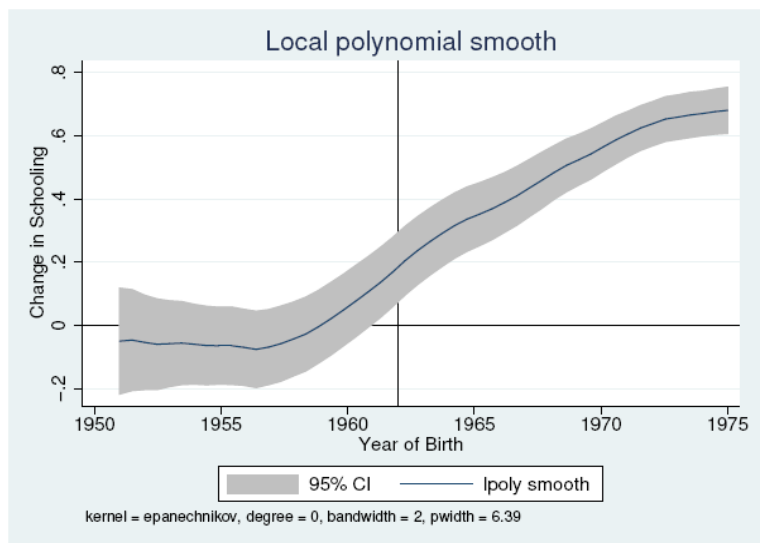
Figure 2. Education with confidence intervals: Coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.

Moreover, we find substantial increases intolerance for individuals born after 1962, i.e. individuals most likely to benefit from the INPRES program. Figures for all three main dependent variables with and without confidence intervals are displayed in figures 3 to 8. In line with the hypothesis that the treatment effects are driven by individuals exposed to the Pancasila ideology at school we find the largest increases in tolerance for individuals born after 1965.

Figure 3. Tolerance Index: MA(3) of the coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.
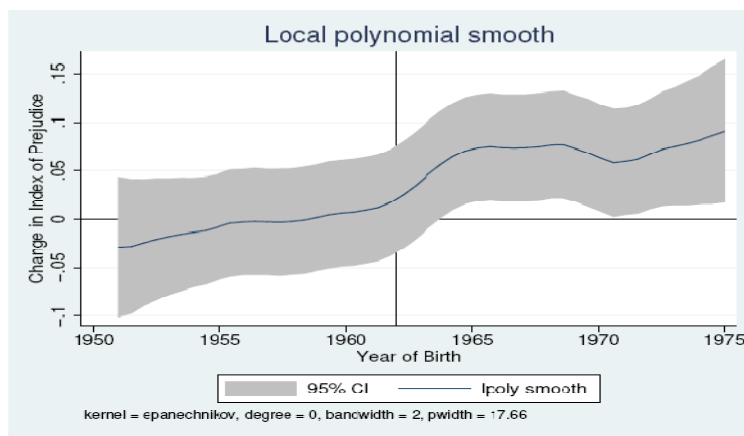
Figure 4. Tolerance Index with confidence intervals: Coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.
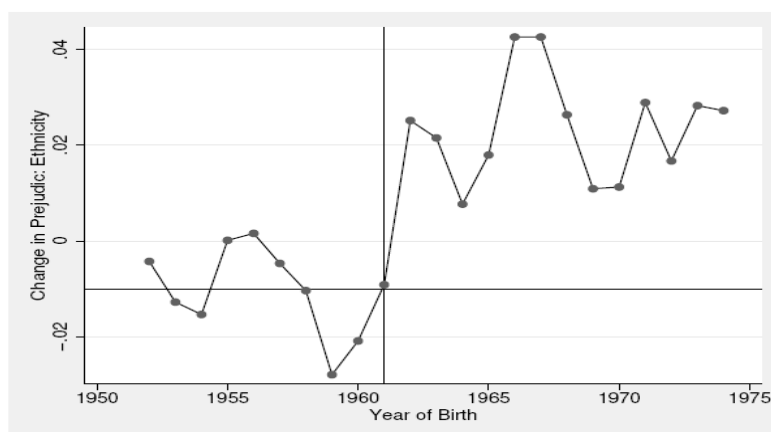
Figure 5. Tolerance Ethnicity: MA(3) of the coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.

Figure 6. Tolerance Ethnicity:; Coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.

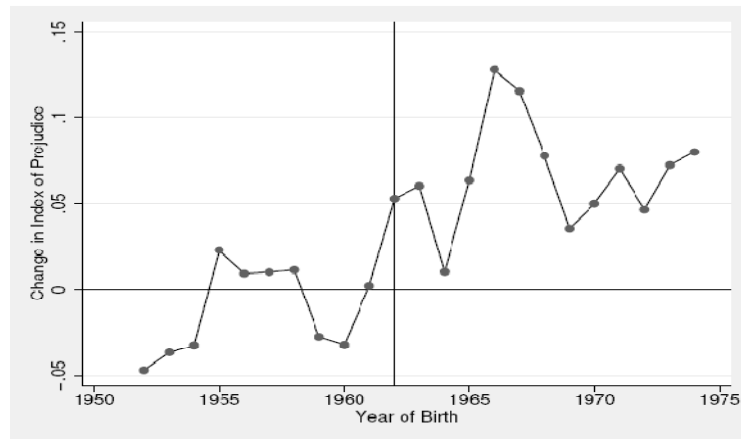**Figure 7. Tolerance Religion: MA(3) of the coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.**

Figure 8. Tolerance Religion with confidence intervals: Coefficient on High Treatment intensity interacted with a year dummy conditional on district of birth and cohort fixed effects plotted by year.
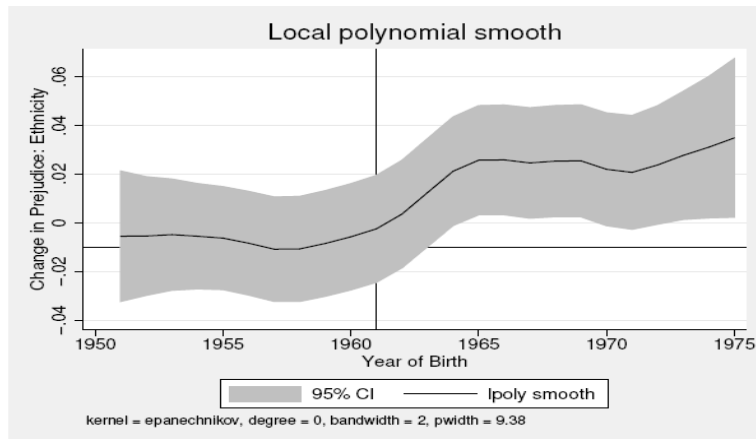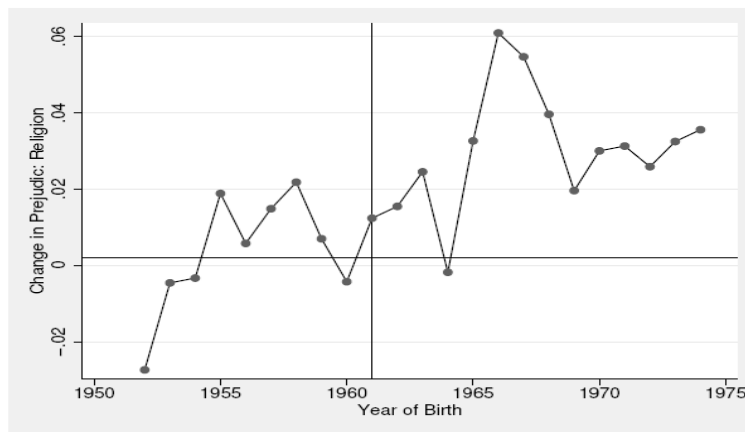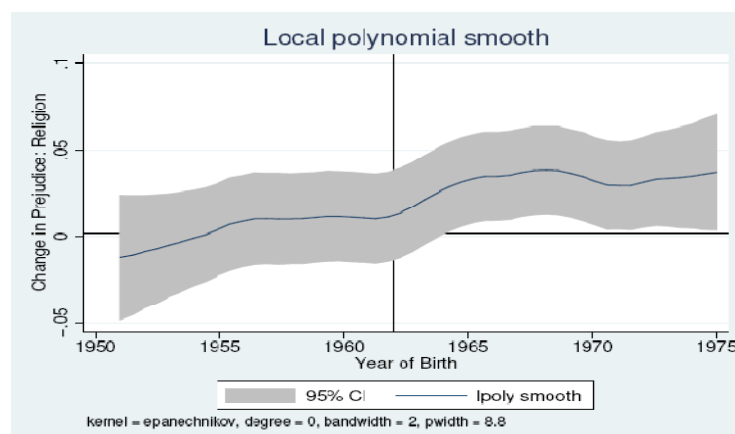
3.2 Instrumental Variables

Furthermore, we use an instrumental variable specification based on the generalized-difference-in-differences specification. We do so in order to assess the magnitudes of the effect of schooling on tolerance.

In our preferred specification of interest we instrument schooling with the interaction of a dummy indicating a high intensity district and a treatment indicator, $Treat_{idt} \times H_d$, conditional on both district of birth and cohort fixed effects. The result from this specification is displayed in Panel A of table 4. As argued in section 2, the instrument is valid conditional on cohort and district of birth fixed effects if the common trend assumption holds. I conduct two placebo exercises in section 4 to provide evidence that this key identifying assumption is valid.

To test the validity of the over-identifying assumption, we then use two instruments. We use a dummy indicating that an individual is born between 1966 and 1969 and an indicator for individuals born between 1970 and 1972, both interacted with a high treatment intensity indicator. In a third specification, we use the INPRES intensity interacted with a treatment indicator, $Treat_{idt} \times Int_d$.

All of our instrumental variable estimates suggest that an increase in schooling substantially affects attitudes towards individuals from a different ethnicity and religion. Specifically, an increase in schooling by one year results in about a 0.15 standard deviation increase in the "tolerance index" for all three specifications. This suggests a large causal effect of education on tolerance. It implies that a one standard deviation increase in schooling results in almost half a standard deviation increase in tolerance towards people from a different ethnicity and religion.

Moreover, we examine the validity and informativeness of our instruments. First, we investigate whether our instruments are weak: For this purpose, we employ the Angrist-Pischke cluster-robust first-stage statistic, which takes account of the non-iid structure of the errors [11]. Panels A to C in Table 4 show that the first-stage F-stat is well above the critical cut-off of 10. Indeed in our preferred specification in Panel A of table 4, the first-stage F-stat is well above 20. Second, we test the validity of instruments by use of specification B where we make use of two instruments. This allows us to test for the internal consistency[19] of the over-identifying restrictions by means of the Sargan/Hansen test statistic: Panel B in Table 4 shows that there is no evidence that the over-identification restrictions are violated.

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[19]The Sargan/Hansen test only allows us to answer the following question: Assuming that some of the instruments are valid, are all of them valid?

Table 4. Main Results: Instrumental Variables

	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A:			
Schooling	0.147**	0.068**	0.059**
	(0.066)	(0.029)	(0.029)
N	27203	28301	27741
Angrist-Pischke F-stat	24.70	22.65	26.81
Panel B:			
Schooling	0.136**	0.066**	0.053*
	(0.067)	(0.029)	(0.030)
N	27203	28301	27741
Angrist-Pischke F-stat	12.66	11.58	13.67
Sargan/Hansen test (p)	0.2784	0.6684	0.2095
Panel C:			
Schooling	0.136**	0.066**	0.053*
	(0.067)	(0.029)	(0.030)
N	27203	28301	27741
Angrist-Pischke F-stat	19.07	19.97	19.26
Cohort FE	Y	Y	Y
District of birth FE	Y	Y	Y
Population in 71 × Cohort FE	Y	Y	Y

Standard errors are clustered by the district of dirth. In Panel A we use one instruments: An indicator that an individual was born between 66 and 72 interacted with an indicator for high treatment intensity. In Panel B, we use two instruments: the first is an indicator that the individual is born between 66 and 69 interacted with a high treatment intensity indicator; and the second is an indicator that the individual is born between 70 and 72 interacted with a high treatment intensity indicator. In Panel C we use one instrument: An indicator that an individual was born between 66 and 72 interacted with spatial treatment intensity of INPRES. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

IV. ROBUSTNESS

In this section we conduct a variety of robustness checks. First, we show that our results are robust to including further control variables. Then, we show that our results are not significantly affected by clustering standard errors at different levels. Subsequently, we provide evidence on the validity of the common trend assumption. We show that the results are similar when taking into account of the ordered nature of the outcome variables. Subsequently, we show that our results are robust to using different definitions of treatment and control. Finally, we investigate the potential confounding factor of missing values and conduct a bounding exercise.

4.1 Additional Controls

We include data on the intensity of treatment of districts with a large sanitation program from Indonesia. In other words, we control for potential confounding effects of this health intervention. In addition, we interact the initial enrollment rate in 1971 with cohort-fixed effects to control for differential trends by initial enrollment rates. One could argue that districts with initially lower enrollment rates had differential trends from those with high initial enrollment rates. We find no notable changes in the coefficient estimates of interest; there are even slight increases in the coefficient estimates. We take this as evidence that the results are not-upward biased as a result of either differential trends or omitted variables. It is also possible that our results are driven by district of residence unobservables affecting levels of tolerance. Therefore, I have made sure that the results are robust to the inclusion of district fixed effects. These results are displayed in Panel H of Table 5. Finally, there are some concerns regarding the data quality in Papua. Therefore, I show that my results are not driven by the subsample from Papua. Indeed as can be seen in Panel I of Table 5, my estimated coefficient estimates even slightly increase once I exclude Papua.

Moreover, we take account of the ordered nature of the two attitudinal variables. In other words, we employ exactly the same identification strategy as in our main reduced form specification, but employ an ordered logit model. Thus, we can relax the equidistance assumption underlying the OLS estimates: these estimates implicitly rely on the assumption that, for example, the difference between 1 ("quite unhappy") and 2 ("unhappy") and 3 ("happy") and 4 ("very happy") is equivalent. As can be seen in Panel G of Table 4, our main results are robust to taking account of the non-linear nature of the dependent variable.

4.2 Standard Errors

Our results are robust to clustering standard errors in a variety of different ways. First, we cluster the standard errors by current district of residence as evidenced in Table 4 Panel C. Second, we cluster standard errors by the birth cohort as we might be concerned that there is correlation of error terms for people born in the same cohort. As we only consider 22 cohorts in our main specification, we apply a wild-cluster bootstrap correction of standard errors in order to avoid over-rejection of the null hypothesis [11, 20, 19].

Moreover, we take account of potential correlation in errors in different non-nested clusters by employing two-way clustering methods [19]. Specifically, we two-way cluster standard errors by district of birth and district of residence as can be seen in Panel E. Finally, as illustrated in Panel F, we employ two-way clustering by district of birth and cohort. The standard errors barely change as a result of two-way clustering. All in all, the small changes of standard errors for different levels of clusters is comforting evidence for valid inference.

Table 5. Robustness of the main results

	Schooling	No Schooling	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A Main Specifciation.					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.136)	(0.015)	(0.044)	(0.018)	(0.020)
N	31616	31616	27203	28301	27741
Panel B: With Controls					
Treatment ×	0.666***	-0.049***	0.101**	0.044**	0.041**
High Intensity	(0.138)	(0.016)	(0.045)	(0.018)	(0.021)
N	30579	30579	26249	27319	26781
Panel C: Cluster: Cohort					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.122)	(0.013)	(0.044)	(0.018)	(0.021)
N	31616	31616	27203	28301	27741
Panel D: Cluster: Current district					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.073)	(0.007)	(0.043)	(0.019)	(0.019)
N	31616	31616	27203	28301	27741
Panel E: Two-way Cluster A					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.136)	(0.015)	(0.044)	(0.018)	(0.020)
N	31616	31616	27203	28301	27741
Panel F: Two-way Cluster B					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.120)	(0.014)	(0.042)	(0.018)	(0.017)
N	31616	31616	27203	28301	27741
Panel G: Ordered Model					
Treatment ×	0.440***	-	-	0.191**	0.156*
High Intensity	(0.098)	-	-	(0.085)	(0.080)
Panel H: Current Districts FE					
Treatment ×	0.668***	-0.052***	0.105**	0.049***	0.040*
High Intensity	(0.121)	(0.014)	(0.045)	(0.018)	(0.021)
Panel H: without Papua					
Treatment ×	0.670***	-0.052***	0.100**	0.044**	0.041*
High Intensity	(0.120)	(0.014)	(0.046)	(0.019)	(0.022)
Cohort FE	Y	Y	Y	Y	Y
District of birth FE	Y	Y	Y	Y	Y
Population in 71 × Cohort FE	Y	Y	Y	Y	Y
N	31616	28301	27203	28301	27741

Standard errors are clustered at the district of birth unless otherwise specified. In Panel B we control for intensity of a health and sanitation program and enrolment rates, both interacted with cohort dummies. we Panel C we cluster standard errors by cohort and in D by current residence district. In Panel E we two-way cluster standard errors at the district of residence and district of birth. In Panel F we two-way cluster standard errors by cohort and district of birth. In Panel G we use an ordered logit model to take account of the ordinal nature of the outcome variables of interest. In Panel H I include both district of residence and district of birth fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 The Common Trend Assumption

We test whether the key identification assumption of a common trend in schooling and attitudes is valid. Specifically, we conduct two placebo tests for cohorts *not differentially* affected by INPRES. First, we use individuals born between 1950 and 1960. All of these individuals already should have left primary school by the time the first INPRES school was built. We create a pseudo-treatment indicator for individuals born between 1956 and 1960. We interact the pseudo-treatment indicator with a dummy variable indicating high INPRES treatment intensity[20] and control for cohort and district of birth fixed effects.

Second, we use data on individuals born between 1973 and 1985, i.e. cohorts that all should benefit from the INPRES program. We create a pseudo-treatment variable for those born after 1979. Then, we interact this pseudo-treatment indicator with a dummy variable indicating high treatment intensity. If the assumption of a common trend in schooling and interethnic and interreligious attitudes is valid, we would expect there to be no significant treatment effects in this placebo specification. As is evidenced in Panels A and B of table 5, this is exactly what we find in the data. None of the "pseudotreatment" coefficients is significantly different from zero; 6 out of the 10 tested coefficients are negative, providing further credibility to the common trend assumption.

Table 6. Common trend assumption

	Schooling	No Schooling	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A					
Pseudotreatment ×	0.0432	-0.0056	0.0299	-0.0037	0.0266
High Intensity	(0.1604)	(0.0175)	(0.0730)	(0.0280)	(0.0362)
N	11851	11851	10085	10526	10301
Sample B					
Pseudotreatment ×	0.0403	-0.0000	-0.0376	-0.0081	-0.0205
High Intensity	(0.0675)	(0.0051)	(0.0493)	(0.0193)	(0.0245)
N	24226	24226	20941	21751	21326

Standard Error clustered at the district of birth in parentheses. In Panel A we interact a pseudo-treatment dummy indicator (for cohorts born between 56 and 60) with a dummy indicating high treatment intensity. The control group in Panel A are cohorts born between 50 and 55. For Panel B we interact a pseudo-treatment dummy indicator (for cohorts born between 79 and 85) with a dummy indicating high treatment intensity. The control group in Panel B are cohorts born between 73 and 78.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Different Definitions for Treatment and Control

We show that our results are robust to using different definitions of treatment and control. We demonstrate that the results are quite robust to using different cutoffs for the treatment definitions – which is also clearly evidenced in figures 1 to 8 where we plot the "treatment effects" for every year individually. Using cohorts born in 1961 in the control group does not significantly affect our estimates. This can be seen in Table 7, Panel B. In Panels C and D, we use different definitions of treatment. Specifically, we show that including individuals born in

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[20]Results from this placebo exercise are very similar when using treatment intensity.

1973 (Panel C) and between 1973 and 1975 (Panel D) in the treatment group does not significantly affect results. Indeed, the coefficient estimates seem to increase slightly. Overall, the robustness of coefficient estimates provides further credibility to the estimated treatment effects.

Table 7. Sample Definitions: Robustness

	Schooling	No Schooling	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A Main Specification.					
Treatment ×	0.683***	-0.054***	0.103**	0.045**	0.042**
High Intensity	(0.136)	(0.015)	(0.044)	(0.018)	(0.020)
N	31616	31616	27203	28301	27741
R^2	0.014	0.008	0.001	0.001	0.001
Sample B					
Treatment ×	0.630***	-0.048***	0.096**	0.044***	0.036*
High Intensity	(0.130)	(0.014)	(0.042)	(0.017)	(0.020)
N	31616	31616	27203	28301	27741
R^2	0.014	0.007	0.001	0.001	0.001
Sample C					
Treatment ×	0.702***	-0.056***	0.101**	0.041**	0.045**
High Intensity	(0.136)	(0.016)	(0.043)	(0.018)	(0.020)
N	33584	33584	28911	30062	29489
R^2	0.016	0.008	0.001	0.001	0.001
Sample D					
Treatment ×	0.748***	-0.059***	0.111***	0.048***	0.044**
High Intensity	(0.137)	(0.016)	(0.042)	(0.017)	(0.020)
N	37728	37728	32498	33780	33138
R^2	0.019	0.010	0.001	0.001	0.001

Standard Error clustered at the district of birth in parentheses. In Panel A we report the main specification. In Panel B, we include 1961 cohort as a control year. In Panel C we include 1973 as a treatment year. In Panel D we include cohorts born 1973 - 1975 in the treatment group.

4.5 Missings and Partial Identification

About 14 percent of our sample have missing values for the question on tolerance. We investigate whether treated individuals are more likely to have missing values. As is evidenced in Table 8, treated individuals receiving more education are more likely to respond to the question. One interpretation is that individuals with more education have less difficulties in understanding the question, and are therefore more likely to reply to the question. Yet, there is a second interpretation: Not responding to the question may be a sign of negative attitudes towards individuals from a different ethnicity and religion if we are willing to assume that individuals experience a disutility in expressing negative inter-ethnic and inter-religious

attitudes.[21] If we assume that those individuals who refused to answer the question have a less positive attitude towards people from a different ethnicity, then this would imply that our estimates are downward biased, i.e. that the true population "treatment effect" of interest is larger than the estimates from our main specification.

To investigate the sensitivity of our results to these missing values we conduct a partial-identification exercise. We bound our estimated effects as follows: First, we set all of the missings to the highest (lower bound of the estimate) and then to the lowest possible value (upper bound of the estimate). Table 8 shows that these bounds range from ca. 0.07 to 0.01 for both outcome variables of interest. Given that people might experience disutility from expressing a negative attitude towards people from a different ethnicity, those might be more likely not to respond. This would imply that the "true treatment effects" are closer to the upper rather than the lower bounds given by this partial identification exercise given that we find that less educated individuals are less likely to respond.

Table 8. Missings and Partial Identification

	(1)	(2)	(3)	(4)	(5)	(6)
	Missing: Tolerance		**Upper Bound: Tolerance**		**Lower Bound: Tolerance**	
	Ethnicity	**Religion**	**Ethnicity**	**Religion**	**Ethnicity**	**Religion**
Treatment ×	-0.017	-0.022*	0.069***	0.073***	0.019	0.008
High Intensity	(0.011)	(0.012)	(0.024)	(0.026)	(0.022)	(0.025)
N	31616	31616	31616	31616	31616	31616
R^2	0.096	0.107	0.100	0.158	0.047	0.062

Standard errors clustered at the district of birth in parentheses. In columns (1) and (2), results on the probability of missing values of the two tolerance questions are reported. In columns (3) and (4) we set all of the missing values to the lowest possible value (1). In columns (5) and (6) we set all of the missing values to the highest possible value (4).

V. MECHANISMS AND HETEROGENEITY

In this section we analyze potential mechanisms underlying the estimated effect of education. First, we show that moral education on the state ideology under Suharto is a key mechanism underlying the results. Then we investigate several further mechanisms, such as migration, occupational choice and income effects. Finally, we rule out alternative explanations based on general pro-sociality, socialization and well-being.

5.1   Pancasila Moral Education

In what follows we investigate whether the educational content, i.e. Pancasila moral education, is important for the estimated relationship between schooling and tolerance. Specifically, we compare differences in differences between individuals with schooling and

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[21]Clearly, this is also reliant on the assumption of substantial lying costs [1]

whether they were born before 1988. Those born in or after 1988 would only be slightly or not at all affected by the Suharto regime's attempts for indoctrination in primary schools.[22]

Specifically, as Suharto fell in 1998 only those born before 1985 would still be indoctrinated with Pancasila during their whole education at primary school. Those born between 1985 and 1987 would be partially treated; and those born in or after 1988 would only be marginally treated or not treated at all. To identify the effect of changes in educational content as a result of the Suharto regime, we employ the following strategy: we use variation in schooling as well as temporal variation to differentiate between individuals born before or after 1988.

Specifically, we create a treatment indicator TR_{idt} for individuals born before 1985 and after 1973, a partial treatment indicator for individuals born between 1985 and 1987, PTR_{idt} and interact these with a variable indicating the amount of schooling, SC_{id}. We employ a strategy controlling for district of birth[23], α_d, and cohort fixed effects, β_t.

$$y_{idt} = \pi_1(TR_{idt} \times SC_{id}) + \pi_2(PTR_{idt} \times SC_id) + \pi_3SC_{id} + \alpha_d + \beta_t + \varepsilon_{idt} \qquad (6)$$

We cluster the standard errors at the district-of birth level and have made sure that the results are robust to clustering at different levels. Table 11 presents the results: we find that those with higher levels of education born before 1985 and 1989 respectively display significantly higher levels of tolerance towards people from a different ethnicity and religion. This provides further evidence for the importance of educational content under Suharto, i.e. Pancasila moral education, as a mechanism underlying the estimated treatment effects.

One could argue that those with low schooling born in 1989 differ substantially from those with low schooling born in 1973. One could argue that comparing those born between 1985 and 1988 with those born after that should address concern of time-varying unobservables of those with low schooling. In other words, one can compare those individuals born within a time window of a few years and interpret the results in the spirit of a regression discontinuity design. As can be seen in Table 11, our results are robust for those individuals that were partially treated, i.e. born between 85 and 88 (inclusive). Indeed, the estimated treatment effects are quite similar for partially vs. fully treated individuals.

In addition, we conduct two placebo exercises to address further caveats regarding time-varying unobservables of individuals with low levels of schooling. Specifically, we use individuals born between 1950 and 1960 (individuals not affected by Pancasila) and define those born between 1956 and 1960 as "pseudo-treated". We interact the pseudo-treatment indicator with the schooling variable. As can be seen in Panel B of Table 10. We find no treatment effects. Finally, we use individuals born between 1970 and 1979 (all similarly affected by Pancasila moral education at schools) to conduct a further placebo. We use those born after 1975 as "pseudo-treated" and interact this indicator with the years of schooling. Panel C of Table 10 shows that we find no treatment effects for this falsification exercise.

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[22]It should be noted that a certain much less pronounced and intensive form of Pancasila moral education is still taught in schools in Indonesia today. It enjoys a much lower importance than in the Suharto period.

[23]The results are robust to controlling for district of residence rather than district of birth.

Table 9. Pancasila Moral Education

	Index: Tolerance	Tolerance: Ethnicity	Tolerance: Religion
Panel A			
Treatment ×	0.032***	0.011***	0.016***
Schooling	(0.010)	(0.004)	(0.005)
Partial Treatment ×	0.032**	0.009	0.015***
Schooling	(0.013)	(0.005)	(0.006)
N	27031	28099	27511
Panel B: Placebo 1			
Pseudo-treatment ×	-0.005	-0.001	-0.004
Schooling	(0.009)	(0.004)	(0.004)
N	10147	10588	10364
Panel C: Placebo 2			
Pseudo-treatment ×	-0.008	-0.001	-0.004
Schooling	(0.010)	(0.004)	(0.005)
N	17527	18178	17864
District of birth FE	Y	Y	Y
Cohort FE	Y	Y	Y
N	27031	28099	27511
R^2	0.106	0.067	0.130

Standard Error clustered at the district of birth in parentheses. The fully treated individuals are defined as those born between 1973 and 1984. The partially treated individuals are those born between 1985 and 1987. The control individuals are those born after 1988. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Labor market mechanisms and income effects

The availability of primary education may substantially affect individual's life in terms of occupational choice, migration into cities and earned income. First, we look into the effects of INPRES on occupational choice. We find that those individuals with exogenously increased levels of education are more likely to work in trade and industry and less likely to work in agriculture. Moreover, "treated individuals" are more likely to migrate[24], and to live in cities. This in turn has dramatic consequences on individual's social environment as they are exposed to a higher degree of ethnic and religious heterogeneity.

Moreover, treated individuals have have higher total expenditures. It may be that this increase in expenditures affects individuals preferences: first, it may be that there is a direct link between income and tolerance. Second, it may be that the opportunities brought about by an increase in income, for example travel, affect tolerance and social preferences. This mechanism is in line with [24]. They show that negative exogenous economic shocks result in more right wing voting in Germany.

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[24]Here, we employ the following definition of migration: the dummy migration takes value one if an individual currently does not live in the district in which he or she was born. This variables is noisily measured as it does not capture within-district migration and it does not account for multiple migrations.

Table 10. Labor Market Mechanisms

	(1) Log Expenditure	(2) Migration	(3) Agriculture	(4) Industry	(5) Trade	(6) Employee	(7) Rural
Panel A:							
Treatment ×	0.033*	0.753*	-0.025*	0.026***	0.021*	0.008	-0.032***
High Intensity	(0.019)	(0.416)	(0.013)	(0.008)	(0.012)	(0.013)	(0.012)
N	32035	32035	24017	24017	24017	24017	32035
Panel B:							
Treatment ×	0.000***	0.002*	-0.000**	0.000***	0.000***	0.000	-0.000***
Intensity	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	31750	31750	23787	23787	23787	23787	31750

Standard errors clustered at the district of birth in parentheses. In columns (3) to (6) we only consider individuals currently in the labor market. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Alternative Mechanisms

In this subsection, we examine alternative mechanisms that could be driving the treatment effects. We investigate how the treatment affects several variables that might constitute alternative mechanisms underlying the effects on tolerance. Finally, we analyze heterogeneous effects of INPRES by the ethnic segregation of the district of birth.

It might be that individuals that are more educated are generally happier, more pro-social and more sociable. [49] have recently pointed out that it is more powerful to use noisily measured potential confounding variables on the left-hand side of the candidate equation rather than including them as 'endogenous controls' in the main specification. Therefore, we employ our main generalized difference-in-differences specifications (both based on intensity and one based on a high intensity dummy) to investigate the effect of INPRES on (i) social club memberships, (ii) willingness to help neighbors and (iii) happiness.[25]

It could be that individuals with higher levels of education acquire skills of socializations as a result of higher levels of schooling. In other words, in addition to human capital they might build up social capital. To test for this particular mechanism, I examine whether individuals with higher levels of schooling join more social and religious organizations. As can be seen in table 11, we find no evidence that those individuals affected by INPRES increase their activities in religious and social organizations. The estimated treatment effect is not statistically significant.

Second, recent evidence by [39] suggests that education has a causal impact on social preferences. Therefore, we investigate whether schooling might have increased pro-social attitudes more generally. For this purpose we investigate the effect of receiving more education on willingness to help neighbors and to share with neighbors. We find no evidence for in favor of this particular mechanism.

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[25]We have made sure that our results are similar for an alternative measure of mental health.

Third, it could be hypothesized that higher income and the resulting increase in well-being and reduction in anxiety of potentially losing a job, could reduce racism. We find no evidence for this mechanism: specifically, our estimates suggest that the treated group does not display higher levels of well-being.

Table 11. Alternative Mechanisms

	Willingness to help	Mutual Support	Religious Organization	Social Organization	SWB
Panel A:					
Treatment ×	0.004	-0.021	0.006	0.008	0.008
High Intensity	(0.013)	(0.018)	(0.020)	(0.024)	(0.013)
N	32035	32035	31511	29646	32035
Panel B:					
Treatment ×	-0.000	-0.000	-0.000	0.000	0.000
Intensity	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	31750	31750	31234	29387	31750

Standard errors clustered at the district of birth in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Furthermore, there might be a direct socialization mechanism through which education could affect interethnic and interreligious attitudes: specifically, attendance of primary school might expose individuals to interactions with individuals from a different ethnicity and religion [48]. To test whether interactions with individuals from a different ethnicity are an important mechanism in this context, we make use of a measure of ethnic segregation [6]. Specifically, we conduct a median-split by ethnic segregation in order to assess treatment heterogeneity. We find that the treatment effects are very similar for individuals born in high ethnic segregation districts vs. low ethnic segregation districts. Therefore, we can rule out an explanation based on the contact hypothesis based on our data.

Table 12. Heterogeneity by ethnic segregation of district of birth

	Schooling		No Schooling		Index of Prejudice	
High Ethnic Segregation	yes	no	yes	no	yes	no
Treatment	0.774***	0.523***	-0.053***	-0.053**	0.085	0.120*
	(0.173)	(0.194)	(0.020)	(0.024)	(0.058)	(0.067)
P-value of difference	0.363		0.903		0.717	
N	15960	14876	15960	14876	13451	13100
R^2	0.013	0.020	0.009	0.009	0.002	0.002
	Tolerance: Ethnicity		**Tolerance: Religion**			
High Ethnic Segregation	yes	no	yes	no		
Treatment	0.044*	0.048*	0.026	0.053*		
	(0.024)	(0.027)	(0.027)	(0.031)		
P-value	0.890		0.569			
N	13947	13677	13819	13270		

Standard errors clustered at the district of birth in parentheses. Here, we conduct a median split method and split the sample by the median level of ethnic segregation (in which the segregation index takes value .25) in an individual's district of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VI. CONCLUSION

This paper is the first to provide causal evidence on the relationship between education and interethnic and interreligious tolerance. We use a unique large cross-sectional dataset with attitudinal data and show that an exogenous increase in education results in higher tolerance and pro-social attitudes towards individuals from a different religion and ethnicity.

We provide evidence that moral education in primary schools under Suharto explains some portion of the treatment effects: cohorts born before 1988 were exposed to an indoctrination program by the Indonesian government that emphasized the unity of Indonesia and tried to lower both religious and ethnic cleavages. In addition, we emphasize important labor market mechanisms through which INPRES may have affected tolerance: individuals more strongly exposed to the program are more likely to migrate into cities, to work in trade and industry. These factors in turn imply that the religious and ethnic diversity of treated individuals is increased. Furtermore, treated individuals earn substantially higher incomes.

This paper has important policy implications: it provides clean evidence on the endogeneity of preferences [5, 46, 33] to schooling. It shows that education has additional positive benefits in terms of increased tolerance in addition to well-documented benefits, such as increased citizenship and lowered delinquencies [41, 40]. It shows that education at school can substantially affect attitudes towards people from a different ethnicity and religion. This in turn implies that moral education at schools may be a viable tool for governments to increase cohesion among citizens.

Future research should try to extend this work along two dimension: first, researchers should focus on understanding the exact mechanisms through which education affects attitudes. Second, researchers should test for the external validity of these results in a developed country context: For example, researchers could exploit exogenous variation in education in a developed country context (through compulsory schooling laws) to test for changes in tolerance and racism.

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