Perils or Promise of Ethnic Integration?

Appendix of Supporting Materials

Cyrus Samii
New York University
January 24, 2013

Table and Figure numbering is carried over from the main text. Any references made below also refer to references listed as part of the main text.

A Sampling procedures

The relevant data are from ex-FAB who were integrated into the new army as well as those who were not integrated and, by the time of interview in the summer of 2007, demobilized. The sampling strategies were slightly different for demobilized FAB versus those in the integrated military. Demobilized FAB were selected through a multistage random sample from lists of demobilized ex-combatants registered to receive reintegration benefits through Burundi's national demobilization, disarmament, and reintegration (DDR) program. The first stage involved randomly selecting 66 out of Burundi's 129 *communes*—Burundi's second-tier administrative unit—from a list of communes stratified by Burundi's 17 provinces. We then set a target number of ex-combatant interviews to complete in each commune, with targets proportional to the national proportion of ex-combatants registered with the DDR program in the commune. Targets ranged from 2 to 33. Then, we obtained from the national DDR office the complete lists of ex-combatants registered as residents of each of the selected communes. We then drew a simple random sample (with a random number generator in the software package, R) of the desired number of interviews from each of these commune lists. We created a randomly selected reserve list for each commune to use in the case of non-response. Selected participants were contacted and brought to the respective Provincial Bureau by DDR program staff for interview on a scheduled date by our enumeration team.

Active members of the military were selected through a separate multistage process. In the same 66 communes as described above, enumerators approached police station chiefs with a letter from the Ministry of Interior explaining that they were to list all officers stationed in the commune. Then the enumerators randomly selected a target number of police which was proportional to the

population size of the commune. The reason is that police assignments are given in a manner proportional to commune population. This creates as close to a self-weighting, representative sample as possible with available information. For the active members of the national army, we identified all camps listed in the 66 selected communes. Enumerators then approached the barrack with a letter from the Minister of Defense instructing them to cooperate with the enumeration team to use interval sampling to select non-commissioned officer and rank-and-file soldiers from the camp's register, with the number selected in each camp proportional to the total in the camp, as inferred from information provided by the Defense Ministry.

The rate at which first choices from the respective sample frames were interviewed in each of these samples (equivalent to the American Association of Public Opinion Professionals definition "RR2") was very high—around 90% for each—and so I assume no need for further adjustment to account for non-response. This very high response rate was likely due to a few factors: (1) respondents probably took the interview to be a requirement given the formal manner in which they were approached; (2) we accommodated schedules by setting dates for interviews well in advance; (3) the fieldwork was conducted during the idle interim period between planting and harvesting seasons, and so demobilized soldiers working in the agricultural sector faced few competing demands on their time; and (4) for the demobilized soliders, while participation was voluntary, a "transport allowance" of about 2 US dollars was provided to each respondent after they completed the interview, thus making it worthwhile for respondents to sit through the entire interview. (Enumerators purchased cases of soft drinks for police stations and army camps.)

B Sources of "fuzziness" in the application of the age requirement

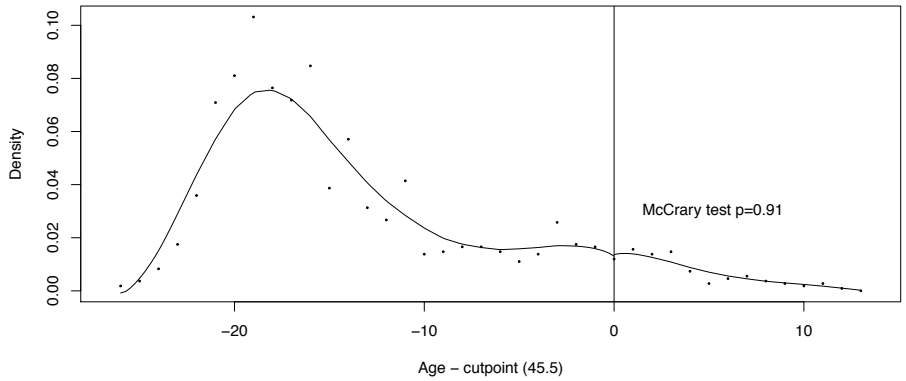
At the time of fieldwork in June-August 2007, eligibility for participation was restricted to those below the age 45 as of September 2006, the timing of the last application of the eligibility threshold for demobilization prior to fieldwork. Those who were less than 45 years of age had not yet been subject to the eligibility cut-off from the previous year. Those aged 45 and in the military would be subject to the eligibility criterion sometime shortly after field work as part of the rationalization process. Some of these people would have turned 45 in the months between September 2006 and the field work period of June-August 2007, and so the maximum age among those deemed eligible by the rules should be 45, and the youngest age for which there should be no ambiguity of ineligibility is 46, with some indeterminacy about ineligibility for those aged 45. I set the cut-off at 45.5 years to account for such coarseness. Looking at Figure 2, we see that this results in some fuzziness at the cut-point.

Individuals in the military at the time of fieldwork would have been subject to about 18 months of experience in the integrated army. The age-based eligibility threshold was only applied with regularity during the transition as of late 2005, just at the onset of integration. Our sample includes individuals who were demobilized as part of a wave of applications of the eligibility threshold in October-November 2005 (accounting for 55 out of the 85 retirements in the sample), with a second, smaller wave occurring in March 2006, and sporadic retirements otherwise. Thus, a few of the 45 and 46 year-old demobilized soldiers in the sample had the chance to participate in the integrated army for some period up to mid/late 2006, implying that they would have had some 6-9 months

of exposure, although this would have ended at least 6 months prior to field work. Nonetheless, to make the estimation more straightforward, I consider these to be on the "non-treated" side of the eligibility cut-off. This coding will dilute the effect estimates to a certain extent, but the payoff is to make the estimation much more straightforward.

C Density test for sorting and manipulation at the cut-point

Figure 5: Forcing variable density analysis



Refer to McCrary (2008) for a description of the density analysis. The analysis uses a histogram bin width of 1 year (the smallest possible, given the coarseness of the age variable) and applies a local regression linear smooth with a bandwidth of 5 years. The *p*-value is for a test of the null hypothesis of smoothness in the density of the forcing variable. Sorting or manipulation around the cut-point typically results in stacking of the density on one or another side of the cut-point. The *p*-value of .91 suggests no evidence of this.

D Questions used for non-response index

Table 5: Questions used to construct the non-response index

Question id	Question	Overall non-resp. rate	Non-resp. rate in 5-year bandwidth
dm26	In 1993, which political party or movement did you support?	0.76	0.71
dm27	Which political party do you currently support?	0.73	0.76
cm5	Where you live now, how wealthy are you compared to others?	0.03	0.04
cm6	...compared to other Hutus?	0.04	0.06
cm7	... compared to other Tutsis?	0.03	0.06
cm8	Before the war in your community, how wealthy was your family compared to others?	0.05	0.04
cm9	...compared to other Hutus?	0.05	0.06
cm10	...compared to other Tutsis?	0.05	0.06
deathroster1	Did anyone in your immediate family die during the war?	0.04	0.05
pf30	What was the main cause that the CNDD-FDD was fighting for?	0.28	0.39
pf31	...that the FNL was fighting for?	0.39	0.39
pf32	...that the FAB was fighting for?	0.02	0.08
tr3	Did you witness civilians being killed?	0.01	0.04
tr6	How many members of your family were killed in the war?	0.03	0.07
tr7	...friends were killed in the war?	0.06	0.10
uo1	What was the name of your fighting unit?	0.01	0.04
lb2intara	Where did your last combat engagement take place?	0.09	0.21
re1	How do people in your community look upon former rebels?	0.03	0.04
re2	...former FAB?	0.02	0.03
re4	Some people say that former combatants who killed civil populations or who raped women should not be accepted in their families in any case and they should be punished. Some others say that they should be accepted and what happened should be forgotten. A third group says that they could be accepted if they beg for forgiveness. Which of the three groups do you support?	0.01	0.01
cp3	Comparing with the situation before the war, do you think you have more, fewer or the same political rights?	0.08	0.07
cp6	If there are persons who were rich before the war due to ethnical, regional or political exclusion, do you think that: [read the two options] 1. The government should seize their properties in order to use them for public interests. – or – 2. The government does not have the right to take those properties, as is the case for any other person?	0.04	0.03
cp8	Which one of the following statements do you support? 1. The government should ensure equal access to higher education as well to government jobs for all ethnic groups according to the proportions of the populations in the country - - or – 2. The government should not consider ethnicity when recruiting for jobs or higher education institutions?	0.01	0.01
qe1	[To enumerator] Did the respondent seem distracted?	0.06*	0.07*
qe2	[To enumerator] Was the respondent readily willing to answer questions?	0.12*	0.17*
qe3	[To enumerator] Were you uncomfortable with this respondent?	0.10*	0.12*

*For these questions, the rate reported is not the non-response rate, but the rate at which enumerators marked an answer indicative of the subject being reluctant to respond.

Constructing the non-response index required determining a set of "sensitive" questions. A first cut list was developed based on discussions with our enumerators before and after fieldwork. The enumerators were not aware that this assessment of sensitivity would be used to construct

a measure of prejudice. We did not change the questions either. Rather, we emphasized to the enumerators that at the beginning of each module in the questionnaire, they were to explain again that the respondent had the right to refuse to answer or indicate "I don't know" for any question posed. In constructing the non-response measure I do not distinguish between "refuse" and "don't know" responses, because as our enumerators explained, a common way for a respondent to dodge a question would be to say "don't know." This introduces a bit of measurement error. I see no reason to think that the error would bias the analysis, although it does contribute to some loss of statistical power. I then narrowed the list to a set that exhibited at least some non-response. (Questions with no non-response provide no information, and so their exclusion does not contribute to bias.) The result was a list of 23 "sensitive" questions, which are shown in Figure **??**. Note that these questions typically served as lead-in questions that determined survey "skip patterns"; that is, responses to these questions would determine whether a subsequent battery of (typically, sensitive) questions was administered. Because non-response on these latter questions automatically followed non-response from the lead-in questions, I do not include non-response on the latter questions to avoid exaggerating the level of non-response. Three additional questions posed to the enumerator directly asked about levels of responsiveness and comfort during the interview. These were also added to the index.

E Enumerator coethnicity assignment, weighting, and enumerator characteristics

Our enumeration team consisted of seven enumerators who identified as Hutu, and 17 who identified as Tutsi. The ethnic distribution of our enumeration team differs greatly from the ethnic distribution of the population, in which Hutus are the large majority (popularly believed to make up 85% of the population).[25] Given that we recruited enumerators from among the university-educated, the disparity between our team's ethnic distribution and that of the population is a testament to the deep legacy of Hutu exclusion in the education system.Our enumerator assignment protocol was such that enumerators were to be randomly assigned to respondents, who themselves were randomly selected. In this way, the protocol provided for the random assignment of enumerator-respondent pairs to be either coethnic (both having the same ethnicity) or non-coethnic (ethnicity of enumerator and respondent differ). Because we were not present to monitor enumerators at all times, there is some question as to whether enumerators may have violated this protocol, and sorted their interviews on the basis of ethnicity or some other factor that may confound the coethnicity assignments. I assessed this possibility by performing a randomization test. Our respondent data contain 1086 total interviews with still-active and demobilized FAB soldiers. Of them, 902 identify as Tutsi, and 184 as Hutu. Given those proportions, as well as our 7 to 17 ratio of Hutu to Tutsi enumerators, purely random assignment would lead us to expect 64% of enumerator-respondent pairs to be ethnically concordant. In my sample, the figure is 65%. An exact binomial test indicates that probability of seeing this large of a departure given random assignment is 0.39, well within reasonable bounds. I think it is reasonable to believe that the scope for sorting on ethnicity was limited.

[25] Refer to fn 2.

We trained our enumerators extensively on the protocol, and emphasized that compensation and retention in the project would be based on their abiding by the protocol.

Because the proportion of Hutu and Tutsi enumerators was not 50-50, the probability of having a coethnic enumerator varies depending on the ethnicity of the subject. For Tutsi subjects, the randomization procedure implies that the probability of having a coethnic interviewer is $17/24$ and a non-coethnic is $7/24$. For Hutu respondents, these probabilities are reversed. In order to ensure an unbiased estimate of the sample average effect of coethnicity, units are weighted by the inverse of their probability of being assigned to the condition that they received.

All enumerators were recruited through *Iteka–Ligue Burundaise des Droits de l'Homme* (Burundian League for Human Rights), a non-partisan, nationally prominent, and nationally syndicated human rights organization. The organization enjoys a reputation in Burundi of being a neutral advocate for human rights, taking stances at times in opposition of all political tendencies in the country. *Iteka* members hail from all parts of the country, although their operations are centered in the capital, Bujumbura. The enumeration staff included individuals from all regions of the country, although they were all recruited at *Iteka*'s central offices in Bujumbura. The enumerators ages ranged from 25 to 45, and included 8 women (of which 1 was Hutu). All enumerators had at least a university degree, which is rare in Burundi (about 1% of adults according to our survey).

F Summary statistics

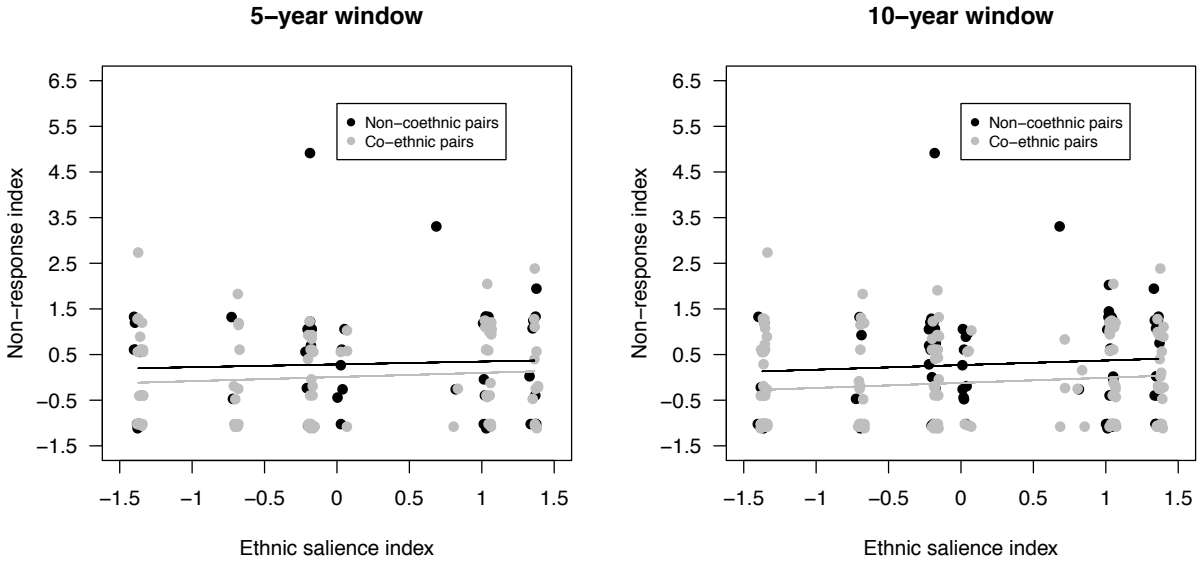
Table 6: Summary statistics for whole sample, 10-year, and 5-year bandwidths

	Whole Sample			10-year Bandwidth			5-year Bandwidth		
Variable	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
Hutu	0.17	0.38	1086	0.07	0.25	265	0.07	0.25	161
Age-45.5	-13.15	7.65	1086	-1.94	4.81	265	-0.33	2.65	161
Non-response index	0.00	1.00	1086	0.03	1.03	265	0.14	1.08	161
Non-response proportion	0.12	0.09	1086	0.13	0.11	265	0.14	0.12	161
Support equal access	0.12	0.32	1071	0.10	0.30	260	0.07	0.25	159
Ethnic salience index	0.00	1.02	980	0.03	1.00	239	0.01	1.01	143
Support co-ethnics' ideas	0.32	0.47	1055	0.32	0.47	254	0.36	0.48	153
Own wellbeing depends on coethnics'	0.38	0.48	1029	0.37	0.48	251	0.36	0.48	151
What happens to coethnics matters to me	0.64	0.48	1024	0.65	0.48	251	0.65	0.48	152
Age $<$ 45.5	0.91	0.28	1086	0.66	0.47	265	0.57	0.50	161
Non-coethnic interviewer	0.35	0.48	1086	0.33	0.47	265	0.32	0.47	161
Integrated	0.68	0.47	1086	0.6	0.49	265	0.56	0.50	161
NCO	0.72	0.45	1017	0.97	0.18	235	0.99	0.12	139
Years in military	12.68	6.82	1074	21.85	7.06	257	24.14	5.69	153
Pre-war education (years)	5.83	2.22	1086	7.45	2.21	265	7.39	2.19	161
Unit death rate	0.09	0.14	1002	0.09	0.17	240	0.08	0.16	144
Family death rate	0.12	0.17	1086	0.14	0.17	265	0.15	0.18	161
Economic conditions worsened	0.50	0.50	946	0.52	0.50	236	0.54	0.50	138
log(Income/month + 1)	10.55	0.77	1029	10.57	0.84	248	10.57	0.86	147
Placebo non-response rate	0.10	0.07	1086	0.11	0.10	265	0.12	0.11	161

G Relationship between outcome measures

Figure 6 shows the relationship between the non-response index and the ethnic salience index for subjects with ages within 10-year and 5-year windows around the cut-point. The upper line in both graphs is the regression fit for subjects in the non-coethnic interview condition, and the lower line is the regression fit for subjects in the coethnic interview condition. The non-response index rises slightly in levels of salience and those with non-coethnic interviewers have higher non-response index scores. The correlation is moderate, at about 0.10 (significant with $p < .05$ for the 10-year bandwidth, but not significant for the 5-year bandwidth). If the two measures were measuring a single underlying trait, we would expect that the magnitude of the coethnicity effect would be larger at higher levels of salience. This is not so, suggesting that attitudes and behavior associated with prejudice and ethnic salience do not reduce to simple relationships.

Figure 6: **Relationship between the non-response and ethnic salience indexes**



H Robustness Checks for Regression Discontinuity Estimates

Table 7: Effects of integration on prejudice, using the raw non-response scores

	(4-year bandwidth) Non-resp. prop.	(5-year bandwidth) Non-resp. prop.	(10-year bandwidth) Non-resp. prop.
Integrated	-0.11	0.01	-0.02
	(0.22)	(0.06)	(0.05)
IntegratedXNon-coeth.	-0.04	-0.09*	-0.08**
	(0.10)	(0.05)	(0.03)
Non-coeth. pair	0.06	0.10***	0.09***
	(0.06)	(0.03)	(0.02)
Age-45.5	-0.03	-0.01	-0.02
	(0.05)	(0.01)	(0.02)
(Age<45.5)X(Age-45.5)	0.01	0.00	0.01
	(0.02)	(0.01)	(0.02)
$(\text{Age-45.5})^2$			0.00
			(0.00)
$(\text{Age<45.5})\text{X}(\text{Age-45.5})^2$			-0.00
			(0.00)
Constant	0.19	0.12***	0.15***
	(0.14)	(0.04)	(0.04)
Observations	110	161	265

Standard errors in parentheses

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

Weighted two-stage least squares estimates with standard error estimates that account for clustering by interview location/barrack. The first column corresponds to the Imbens-Kalyanaraman optimal bandwidth.

Table 8: Effects of integration on prejudice, triangular kernel estimates

	(4-year bandwidth) Non-resp. index	(5-year bandwidth) Non-resp. index
Integrated	-0.21	-0.14
	(1.52)	(0.97)
IntegratedXNon-coeth.	-0.94	-0.99
	(0.89)	(0.61)
Non-coeth. pair	0.92*	0.93**
	(0.50)	(0.37)
Age-45.5	-0.06	-0.08
	(0.33)	(0.19)
(Age<45.5)X(Age-45.5)	-0.09	-0.04
	(0.15)	(0.11)
Constant	0.02	0.01
	(0.90)	(0.57)
Observations	141	161

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(With triangular kernel)

Imbens and Lemieux (2007:11) argue that given a sufficiently tight window, a local linear regression with a rectangular kernel (that is, an ordinary linear regression within the window) should be adequate. Some modicum of bias reduction may be gained with a more sophisticated kernel—e.g. a triangular kernel, equivalent to using weights that decrease linearly in distance from the cut-point—although at a cost to efficiency. Because efficiency concerns predominate in my analysis, I use rectangular kernel estimation as my primary strategy for modeling effects. We see that the triangular kernel, applied to the 4-year and 5-year bandwidth models, does not change the point estimates appreciably, although the standard errors increase considerably.

Table 9: Effects of integration on prejudice, controlling for enumerator fixed effects

	(4-year bandwidth) Non-resp. index	(5-year bandwidth) Non-resp. index	(10-year bandwidth) Non-resp. index
Integrated	-0.35	0.03	-0.31
	(0.78)	(0.50)	(0.49)
IntegratedXNon-coeth.	-0.96*	-0.89*	-0.77**
	(0.52)	(0.53)	(0.30)
Non-coeth. pair	0.82*	1.04**	0.98***
	(0.49)	(0.42)	(0.27)
Age-45.5	-0.11	-0.04	-0.11
	(0.18)	(0.12)	(0.19)
(Age<45.5)X(Age-45.5)	-0.09	-0.04	-0.05
	(0.14)	(0.15)	(0.23)
$(\text{Age-45.5})^2$			0.01
			(0.02)
$(\text{Age<45.5})\text{X}(\text{Age-45.5})^2$			-0.02
			(0.02)
Observations	139	158	262

Standard errors in parentheses

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

(With enumerator fixed effects)

Table 10: Effects of integration on ethnic salience, controlling for co-ethnicity of interviewer

	(4-year bandwidth) Eth. salience	(5-year bandwidth) Eth. salience	(10-year bandwidth) Eth. salience
Integrated	-0.33	-0.03	-0.09
	(0.74)	(0.56)	(0.33)
Age-45.5	-0.07	0.00	0.06
	(0.17)	(0.11)	(0.04)
(Age<45.5)X(Age-45.5)	0.07	0.08	-0.06
	(0.20)	(0.15)	(0.05)
Non-coeth. pair	0.21	0.15	0.27**
	(0.22)	(0.20)	(0.12)
Constant	0.21	0.10	-0.08
	(0.45)	(0.34)	(0.22)
Observations	126	143	239

Standard errors in parentheses

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

Table 11: Effects of integration on ethnic salience, controlling for enumerator fixed effects

	(4-year bandwidth) Eth. salience	(5-year bandwidth) Eth. salience	(10-year bandwidth) Eth. salience
Integrated	-0.10	-0.01	-0.24
	(0.73)	(0.53)	(0.29)
Age-45.5	0.08	0.10	0.05
	(0.18)	(0.12)	(0.03)
(Age<45.5)X(Age-45.5)	-0.05	-0.06	-0.06
	(0.17)	(0.16)	(0.04)
Observations	124	140	236

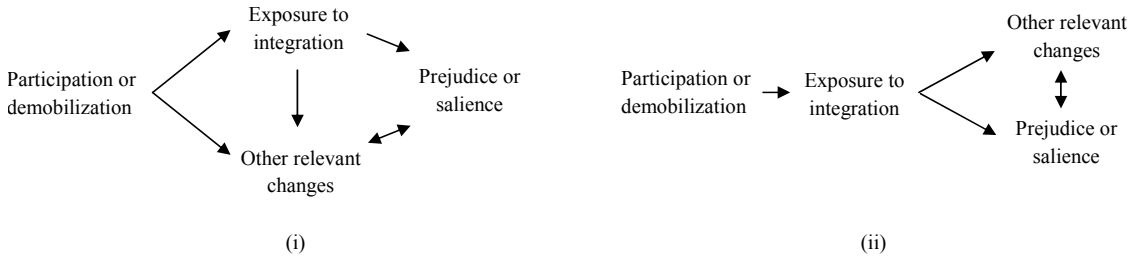
Standard errors in parentheses

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

(With enumerator fixed effects)

H.1 Checking exclusion restrictions

Figure 7: **Alternative causal pathways relating participation/demobilization to prejudice and salience**



Graph (i) on the left shows a causal pathway where effects other than exposure to quota-based integration may affect prejudice or salience. Graph (ii) on the right shows a causal pathway where exposure to integration is a primary effect of participation, and prejudice, salience, and other relevant changes follow from that.

The figure displays graphs of two causal pathways. Graph (i) on the left displays a causal pathway showing that participation versus demobilization may cause changes in other relevant variables that ultimately affect expressions of ethnic prejudice or salience. It may be that it is these other relevant changes that are important, and that the pathway that flows through "exposure to integration" is of little importance. For example, participation in the military may cause exposure to military norms or training that would mute expressions of prejudice or salience even were there no exposure to integration. Alas, the current data do not allow me to assess this possibility. Alternatively, demobilization may result in changes to one's material well-being or psychological state that heighten expressions of prejudice or salience, in which case our "optimistic" interpretation of the findings above may be invalid. This is something that I can study to a certain extent, and I do so below. Graph (ii) on the right displays an alternative causal pathway. Here, exposure to integration is the primary thing that determines whether military participants will differ from their demobilized

counterparts in their expressions of ethnic prejudice or salience. It may also be the case that such exposure affects other outcomes, but this is of no consequence for our interpretation. If the latter graph accurately characterizes what is actually happening, then the "optimistic" interpretation of the findings thus far is valid. The latter graph entails an "exclusion restriction," whereby there are no pathways circumventing "exposure to integration" that link participation/demobilization to expressions of ethnic prejudice or salience.

To test for alternative pathways, I use responses from the survey to a question of whether respondents considered their current economic conditions to be very bad, bad, good, or very good. Given that there were very few responses in either the very bad or very good categories (about 6% of responses in total), I constructed a binary variable that distinguished very bad and bad responses from very good and good responses. I applied the same specification as used above for the analysis of salience and the placebo checks. The estimates show no substantial differences at the cut-point in these perceptions (Table 12 in the Appendix). The survey data contain demobilized soldiers' responses to a question asking about whether they think things are better or worse for them as compared to their counterparts who remain in the military. Looking only at the responses of demobilized soldiers within the window of 45-50 years of age (within 5 years of the cut-point), 56 out of the 61 demobilized soldiers in this subgroup indicated "worse" (data were missing for 3 of the respondents), suggesting some bitterness among this group. This question was only asked of demobilized soldiers, however, so I cannot use it to construct a test for exclusion restriction violations. That said, members of the military and demobilized soldiers alike respond frequently that their current economic conditions are "bad" or "very bad." Limiting the analysis again to respon-

dents within 5-years of the cut-point, 50% of military respondents indicated that their economic conditions were "bad" or "very bad," while the percentage was 58% for demobilized.[26] One could chalk up demobilized soldiers' apparent bitterness about not being the military to general dissatisfaction with their economic conditions—a dissatisfaction that is also strongly felt among current soldiers.

To assess possible exclusion violations due to changes in material conditions, I estimated changes in the natural log of (self-reported) monthly income at the cut-point, again using the specification from the analysis of salience and placebo outcomes.[27] Income is an interesting outcome to examine, in this case, because there is no reason to believe that it would be affected by exposure to ethnic integration within the military. Thus, any differences that we measure must be due to income shocks associated with being demobilized versus remaining in the military. The estimates show no significant shock at the cut-point (Table 13 in the Appendix). This is to be expected for reasons specific to the Burundi case. Demobilized soldiers from the national army in Burundi were afforded a combination of pension benefits and "reintegration" assistance. An income allowance was provided so as maintain a subsistence level comparable to that of military members for 2 years after being discharged. In addition, a World Bank supported reintegration program provided demobilized soldiers with financial capital, start-up materials, and training for establishing a civilian livelihood.[28]

[26] A Chi-square test fails to reject the null at 33% significance.

[27] The rate of missingness was about 5% overall for the income measure, and so I simply omit those observations

[28] Gilligan et al (2012) study the impact of this assistance program on the economic and political reintegration of former rebel soldiers. Former national army members qualified for this same assistance as well as additional perks due from the national army pension scheme.

Table 12: Effects of integration on perception that "economic conditions are worse"

	(4-year bandwidth) Neg. ec. perc.	(5-year bandwidth) Neg. ec. perc.	(10-year bandwidth) Neg. ec. perc.
Integrated	-0.16	-0.08	-0.07
	(0.33)	(0.26)	(0.16)
Age-45.5	0.06	0.08*	0.01
	(0.07)	(0.04)	(0.02)
(Age<45.5)X(Age-45.5)	-0.13	-0.14**	-0.01
	(0.08)	(0.05)	(0.02)
Non-coeth. pair	-0.11	-0.18*	-0.13*
	(0.11)	(0.09)	(0.07)
Constant	0.50**	0.48***	0.60***
	(0.21)	(0.17)	(0.11)
Observations	121	138	236

Standard errors in parentheses

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

Table 13: Effects of integration on monthly income (Burundian Francs/month, log scale)

	(5-year bandwidth) Log(income+1)	(10-year bandwidth) Log(income+1)
Integrated	0.42	0.22
	(0.45)	(0.45)
Age-45.5	-0.04	-0.09
	(0.09)	(0.12)
(Age<45.5)X(Age-45.5)	0.04	0.06
	(0.08)	(0.13)
$(\text{Age-45.5})^2$		0.00
		(0.01)
(Age<45.5)X$(\text{Age-45.5})^2$		-0.00
		(0.02)
Constant	10.38***	10.51***
	(0.29)	(0.30)
Observations	147	248

Standard errors in parentheses

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

Table 14: Effects of integration on prejudice, controlling for economic outcomes

	(4-year bandwidth) Non-resp. index	(5-year bandwidth) Non-resp. index	(10-year bandwidth) Non-resp. index
Integrated	0.07	0.31	-0.10
	(0.82)	(0.59)	(0.45)
IntegratedXNon-coeth.	-0.66	-0.70*	-0.36
	(0.46)	(0.38)	(0.35)
Non-coeth. pair	0.95***	0.98***	0.84***
	(0.29)	(0.27)	(0.23)
Age-45.5	-0.06	0.03	-0.08
	(0.14)	(0.12)	(0.16)
(Age<45.5)X(Age-45.5)	0.04	-0.05	-0.02
	(0.13)	(0.12)	(0.19)
Neg. ec. perc.	0.10	0.16	-0.05
	(0.13)	(0.12)	(0.10)
Log(income+1)	0.20*	0.15	0.09
	(0.11)	(0.10)	(0.08)
$(\text{Age-45.5})^2$			0.01
			(0.02)
$(\text{Age}<45.5)\text{X}(\text{Age-45.5})^2$			-0.02
			(0.02)
Constant	-2.29**	-2.10**	-1.05
	(0.90)	(0.88)	(0.69)
Observations	109	126	221

Standard errors in parentheses

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

Table 15: Effects of integration on ethnic salience, controlling for economic outcomes

	(4-year bandwidth) Eth. salience	(5-year bandwidth) Eth. salience	(10-year bandwidth) Eth. salience
Integrated	-0.39	-0.02	-0.19
	(0.84)	(0.62)	(0.36)
Age-45.5	-0.04	0.03	0.06*
	(0.19)	(0.12)	(0.04)
(Age<45.5)X(Age-45.5)	-0.00	0.04	-0.08*
	(0.21)	(0.17)	(0.04)
Neg. ec. perc.	0.02	0.11	0.03
	(0.23)	(0.19)	(0.14)
Log(income+1)	-0.09	-0.06	-0.01
	(0.16)	(0.15)	(0.11)
Constant	1.14	0.64	0.08
	(1.58)	(1.47)	(1.10)
Observations	99	114	202

Standard errors in parentheses

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