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Radical Religious Rule and Human Capital: Evidence from the Taliban Control in Afghanistan (1996-2001)

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Discussion Paper No. 2023-01

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January 2023‡

Abstract

This paper estimates empirically in a robust way the effect of a radical religious rule, namely the Taliban control in most of the Afghan provinces between 1996 and 2001, on human capital accumulation. Human capital is proxied by three standard educational attainment variables, which are considered separately for women and men, as the Taliban rule particularly banned girls from schools after the age of 8 years. We use data for all provinces of Afghanistan, where two were not under Taliban rule in 1996-2001, and apply a difference-in-differences methodology, followed by a technique akin to an event study that goes deeper into some refinements. Our key contribution is to uncover that the negative human capital accumulation effect is mostly generated in the early childhood of women, and – less so – men: more precisely, at the start of schooling. Those girls who missed out on the chance of beginning education around the age of turning 6 years because of the Taliban ban were considerably disadvantaged in the long run. We quantify this "scarring" damage to be of the order of nearly 50% reduction in the mean value of their years of schooling, literacy probability and primary school completion probability, compared to women in the control provinces that were not under Taliban rule in 1996-2001. The policy relevance of our results is huge and immediate, in Afghanistan more directly where the Taliban returned to power in August 2021, forbid education of girls above sixth grade in March 2022 and banned women from all universities in December 2022, but also in many countries where radical religious doctrines still deprive many millions of children of their basic human right to education and, hence, better career and life prospects.

Keywords: radical religious rule, Taliban control in Afghanistan (1996-2001), subjugation of women, human capital accumulation, primary schooling, educational attainment
JEL codes: J16, I2, Z12, D1

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‡We are particularly indebted to Dominic Rohner for valuable feedback on earlier drafts of this paper. We also thank Mark Casson, Bipasha Maity and Alireza Naghavi for useful comments on later versions. Any remaining errors or misinterpretations are our sole responsibility.

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1 Introduction

Education is one of the very basic human rights. It empowers people to invest in their human capital and attain skills and qualifications that can propel their career to higher earnings in the labor market and, hence, higher standards of living. Religion, via vertical and horizontal cultural transmission of social norms and their interplay with institutions (Galor, 2022) can play a decisive role in promoting or hindering education.[1] In turn, education has a great potential to preserve diversity, instill tolerance, and contribute to diminishing the probability of conflict, enhancing at the same time the occurrence of new ideas and technological progress (Ogaki and Mihailov, 2021). Similarly to religion, education and the related formation and transmission of beliefs and values in a society are also strongly influenced by the political system, e.g., Aslam, Farvaque and Mihailov (2020).

Most would readily agree that giving an equal opportunity to primary and secondary education for every child at pre-school and schooling age is, therefore, the bare minimum of a benevolent government's policy embracing seriously the goal of fairness and human development. Yet, some governments do not endorse and implement such a general guiding principle, succumbing to religious or other considerations, and often discriminating and disadvantaging women against men. The Taliban[2] takeover of control over most of Afghanistan during 1996-2001 is perhaps the best known and striking example of such policies across the modern world. This is in stark contrast with the situation in the country in the few decades prior to the coming to power of the Taliban, when – according, e.g., to Herzer (2001), p. 18 – "[...] Afghan women participated in their communities' social, political and cultural life. Fifty percent of the students and 60 percent of the teachers at Kabul University were women. Similarly, women constituted 70 percent of all school teachers. Forty percent of the doctors in Kabul were women as were 50 percent of the civil government workers. [...]". In contrast, while the country was under Taliban rule, women in particular were subjugated, excluded from public life and denied access to education or labor market positions outside their home (Rashid, 2010).[3] The ban for girls older than 8 years to go to school,[4] in accordance with the Taliban own radical interpretation of the *Sharia* law, has generally been blamed for the resulting collapse of the enrollment and educational attainment of women. Ramadurai (2012), among others, writes that "[...] [s]ince

[1] Fundamentally, history, geography and trade have shaped out the adoption and spread of religions over the centuries, e.g., Michalopoulos, Naghavi and Prarolo (2018).

[2] The Taliban is a radical religious and political group that governed Afghanistan from 1996 to 2001, and now again, starting in August 2021. For a historical account, see, e.g., the summary in Olesen (1996) or the details in Rashid (2010); for a narrower focus on women's plight during the Taliban rule, see, e.g., Telesetsky (1998), Ghasemi (1999) or Middleton (2001).

[3] Farhournand-Sims (2007), p. 655, quotes Ahmed Gosh (2003) to clarify that "the situation of women in Afghanistan cannot be categorized neatly into pre-Taliban and post-Taliban frameworks of analysis, but requires an understanding that there is a long history of women's subjugation in that country. Afghan women have been subject to patriarchal attitudes and traditional customs that have greatly affected their ability to participate in society and enjoy equal access to education, health, marriage rights, and equality before the law". Further on, p. 656, the same author acknowledges that "[t]he first efforts to advance women's rights were introduced by King Amanullah (1919-29) who was determined to modernize Afghanistan socially and economically. In a bold move, he reformed the 1921 family code in order to provide Afghan women with improved legal standing in the private sphere, while also commissioning the drafting of a new constitution (1923) that made generic reference to gender equality and made education compulsory for all Afghans." (p. 556).

[4] Yet, some girls have begun attending clandestine schools as a consequence: see, e.g., Unterhalter (2022).

1996, the Taliban regime has been the root cause for the demise of the Afghani educational system as evidenced by literacy rates of men and women. The literacy rate of Afghani women is resting at an appalling level of 2.8 percent, whereas the literacy rate of Afghani men is standing at a literacy rate of 18.7 percent." (pp. 82-83).

Since the end of the Taliban rule in 2001, the country has made decisive strides ahead to repair this deplorable situation. Indeed, UNESCO (2021) reports that whereas "Afghanistan has the youngest population in South and West Asia and is one of the poorest countries in the world" (p. 7), "[t]he Human Development Index (HDI) value increased from 0.35 in 2001 to 0.511 in 2019 and the mean years of schooling increased by 2.4 years[5]" (p. 7). In particular, "[t]he number of girls in primary school has increased from almost zero in 2001 to 2.5 million in 2018. Today 4 out of 10 students in primary education are girls." (ibid., p. 7). Moreover, "[t]he number of girls in higher education increased from around 5,000 in 2001 to around 90,000 in 2018." (ibid., p. 7). Finally, and as a consequence of this progress following the end of the Taliban rule in 2001, " [t]he female literacy rate almost doubled in a decade, from 17% in 2011 to 30% in 2018." (ibid., p. 7)

With the recent return of the Taliban to power in Afghanistan,[6] the international community has begun to worry again, especially for women and their rights. After the first year and a half since then, it appears that the Taliban are likely to go down a similar road as in their previous rule. Initially, in the few months since return to power, they tried to leave an impression that their policies were this time more organized and more progressive relative to the 1996-2001 spell in power.[7] In effect, no one can really be sure at present as to where the future of Afghanistan is heading. UNESCO (2021) has also expressed concern, no matter the acknowledgment of considerable achievements in the interim period since 2001: "Afghanistan has achieved appreciable progress over the past two decades yet the task of building an inclusive and equitable education system remains colossal and fears of regression and a looming humanitarian catastrophe run high since August 2021." (p. 8). A year after the return to power of the Taliban, the initial hopes seem to have vanished, as claimed by Mines and Jadoon (2022): "The Taliban promised to respect girls' education and women's rights, and to not allow the country to become a breeding ground for terrorism, as it had been in the Taliban's previous stint in government before the 2001 U.S. intervention. But for a year and a half after the fall of Kabul, the Taliban has failed to deliver on these promises and has gradually become more repressive as it tries to consolidate power in the country." And to confirm and increase such fears, the Taliban banned women from all Afghan universities on December 2022: according to Berger and George (2022), "A spokesman for the Ministry of Higher Education announced the

[5] Average number of completed years of education of a country's population aged 25 years and older, excluding years spent repeating individual grades.

[6] The Taliban regained political power over Afghanistan seizing the capital Kabul on 15 August 2021 after the withdrawal of the last US troops from the country.

[7] In contrast to the first Taliban rule, women are now, for instance, not forced to wear a *burkha* (i.e., head-to-toes veil) nor are they completely prohibited to work, if it can be ensured that they are segregated from men (Agence France Presse, 2022*a*). In practice, however, restrictions on employment and fear of reprimands effectively bar them from employment, particularly in public positions (Agence France Presse, 2022*a*). Moreover, in its quest for international recognition and humanitarian aid, the Taliban regime has to face demands for human rights guarantees, and discussions on the matter are ongoing (Keystone-ATS, 2022).

suspension, effective immediately and in place until further notice, in a statement released after a Taliban leadership meeting. [...] With the ban on university attendance, nearly all Afghan women above the age of 12 are now barred from formal education. The move is also expected to further restrict the ability of Afghan women to participate in the workforce and other aspects of public life." In any case, there might be lessons to be drawn from the past, and in this lies part of the motivation for the present paper.

While many aspects of the Taliban rule would be worth investigating and quantifying with regard to human development concerns, we have chosen to analyze its long-term impact on human capital accumulation because the prohibition of girls' education was not only unjust and devastating, but may also still prove persistent.[8] Moreover, boys' education might be endangered as well, since history reveals that the interdiction for female teachers to practice their profession also challenged boys' education during the 1996-2001 Taliban regime (Rashid, 2010). Stakes are thus high. First, prohibiting education is a clear violation of the universal human rights: see General Assembly of the United Nations (1948), art. 26; and, second, thereby lowering the country's human capital raises deep economic concerns. Indeed, the importance of human capital as *proximate*[9] determinant of growth is widely documented in economics (e.g., Mankiw, Romer and Weil, 1992; Gennaioli et al., 2013; Squicciarini and Voigtländer, 2015). Furthermore, the development and growth literature showed that institutions, violent conflicts and culture (including religion) are important *fundamental* determinants of growth (e.g., Acemoglu, Johnson and Robinson, 2001; Przeworski and Limongi, 1993; Tabellini, 2010; Akbulut-Yuksel and Yuksel, 2015; Rohner, Thoenig and Zilibotti, 2013; León, 2012; Becker and Woessmann, 2009), which intrinsically relate to the Taliban regime in Afghanistan. Our paper thus contributes to the literature on these *fundamental* determinants of growth in quantifying the long-term causal impact of a radical religious rule on human capital accumulation and labor market participation of both men and women.

In examining empirically the aforementioned key research question, our paper also relates to the general literature on the impact of conflicts on education (e.g., León, 2012; Chamarbagwala and Morán, 2011; Shemyakina, 2011; Justino, Leone and Salardi, 2014; Brown and Velásquez, 2017; Monteiro and Rocha, 2017; Brück, Di Maio and Miaari, 2019). It also belongs, more narrowly, to the literature on religion economics – for a survey, see Iyer (2016) – and in particular to the functioning of sects and radical religious militias. Indeed, the Taliban's radical practice of Islam clearly departs from the traditional Muslim faith by its extremism and lack of tolerance, hence categorizing them as a sect. Moreover, their use of violence also qualifies them as a radical religious militia. While the academic literature took interest in explaining seemingly

[8] After returning to power in 2021, the Taliban once again suspended female education. However, under pressure of the international community, they announced in mid-January 2022 that girls should be back to school as of late March 2022 (Al Jazeera, 2022). On 23 March 2022, a few hours after all girls' schools officially reopened, the Taliban administration backtracked and forbid education of girls above sixth grade again "until a plan was drawn up in accordance with Islamic law and Afghan culture" (Agence France Presse, 2022*b*; Reuters, 2022).

[9] To fix the terminology, let us remind that according to the more recent growth literature – see, e.g., the graduate textbook by Acemoglu (2009), pp. 109-112 – *proximate* determinants of growth are the well-established growth determinants typically entering neoclassical and modern economic growth models: physical capital, human capital and technology; *fundamental* determinants, on the other hand, are the deeper variables – such as history, geography, institutions, policy, culture, religion – that actually explain the accumulation of the *proximate* determinants of growth themselves, and thus ultimately growth.

irrational acts of zealotry, costly prohibitions, sacrifices and terrorism imposed by radical and violent religious groups (Berman, 2003; Berman and Laitin, 2008), only few papers attempt to quantify the socioeconomic damage of these acts for members and non-members of these radical groups (e.g., Bertoni et al., 2019; Ekhator-Mobayode et al., 2022; Alfano and Görlach, 2019; Stoelinga, 2022).

To the best of our knowledge, only two papers have analyzed the long-term impact of the Taliban rule in Afghanistan on socioeconomic variables. The first one, Noury and Speciale (2016), which shares many similarities with our study, focuses on the impact of the Taliban regime on women's education, labor market participation and fertility outcomes. They conduct a difference-in-differences analysis on cohorts of birth and provinces of residence using data from the 2007/2008 National Risk and Vulnerability Assessment Survey (NRVA), and find that one additional year of exposure to the Taliban regime while of school age reduces women's likelihood of completing basic education by about 2 percentage points (pp), literacy probability by about 3 pp, and total years of education by about 0.2 years. These authors report larger effects for women living in Pashtun and in rural areas and no statistically significant effect for men. In terms of work and fertility outcomes, Noury and Speciale (2016) observe that exposure to the Taliban rule while of school age reduces women's probability of employment outside the household, increases their probability of having an agricultural job within the household, lowers their age at first marriage and increases their total number of children. Their results are robust to controls for emigration and violence at the province level.

The second paper, Maity and Shukla (2022), uses data from the 2015 Afghanistan Demographic and Health Survey (DHS) to investigate the impact of the Taliban regime on women's age at first marriage and at first childbirth. Given the strict gender policies implemented by the Taliban, such as the education ban for girls older than 8 years and the obligation for women to be accompanied by a male relative or husband when traveling outside the home, the authors argue that parents might have seen marriage as a way to ensure the security and mobility of their daughters. Comparing, across ethnic groups, women old enough to already be married at the onset of the Taliban rule with younger women yet to enter the marriage market, Maity and Shukla (2022) find an increase of about 7 months in the age at first marriage of Tajik and Uzbek women (the movement politically opposed to the Taliban and known as Northern Alliance was mainly composed of Tajik and Uzbek Afghans) relative to other ethnic groups, notably the Pashtuns, the majority ethnicity among the Taliban. Hence, Maity and Shukla (2022) results indicate that women belonging to ethnic groups politically opposed to the Taliban were less likely to be negatively affected by the Taliban rule and its radical gender policies. The authors find analogous results regarding age at first childbirth.

It is worth noting that, contrary to most papers analyzing the socioeconomic impact of radical religious militias, the two papers discussed above, along with our paper, do not investigate the impact of radical religious militias' acts of violence, but rather the impact of their extreme institutions. Indeed, by not using as independent variable/treatment a measure of insurgency violence, but a measure of territorial occupation, one examines the impact of a differing institutional *regime* and associated norms on the local population. This approach is especially

relevant in the case of the Taliban, as they eventually ruled 90% of Afghanistan and established the Islamic Emirate of Afghanistan in 1996 (Rashid, 2010). In occupied territories, the Taliban could enforce their very own interpretation of the *Sharia* law (Rashid, 2010) and no alternative government law had weight. In other words, their radical norms were formally imposed on all and not only brought to the population informally through the influence of insurgency violence, terror and destruction. One should however still acknowledge the role violence played in the Taliban's politics and in Afghanistan, as the civil war was still raging and the Taliban's religious police typically violently enforced new norms of behavior.

In order to quantify the causal impact of the Taliban institutions on human capital accumulation and labor market participation, we also use individual data from the 2015 Afghanistan Demographic and Health Survey (DHS). We measure human capital employing three standard educational outcome variables: years of schooling completed, literacy, and primary school completion. Labor market participation in the long run is measured by an individual's probability to be working at the time of the survey. Additionally, we use the data provided in Noury and Speciale (2016) to determine the timing of Taliban control at the province level in Afghanistan between 1994 and 2001. During that period, the Taliban took Afghan provinces one by one and seized Kabul in 1996, thereby establishing the Islamic Emirate of Afghanistan. They implemented the "strictest interpretation of the *Sharia* law ever seen in the Muslim world", to quote Rashid (2010), p. 29, and were notably criticized by the international community for their extreme gender policies. We take advantage of the differences in Taliban control across provinces as if in a natural experiment. We thus compare, across provinces administered by the Taliban and the Northern Alliance, individuals who were too old at the Taliban's arrival in their province of residence for their education to be affected by the Taliban institutions with individuals of compulsory school age (6 to 14) or of preschool age (0 to 5) at the time.

We apply a difference-in-differences (DiD) methodology, followed by a technique akin to an event study that goes deeper into some refinements. When accounting for the number of years individuals were exposed to the Taliban regime, we find that, on average, one additional year of exposure to the Taliban rule while of compulsory school age decreases women's completed years of schooling by 0.08 years, literacy probability by 1.07 pp and primary school completion probability by 0.56 pp. Interestingly, we find that exposure to the Taliban rule while of preschool age has a much larger impact. One additional year of exposure while of preschool age decreases, on average, women's completed years of schooling by 0.49 years, literacy probability by 4.66 pp and primary school completion probability by 4.43 pp. These figures respectively represent decreases of 32%, 27% and 32% compared to the mean value of the corresponding variables in the control provinces. Furthermore, we confirm the finding that preschool-aged exposure to the Taliban rule is critical for later human capital accumulation in the treatment effect heterogeneity analysis. We observe clearly that for both men and women, the younger the individual at first exposure to the Taliban rule, the larger the negative impact on their education. For the youngest cohorts, this impact is enormous. Our main contribution to the literature is to uncover that the negative human capital accumulation effect of radical religious rule is mostly generated in the early childhood of women, and – less so – men: that is, in pre-school age and at the start

of schooling. Other studies have not considered pre-school age, so we introduce an important refinement. Namely, those girls who missed out on the chance of embarking on education around the age of turning 6 years because of the Taliban ban were considerably disadvantaged by that act, in a sort of long-term "scarring" effect: we quantify this damage to be of the order of nearly 50% reduction in the mean value of their years of schooling, literacy probability and primary school completion probability, compared to the control provinces that were not under Taliban regime in 1996-2001. The policy relevance of our results is huge and immediate, in Afghanistan more directly where the Taliban returned to rule in August 2021 and resumed their restrictive policies on women's education, but also in all countries where radical religious doctrines deprive many millions of children of their basic human right to education and, hence, better career and life prospects.

In light of the summarized empirical findings, our paper contributes directly to the literature on the critical importance of early childhood for later life socioeconomic outcomes (e.g., Garbarino and Kostelny, 1996; Kuterovac-Jagodić, 2003; Barenbaum, Ruchkin and Schwab-Stone, 2004; Gould, Lavy and Paserman, 2011; León, 2012; Heckman, Pinto and Savelyev, 2013; Couttenier et al., 2019). The results of our heterogeneity analysis however confirm that exposure to the Taliban rule between the ages of 0 and 14 has had no impact on the labor market participation of women in the long run, while it has slightly increased men's. Our results are shown to be robust to alternative estimation samples, varying school age definitions, different Taliban control timing and two-way clustering. Moreover, they remain robust when partially treated provinces (i.e., provinces that were only partially administered by the Taliban) are included in the control group.

The rest of this paper is organized as follows. Section 2 presents the data, while the identification strategies are explained in section 3. Section 4 reports the baseline results, as well as some robustness tests, section 5 suggests our key interpretations, and section 6 concludes. An online appendix overviews Afghanistan's history (in section A), the academic literature on radical religious groups and that on the impact of conflicts on education (in section B) and collects figures and tables with additional robustness checks (in section C).

2 Data

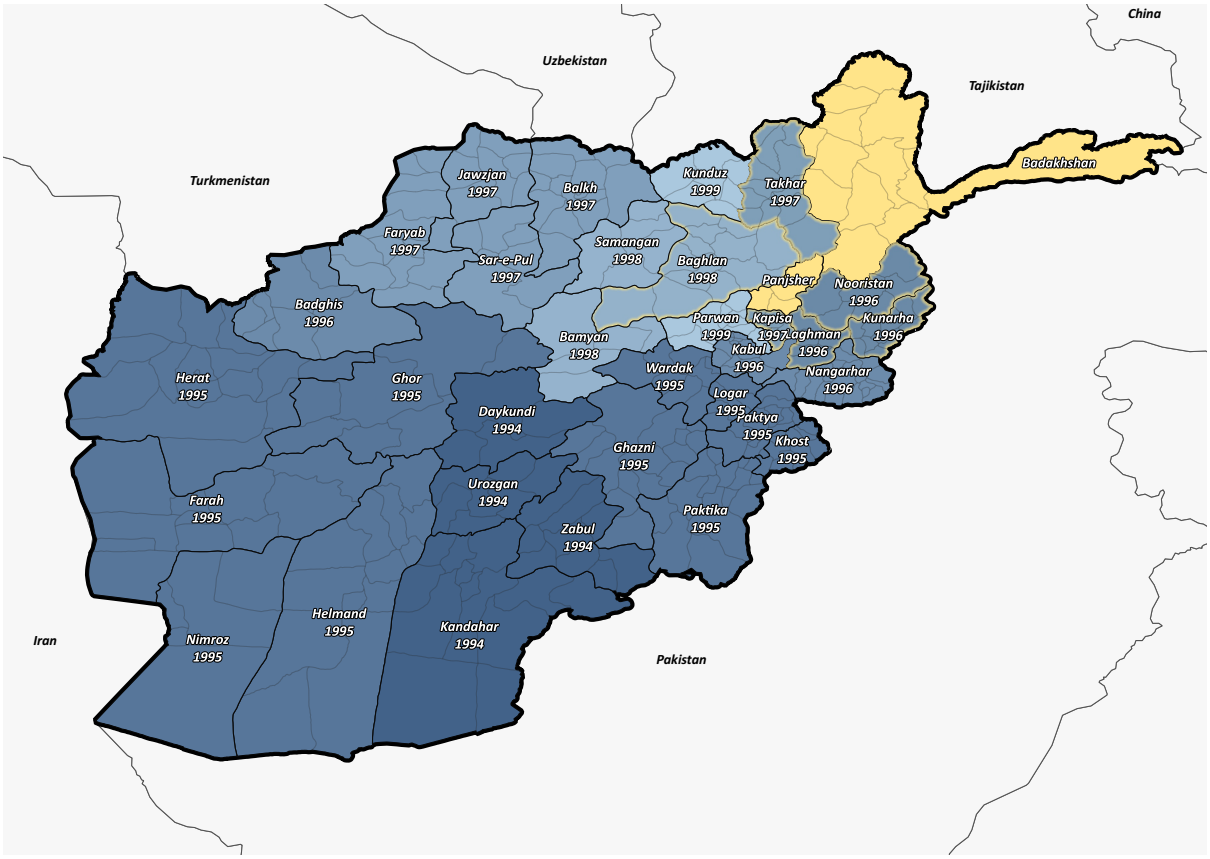
This section introduces the data we used and illustrates them with key descriptive statistics.

2.1 Taliban Control

The Taliban controlled some 90% of Afghanistan's territory at the peak of their rule, but they did not seize all provinces simultaneously – see, e.g., Rashid (2010). As to the timing of the Taliban occupation of the different provinces of Afghanistan, we employed the data in Noury and Speciale (2016). In turn, these authors collected and combined their data from Rashid (2010) and CNN (2001)[10] and in communicating with several experts. We report these data in Figure 1.

[10]This source refers to information provided by the US government.

Figure 1: Timing of the Taliban Occupation at the Province Level



Notes: This figure reports years in which the Taliban occupation began in the different Afghan provinces. It lasted in all "treated" provinces (in the sense of our methodology, described later on) until late 2001. Badakhshan and Panjsher, in yellow on the map, never were under Taliban control. The light yellow outlines on the borders of Takhar, Baghlan, Nooristan, Kunhara, Kapisa and Laghman indicate that these provinces were only partially administrated by the Taliban. Source: Noury and Speciale (2016) on the timing of Taliban occupation and Hijmans, University of California Berkeley and Museum of Vertebrate Zoology (2015*a*,*b*,*c*) on Afghanistan's administrative boundaries.

The first provinces seized by the Taliban were Kandahar, Zabul, Urozgan and Daykundi in 1994; followed by Helmand, Nimroz, Farah, Ghor, Herat, Khost, Paktika, Paktya, Logar, Wardak and Ghazni in 1995; Kabul, Nangarhar, Badghis, Kapisa, Laghman, Nooristan and Kunarha in 1996; Takhar, Balkh, Sar-e-Pul, Jawzjan and Faryab in 1997; Bamyan, Baghlan and Samangan in 1998; and Parwan and Kunduz in 1999. The Taliban rule in these provinces lasted until the end of 2001, when the US and NATO military forces launched *Operation Enduring Freedom* following the September 11 terror attacks.

2.2 2015 Afghanistan DHS Survey

To conduct our analysis, we use data from the 2015 Afghanistan Demographic and Health Survey. The Demographic and Health Surveys (DHS) Program is a USAID[11] funded program collaborating with government agencies in more than 90 countries worldwide to carry out stan-

[11] United States Agency for International Development.

dardized population surveys, thereby collecting key information on health and demographic indicators to improve policy decisions and research (USAID, 2021).

The 2015 Afghanistan DHS was conducted by the Central Statistics Organization (CSO) of Afghanistan and the Afghan Ministry of Public Health (MoPH) between 15 June 2015 and 23 February 2016[12] with the technical assistance of the Inner City Fund (ICF) and The United Nations Children's Fund (UNICEF) (Central Statistics Organization (CSO), Minitry of Public Health (MoPH) and ICF, 2017). This survey is the first *Standard DHS Survey* completed in Afghanistan, and remains the only one to date. It collects data for 24,395 households, 29,461 ever-married women aged 15 to 49 years and 10,760 ever-married men aged 15 and 49 years. The survey covers all 34 Afghan provinces and contains information on a large number of health and demographic variables, including age of respondent, province of residence, literacy, completed years of schooling and labor market participation. It also includes sample weights accounting for the probability that a specific individual is sampled. Using these weights allows the production of representative statistics at the national and provincial level, as well as for rural and urban areas (Central Statistics Organization (CSO), Minitry of Public Health (MoPH) and ICF, 2017).

2.3 Descriptive Statistics

Table 1 (to be found in appendix section C) reports descriptive statistics on educational outcomes, labor market participation and exposure to the Taliban rule of the survey respondents. The observation samples used for the construction of these statistics are the ones used in the baseline econometric analyses hereafter (from which results are reported in tables 2 through 5 and in Figure 3). They include ever-married women, respectively men, aged 20 to 49 at the time of the survey. Table 1 is broken down by provinces that ever were under Taliban rule (to be referred to as "treated" provinces from a methodological point of view) and those that never were (to be referred as "control" provinces). It also allows a comparison between men and women. All figures provided in our study are generated using sample weights.

[Insert Table 1 about here]

One can see that only 3.56% of the women and 3.08% of the men surveyed reside in provinces that never were under Taliban rule, i.e., Panjsher and Badakhshan. Hence, their exposure to the Taliban rule is invariably set to zero. The remaining people surveyed reside in provinces that eventually were taken under Taliban control. Note, however, that this table is not a direct comparison of people whose education was affected or not by the Taliban rule; rather, it just depicts the provinces that ever or never were under Taliban occupation. Consequently, not all people residing in treated provinces were treated, since some people were already out of school at the time the Taliban seized their province of residence. In fact, in our estimation sample, about 59% of women living in eventually treated provinces were of compulsory school age (6 to 14 years old) for at least one year under Taliban rule (and thus considered as treated for the purposes of our study) and about 23% of them were of preschool age (0 to 5 years old) for at

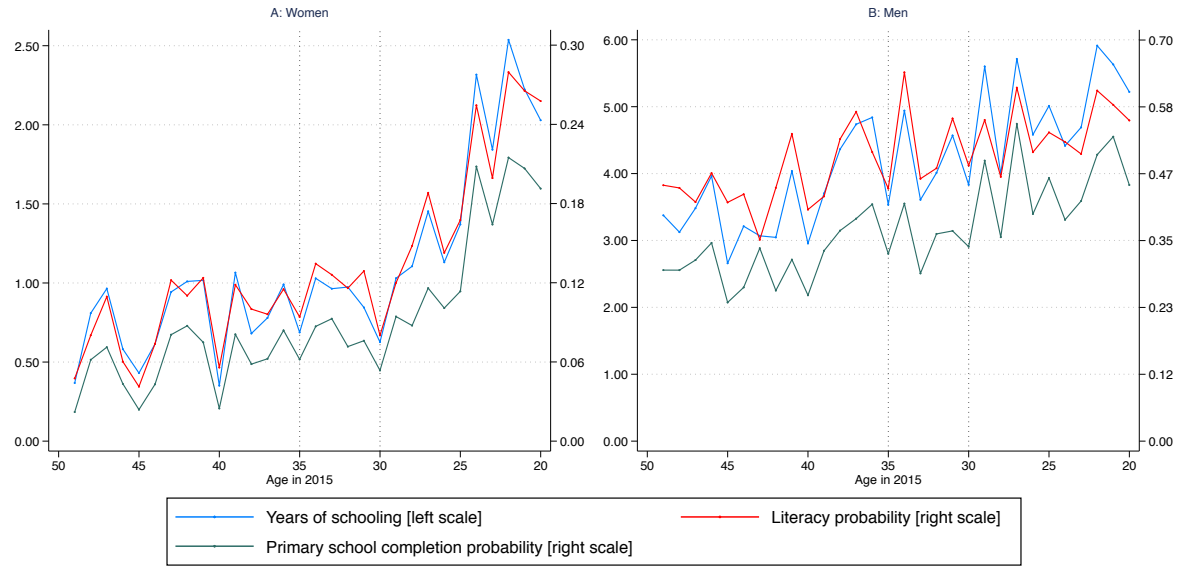
[12] Afghanistan does not use the Georgian calendar. On the Afghan calendar, the survey was conducted between Jawza 25, 1394 and Hout 4, 1394.

least one year under Taliban rule (and thus also considered as treated). These two treatments by age range are not mutually exclusive. For men, the respective two fractions amount to 51% and 13%. In our sample, women living in provinces that ever were under Taliban rule have on average 2.39 years of exposure to the Taliban rule while of compulsory school age (with a standard deviation of 2.49 years) and 0.74 years of exposure to the Taliban rule while of preschool age (with a standard deviation of 1.58 years). The corresponding average statistics for men are 2.21 years of compulsory school-aged exposure (with a standard deviation of 2.56 years) and 0.38 years of preschool-aged exposure (with a standard deviation of 1.14 years).

Moreover, Table 1 shows that human capital accumulation is generally low in Afghanistan, and that gender inequalities are important. While women in our sample complete on average 1.17 years of schooling, men complete on average 4.12 years. It also appears that, on average, women living in the control provinces (Panjsher and Badakhshan) complete 0.36 years of education more than women living in provinces that were under Taliban rule at some point. For men, the reverse is true. That is, men living in control provinces appear to complete on average 0.47 years of education less than men in treated provinces. The gender gap is also clear when looking at literacy and primary school completion rates. In our sample, 14% percent of women are literate, compared to 49% of men. Again, women residing in control provinces reveal a higher degree of literacy than women in treated provinces, while the opposite holds for men. Additionally, only 10% of women in our estimation sample completed primary school, versus 36% of men. As before, we observe that women in control provinces are on average more likely to complete primary school than in treated provinces, whereas for men this observation reverses.

While these descriptive statistics are instructive regarding the low levels of education in Afghanistan, it should also be kept in mind that they are averages over people aged 20 to 49 years in 2015. In fact, the education attainment of both men and women in Afghanistan increased over time, so that younger generations are typically more educated than older ones. This can be seen in Figure 2, which plots the mean values of the educational outcomes by age cohort for all provinces combined. To give very broad numbers, women aged 40 to 50 in our sample have between 0.5 and 1 year of schooling on average, a literacy probability oscillating around 10% and a primary school completion rate of about 5%. Younger women aged 20 to 25 on the other hand have between 2 to 2.5 years of schooling on average, a literacy probability around 25% and a primary school completion rate of about 20%. For men, the increase in human capital accumulation across cohorts is smoother than for women, for whom there is a sudden increase around the generation of people aged 20-25 in 2015. Men aged 40 to 50 in our sample have broadly between 3 and 4 years of schooling, a literacy probability oscillating around 43% and a primary school completion rate of about 30%. By contrast, younger men aged 20 to 25 have between 4.5 and 6 years of schooling on average, a literacy probability vacillating around 55% and a primary school completion rate of about 47%. While these remain only rough numbers, the trends clearly show that human capital in Afghanistan generally increased across age cohorts.

Figure 2: Education Across Age Cohorts (All Provinces Combined)



Notes: For each age cohort, the figure plots the mean value of individuals' completed years of schooling (blue line), primary school completion (green line) and literacy (red line). The mean value of the variable *completed years of schooling* is reported in years for each age cohort on the left scale of each panel, while the mean values of the variables *primary school completion* and *literacy* are reported in percentage points on the right scale of each panel. Note that the scales used for the two panels are not the same. The statistics are computed using sample weights and include people from all Afghan provinces. As such, they do not distinguish between provinces that ever were or never were under Taliban occupation, nor between treated and non-treated individuals. Note, however, that: all individuals aged 36 years or more in 2015 finished their compulsory schooling before the Taliban arrival and, hence, are untreated; all individuals aged 30 years or less in 2015 and living in ever treated provinces have been of compulsory schooling age for at least one year under Taliban rule and are thus considered as treated (while those living in control provinces are untreated); and that some individuals aged 31 to 35 years in 2015 may be treated depending on the timing of the Taliban occupation in their province of residence. These age cutoffs are depicted by the two dotted vertical lines. Source: DHS.

Differently from Figure 2, Figure 4 later on distinguishes between "treated" and "control" provinces when plotting the evolution of educational outcomes across birth cohorts.[13] For now, we note that Figure 4 additionally reveals that the divergence between treated and control provinces in the intensity of the human capital accumulation over time first arises for the generation of people aged approximately 30 years in 2015 (at least for women). These people were, depending on their province of residence, 9 to 14 years old when the Taliban occupation began locally. Theoretically, the oldest individuals whose education was affected by the Taliban rule (i.e., oldest treated individuals) are the ones who were in their last year of compulsory schooling at the time the Taliban occupation began locally, i.e., people who were 14 years old at the Taliban arrival in their province of residence. This corresponds to people aged 35 years in 2015 for provinces seized in 1994; 34 years in 2015 for provinces seized in 1995; 33 years in 2015 for provinces seized in 1996; 32 years in 2015 for provinces seized in 1997; 31 years in 2015 for provinces seized in 1998; and 30 years in 2015 for provinces seized in 1999. Hence, the timing of divergence between treated and control provinces appears to correspond, although possibly

[13] Figure 4 and its layout are more extensively discussed in section 4.4.

slightly delayed, to the timing of the Taliban control at the province level. Note also that people aged 20 years in 2015 were aged 6 years in 2001, the last year of the Taliban rule for all treated provinces. Therefore, 20 year-olds in 2015 living in treated provinces have exactly one year of compulsory school-aged exposure to the Taliban regime. In consideration of the above, all individuals in our sample aged 20 to 30 years in 2015 and living in ever treated provinces have at least one year of exposure to the Taliban regime while of compulsory school age and are therefore considered as treated. Depending on their province of residence, some individuals aged 31 to 35 years old in 2015 are also treated.

Coming back to Figure 2, it is not surprising that educational outcomes in Afghanistan were particularly low already before the Taliban arrival, i.e., for people aged above 30-35 years in 2015. Indeed – as, e.g., Rashid (2010) highlights – the country had suffered more than 20 years of civil war before the emergence of this radical religious group, creating unfavorable socioeconomic conditions for human capital accumulation. By the mid-1990s, the country had "one of the lowest rated indices for the human condition in the world" (Rashid, 2010, p. 107).[14] Hence, the same author argues that "the Taliban [...] only worsened an ongoing crisis" (ibid.). It is still true nowadays – as was briefly discussed – that Afghanistan is one of the least developed countries in the world. This is revealed, for example, by its human development index (HDI) for 2019, which equaled 0.511 and ranked 169 out of 189 countries (UNDP, 2020).

Finally, with regards to labor market participation in 2015, Table 1 shows that women in Afghanistan do not work much, as only 13% of women in our sample declare any job activity. In contrast, almost all sampled men worked in 2015, with a weighted sample average amounting to 97%. Note that while there is no difference on average in labor market participation rate of men at the time of the survey between treated and control provinces, there is an important one for women. In 2015, only about 2% of sampled women worked in control provinces, while about 14% worked in treated provinces.

3 Methodology

This section outlines the two complementary methodological approaches that we employ.

3.1 Generalized Difference-in-Differences

In order to study the long-term impact of exposure to the Taliban rule on educational and labor market outcomes of both men and women, we first make use of a difference-in-differences strategy. Exploiting variation in exposure to the Taliban rule across birth cohorts and provinces of residence, we compare – between provinces which were and were not administered by the Taliban – people who were of school and/or preschool age during the Taliban rule with people who were too old for their human capital to be affected by the Taliban's female education ban and the related diverse schooling discouragements and impediments. The difference in outcomes

[14] Note that the figures for illiteracy reported by Rashid (2010) are coherent with what we find in our sample. He reports that before the Taliban appeared in the mid-1990s, about 90% of women and 60% of men were illiterate (ibid.). This is consistent with what we observe in our sample for people aged more than 35 at the time of the survey (and who thus were, approximately, out of school at the arrival of the Taliban).

across "treated" and "control" provinces can also be due to a reduced supply of education (less teachers) or to the increased risk of becoming a victim of violence on the way to school.

3.1.1 Without Treatment Intensity

As a first step and since the timing of the "treatment" (i.e., Taliban being in power) varies across provinces, we make use of a simple empirical model, namely *generalized* difference-in-differences. Its baseline equation reads as follows:

$$\begin{aligned} Outcome_{ibd} = \alpha_b + \delta_d + \beta_1(TalibanControl_p * SchoolAged_{bp}) \\ + \beta_2(TalibanControl_p * PreschoolAged_{bp}) + \mathbf{X}_{ibd}\gamma + \epsilon_{ibd} \end{aligned} \tag{1}$$

where α_b and δ_d are, respectively, birth cohorts b and districts d fixed effects;[15] $TalibanControl_p$ is an indicator variable taking the value of one if the province p ever was under Taliban rule; $SchoolAged_{bp}$ is an indicator variable taking the value of one if individuals of the birth cohort b in province p were of compulsory school age (6 to 14 years old) for at least one year during the Taliban regime; $PreschoolAged_{bp}$ is an indicator variable taking the value of one if individuals of the birth cohort b in province p were of preschool age (0 to 5 years old) for at least one year during the Taliban regime;[16] and $\mathbf{X}_{ibd}$ is a vector of potential individual i covariates varying both across cohorts b and districts d. It includes ethnicity and language dummies,[17] as well as an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Errors are clustered at the province level and sample weights are used in all regressions.[18]

The coefficients of interest are β_1 and β_2. If the identification assumptions hold,[19] β_1 captures the average causal impact of having been of compulsory school age (6 to 14 years old) for at least a year under Taliban rule on the outcome variables considered; and β_2, similarly, the average causal effect of having been of preschool age (0 to 5 years old).

The variable $Outcome_{ibd}$ refers to each of the dependent variables we examine separately. We look at (1) number of completed years of education, (2) literacy, (3) probability of completing primary school, i.e., 6 years of schooling, and (4) labor market participation probability as reported at the time of the survey. The number of completed years of education is given in the 2015 Afghanistan DHS. Whether someone is literate is measured in the 2015 Afghanistan DHS by presenting to respondents a short piece of text and observing whether they can read it,

[15] The advantage of using districts fixed effects instead of provinces fixed effects, although the treatment is at the province level, is that in addition to capturing whether a province (i.e., all districts of this province) was treated or not, it also controls for finer differences between regions that are invariant across birth cohorts than provinces fixed effects would.

[16] Each individual's age at the time of the Taliban regime was computed based on both their age at the time of the survey and the timing of the Taliban rule at the province level. Tables A1 to A3 in the appendix illustrate these computations. Given the above, both $SchoolAged_{bp}$ and $PreschoolAged_{bp}$ account for the differing timing of Taliban occupation across provinces. In provinces that never were controlled by the Taliban, a timing equivalent to the official Taliban rule, i.e., 1996 to 2001, is artificially used (but the interaction term is always equal to zero).

[17] Even if ethnicity and language distributions are likely stable in a given district throughout time, and thus accounted for by districts fixed effects, we still include them in the eventuality that within a district, the proportion of people *surveyed* of a given ethnicity or speaking a given language varies across birth cohorts.

[18] Unweighted baseline results are also available in the appendix.

[19] See discussions in sections 4.4 and 5.2

partially read it, or not read it at all. We transformed this variable into an indicator variable taking the value of one if the respondent was able to read, optimistically including people partially able to read as literate. For primary school completion, we then created a dummy variable taking the value of one if the individual completed 6 or more years of schooling. Finally, we computed the labor market participation probability by creating an indicator variable taking the value of one if the respondent reported any type of work activity in 2015.

3.1.2 With Treatment Intensity

Since we expect people being exposed for longer periods of time to the Taliban rule to be more strongly affected by the Taliban norms that were imposed on them, as a second step in our generalized DiD methodology we conduct the same analysis, but now accounting for the intensity of treatment. We proxy the treatment intensity by the number of years individuals were of compulsory school age and/or preschool age during the Taliban regime. Individuals' number of years of exposure can be computed based on their birth year and province of residence.[20] The baseline equation now reads as follows:

$$\begin{aligned} Outcome_{ibd} = \alpha_b + \delta_d + \beta_1(TalibanControl_p * YearsSchoolAgedExposure_{bp}) \\ + \beta_2(TalibanControl_p * YearsPreschoolAgedExposure_{bp}) + \mathbf{X}_{ibd}\gamma + \epsilon_{ibd} \end{aligned} \tag{2}$$

where, as before, α_b and δ_d are, respectively, birth cohorts b fixed effects and districts d fixed effects; $\mathbf{X}_{ibd}$ is a vector of potential covariates; and $TalibanControl_p$ is an indicator variable equal to one if the province p ever was under Taliban rule. $Outcome_{ibd}$ refers to the four outcome variables described above. This empirical model still clusters errors at the province level and uses sample weights. $YearsSchoolAgedExposure_{bp}$ is a variable ranging from 0 to 8 and indicating the number of years an individual was between the ages of 6 and 14 at the time of the Taliban regime in their province of residence; and $YearsPreschoolAgedExposure_{bp}$ is a variable ranging from 0 to 6 and, similarly, indicating the number of years an individual was between the ages of 0 and 5 during that time.

As before, β_1 and β_2 are the coefficients of interest. If the identification assumptions hold,[21] β_1 captures the average causal impact of being exposed for one additional year while of compulsory school age (6 to 14 years old) to the Taliban rule on the outcome variables considered; and β_2 captures, similarly, the average causal impact of being exposed for one additional year while of preschool age (0 to 5 years old).

We estimate both baseline models (1) and (2) separately for men and women. Moreover, we restrict the estimation sample in the baseline analysis to individuals aged 20 years or more at the time of the survey. The resulting estimation samples count respectively 27,632 ever-married women aged 20 to 49 years and 10,602 ever-married men aged 20 to 49 years at the time of the survey. There are several reasons for excluding individuals younger than 20 years of age from the analysis, the main one being that these individuals might not be definitively done

[20] See again Tables A1 to A3 in the appendix.
[21] See footnote 19.

with their schooling (accounting for a general definition of schooling age). Since one of the dependent variables of interest is total years of schooling, we want to include only people who have completed their education spell in life. Other reasons are that the number of observations for ages below 20 years is smaller (especially for men), and that information for people aged 18 years or below is missing for several provinces.[22]

There are several advantages in using such a difference-in-differences estimator, independently of whether one accounts for treatment intensity or not. By conditioning on districts fixed effects, the model removes biases due to enduring differences across districts such as geography, regional culture, ethnicity – which, as highlighted by Rashid (2010), plays a very important role in Afghanistan – and distance to nearest city. Moreover, including birth cohorts fixed effects removes biases that would otherwise arise from the comparison between younger and older individuals, typically from the fact that educational attainment increased across generations in every province (see Figure 2). However, for the DiD methodology to be valid and correctly identify the average treatment effect on the treated (ATT), the common trends assumption and a few other assumptions have to hold. As mentioned, these are discussed in sections 4.4 and 5.2.

3.2 Treatment Effect Heterogeneity by Age at First Exposure to the Taliban Rule

To expand and complement our DiD analysis outlined above, we next use a strategy akin to an event study in order to investigate finer heterogeneous effects by age at the Taliban arrival in one's province of residence. The corresponding baseline equation now reads:

$$Outcome_{ibd} = \alpha_b + \delta_d + \sum_{\substack{j=0 \\ j \neq 15}}^{30} \beta_j AgeAtTalibanArrival_{bpj} + \mathbf{X}_{ibd}\gamma + \epsilon_{ibd} \tag{3}$$

where $Outcome_{ibd}$ describes the same four dependent variables as before; α_b and δ_d still are, respectively, birth cohorts b and districts d fixed effects, and $\mathbf{X}_{ibd}$ still is our vector of potential covariates. As before, errors are clustered at the province level and sample weights are used. $\forall$ $j \in [1, 29]$, $AgeAtTalibanArrival_{bpj}$ is thus an indicator variable equal to one if individuals of cohort b in province p were aged j at the time the Taliban took control over their province of residence.[23] Schmidheiny and Siegloch (2020) highlight the importance of binning the endpoints for identification in an event study. Therefore, $AgeAtTalibanArrival_{bp30}$ is an indicator variable equal to one if individuals of cohort b in province p were aged 30 or more at the time the Taliban arrived in their province of residence. Correspondingly, $AgeAtTalibanArrival_{bp0}$ is an indicator variable equal to one if individuals of cohort b in province p were aged 0 or were yet to be born at the time the Taliban arrived in their province of residence. Table A1 in the appendix illustrates the apportionment of the $AgeAtTalibanArrival_{bpj}$ leads/lags at the age-province level. Still

[22] Table A1 in the appendix reports the number of respondents of each age in each province.

[23] For provinces that never were controlled by the Taliban, $AgeAtTalibanArrival_{bpj} = 0$, $\forall j$ $[0, 30]$.

following Schmidheiny and Siegloch (2020), the first lag (i.e., $AgeAtTalibanArrival_{bp15}$) is dropped and age 15 at the Taliban arrival is thus used as reference.[24] The effect window [0 to 30] is set such as to be the largest possible while still paying attention to having a relatively large number of observations at the endpoints.[25]

As before, the analysis is conducted separately for men and women and the β_j are the coefficients of interest. Each β_j reports the average difference in the considered outcome between treated and nontreated individuals aged j at the beginning of the Taliban rule, with respect to the base (age at Taliban arrival = 15). In other words, the β_j capture the effect of exposure to the Taliban rule when this exposure starts at age j.

4 Results

This section reports our baseline estimates resulting from the application of the methodologies discussed in the preceding section. Supporting evidence for the common trends assumption is also presented, as well as different robustness tests.

4.1 Baseline Difference-in-Differences Results

Table 2 reports the baseline results of the difference-in-differences analysis without treatment intensity, i.e., the results of the estimation of eq. (1). Panel A of the table displays results for women and Panel B for men. The dependent variable of interest is completed years of schooling in column (1); literacy probability in column (2); primary school completion probability in column (3); and labor market participation at the time of the survey in column (4).

[Insert Table 2 about here]

The estimated coefficients of the interaction terms *Taliban control x School-aged* ($\hat{\beta}_1$) and *Taliban control x Preschool-aged* ($\hat{\beta}_2$) quantify the average effect on each of the four alternative dependent variables of having been respectively of compulsory school age (6 to 14 years old) or preschool age (0 to 5 years old) under Taliban rule for at least one year. For women, both of these treatments do not have a statistically significant effect on the four investigated outcomes. In other words, we observe no impact of the Taliban rule on women's education and labor market participation when ignoring the intensity of treatment, i.e., the number of years a woman was exposed to the Taliban regime while of preschool and/or compulsory school age.

For men as well, most coefficients are not statistically different from zero. We observe, however, a significant effect of compulsory school-aged exposure on men's primary school completion probability. On average, this probability decreases by 6.49 percentage points (pp) for men who were exposed to the Taliban regime for at least one year between their 6 and 14 years

[24] In a standard event study, lags are periods preceding the treatment and leads periods following it. Since the people aged 14 at the time the Taliban arrived in their province of residence are the "just treated" individuals, $AgeAtTalibanArrival_{bp14}$ can be compared to the treatment timing. Thus, $AgeAtTalibanArrival_{bp15}$ is the first lag, $AgeAtTalibanArrival_{bp16}$ is the second lag, etc. Similarly, $AgeAtTalibanArrival_{bp13}$ is the first lead, $AgeAtTalibanArrival_{bp12}$ is the second lead, etc. Dropping the indicator for the first lag normalizes β_{15} to zero. It hence becomes a baseline.

[25] See Table A3 in the appendix. Round numbers are also chosen as endpoints for aesthetics.

of age compared to men who were not. We also observe a decline of 0.58 years of schooling, on average, for men exposed to the Taliban regime while of compulsory school age as compared to men who were not.

[Insert Table 3 about here]

The methodological refinement of accounting for treatment intensity applied next turns out to be crucial, and leads us to come to a different, expected, intuitive and well-supported conclusion. Table 3, formatted like Table 2, reports the baseline results of the difference-in-differences analysis with treatment intensity, i.e., the results of the estimation of eq. (2). Panel A now shows that both one additional year of compulsory school-aged exposure ($\hat{\beta}_1$) and one additional year of preschool-aged exposure ($\hat{\beta}_2$) have a statistically significant negative effect on all three women's educational outcomes considered. Moreover, the effect of one additional year of preschool-aged exposure is of much larger magnitude than that of one additional year of compulsory school-aged exposure. We find that, everything else held constant, one additional year of exposure to the Taliban regime between the ages of 6 and 14 reduces women's completed years of education by on average 0.08 years, women's literacy probability by on average 1.07 pp, and women's primary school completion probability by on average 0.56 pp. These effects are important in terms of their economic meaning since they respectively represent reductions of 5%, 6% and 4% compared to women's average number of completed years of schooling, women's literacy rate and women's primary school completion rate in the control provinces. But as already stated, the negative impact of one additional year of exposure to the Taliban institutions between the ages of 0 and 5 is even larger. We find that, on average and everything else held constant, one additional year of preschool-aged exposure decreases women's completed years of schooling by 0.49 years, women's literacy probability by 4.66 pp and women's primary school completion probability by 4.43 pp. These effects are more important as well in terms of economic interpretation and amount to decreases of respectively 32%, 27%, and 32% compared to the mean value of the corresponding outcome variables for women in the control provinces. Given that the average woman in provinces that eventually were under Taliban occupation has, in our estimation sample, 0.74 years of preschool-aged exposure to the Taliban rule and 2.39 years of compulsory school-aged exposure to it, she completes 0.55 years (slightly more than 6 months) of education less, has a literacy probability lowered by 6.01 pp and a primary school completion probability reduced by 4.63 pp compared to control women. Concerning labor market participation in the long run, we find that one additional year of exposure to the Taliban regime while of preschool and/or compulsory school age has no significant impact for women.

Turning to men, we first observe no statistically significant effect of either one additional year of preschool-aged exposure or compulsory school-aged exposure to the Taliban regime on total years of schooling and on literacy probability. By contrast, we find that one additional year of preschool-aged exposure to the Taliban rule significantly decreases men's primary school completion probability by 3.53 pp on average. This represents a sizable decline of 12% compared to the male primary school completion rate in the control provinces. Given that the average

treated man in our estimation sample has 0.38 years of preschool-aged exposure to the Taliban regime, his primary school completion probability is lowered by 1.34 pp compared to men who were not under Taliban regime in their early childhood. In terms of labor market participation, we find that one additional year of Taliban exposure while of compulsory school age increases men's probability to participate in the labor market in the long run by 0.63 pp on average. This is a small effect in economic terms, however, as it translates into a 0.66% increase compared to the labor market participation rate of men in control provinces.

Comparing the results of tables 2 and 3 leads after all to intuitive conclusions, even if they may seem contrasting at first. We remark that while exposure to Taliban institutions appears to have no statistically significant effect on women's educational attainments when ignoring treatment intensity, it greatly matters when accounting for the number of years a woman was exposed to the Taliban regime. In other words, we revealed that exposure to the Taliban rule had no effect *on average* on women's education in our sample, but that it had an important *marginal* effect. Moreover, if the results for men are not as controversial, we still observe differences between the *average* effect of Taliban exposure on education estimated in Table 2 and the *marginal* effect of one additional year of exposure estimated in Table 3. One explanation that could reconcile these findings is that the effect of the Taliban regime on educational and labor market outcomes is heterogeneous across different groups of people, such that the average effect for women is inconclusive. People indeed differ in terms of exposure length (which, given the significant marginal effects estimated in Table 3, matters), but also in the timing of exposure in their life. The fact that the coefficients of exposure while of preschool age are larger than those of compulsory school-aged exposure is a first hint that the age individuals were under Taliban occupation might be critical to understanding the impact of the regime on Afghan men's and women's education. Consequently, this dimension of our empirical analysis is investigated with more precision in the next section.

4.2 Treatment Effect Heterogeneity by Age at First Exposure to the Taliban Rule

To disentangle further what we already reported, we here embark on a strategy akin to an event study. It allows to examine the heterogeneity of the treatment effect by age at the beginning of one's exposure to the Taliban regime. The results of this aspect of our analysis, which corresponds to the estimation of eq. (3), are reported visually in Figure 3. The same results are also presented under a different format in Table 4.

[Insert Figure 3 and Table 4 about here]

Panels A1-4 in Figure 3 display the results for women, while Panels B1-4 depict them for men (in Table 4 respectively, columns (1) to (4) present the results for women and columns (5) to (8) for men). Panels A1 and B1 in Figure 3 (columns (1) and (5) in Table 4) report the results with completed years of schooling as dependent variable; Panels A2 and B2 (columns (2) and (6)) report the results with literacy as dependent variable; Panels A3 and B3 (columns (3) and (7)) report the results with primary school completion probability as dependent variable;

and Panels A4 and B4 (columns (4) and (8)) report the results with labor market participation in 2015 as dependent variable.

Figure 3 plots the coefficients ($\hat{\beta}_j$) of the corresponding $AgeAtTalibanArrival_{pbj}$ leads/lags. They quantify the average difference in educational or labor market outcomes, respectively to the base (age at Taliban arrival = 15),[26] between people for whom the Taliban rule started in their province of residence as they were aged j, and similarly aged people living in the control provinces. The light blue bars around the coefficients report their 95% confidence interval and the black vertical line on the graph highlights the "just treated" cohort, i.e., people who were 14 years old and thus were in their last year of compulsory schooling at the start of the Taliban rule in their province of residence.

The first thing to note is that we globally observe no significant effect of exposure to the Taliban regime on educational and labor market outcomes of people for whom this exposure started when they were already older than 14 years of age, i.e., on the left of the black vertical line. This is supporting evidence for the common trends assumption and will be further discussed in section 4.4. Admittedly, however, this claim is stronger in Panel A for women than it is in Panel B for men, where, from time to time, a statistically significant coefficient is observed. Yet, because there is no clear pattern in those few significant coefficients, we could argue that they are likely due to noise in the data and a lower number of observations at given ages.[27]

Furthermore, not only is there no significant effect of exposure to the Taliban rule for women who were older than 14 years at the Taliban arrival in their province of residence, but we also do not observe any effect of exposure to the Taliban rule for women aged 7 to 14 years at that time. While the downward trend in $AgeAtTalibanArrival_{pbj}$ coefficients starts around the age of 8 years at first exposure, the coefficients become statistically different from zero only for women aged 6 years old or less at the time the Taliban occupation started in their province. That is, exposure to the Taliban institutions appears to only have affected the human capital accumulation of women who were very young when first exposed.

Moreover, the magnitude of the effect is larger the younger the girl was at first Taliban exposure, culminating with newborn and yet to be born treated girls at first exposure to have, on average, 3.51 years of education less than untreated women, a literacy probability 36.4 pp lower and a primary completion probability 32.6 pp lower. These upper bound effects are huge. Indeed, they represent respectively a 232%, 214% and 233% decrease in the corresponding outcome variables compared to their mean value in the control provinces. We also observe that exposure to the Taliban regime has no significant effect on the probability of participating in the labor market in the long run, independently of a woman's age at first exposure.

Turning to men, one should first acknowledge that the coefficients are more volatile and less precisely estimated, which might be due to the lower number of observations collected for men than for women. However, as for women, there seems to be a negative effect of Taliban exposure on all three educational outcomes, as depicted by the smooth downward trend in the coefficients

[26] As mentioned, the coefficient of the first lag, $AgeAtTalibanArrival_{bp15}$, is dropped (normalized to zero) and age 15 at the time of Taliban arrival is thus used as reference.

[27] Tables A1 to A3 in the appendix count the number of observations in our samples for given ages and in given provinces or treatment groups.

starting at age 14 at the Taliban arrival. The coefficients, however, remain mostly statistically insignificant, except for the youngest boys at first exposure. Similarly to what we reported for girls, the magnitude of the effect is larger the younger the boy was at the beginning of the Taliban rule in his province of residence. At the upper bound of the effect, newborn and yet to be born treated boys at first exposure have, on average, a literacy probability lowered by 18.9 pp relative to untreated boys and a primary school completion probability lowered by 24.3 pp (the 1.96 years of schooling now come out as statistically insignificant). These effects are much smaller than for girls, yet they are far from negligible. Indeed, they represent respectively a 43% and 84% decrease in the corresponding outcomes compared to their mean value in the control provinces (and 54%, but insignificant). Perhaps surprisingly, we observe in Table 3 that young boys at the beginning of the Taliban occupation in their province have a higher probability of participating in the labor market at the time of the survey.

Note that the results of Figure 3 and Table 4 are consistent with the ones of the difference-in-differences methodology. In light of Figure 3, namely that the education of women older than 6 to 8 years at first exposure is not affected by the Taliban rule, it appears logical, if the majority of women in our estimation sample belong to older age cohorts, that we do not find any *average* effect of having been of compulsory school age for at least a year in Table 2; and that we only find a small *marginal* effect of one additional year of compulsory school-aged exposure compared to that of preschool-aged exposure in Table 3. On the contrary, since it appears that exposure along all school grades somewhat mattered for men, although the coefficients are mostly insignificant, it seems plausible that we rather observe an *average* effect of compulsory school-aged exposure rather than a *marginal* one. Note that both methodologies consistently confirm that exposure to the Taliban rule has a positive effect on men's labor market participation in 2015 and a negative impact on women's educational outcomes (the estimated analogous effects for men are mostly statistically insignificant). Finally, both younger boys and girls at first Taliban exposure, which Figure 3 shows to be more strongly affected, have on average accumulated more years of Taliban exposure,[28] especially while of preschool age. And Table 3 further reveals that the latter conclusion matters the most.

4.3 Treatment at the Start of Schooling

The beginning of the Taliban occupation coincides with a different stage in the education of every individual. Since we observe that the Taliban rule mostly affected the educational outcomes of people who were not yet schooled at first exposure, it is worth investigating next whether the effect mostly acts at the start of schooling. Indeed, because we use samples of people aged 20 to 49 years at the time of the survey, all treated respondents have at least one year of compulsory school-aged exposure to the Taliban rule. This in turn means that all individuals included in the estimation samples and aged 6 years or less at first Taliban exposure began or should have begun their schooling during the Taliban regime. If people who would have begun their education if the Taliban had not occupied their province of residence did not, and hence missed

[28]The actual number of years an individual was exposed to the Taliban rule depends also, however, on how long their province of residence was under Taliban rule.

their chance to an education, there might be a strong effect at the start of schooling driving the results. This is especially relevant for women, whose education was prohibited after age 8 (New York Times, 1998). Moreover, people who never received any education before the change in institutions might be less likely to resume schooling later on (after the fall of the Taliban regime) than people who already went to school for a few years; and be less likely to catch up in the long run with untreated individuals in terms of human capital (see León, 2012; Stoelinga, 2022).

We test the hypothesis above by comparing people who turned 6 with people who were already older at the time of the Taliban rule, between provinces administered by the Taliban and provinces administered by the Northern Alliance. In other words, we conduct a difference-in-differences analysis similar to the one described in eq. (1):

$$Outcome_{ibd} = \alpha_b + \delta_d + \beta(TalibanControl_p * Turned\ 6_{bp}) + \mathbf{X}_{ibd}\gamma + \epsilon_{ibd} \quad (4)$$

where the notation is as before but now $Turned\ 6_{bp}$ is an indicator variable taking the value of one if an individual was aged 6, and therefore began or should have begun schooling at some point during the Taliban rule.

[Insert Table 5 about here]

The results of this analysis are reported in Table 5. As before, panel A displays women's results and Panel B men's. The four dependent variables are the same.

For women, we observe in Panel A that the coefficient of *Taliban control x Turned 6* ($\hat{\beta}$) is negative and statistically significant at the 5% level for all three educational variables considered. Everything else held constant, we hence find that, on average, a woman turning 6 years under Taliban rule – age at which one typically starts their schooling – has 0.67 years of education less, a literacy probability lowered by 7.56 pp and a primary school completion probability reduced by on 6.69 pp. Again, these are large effects in economic terms, representing respectively a 44%, 44% and 48% decline compared to the mean value of the corresponding outcome variables in the control provinces. Therefore, our results are coherent with an effect working mostly at the start of schooling for women, i.e., with the observation that our results might be mostly driven by the many girls who would have started school but did not because of the Taliban rule. On the other hand, our results show that such explanation is unlikely for men. Indeed, we do not observe any statistically significant effect of turning 6 years of age and having to begin school under Taliban rule on men's educational variables. The treatment, however, appears to increase the labor market participation probability of men in the long run. This effect is significant at the 10% level and represents a 4% increase compared to the 2015 labor market participation rate of men in control provinces.

4.4 Supporting Evidence for the Common Trends Assumption

For any of the results we reported to be valid, the common trends assumption must hold. In other words, we must assume that if the provinces that eventually were under Taliban control

had not been, the educational and labor market outcomes of the people living in these provinces would have evolved across generations the same way they did in provinces that never were under Taliban control. While this assumption cannot be formally tested, it is nevertheless possible to present some supporting evidence for its validity, and we do it in what follows.

The first way to provide supporting evidence for the common trends assumption is to visually plot the means of the outcome variables across generations for the treated and control provinces, and observe whether the outcomes of interest follow parallel trends for some time before the beginning of the treatment. If they did, we can then argue that without the treatment, they would have continued to follow such similar trends. This is exactly what is done in Figure 4. No matter that we know from Figure 1 that there are differences in the timing of Taliban occupation across provinces, we now disregard them in Figure 4 for simplicity, which we did not do in our earlier analysis based on difference-in-differences and event study techniques. Here, by exception, we consider that the treatment started in each affected province in 1996. This transposes into considering that all people aged 20 to 33 at the time of the survey were of compulsory school and/or preschool age for at least one year at the time of the Taliban regime. This cutoff is represented by a solid black vertical line[29] in Figure 4. Although the series resulting from the plotting of the means are somewhat spiky, especially when the number of observations for a given age is smaller[30] (e.g., men in the control provinces, Panels B1-4), they do not clearly violate the common trends assumption. In other words, the trends, although imprecise, appear rather parallel. This gives us a first indication that the common trends assumption is credible. Because of the volatility of the means, however, it is not sufficiently convincing and more support is desirable.

[Insert Figure 4 about here]

The second piece of supporting evidence for the validity of the common trends assumption, and the strongest one, is given by the absence of pre-event trend in Figure 3. As already discussed, the coefficients plotted on the left of the vertical line in the mentioned figure are not significantly different from zero. This means that there is, in relation to the base, no statistically significant difference between people in treated and control provinces in educational and labor market outcomes of individuals too old to be treated, i.e., individuals who were already older than 14 years at the Taliban arrival in their province of residence. In other words, the educational and labor market outcomes of these individuals followed similar trends in treatment and control provinces before the emergence of the Taliban.[31] We can thus argue that without the Taliban, this similarity would have continued. Note, however, that our claim here is stronger

[29] In reality, this cutoff varies between ages 30 and 35 in 2015 depending on individuals' province of residence. This is graphically represented in Figure 4 by the two dotted vertical lines.

[30] A similar figure is available in the appendix, where the control group alternatively consists of both provinces that never were under Taliban rule (Panjsher and Badakshan) and provinces that only partially were under Taliban rule (Baghlan, Takhar, Kapisa, Laghman, Kunarha and Nooristan). Doing this increases the number of observations in the new control group, hence flattening out the observed spikes. The divergence in outcomes between control and treated provinces however diminishes, as expected, since partially treated provinces are artificially added as not treated.

[31] When looking at Figure A4 in the appendix, which reports unweighted results, we do see a pre-trend. This highlights the importance of using sample weights for the validity of our analyses.

for women than for men, for which a few lags are significantly different from zero. Yet, there is no clear pattern in these coefficients, such that we can argue that they are mostly due to outliers combined with a smaller number of observations for given ages at first exposure to the Taliban rule.

To produce a last piece of supporting evidence for the validity of the results, we conduct a few placebo regressions. There are two ways of running a placebo analysis: (i) either by showing that the treatment has no effect on a "fake" outcome, which provides supporting evidence that it is not other unobserved differences between treatment and control groups that drive the results; (ii) or in artificially attributing the treatment to untreated individuals, and showing that when the treatment is thereby wrongly attributed, no effect is observed. Both these approaches are here adopted as placebos. In Panel A of Table 6, the treatment is artificially attributed to people who were aged 21 to 24 years at the Taliban arrival,[32] that is, to people who were clearly too old to be affected by the education policies of the Taliban even accounting for a very general definition of schooling age (6 to 20 years old). They are then compared, in the treated and the control provinces, to people who were then even older. Younger individuals are removed from the analysis. As the results of this difference-in-differences placebo strategy show, we find no significant effect of this artificial treatment on any of the dependent variables of interest, which provides additional support for the common trends assumption.

[Insert Table 6 about here]

The second approach to run a placebo test is adopted in Panel B, where for a "fake" outcome we here use tuberculosis and cancer occurrences, two variables available in the 2015 Afghanistan DHS. The diagnostic of any of these diseases over the course of one's life should arguably not be impacted by exposure to the Taliban rule in preschool and compulsory school years specifically. Whereas it is conceivable that the rule could have impacted people's health through increased poverty and lowered food intake, sanitation and healthcare,[33] there is still no reason to believe that only school-aged or preschool-aged individuals would be impacted and thus more likely to develop cancer or be diagnosed with tuberculosis. Indeed, tuberculosis being caused by a bacteria and cancer's causes being various, if the Taliban rule would have aggravated the occurrences of these diseases for some time or even in a longer run, it would likely have been for all. Moreover, we look at diagnostic occurrences and not death rates caused by the diseases, which are more likely to have been influenced by exposure to the Taliban rule in the vulnerable early years of life. Hence, if significant difference-in-differences placebo coefficients are found, the comparison groups are likely flawed and the validity of the results presented in the preceding sections would be jeopardized. But we observe no significant effect, which is a final piece of supportive evidence for the common trends assumption we presented here.

[32] This corresponds to people aged, at the time of the survey, 37 to 40 in provinces seized in 1999; 38 to 41 in provinces seized in 1998; 39 to 42 in provinces seized in 1997; 40 to 43 in provinces seized in 1996; 41 to 44 in provinces seized in 1995; and 42 to 45 in provinces seized in 1994.

[33] This, however, is not demonstrated and remains outside the scope of the present study.

4.5 Robustness Tests

To check the sensitivity of our results and how the different assumptions made along the way affect them, we next conduct several robustness tests. Tables 7 and 8 report the outcomes of these tests for the DiD regressions without treatment intensity for women and men, respectively; and Tables 9 and 10 present these for the DiD regressions with treatment intensity for women and men, respectively. Robustness checks were also conducted for the results that highlight the importance of the start of schooling, reported in tables A12 and A13 in the appendix, and for the event study technique, provided in tables A8, A9, A10 and A11 in the appendix.

[Insert tables 7 to 10 about here]

The first column in tables 7 to 10 replicates the baseline results of tables 2 and 3. The second and third columns re-estimate the models with different estimation samples. Individuals aged 15 to 49 (the whole 2015 Afghanistan DHS sample) are used to produce estimates in column (2) and individuals aged 20 to 40 to produce estimates in column (3). Even if as discussed, excluding the individuals aged less than 20 years at the time of the survey is intentional, it is still interesting to see how sensitive the results are to the estimation sample used. Similarly, one may want to remove older individuals from the estimation sample, arguing that these generations are too different from younger ones (even if age dummies should theoretically account for differences across generations that are generalized across provinces already). Comparing the results obtained in the baseline with those obtained with the alternative samples, we broadly reach the same conclusions. When ignoring treatment intensity in tables 7 and 8, we observe that effects remain statistically insignificant and those that come out as significant are of a similar magnitude.[34]

Similarly, when accounting for treatment intensity in tables 9 and 10, the effects of both school-aged and preschool-aged Taliban exposure on the three educational outcomes considered remain significant and of comparable magnitude for women[35] and insignificant for men; with the exception of, as before, the effect of one additional year of preschool-aged exposure on men's primary school completion probability and the effect of one additional year of compulsory school-aged exposure on men's labour market participation in the long run.[36] Overall, therefore, the results remain comparable and the analysis is rather robust to the use of these alternative estimation samples.

Next, column (4) in tables 7 to 10 reports results when an alternative group of control provinces is used. Indeed, as reported in Figure 1, few provinces in Afghanistan were in fact not completely under Taliban control, but only partially. Unfortunately, we do not have unambiguous information on which districts in these provinces were and were not under Taliban rule. The concerned provinces are Baghlan, Takhar, Kapisa, Laghman, Kunarha and Nooristan. In

[34] With the exception of the effect of school-aged exposure on completed years of schooling when removing from the analysis the 41-49 year-olds.

[35] With the exception of the effect of one additional year of compulsory school-aged exposure on primary school completion and years of schooling completed when using the whole Afghanistan 2015 DHS sample. This might be due to the inclusion of people who are possibly not yet done with their education.

[36] Note however that this last effect becomes insignificant when the 41 to 49 years old at the time of the survey are excluded from the estimation sample.

the baseline analysis, these provinces were assumed to be fully under Taliban control. Following Noury and Speciale (2016), we now reverse this assumption in including them in the control group alongside Panjsher and Badakhshan, in effect artificially setting the years of exposure of all their inhabitants to zero. Doing so, we expect to find lower treatment effects and this is indeed what is observed in all four tables. In Table 9, the effect of one additional year of compulsory school-aged exposure on women's educational outcomes becomes insignificant, while the effect of one additional year of preschool-aged exposure decreases in magnitude but still remains significant. The effects previously observed on men's educational outcomes are driven to statistical insignificance, except those on long-term labor market participation (see tables 8 and 10).

Column (5) in tables 7 to 10 questions the timing of arrival of the Taliban at the province level. Indeed, the data provided in Figure 1 only indicate the year at which each province was seized. Lacking more precise information, we counted the year of arrival as treated. However, if the Taliban arrived in a given province in December of a given year, for instance, it would be more accurate to account for the year of arrival as non-treated and only the following year as treated. This implicit assumption might affect the results. We thus shifted the exposure by one year, this time assuming that the treatment (Taliban rule) began the year following the information provided in Figure 1. Of course, this shift would in reality be necessary for some provinces while not for others, but we do not know for which ones precisely. Therefore, we can expect that the true effect lies somewhere between the estimation results obtained in the baseline and the ones obtained in this shifted exposure test. In tables 7 and 9, we observe that the shifted exposure increases the magnitude of the negative effect of Taliban rule between the age of 0 and 14 for women, and this accounting or not for the treatment intensity. We therefore conclude that the effects found in the baseline are a lower bound to the true effects of the Taliban rule on the educational variables considered. For men, the results of tables 8 and 10 are slightly less straightforward. A shift in the treatment effect distribution is observed in Panel C of Table 8, where the effect of school-aged exposure is mitigated while the effect of preschool-aged exposure is magnified and made significant at the 1% level. For the rest, the shift in Taliban exposure generally increases the magnitude of the already significant effects while of preschool and compulsory school age on men's outcomes. It also causes some effects on men's human capital accumulation to become statistically significant. The effect on labor market participation remains unchanged. In consideration of the above, we can generalize the results to say that the shift in treatment exposure mostly increases the magnitude of the downward impact of the Taliban rule on education for both men and women, although it is less clear for men. Hence, our baseline results appear to be rather a lower bound to the true effects of the Taliban rule.

A different test is conducted in columns (6) and (7) of tables 7 to 10. Here, compulsory school age and preschool age definitions are redefined. Indeed, even if a law in 2008 made schooling compulsory for children aged 6 to 14 (NUFFIC, 2015), schooling was not yet compulsory for most of the people in the estimation sample. Moreover, we know from NUFFIC (2015) that in Afghanistan some children typically start school later. Additionally, some students may not yet

be done with their education at 14 years old, as they may continue with upper secondary education, vocational training or even higher education. This is why we investigate how the results change when accounting for a different definition of schooling age (and, by extension, preschool age). Column (6) looks at a more general definition of schooling age, i.e., 6 to 20 year-olds, to account for people who might study for longer time periods, while column (7) on the other hand shifts the schooling age definition to 7 to 15 year-olds, to account for people who might begin their primary education later.[37] In column (6) of the four robustness tests tables, the impact of preschool-aged exposure quite logically does not importantly change (the definition of preschool-aged exposure remains the same) regardless of whether treatment intensity is accounted for or not. The effect of compulsory school-aged exposure on the educational outcomes, however, decreases in magnitude both when accounting for treatment intensity and when not, and even becomes insignificant. Where the effect already was insignificant, it remains so. These results are consistent in light of the findings of our heterogeneity analysis. Indeed, knowing that only the younger cohorts were affected by the Taliban rule, including older cohorts into the treatment group, which we know were unaffected by the regime, drives the average effect of compulsory school-aged exposure to zero. When defining compulsory schooling age as 7 to 15 year-olds in column (7), we observe, for women, that the negative effect of preschool-aged exposure, when neglecting treatment intensity, increases in magnitude and becomes significant, while the effect of compulsory school-aged exposure remains insignificant.

When accounting for treatment intensity, the effect of one additional year of compulsory school-aged exposure for women decreases in magnitude and becomes insignificant, while the effect of one additional year of preschool-aged exposure does not importantly change. This reflects the fact that, as observed in Table 4, only women aged 6 years or less at the beginning of the Taliban rule were significantly affected, and now the preschool age definition includes people aged 6, such that the effect of compulsory school-aged exposure is no longer observed. Moreover, this confirms our results that preschool-aged exposure, as well as early school-aged exposure, matter more than later school-aged exposure. For men, the coefficients which were not statistically different from zero remain insignificant, and the size of the effects of compulsory school-aged exposure decreases while that of preschool-aged exposure remains mostly unchanged.

The last column of tables 7 to 10 tests how the significance of the results changes when clustering at the province-cohort of birth level instead of the province level. When using data spanning over many periods (in our case birth cohorts), it is likely that errors are serially correlated within a group (in our case a province). For this reason, it is important to cluster the standard errors in order to not overestimate the statistical significance of the results. In our baseline analysis, we did accordingly cluster the standard errors at the province level. Now, if the errors happened to also be correlated across provinces, one should rather cluster the errors at the province-cohort level (two-way clustering). This is what we do here as a robustness test. We observe that clustering at the province-cohort level instead of the province level does not drastically change the size of the coefficients' standard errors. They are sometimes a little

[37] We take age 7 as a middle ground between 6 and 8, since NUFFIC (2015) mentions that children in Afghanistan typically start school between 6 and 8 years old.

smaller, and sometimes a little larger. Moreover, the results that were significant at the 5% or 1% level remain significant at either of these; and only results that were significant at the 10% level become insignificant in some instances. On the contrary, some results that were insignificant become significant at the 10% level, and some results that were significant at the 10% level become significant at 5% instead. In consideration of the above, we conclude that the significance of the *main* results of the study is robust to two-way clustering.

Similar conclusions as the ones presented above are reached in the robustness tests performed on the analysis of treatment at the start of schooling[38] and on the event study approach.[39] Details are not presented here, but the outcomes of all robustness tests are available in the appendix. Yet one thing worth highlighting is the results of the effect heterogeneity by age at first Taliban exposure when looking at the whole Afghanistan 2015 DHS sample. This is interesting because adding people aged 15 to 19 years at the time of the survey into the estimation sample allows us to look at how the Taliban regime affected yet to be born individuals at the Taliban arrival and individuals who did not turn 6 years at the time of the Taliban regime. Note that the binning of the endpoints had to be adapted in order to perform this analysis. In comparing Figure 3 and Figure 5, we observe visually that the inclusion of the 15 to 19 year-olds in the estimation sample does not importantly change the coefficients we obtained without them. But we can now see how the Taliban rule affected the educational and labor market outcomes of people even younger (yet to be born) at the time the Taliban seized their province of residence. What we interestingly observe is that, for both men and women, the effect persists for cohorts who were born under Taliban rule but who were never exposed to the Taliban's institutions while of compulsory school age. This implies that the effect cannot fully act at the start of schooling. We will return to this in the next section. Additionally, we observe that the effect seems to eventually dampen. This is more obvious for women than men. For the latter this decrease in the magnitude of the effect is not clearly distinguishable, but the coefficients tend to become statistically insignificant at the endpoints again (although this could also be due to a lower number of observations at the endpoints yielding less precisely estimated coefficients).

[Insert Figure 5 about here]

5 Discussion

This section discusses the results, suggests interpretations to explain them and compares them to the existing literature. Limitations of the study are then outlined, along with ideas on how to take this research further.

[38] Except for the shifted exposure test, but the effects remain mostly insignificant.

[39] Note, however, that when excluding the 41-49 years old at the time of the survey, effects for men are somewhat larger in magnitude.

5.1 Interpretation of Our Results and Comparison with the Existing Literature

Throughout the study, our results indicate that the Taliban occupation mostly affected the education of individuals who were very young when first exposed to it. Therefore, the central and original contribution of the present paper is that the accumulation of years of preschool-aged exposure to the radical religious Taliban rule was particularly critical for individuals' lifelong human capital accumulation. This is especially true for women.

5.1.1 Human Capital Accumulation

Indeed, the results of the difference-in-differences estimation in Table 3 show that if one additional year of compulsory school-aged exposure also has a substantial negative impact on women's educational outcomes, the average negative impact of one additional year of preschool-aged exposure is much larger. This result is confirmed by the subsequent treatment effects heterogeneity analysis. Figure 3 clearly shows that only women aged 8 years or younger at the beginning of the Taliban rule in their province of residence were affected by exposure to the regime, while the effect becomes statistically significant only for women who were aged 6 years or less at the Taliban arrival. Moreover, we find that the younger the girl at first exposure to the Taliban rule, the larger the impact of the regime on her educational outcomes. This all in all means that it is mostly years of preschool-aged exposure that matter, along with exposure in the first grades of school, rather than later grades (such that the *average* effect of one additional year of compulsory school-aged exposure in the difference-in-differences analysis is smaller). Moreover, if the effects described above are clearer and larger for Afghan girls, they are far from negligible for Afghan boys.[40] It indeed appears in Figure 3 that the younger a boy at the Taliban arrival in his province of residence, the larger the negative impact of the Taliban rule on his educational outcomes too. While most effects found for boys are insignificant in Table 3, we still globally observe larger impacts of one additional year of preschool-aged exposure than of compulsory school-aged exposure. For boys, however, it seems that school-aged exposure across all compulsory grades also somewhat mattered, but still less importantly than preschool-aged exposure. Indeed in Figure 3, although most coefficients are statistically insignificant, the decrease in human capital across treated generations appears smoother for boys and to start at age 14 years old at first exposure.

Overall, the results of the difference-in-differences analysis with treatment intensity reported in Table 3 show that length of exposure to the Taliban institutions matters to quantify the effects of the Taliban regime on educational and labor market outcomes of Afghans. They also give a first indication of the importance of the timing of exposure for this quantification, as the estimated effects of one additional year of preschool-aged exposure are larger than those of one additional year of compulsory school-aged exposure. The importance of the timing of

[40] If boys' education was not prohibited under the Taliban rule, many boys' schools had to close too because many teachers, especially at the primary school level, were women, and women were prohibited to work outside of their home (see, e.g., Rashid, 2010, p. 106). Thus, boys' education was also jeopardized, although less drastically than the one of girls.

exposure is further assessed in the heterogeneity analysis by age at the Taliban arrival, where we clearly observe that exposure at a younger age is much more damaging than at a later age. The fact that the length of one's exposure to the Taliban rule, as well as the timing of exposure in one's life, matter, helps explain the results of Table 2. Mostly insignificant, these results are rather difficult to interpret for both men and women. We could explain them by our findings that, since the effect of Taliban exposure on education is so heterogeneous across birth cohorts, especially for women, the fact that this heterogeneity is completely neglected in Table 2 leads to inconclusive results. In the case of women specifically, knowing that the education of older individuals at first exposure was not affected at all by the Taliban regime in the long run, it seems plausible that the *average* effect of Taliban exposure on educational outcomes is statistically insignificant if most women in our sample belong to older cohorts, whose education was not influenced by the regime, and only a smaller proportion of the sample are younger individuals, whose education was affected by the regime. A similar argumentation could help us explain why we observe an *average* effect of exposure to the Taliban rule on men's completed years of schooling rather than a *marginal* one.

Noury and Speciale (2016), who also study the impact of the Taliban rule on women's education using a comparable difference-in-differences strategy, do not find, in contrast to our study, that it is mostly younger cohorts that were affected by the Taliban regime. And for a good reason: they do not investigate the impact of preschool-aged exposure at all nor do they look at the heterogeneity of the treatment effect by people's age at the time of exposure. Their unique treatment is thus school-aged exposure, which they define as being exposure between the ages of 6 and 15 years old. Restricting their estimation sample to individuals born between 1976 and 1992,[41] Noury and Speciale (2016) find that one additional year of school-aged exposure reduces literacy by 2.33 to 2.82 pp and total years of education completed by 0.21 to 0.23 years. In comparison, we find that one additional year of compulsory school-aged exposure decreases women's completed years of schooling by 0.08 years, a smaller effect. On the other hand however, we find that one additional year of preschool-aged exposure decreases total schooling by 0.49 years, which is an effect more than twice stronger in magnitude than what Noury and Speciale (2016) found. In terms of literacy, we observe a similar pattern. We find that one additional year of compulsory school-aged exposure decreases literacy by 1.07 pp, which is more than twice lower than what they found, but we find that one additional year of preschool-aged exposure decreases women's literacy probability by 4.66 pp, which is almost twice larger than what they found. The size of the estimated effects by Noury and Speciale (2016) therefore lie somewhere in-between the sizes of our preschool-aged exposure and compulsory school-aged exposure effects. Moreover, the coefficients they estimated with regard to the effects on men's outcomes are also in line with ours. They find that one additional year of school-aged exposure decreases men's completed years of schooling by 0.048 years (significant only at the 10% level) while in our case we find statistical insignificance. Additionally, they find no significant effect of one additional year of school-aged exposure on men's literacy probability, as we do. In light

[41] This would correspond in our case to including in the estimation sample individuals aged 23 to 39 in 2015.

of the above, our key finding that early exposure to the Taliban rule had a critical impact on individuals' education is compatible with the existing literature, but novel.

It remains quite puzzling, however, that we do not find any effect of the Taliban rule for girls whose exposure started as they were aged more than 8 years old, while it is these girls' education which was specifically prohibited (New York Times, 1998). A potential answer may be that older girls at first exposure were on average already out of school anyway. Indeed, as reported in Table 1, a large majority of Afghan women do not go to school or quit school quite early. In our sample, the average total years of schooling for women is 1.17 years and their primary school completion probability amounts to 10%. We also observe in Figure 2 that education is low for most generations and has only increased recently. Thus, it is possible that for many cohorts schooling was not widespread in control and treated provinces alike, so that people would not have gone to school regardless of the Taliban rule. Then, as education democratized later on, it possibly increased in control provinces more than in treated provinces because the Taliban rule had stopped this development in the latter, but not in provinces that were administered by the Northern Alliance. Another hypothetical explanation may be that people who were older at first exposure, and who possibly already went to school for a few years before the change in institutions, could, in the long run, catch up in terms of human capital with untreated individuals, while younger people at first exposure could not (see León, 2012; Stoelinga, 2022).

What mechanisms drive our results? One interpretation, which we test, is that the effect might actually work upon exposure at the start of schooling, especially for women. What we mean is that if, on average, people who would have begun school did not because of the Taliban occupation, and hence missed their chance to get any education,[42] we can expect a large impact of exposure at the start of schooling to possibly drive our results. People might have not begun school for different reasons, including principally the Taliban's female education ban, but also reduced school supply (women being prohibited to work outside of their home, there was a shortage of teachers and closures of some boys' schools too) or anticipation of schooling disruptions. The results of Table 5 confirm that exposure at age 6, at which one typically starts school, has a large negative effect on all three educational outcomes of interest for women. It is however not significant for men. This is in line with the results of Figure 3, where we observe that for men, exposure to the Taliban regime during all compulsory school grades somewhat matters, as depicted by the smooth downward trend in coefficients, although most coefficients are statistically insignificant. The effect for women on the other hand only kicks in suddenly for girls aged 6 to 8 years old at first exposure to the Taliban rule. Hence, it appears plausible that our results are driven by a large effect working at school start for women, while not for men. This likely reflects the consequences of the female schooling ban after age 8 (New York Times, 1998), which by definition affected girls but not boys.

Yet, even if it turns out to be true that the effect partially works at school start, it cannot be the whole story, for several reasons. First, we also observe an effect for men, which, as discussed above, is not observed specifically at school start. There must thus be at least another

[42] León (2012) and Stoelinga (2022) argue that people who never received any education before a disruption or a war may be less likely to resume/start schooling once it ends.

mechanism at play for men too. Second, if only the schooling ban prevented women to get education, i.e., if only exposure at the start of school matters, what could explain the fact that the impact of Taliban exposure is larger for women first exposed at age 1 than those first exposed at age 6? Or in other words, what could explain the fact that additional years of preschool-aged exposure (i.e., the intensity of treatment) worsen the impact of the Taliban institutions on lifelong human capital accumulation? Finally, and most importantly, when including in the estimation sample people aged 15 to 19 years at the time of the survey, we still observe large effects on their educational outcomes (see Figure 5). People aged less than 20 years at the time of the survey were too young to have any year of compulsory school-aged exposure to the Taliban rule. If all individuals in our samples were born under Taliban rule (and thus have some preschool-aged exposure to the Taliban's institutions[43]) individuals aged 15 to 19 years at the time of the survey did not turn 6 years under Taliban rule and effectively started (or should have started) school after the fall of the Taliban regime in late 2001. The Taliban rule could thus not have *directly* prevented them to begin their education, with a schooling ban, for instance. If the effect eventually recedes as seen in Panel A of Figure 5 (which is harder to observe in Panel B), maybe as a result of a lower number of years of preschool-aged exposure, there still is a persisting effect, which cannot act at school start. Therefore, it appears that exposure to the Taliban institutions in the early years of life impacted later human capital accumulation of both boys and girls in Afghanistan through a form of "scarring" effect. This could be explained, for instance, by a form of cultural assimilation of the Taliban norms, persistent fears or social pressure caused by the experience of the rule, and/or lowered expected returns to education. Whether it affected the individuals themselves and their future education preferences, or their parents and their decisions for their children, or both, is debatable. In any case, the Taliban rule could likely have impacted people's expectations for the future, and set new norms, fears, and examples. In addition to the above, other mechanisms possibly underlying the relationship between Taliban occupation and education both immediately and persistently could be, for instance, a decrease in school supply (shortage of teachers, school closures), insurgency violence and heightened financial needs as a result of the economical crisis triggered by the Taliban regime and the war. Parents might have taken their boys out of school to work and help bring financial resources home; and they might also have married their girls away, in exchange of some money. These are only hypotheses, and further research is needed to confirm or discard them. There could also be other channels which are not considered here and further studies could focus on identifying these too.

Our key novel empirical finding that the youngest individuals were the most strongly affected ones by the Taliban rule is in line with Rashid (2010). He writes: "Women and children face the brunt of the conflict [...]. Most children had witnessed extreme violence and did not expect to survive. [...] [A majority] had lost a family member and no longer trusted adults. They all suffer[ed] from flashbacks, nightmares and loneliness. Many said they felt their life was not worth living anymore [...]. Every norm of family life had been destroyed [...]. When children cease to trust their parents or parents cannot provide security, children have no anchor in the

[43]Note that all people aged 15 at the time of the sample have 2 years of preschool-aged exposure, but none of compulsory school-aged exposure.

real world. [M]any [children] were orphans with no hope of having a family, an education, or a job except soldiering" (Rashid, 2010, p. 109). Moreover, our study is not the only one finding that early childhood is critical for one's later life behaviors, beliefs and preferences. The psychology literature demonstrated for instance that children exposed to wars and violence between the ages of 5 and 9 years are more vulnerable to war trauma (e.g., Garbarino and Kostelny, 1996; Kuterovac-Jagodić, 2003; Barenbaum, Ruchkin and Schwab-Stone, 2004). To cite some examples in the economics literature, Couttenier et al. (2019) find that exposure to violent conflicts during childhood makes migrants more violence-prone in their host country, and that this effect is stronger if the exposure took place in the first five years of life. León (2012) reports that while exposure to civil conflicts has in the short-term a negative impact on human capital accumulation of children exposed while in utero, of preschool or of primary school age, only children exposed while in utero or of preschool age are affected in the long run. Children who were older at exposure to conflicts succeed to catch up with their peers who did not experience violent conflicts. Aside from exposure to violence, studies also reveal that the economic, social and political contexts in which young children grow up affect their later life socioeconomic outcomes. Gould, Lavy and Paserman (2011) report that growing up in a modern environment in the first five years of life has favorable impacts on later life socioeconomic outcomes, including human capital accumulation. Similarly, Heckman, Pinto and Savelyev (2013) show that intervention programs in early childhood (3 to 4 years old) can boost adult outcomes, including human capital, via their influence on personality skills. Moreover, studies show that the formation of pro-social preferences is very dynamic during childhood (e.g. Fehr, Bernhard and Rockenbach, 2008; Fehr, Glätlze-Rützler and Sutter, 2013; Bauer, Chytilová and Pertold-Gebicka, 2013), possibly through the absorption of normative rules of the society surrounding them (Fehr, Bernhard and Rockenbach, 2008). Our results contribute in some sense to this literature, showing that early childhood exposure to destructive and violent radical religious institutions strongly impacts people's lifelong human capital accumulation.

5.1.2 Labor Market Participation in the Long Run

Our results also show that Taliban exposure while of preschool and/or compulsory school age has no impact on women's long-term labor market participation probability: in all of our specifications, the estimated coefficients are insignificant. For men, however, we find that exposure to the Taliban rule increases their probability of working at the time of the survey.

To compare our results with the existing literature, Noury and Speciale (2016) report that one additional year of school-aged exposure to the Taliban rule slightly increases a woman's labor market participation probability in the long run. They do not examine the effect of Taliban exposure on men's labor market outcomes.

The fact that we do not find any effect of Taliban exposure while of preschool and/or compulsory school age on labor market participation of women in the long run is not necessarily implausible. It is possible that having been exposed to the Taliban regime in 1996-2001 does not affect at all whether a woman works or not in 2015, i.e., 15-20 years later. It could, however – since Taliban exposure clearly decreases women's educational attainments – affect

the composition of women's labor. Treated women could, for instance, be less likely to work in skilled positions or outside of their household in the long run. While this was not investigated in our study, Noury and Speciale (2016) indeed report that one additional year of school-aged exposure to the Taliban rule reduces a woman's probability of working outside of the household and increases her probability to work an agricultural job within the household or to perform unpaid family work.

5.2 Limitations

As discussed, for the DiD methodology to be valid and correctly identify the average treatment effect on the treated (ATT), the main identifying assumption that has to hold is the common trends assumption. If this assumption cannot be formally tested, it is nevertheless possible to present some empirical evidence for its credibility. This is what has been done in section 4.4, where we showed that the common trends assumption held before the arrival of the Taliban. We can thus credibly assume that if the Taliban would not have gained control over the concerned provinces in Afghanistan, the evolution of our variables of interest would have continued to follow such parallel trends between control and treated provinces.

But the common trends assumption is not the only assumption that should hold for the difference-in-differences strategy to produce unbiased results. Namely, one should for instance assume that no other event influencing the dependent variables happened at the same time as the one evaluated, otherwise we estimate the joint impact of the simultaneous events. This should not be a big issue in our case, as we evaluate the overall impact of a change in territorial control and institutions in a broad sense.

For our results to be unbiased, the timing of the treatment must also be exogenous. This means that the fact the Taliban seized a province rather than another should not be determined by the dependent variables of interest. If the Taliban had for instance targeted provinces where schooling was especially high aiming at reducing it, then the treatment would be endogenous. This, to the best of our knowledge, was not the case, such that it should not be an issue. However, it is possible that the Taliban targeted, at least initially, provinces where a majority of Pashtun Afghan lived, given that most of its members were themselves of this ethnicity (Rashid, 2010) and the group possibly had thus more sympathies from civilians in these areas. If there are then cultural and ethnic differences in preferences for human capital accumulation across ethnicity or cultural/linguistic groups, then the treatment might be endogenous. A similar reasoning applies if the Taliban targeted poorer (respectively richer) or rural (respectively urban) areas, where schooling is typically lower (respectively higher). Rationale for this could be that poorer regions were easier to control or respectively that richer regions presented higher rent capture incentives. In order to avoid such possible omitted variable biases, ethnicity and languages dummies, as well as a wealth index and a variable indicating whether individuals lived in a rural or urban area, were added in our regressions. Another variable which could have simultaneously determined which provinces were under Taliban control and which can be linked to people's education level (assuming that people living in richer, less remote areas have more education on average) is the economic value of an area due to its opium production, road network

and strategic situation for trade or warfare. Assuming that these variables remained relatively constant across generations, districts fixed effects already account for them. Finally, even if the observation of parallel trends before the start of the treatment is reassuring, violence remains a concern. If the Taliban controlled provinces that experienced more violence on average, and knowing that violence negatively impacts schooling, violent acts might bias our results. This is further discussed below.

The last two important assumptions for our results to be unbiased are, first, that there was no spillover of treatment, and second, that the composition of the treatment and control groups does not change as a result of the treatment. These two assumptions likely do not hold in our study. Indeed, the Taliban's presence in neighboring provinces, and the norms, fears and violence associated with it, might have spilled over on provinces administered by the Northern Alliance, which would find themselves indirectly and partially treated. In the presence of such spillovers, however, one can expect that Taliban's institutions and violence indirectly reduced educational attainments of the residents of provinces administered by the Northern Alliance. Therefore, one should expect that the true causal effect is even larger than observed; or in other words that our results underestimate the true causal effect of the Taliban rule on human capital accumulation in the long run.

A more worrying threat is the one caused by migrations. Indeed, if people migrated both within Afghanistan (across provinces) or to neighboring countries in reaction to the Taliban occupation, the composition of the treatment and control groups change as a result of the treatment, and our estimates would be biased. We know that the situation in Afghanistan triggered important human displacements, and specifically more educated families attempted to flee (e.g., Rashid, 2010, p. 106). In such a case, our results may overestimate the true causal impact of the Taliban rule, as the selection into migration of educated families artificially decreases the average educational attainments in occupied territories. While we did not attempt to discard this issue in our study, Noury and Speciale (2016) did. They found that despite migrations, the effect of the Taliban rule on women's education remains significant and of similar magnitude. Their result, hence, reassures that migrations likely do not importantly bias our results either.

Aside from the discussion of identification assumptions, there are a few other limitations of the study that should be acknowledged. First, and still in relation with migrations, the data we used provide an indication on the current residence of respondents only, not on their childhood residence, which is the one that really matters for exposure to the Taliban rule while of preschool and compulsory school age. Hence, there is a risk that the treatment was wrongly assigned to some individuals. Given the data at our disposal, it is impossible to correct for this. We thus had to assume implicitly that people did not move to another province later in their life.

The 2015 Afghanistan DHS data present some other limitations. A significant one is that both men and women samples are *ever-married* samples. This means that all respondents are or have been married at least once in their life. If this is not a drastic issue for older individuals, it might be for the younger ones, as they might not be representative of a typical young person in Afghanistan. Specifically, it is likely that people who marry earlier also accumulate less

schooling. The average age at first marriage in our samples is 19.4 years old for women and 23.7 years old for men. Thus, this comment especially concerns data for people below 20 to 25 years old. This is also a strong motivation for leaving people younger than 20 years old at the time of the survey out of the analysis. Hopefully, this bias in the data exists for both treated and control provinces alike. It thus should not bias the estimated causal effects too much, but might impede their generalization to the overall population, or at least to the never-married teenagers.

The data also suffer from some rounding bias. Indeed, people's ages are sometimes approximated, as can clearly be seen by the bunching of age around round numbers (e.g. many people state being aged 40 or 45 while much less claim being aged 39, 41, 44 or 46).[44] That is, many people do not know their exact age and simply round it. These approximations might affect the results, and are also partially responsible for the fact that there are less observation points for some given ages, inducing some coefficients, typically in the estimation of eq. (3), to be less precisely estimated and more sensitive to the presence of outliers. Finally, the data provided for the timing of Taliban exposure are given with annual precision. This, combined with people's age as well as available information on schooling age in Afghanistan, forces us to make some approximations as to whose schooling was or was not affected by the Taliban rule.

Lastly, another potential weakness of our results, as already touched upon, is the fact that they may be driven by differences across provinces in the importance of Taliban insurgency violence before, during and after the Taliban rule.[45] Indeed, if provinces which were under Taliban control were more subjected to violent events and terror attacks than provinces which were not, then these violent acts might be the cause of the observed differences in educational attainment across control and treated provinces rather than institutions. If it turns out that insurgency violence drives our results, then their interpretation would be slightly different. If they would still be linked and attributed to the Taliban, they would no longer be due to the institutions (e.g., the prohibition on girls' education) they implemented during their rule, but to insurgency violence and destruction. We are especially concerned by violent acts *after* the end of the Taliban rule in 2001, given that we find a strong effect of Taliban exposure on education for the youngest cohorts mostly, i.e., cohorts which might have begun schooling under Taliban rule, but which effectively completed it after it.[46] Additionally, we observe persisting effects on yet to be born individuals, who did not go to school under Taliban rule at all, but were exposed while of preschool age.[47] This could cast doubt on the true effect of Taliban institutions. We did not test for this in our study, yet Noury and Speciale (2016) did. They report that violence accounts for 10% to 28% of the effect size, while the rest is still attributable to the Taliban rule. Hence, it appears that institutions also matter, even more.

[44] See tables A1 and A2 in the appendix.

[45] A visualization of Taliban insurgency violence through time is available in Figure A7 in the appendix.

[46] People living in treated provinces and aged 20 in our sample have exactly one year of compulsory school age exposure to the Taliban rule.

[47] People living in treated provinces and aged 15 years old in our sample have exactly 2 years of preschool-aged exposure to the Taliban rule and none of compulsory school-aged exposure.

5.3 Avenues for Further Research

To enhance and generalize the findings of this paper and confirm their suggested main interpretations, additional work could be conducted, particularly to discard the possibility that results are driven by insurgency violence or by migrations. A way to test whether the results are driven by violent events committed by the Taliban would be to look at the geographic distribution of the violent events in Afghanistan before, during and after the Taliban regime and show that they are not concentrated in treated or control provinces particularly. Another possibility is to reproduce the analysis in Noury and Speciale (2016) and collect data from the Uppsala Conflict Data (UCDP) until 2015, aggregate the violent events at the province level for the period before, during and after the Taliban rule and include such constructed variables in the regressions (interacting them with the treatments), to see if they explain the treatment effects away completely, partially or not at all. Similarly, one can look for data on migrations rates away from each Afghan province and interact these with the treatments to test whether results remain similar once controlling for migrations.

Once these two concerns removed, further research could focus on, first, the determination of the mechanisms driving the observed relationship, whether it is fear, change in expectations for the future, alteration of cultural norms, peer pressure, destruction of school supply, missed opportunity at the start of school, heightened financial needs or something else. Secondly, one could take interest in the impact of the Taliban rule on other socioeconomic variables. Indeed, if investigating education appears as the salient choice given the clear prohibition of female education and the importance of human capital for economic growth and development, there are many other critical variables to explore. With regards to labor market outcomes, if we observe that there is no impact on women's participation in the labor market in the long run, one may however want to investigate whether there is one on the kind of tasks they are performing, looking for instance at the possibility to work outside of the household or skilled jobs. One could also examine the impact of the Taliban regime on gender roles and gender violence.

It may also be interesting to look at how exposure to the Taliban regime at any given age (and not only beginning at a given age) affected human capital accumulation of treated individuals by including a dummy variable for each concerned age. This would allow to test whether the effect of one additional year of exposure to the rule is non-linear and if for instance only exposure in the first few years of school matters.

6 Concluding Remarks

In this paper, we aimed at estimating the long-term impact of costly norms and prohibitions imposed on the population by a radical religious regime such as that of the Taliban between 1996 and 2001. More precisely, we focused on the impact of the Taliban rule on men's and women's lifelong human capital accumulation and on their long-term labor market participation probability. In light of the return of the Taliban to power in Afghanistan in August 2021, investigating such questions is of utmost importance and immediate policy relevance, as there

might be lessons to be drawn from the past. Moreover, in studying the socioeconomic and long-standing effects of radical religious norms and institutions, this paper contributes also to the literature on the *proximate* determinants of growth. It further strongly relates to the economics of religion, in particular on studies of radical religious groups and militias, as well as to the literature on the impact of conflicts on education.

To address our main research question, we took advantage of the differences in Taliban occupation across provinces between 1994 and 2001 in a *generalized* difference-in-differences framework. We used the data in Noury and Speciale (2016) on the timing of Taliban control at the province level and individual data from the 2015 Afghanistan DHS to estimate the impact of exposure to the Taliban regime between the ages of 0 and 5 (preschool age) and between the ages of 6 and 14 (compulsory school age) on educational attainment and long-term labor market participation. We found that for women, one additional year of compulsory school-aged exposure decreases, on average, their completed years of schooling by 0.08 years (5% compared to the variable's mean in the control provinces), literacy probability by 1.07 pp (6%) and primary school completion probability by 0.56 pp (4%). The effect of one additional year of preschool-aged exposure was estimated to be much larger: it decreases, on average, women's completed years of schooling by 0.49 years (32%), literacy probability by 4.66 (27%) pp and primary school completion probability by 4.43 pp (32%). By contrast, exposure to the Taliban rule while of preschool and/or compulsory school age did not affect in our estimation a woman's probability to participate in the labor market in the long run. For men, preschool-aged exposure to the Taliban rule did not come out as statistically significant. We still found that one additional year of preschool-aged exposure to the Taliban rule significantly decreases men's primary school completion probability by 3.53 pp on average (12%). Hence if men's education was less importantly affected by the Taliban rule than women's, it still was. Differently for women, our results show that one additional year of compulsory school-aged exposure increases men's long-term labor market participation probability by on average 0.63 pp (0.66%).

Having confirmed that the length of one's exposure to the rule matters, we further investigated whether the timing of exposure also does, by implementing a strategy akin to an event study. This allowed to examine the treatment effect heterogeneity by age at first exposure to the Taliban rule. We found that for both men and women, the younger the individual at first exposure to the Taliban rule, the larger the impact on their human capital. The central novel finding of our paper is thus that early childhood exposure to the radical religious Taliban rule was particularly unfavorable for later human capital accumulation. In particular, we documented that, on average a woman turning 6 years under Taliban rule – age at which one typically starts their schooling – has 0.67 years of education less, a literacy probability shrunk by 7.56 pp and a primary school completion probability reduced by on 6.69 pp. Again, these are large effects in economic terms, representing respectively a 44%, 44% and 48% decline compared to the mean value of the corresponding outcome variables in the control provinces. Therefore, our results are coherent with an effect working mostly at the start of schooling for women, i.e., with the observation that our results might be mostly driven by the many girls who would have started school but did not because of the Taliban rule. In conclusion, we revealed, and

estimated in a robust econometric analysis, that both the length of one's exposure (particularly while of preschool age) and timing of exposure to the Taliban rule matter for lifelong human capital accumulation.

Our empirical results provide insights into the importance to target young children with relief programs, and especially girls, to avoid a permanent loss of human capital in Afghanistan. In regards to the large economic magnitude of our estimates relative to the scarce economic literature, we can only stress that the international community should insist on easing the Taliban gender policies and facilitating boys' and girls' access to education. There is hope, as the Taliban have, at least initially, shown some signs of moderation compared to their last power tenure. Moreover, Afghan people worldwide try to stay confident, as illustrated by the words of Ramia Abdul Wakil, a binational Swiss and Afghan woman formerly working for the Swiss Federal Department of Foreign Affairs. She says: "In 20 years, the Afghan population evolved and the Taliban too. They will not be able to act [as] they did before 2001, because people who got a taste of freedom will resist" (Ridard, 2021). Women in Afghanistan are indeed risking their lives to organize protests and fight for their rights (Agence France Press, 2022). Finally, if not for other reasons, the Taliban might have to make concessions with the international community to gain the international recognition and humanitarian help they eagerly seek (Keystone-ATS, 2022).

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Online Appendix to "Radical Religious Rule and Human Capital: Evidence from the Taliban Regime in Afghanistan (1996-2001)"

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January 2023

A Appendix: Background Information

To contextualize this study and ease its understanding, this section presents information on Afghanistan's history, the Taliban, religious radicalism and education in Afghanistan.

A.1 Afghanistan and the Taliban

"Afghanistan, like the Afghans themselves, is a country of contradictions [...]" (Rashid, 2010, p. XV). The geographic location of Afghanistan, at the crossroads of Iran, Central and South Asia, caused the country to be, since the dawn of times, a meeting place, a buffer state as well as a battleground for successive civilizations and empires (Rashid, 2010).[48]. As a result of its history of influences and numerous invasions, Afghanistan developed into a tribal society counting a large number of ethnic, linguistic, cultural and religious groups. Rightfully, Berman (2003) describes Afghanistan as a "famously ungovernable country". Violent rivalries between tribes proved regularly throughout history that nation-building and effective state control in Afghanistan are highly complex tasks. This only reinforces the impressive character of the feat the Taliban accomplished in ruling 90% of the country's territory at the peak of their power.

Before the advent of the Taliban in Afghanistan, the country had already been devastated by more than twenty years of civil war. The monarchy ended in 1973 when Sardar Mohammed Daud established for the first time a Republic in Afghanistan, hence becoming its first president. Eventually, Daud demanded monetary support from the Soviet Union to modernize the Afghan State. But in 1978, he was assassinated by communist sympathizers within the army in a violent military coup that started a war which would ultimately kill 1.5 million people. After the coup, a Marxist rule was established to last, officially, until 1992. But because the communist party lacked understanding to and regards for Afghanistan's complicated tribal society, rural revolts against the party broke all over the country. Regional tribes' warlords and Islamicists declared a cultural and/or ideological *Jihad* (i.e., holy war) against the communists. Simultaneously, divisions within the communist party itself led to the murder of the first communist president, Nur Mohammed Taraki. In 1979, the Soviet Union intervened and Soviet troops invaded Afghanistan, backing up the Afghan communist government. On the other side, first fighting on their own (and sometimes each other), regional warlords eventually formed a

[48] Unless otherwise stated, most information provided in this section A.1 refers to Rashid (2010). Ahmed Rashid is a Pakistani journalist who covered Afghanistan's wars for about 20 years and had a front-row seat to see the Taliban take control of Afghanistan.

loose alliance of Islamic militants known as the *Mujaheddin*, to which was given US and Saudi financial support administered by the Pakistani Interservices Intelligence (ISI). It is actually this aid, in the form of money and arms, that gave Islamicists the means to increase their influence and radicalize the Afghan society in which they barely had grounds before.[49] Within a few months, Afghanistan was precipitated in the Cold War between the Soviet Union and the US. But if for the Americans the interest in supporting the *Mujaheddin* was to reduce the Soviets' sphere of influence, for Afghan tribes, it was yet one more fight to prevent the submission of their religion, culture and society to a foreign system. At last, the *Mujaheddin* successfully forced the withdrawal of Soviet troops in 1989 and, after a few more years of war, overthrew the Soviet-backed government of Mohammed Najibullah in 1992. But when Kabul fell, it was not to Pashtun tribes which used to rule Afghanistan for 300 years, but to the better organised Tajik forces of Burhanuddin Rabbani and his military commander Ahmad Shah Masud, along with Uzbek forces led by General Rashid Dotsum. Almost instantly, a civil war broke between Pashtun tribes led by Gulbuddin Hikmetyar and the interim Islamist government created by Burhanuddin Rabbani. Meanwhile, former *Mujaheddin* warlords fought each other in different parts of the country.

"Afghanistan was in a state of virtual disintegration just before the Taliban emerged [...]. The country was divided into warlord fiefdoms and all the warlords had fought, switched sides and fought again in a bewildering array of alliances, betrayals and bloodshed" (Rashid, 2010, p. 21). In Pashtun Kandahar, the second-largest Afghan city which would soon become the Taliban's general headquarters, belligerent groups were violently battling each other in the street. Moreover, their leaders were not hesitant to pillage homes, enterprises and farms, selling everything off to Pakistani traders, or to impulsively kidnap and rape children. Banditry on the roads was an important issue too, hence making truck transport virtually impossible. It is in this desolating context that the Taliban emerged, led by Mullah Omar, a former *Mujaheddin* warlord of Pashtun ethnicity. The Taliban's agenda, set against the backdrop of a decadent Afghanistan in the early 1990s, declared the will to "restore peace [in Afghanistan], disarm the population, enforce Sharia law and defend the integrity and Islamic character of Afghanistan" (Rashid, 2010, p. 22). *Taliban* actually means *Students of Islam*, as to reflect the fact that most of the group's original members were Islamic students in *madrassas* (i.e. religious boarding schools) in Pakistan. Uneducated and untrained for any profession, most Taliban members joined the movement very young (between 14 and 24 years old). Rashid, (2010), p. 32, writes that "they admired war because it was the only occupation they could possibly adapt to". A majority of them were sons of Pashtun Afghan refugees born and raised in refugee camps, with no war experience nor professional fighting skills. Because of their segregated upbringing in camps and *madrassas*, most of these boys never knew the company of women and the only thing they could relate to them was what the *mullah* taught them, i.e., that women were a temptation and distraction from serving *Allah*.

The first time the Taliban took arms was in 1994 to free two teenage girls that had been abducted and raped by a warlord in Singesar. The Taliban attacked the base the girls were

[49] While virtually all Afghans were religious, 80% of them were of Sunni Hanafi ideology, one of the most liberal creeds.

taken hostage in, freed them and hanged the site commander. Repeated similar interventions made of Mullah Omar the "Robin Hood" of the poor civilians against the warlords of the Kandahar region. However, Pakistani interests seem to also have played an important role in the emergence of the Taliban. Indeed, at the time, the Pakistani Prime Minister Benazir Bhutto and the Pakistani transport mafia were desperately trying to open a safe trade route through Afghanistan to Turkmenistan, Iran and the rest of Central Asia. After having mandated the Taliban to capture the town of Spin Baldak, a critical trucking and fueling stop on the Afghan-Pakistani border so far controlled by Hikmetyar, Pakistan asked the Taliban for help once again when a trial convoy on the newly decided smuggling route was captured by warlords near Kandahar in 1994. The Taliban, well-armed, freed it and the same evening attacked Kandahar, which rapidly fell. The Taliban then went on to operate a secured toll road, to the delight of Pakistani truckers. By the end of 1994, the Taliban had recruited 12,000 students from Islamic Juma'at al Islamiya (JUI) *madrassas* in Pakistan, and within a few months they had conquered 12 provinces in the Pashtun South by fighting, intimidating or bribing local warlords. They brought security to people, disarmed the population and opened safe roads to traffic such that civilians initially saw them as peacemakers. But this view quickly faltered as the Taliban moved out of the South to defeat major warlords in non-Pashtun provinces, while the civil war was still raging around Kabul and ethnic conflicts persisted in the country. The Taliban, treating fallen territories as occupied rather than trying to win hearts and minds, and being too absorbed by war to care about people's economic well-being,[50] became to Afghans nothing more than another warlord party, if not a more radical one. They took Kabul in 1996, thereby establishing the Islamic Emirate of Afghanistan and forcing Burhanuddin Rabbani, Abdul Rashid Dotsum and Ahmad Shah Masud to flee to the North. Leading a bloody war, the Taliban eventually went on to control 90% of Afghanistan's territory. The remaining territory was controlled by a shaky anti-Taliban alliance of warlords called the *United Islamic and National Front for the Salvation of Afghanistan* or *Northern Alliance*.[51] Under the protection of the Taliban, smuggling trade prospered and so did the opium trade, becoming two of the major Taliban's funding sources alongside aid from Pakistan and Saudi Arabia.[52]

Taking power, the Taliban instituted extreme policies in conquered territories. They subjugated women in particular, to the despair of the international community and the UN, but not only. They also discriminated against non-Pashtun ethnic groups, especially the Hazaras.[53] Men were imposed restrictive norms too, but these arguably restrained their rights and mobility less than they did for women. Examples of Taliban's edicts include dress codes: women were required to wear a *burkha* (i.e., head-to-toes veil) in public and were not allowed to put on

[50] "The Taliban continuously insisted that they were not responsible for the population and that *Allah* would provide" (Rashid, 2010, p. 127).

[51] The Tajik Ahmad Shah Masud, the Hazara Karim Khalili and the Uzbek Abdul Rashid Dotsum (who was betrayed by his second in command Malik Pahlawan and fled before Malik, going back on his pact with the Taliban, took his place) were prominent leaders of the Northern Alliance. Masud, dubbed the "lion of Panjsher", could be seen as the Taliban's worst enemy. He is best known for never having surrendered in the Panjsher Valley to the Soviets or the Taliban during his lifetime.

[52] Saudi Arabia eventually stopped providing funds to the Taliban due to a dispute between the parties over the Taliban refusing to hand in Bin Laden, who was at the time under the Taliban's protection, over to Americans.

[53] Hazaras are Shia Muslims, contrarily to Pashtuns who are Sunni Muslims.

make-up, high heels or more generally noisy shoes, and men had to grow beards (which length was regulated) and wear *shalwars* or baggy trousers above the ankle. Everyone had to pray five times a day. The Taliban also banned many forms of entertainment, including music, TV, photography, videos, cards, kite-flying, singing and dancing at weddings, chess, etc. Hanging a painting or photograph at home was an offense. Most sports were forbidden to all, and absolutely all of them were banned for women. Girls' education was forbidden after 8 years of age (New York Times, 1998) and girls' schools were required to close.[54] Working outside of one's home was prescribed for women,[55] and their comings and goings in the street were strictly regulated. For instance, Muslim women were not allowed to leave their home without a *mehram* (i.e., male blood relative), they were banned from the *bazaar*, and they were rarely allowed to venture outside of their home. At the peak of the Taliban rule, householders were even asked to blacken their windows such that women would not be visible from the street. Women were prohibited healthcare by male doctors and thus men and women were segregated into different hospitals.[56] A patrolling religious police from the Department of the Promotion of Virtue and Prevention of Vice enforced all these policies, and punishments for not respecting them were typically held publicly. Afghan women were also kept away from foreigners and humanitarian aid. They were not allowed to be transported in the same car as a foreigner, and their participation in humanitarian programs was prohibited. The UN and NGOs were caught in the dilemma of whether to stop providing food, financial aid and healthcare in sign of disapprobation of the Taliban's gender policies, or to keep helping the poor local population who needed them. Moreover, the Taliban's policies to be respected in order for NGOs to operate made it so difficult to provide humanitarian aid in general, and especially to women, that many programs had to be shut down. Additionally, some Taliban members tried on purpose to provoke crises with the UN to force them out of the country,[57] as they thought that these agencies spread secular Western ideas. From all of the above, a decrease in humanitarian aid resulted, which drastically hit many poor Afghans who could not afford their daily consumption and relied on humanitarian aid to survive.

In the year and a half before the September 11 2001 terror attacks, the economic and social situation in Afghanistan deteriorated quickly. Moreover, the influence of Osama Bin Laden, who at the time was under Taliban protection, increased strongly. Afghanistan had become a base for extremists from all over the world to come and learn battlefield tactics and bomb making in Al Qaeda camps; and Osama Bin Laden[58] convinced Mullah Omar to

[54]The Taliban maintained that they did not want to forbid girls' education but that they did not have sufficient resources and staff to provide separate education for boys and for girls. The ban on girls' education was supposed to be a temporary measure until separate schools could be provided for girls.

[55]In provinces administered by the Northern Alliance, women were allowed to work and girls to go to school. Indeed, if many former *Mujaheddin* warlords, including Masud, also were Islamicists, their ideology lay nowhere near that of the Taliban.

[56]In Kabul, only one hospital was dedicated to women. Working in healthcare was the only profession women were still allowed to perform outside of home.

[57]The Taliban eventually succeeded at forcing the UN to leave Kandahar.

[58]Osama Bin Laden first came to Afghanistan alongside many Muslim radicals from all over the world during the Soviet war to help the *Mujaheddin*, but he then mostly used his wealth to spread *Wahabbism* and set up Al Qaeda. He returned to Saudi Arabia in 1990 and disapproved of the American intervention in Kuwait. The Americans remaining in Saudi Arabia after Kuwait's liberation, he declared a *Jihad* to the US in 1996. He moved back to Afghanistan in 1996 and to Kandahar in 1997. He was then under Taliban protection.

take a stronger stance against Western institutions, as for instance by ordering the closure of foreign hospitals or the shut down of the UN's World Food Programme (WFP) bakeries. On 9 September 2001, Al Qaeda successfully organised the assassination of Ahmad Shah Masud. The murder, originally planned for weeks earlier, aimed at giving the Taliban a chance to defeat the Northern Alliance and take control over the whole of Afghanistan, before the 11 September attacks occurred. In doing this, Bin Laden ensured himself a safe sanctuary in Afghanistan despite anticipated retaliation by the US. And indeed, after the 11 September 2001 terror attacks, everything happened very fast. Within days, the Bush administration declared war to international terrorists and a state of emergency. They demanded to the Taliban to give up power and hand in Bin Laden over, which Mullah Omar firmly refused. The US thus launched with the help of the NATO coalition forces the operation *Enduring Freedom* on 7 October, aiming at ending Al Qaeda's use of Afghanistan as base, capturing Bin Laden and removing the Taliban from power. But the USA were reluctant to send troops on the ground in Afghanistan and instead partnered with the Northern Alliance which was charged with the ground work, while they carried out a bombing campaign. Within approximately one month, the Northern Alliance took control of North, West and Central Afghanistan, forcing the Taliban to retreat to the South. And only a month later, Mullah Omar abandoned Kandahar and fled, along with many other Al Qaeda and Taliban leaders, including Bin Laden. Following a conference near Bonn, a new interim government in Kabul was established, presided by the Pashtun Hamid Karzai, who promised a "broad-based, gender-sensitive, multi-ethnic and fully representative government" (Rashid, 2010, p. 221).

What happened after 2001 is out of the scope of this paper. However, it matters greatly to understand the return of the Taliban to power in August 2021. Indeed, although the Taliban had been defeated in 2001, their leadership structure was almost entirely intact, such that they could regroup in Pakistan, where most fled. After having gathered resources (notably clandestine funding by the ISI), they re-emerged in early 2003 as an insurgency movement, with more elaborated war and propaganda tactics than ever before.[59] Their attacks were at first concentrated in the southern provinces, and the Taliban did not really represent a threat elsewhere. But as the Bush administration refused to send more troops and funding for Afghanistan's reconstruction, the insurgency violence increased from 2006 onwards and the Taliban eventually took more ground; such that in 2009, they had a significant presence throughout the country again (especially in the southern, eastern and some northern provinces) but no territorial control (Roggio, 2022). The Taliban typically targeted the Afghan administration in their attacks. Hence in 2006, 187 schools were burnt down by the Taliban, and 85 teachers and more than 600 policemen were killed. Suicide bombing also became particularly used, creating more and more civilian casualties. When Barack Obama took office in 2009, more military power was devoted to fighting in Afghanistan and training an Afghan army and police. In 2012 at last, the Taliban threat was minimized, and the Taliban were repelled in some southern and eastern regions (Roggio, 2022). However, it was publicly known that the American occupation was only to last for a few years, so the Taliban hid and waited for the withdrawal of the American

[59]The ties the Taliban forged with Al Qaeda transformed them from a peasant army into an organized insurgency with affiliation to international *Jihad* within the movement leadership.

troops (Roggio, 2022; Council on Foreign Relations, 2022). In mid-2013, the responsibility of securing the country was handed over to the Afghan army and the US stroke a deal to maintain some military presence in the country (Council on Foreign Relations, 2022). But as US-led coalition forces were gradually removed from Afghanistan,[60] the Taliban fought to regain territory (Roggio, 2022; Council on Foreign Relations, 2022), despite US-Afghan and intra-Afghan peace talks (Council on Foreign Relations, 2022). By mid-June 2021, they were predominant in most provinces (Roggio, 2022) and when Joe Biden recalled the last American troops, the Taliban moved swiftly such that by August 2021 the Afghan government had collapsed as the Taliban seized Kabul again (Council on Foreign Relations, 2022), this time controlling virtually the whole country (Roggio, 2022).

A.2 Radical Religious Groups, Sects and the Taliban's Radical Islam

In this paper, the terms *sect* and *radical religious group* are used interchangeably to describe a religious group whose practices can be clearly distinguished from predominant and traditional confessions. A sect does not have to be bad, violent, nor of a specific faith, but solely characterizes a group engaging in unusual and/or extremist religious behaviors. The term *radical religious militia* is, by contrast, used to refer to an organized radical religious group using violence to achieve a given goal. Such militia does not have to be of a specific confession either.

Given the above definitions, the Taliban can be labeled both as a radical religious group, or sect, and as a radical religious militia. Their radical practice of Islam clearly departs from the traditional Muslim faith by its extremism, use of violence and lack of tolerance.[61] Moreover, the Taliban have fiercely subjugated women in the name of religion, while Mohammed is traditionally known for emancipating women (Rashid, 2010, p. 107). Generally, the Taliban have drastically augmented traditional Muslim prohibitions and customs (Berman, 2003; Berman and Laitin, 2008), leaving no room for differing interpretations and liberty of choice. No discussion of the Taliban's interpretation of the Koran was tolerated; in fact, the Taliban implemented in Afghanistan the contemporaneous "strictest interpretation of the Sharia law ever seen in the Muslim world" (Rashid, 2010, p. 29).

The Taliban's ideology did not reflect any of the radical Islamicists trends which appeared in the country during the Soviet war, and which were preached by several *Mujaheddin* warlords, including Masud (Rashid, 2010). Their ideology and their *Sharia* law were influenced by an extreme form of *Deobandism*,[62] inherited from the JUI *madrassas* in Pakistan, and by the *Pashtunwali*, i.e., Pashtun tribal code (Rashid, 2010). But the Taliban's practices and beliefs were more extreme than those of Deobandism, which incorporates a tradition of learning and reform. The Taliban, at the opposite, rejected modernism and economic development (Rashid,

[60] Donald Trump, however, paused for some time the withdrawal plan and shifted strategy, announcing that troops withdrawals would be based on conditions on the ground and that the war in Afghanistan may be prolonged with regards to the increase in insurgency violence (Council on Foreign Relations, 2022).

[61] Traditional Islam encourages respect and tolerance and disapproves of the use of violence against fellow Muslims (Berman and Laitin, 2008; Rashid, 2010).

[62] Deobandism is a form of Sunni Islam which appeared in India under the impulsion of Mohammed Qasim Nanautawi and Rashid Ahmed Gangohi; they typically repudiated Shia Muslims and had restrictive views on gender roles (Rashid, 2010).

2010). The latter author, p. 88, reports that "the Taliban represented nobody but themselves and they recognized no Islam except their own". Important for this study is that, contrary to the Taliban, Islamicist trends represented by former *Mujaheddin* warlords, including those leading the Northern Alliance, were not opposed to modernism: thew were relatively forward-looking, wanted to develop an Islamic economy and social system, and encouraged women's education and participation in social life (Rashid, 2010).

A.3 Education in Afghanistan

Since this paper focuses on assessing the long-term impact of the Taliban rule on education, it is important to have some notion of how the Afghan schooling system is designed.

According to NUFFIC (2015), Afghan children typically start school between the ages of 6 and 8. The report mentions that before 1975, children would undergo 6 years of primary education, followed by 3 years of lower secondary education and 3 years of upper secondary education (system 6 + 3 + 3); and that between 1975 and 1990, the education system was transformed into a 8+4 system, before being changed back in 1990 to the 6+3+3 system. After completion of lower secondary school, children can choose as alternative to upper secondary education a vocational training lasting between 3 to 5 years. Primary school is intended for children aged 6 to 11; lower secondary school, or middle school, for children aged 12 to 14; upper secondary school for children aged 15 to 17 and vocational training for children aged between 15 and 19. Higher education does not target a specific age group. In 2008, schooling was legally made compulsory for 9 years, i.e., up to completion of lower secondary education; and for children aged 6 to 14 (NUFFIC, 2015).

In consideration of the above, compulsory school age is defined for the purpose of this study as 6 to 14 years old.[63] We further take ages 6 to 20 as a more general schooling age definition.[64] This is an approximation attempting to account for the fact that some people might have started school later on and/or might have undergone different schooling paths (e.g., vocational training or higher studies).

B Appendix: Literature Review

This section discusses the key academic literature on the rationality of radical religious groups and militias, their socioeconomic impacts and the impact of conflicts on education in general.

B.1 The Functioning of Radical Religious Groups

Many behaviors adopted by radical religious groups, or sects, are puzzling to people who assume rationality in individual decisions. For instance, Jehovah's Witnesses reject blood transfusions (Religion Facts, 2021*b*) possibly at the risk of their life, and sacrifice much of their time door-to-door proselytizing (Religion Facts, 2021*b*); Amish people oppose education above grade eighth

[63] It is hence indirectly assumed that the law defining the legal age of schooling and adopted in 2008 was based on prevalent practices.

[64] This definition is used as robustness test.

and discard electricity and other modern technology (Religion Facts, 2021*a*), hence considerably complicating their daily lives and access to well-paying jobs; and Orthodox Jews respect strict dress codes and dietary restrictions (Religion Facts, 2021*c*), thus encouraging stigmatization and impeding a social life outside of their religious community. Why are such sacrifices and prohibitions, from which individuals derive no direct utility, voluntarily performed and respected in religious sects? Why would members deliberately endure social stigmas, destruct valuable resources and renounce on opportunities? And these examples of practices are not directly hurting non-members. Indeed, even more confusing are terrible acts of violence some radical religious militias commit, such as terror attacks, killings of civilians, and subjugation of specific groups of people (e.g., women, nonbelievers, etc.). Iannaccone (1992) proposed to view religious groups as providers of goods and services to their members; and developed a club good framework to explain the imposition of prohibitions and sacrifices by peaceful sects, to rationalize their voluntary practice by members, and to discard the argument of irrational zealotry or mental disability. Berman (2003) and Berman and Laitin (2008) then extended the club good model to account for violent radical religious groups (e.g., the Taliban, Hamas, Hezbollah, The Jewish Underground), hence throwing light on why sects that start peaceful may turn violent and why they eventually form extremely effective militias. The model can also rationalize seemingly gratuitous acts of violence on non-members, such as the seclusion and subjugation of women, destruction of schools or closure of hospitals (Berman, 2003). It even allows to explain terror attacks and when and why radical religious militias may opt for suicide bombings (Berman and Laitin, 2008).

The club good framework[65] paints prohibitions as an alternative form of taxation. The model assumes that individuals gain utility from secular consumption (S), religious activities (R) and an excludable and non-rival[66] good (A), provided by the government and/or the religious community (e.g., security, schools, health services, etc.)[67]. Its production by the religious group depends on the average religious input (i.e., non-market time) of its members. Accounting for a budget constraint on secular consumption, individuals then decide how to divide their time between secular (e.g., wage work) and religious activities, in order to maximize their utility. Given that each individual maximizes their own utility without accounting for the external benefit of their participation in religious activities to other group members, the average religious input R^*, i.e., the quality of the club good, is lower at the competitive equilibrium than its efficient level R^{**}. To internalize this externality, religious groups would like to either subsidize religious time, or tax market time/consumption, since market work or secular activities are seen as a distraction from the time one could invest in religious activities. This being practically difficult, if not impossible, religious groups thus rely on prohibitions to reach the efficient level of religious participation R^{**}. The goal of these prohibitions (e.g., dress codes inducing stigma, dietary restrictions making socializing outside of the group more difficult and/or more expensive) is thus to lower the utility members can derive from their outside option in order to induce

[65] See Iannaccone (1992); Berman (2003); Berman and Laitin (2008).
[66] Or partially rival.
[67] For instance, the Taliban provided security for traffic and agriculture in Afghanistan.

them to spend more time in religious activities and less in secular ones. Prohibitions are usually enforced by threatening the member's access to the club good.

Sacrifices in the club good framework are to be understood as signaling devices to uncover type and reduce free-riding in the context of unobserved type heterogeneity among potential group members. Indeed, individuals valuing religious activities less and/or having a higher outside option (e.g. a higher market wage), and thus relying on the community less, may be less committed than others and more likely to free-ride. Since free-riding reduces the overall quality of the club good R^*, and with it, members' utility, more committed members may voluntarily undergo a costly sacrifice (e.g., time, resources, options) to reveal one's type and exclude less committed members, for whom the sacrifice is too costly. For this to work, the utility cost of this sacrifice to committed members must be compensated by the increase in utility derived from a higher club good quality following the exclusion of free-riders. Sacrifices are thus used as a tool to reach a separating equilibrium.

The above links to the explanation in Berman (2003) of why radical religious groups form extremely effective militias. Because these groups already impose important sacrifices, they can effectively mitigate the risk of defection, to which militias are extremely sensitive.[68] In fact, sacrifices allow to screen out non-committed and likely to defect members, hence yielding militias more effective and able to tackle higher value projects than other armed groups (Berman, 2003; Berman and Laitin, 2008). The Taliban, for instance, imposed on their members several years of studying the Koran in *madrassas*. This can be seen as an important sacrifice of time for rather poor people who arguably would not be able to use this learnt knowledge for profit (Berman, 2003; Berman and Laitin, 2008).

As for destructive acts of violence performed by radical religious militias on non-members, although they can be understood as signals of commitment, they mostly aim at destroying members' outside option in order to decrease their incentive to defect and increase their dependency on the group. Indeed, by imposing their norms to all and destroying the resources available to non-members, a radical militia ensures that its members know they would not find better conditions among the civilians (Berman, 2003). In the case of Afghanistan, this may explain the closure of schools and hospitals or the attempts to drive NGOs out of the country. In reducing publicly and locally available (through the government, NGOs, etc.) goods and services, radical religious militias increase people's dependency on the mutual insurance brought by the religious community (Berman, 2003). Berman (2003) indeed highlights that radical religious militias mostly emerge and thrive in countries where the government is weak and where market opportunities are poor. The rationale above may also help explain why it was so important for the Taliban to alienate the population and subjugate women. In fact, secluding them and restricting their rights is a way to reduce households' market wages and outside options (Berman, 2003). Rashid (2010), p. 111, indeed states: "Taliban leaders repeatedly told me that if they gave women more freedom or a chance to go to school, they would lose the support of their rank and file [...]". Besides, the subjugation of women and civilians worsened as the Taliban grew in power and control over resources in Afghanistan (e.g., drug trade, smuggling routes) and left

[68] The operations of illegal militias are typically threatened by information the opponent/government might obtain by defectors, for instance in exchange of a reward (Berman and Laitin, 2008).

the southern Pashtun regions (Rashid, 2010). Because increased rents drive incentives to defect and steal resources up, more radical sacrifices and prohibitions are needed to ensure members' commitment (Berman, 2003).

The club good framework was empirically tested and supported for different peaceful religious communities, such as Christian sects (Iannaccone, 1992), Ultra-Orthodox Jews (Berman, 2000), Muslim sects (Berman and Stepanyan, 2004) and Amish communities (Wang, 2020). Empirical support for the model was also provided for radical religious militias in different countries, including Israel and Palestine, Lebanon, Iraq, Chechnya and Sri Lanka (Berman and Laitin, 2008).

B.2 Socioeconomic Impact of the Taliban Rule in Afghanistan and of Radical Religious Militias in General

If, as argued above, apparent acts of zealotry and the imposition of extremist norms by radical religious militias can in fact be rationally explained, they are nonetheless damaging for members and non-members of these groups; and only a few academic papers have attempted to quantify the extent of these damages for the affected people.

As mentioned, most studies investigating the socioeconomic impact of radical religious militias use the prevalence of violent acts as independent variable and thus study the impact of conflicts in the name of radical religious ideas on people. An example of such study, outside of the Afghan context, is Bertoni et al. (2019), which discusses the impact of the Boko Haram conflict in North-East Nigeria on education. Boko Haram is a radical religious militia originally from Nigeria but active also in Cameroon, Niger and Chad. Using a panel fixed-effects model with variation over time and across villages in conflict intensity, Bertoni et al. (2019) find that one standard deviation increase in the number of fatalities in a 5km radius around a child's village reduces their probability of school enrollment by around 3 percentage points (pp). This effect is even larger for children who were no longer of mandatory school age. The authors additionally conduct a difference-in-differences analysis and reveal that the Boko Haram conflict over the 2009-2016 period reduced people's total years of education. They report that this effect is larger for boys.

Ekhator-Mobayode et al. (2022) also investigate the impact of the Boko Haram conflict, but on women's experience of intimate partner violence (IPV), controlling behaviors from husband, autonomy in household decisions and control over their own earnings. Using a kernel-based difference-in-differences model and controlling for different IPV risk factors, the authors find that between 2008 and 2013, the Boko Haram insurgency increased a woman's probability to experience IPV by 3.7 pp, and that of experiencing controlling behaviors from husband by 13.8 pp. It also impeded women's agency.

In a work in progress, Stoelinga (2022) also focuses on Boko Haram using a difference-in-differences approach. She examined the impact of the militia's occupation on educational outcomes. Note that, contrary to the above, she does not study the impact of insurgency violence on human capital, but that of the Boko Haram rule. Her results indicate that children who experienced Boko Haram occupation have 1.24 years of education less than those who

experienced the insurgency but did not live in the caliphate, and are 23 percent less likely to attend school in the years following the occupation. Stoelinga (2022) finds that the negative impact of the Boko Haram rule is larger for boys than for girls in terms of completed years of schooling, but smaller for boys than for girls in terms of school attendance after the occupation. Moreover, she finds that children born between 2003 and 2005 (who thus were 9 to 11 years old when living in the caliphate) were more strongly affected by Boko Haram's occupation in terms of completed schooling. Stoelinga (2022) explores many different potential mechanisms underlying these effects, such as child labor, marriage, health, school supply, labor market trends and returns to education, but none seems to explain the results. She, however, finds some evidence that having a shared social identity with Boko Haram, peer or network effects, and intimidation and fear drive the effects. Thus, Stoelinga (2022)'s research suggests that social identity and social pressure are key to explain how individuals adapt their behaviors in response to occupation.

In a different context, Alfano and Görlach (2019) estimate the impact of Al-Shabaab terror attacks on primary school enrollment and attendance in Kenya. Al-Shabaab is a Somali radical Islamist militia with ties to Al Qaeda and also active across the border in the northeastern regions of Kenya. Alfano and Görlach (2019) exploit variation across Kenyan regions in terrorism increase from the late 2000 onwards in a panel and a difference-in-differences framework. Using different data sets, they find that Al-Shabaab terror attacks importantly decrease enrollment rates and school attendance, in a similar manner for boys and girls. They confirm their results with various instrumental variable strategies, combining distance to Somali border (location prediction of attacks) with either exploitation of Al-Shabaab's position within the Al-Qaeda network, revenue streams for Al-Qaeda derived from Yemen's exports or Al-Shabaab's main source of income, i.e. the export of charcoal (timing prediction of attacks).[69] Alfano and Görlach (2019) are further interested in documenting that the destruction of school supply is not the only mechanism driving the results. They want to highlight the importance of a change in expectations about the risks and returns associated with schooling, which they successfully do in three different ways. First, they show that attacks too far from a child's school for them to be affected by a loss of school infrastructure or personnel still significantly and negatively impact the child's educational outcomes. Secondly, they provide evidence that threats and media coverage of terrorist attacks also negatively impact educational enrollment. Finally, they investigate attitudes directly and find no effect of terror attacks on self-reported experience of violence, but large effects on concerns about violence.

B.3 Conflicts and Education

Aside from the socioeconomic effect of radical religious militias specifically, the academic literature has taken great interest in the impact of violence and conflicts on education in general.

[69] Alfano and Görlach (2019) show that Al-Shabaab closely follows the timing of attacks committed by the Al-Qaeda branch in Yemen, i.e., Al-Qaeda in the Arabian Peninsula (AQAB); and thus can use it as an instrument. Similarly, they show that Yemen's exports of hydrocarbons increase the intensity of attacks by both AQAP and Al-Shabaab (second instrument) and that Somalian coal imports by the UAE increase Al-Shabaab's attacks (third instrument).

For instance, Shemyakina (2011) examines the effect of exposure to the Tajik civil war between 1992 and 1998 on boys' and girls' probability of completing their compulsory schooling and on school enrollment rates just after the conflict. Using data from the 1999 and 2003 Tajik Living Standards Surveys, she exploits differences in exposure to the war across regions, birth cohorts and households and finds that, while boys' education was not significantly affected by the Tajik armed conflict, girls who were of school age during the war and lived in conflict-affected regions are 7.3 percent less likely to complete their mandatory schooling than similarly aged girls unaffected by the war. Moreover, she finds that the school enrollment rate of treated girls in 1999 is significantly lower than that of untreated girls by approximately 12 pp.

In a different context, León (2012) also finds that conflicts have negative effects on human capital accumulation. He studies the short- and long-term impact of the 1980-1993 civil war in Peru on people's educational attainments exploiting temporal and geographical variation in conflict incidence within the country. Using a panel model and relying on a large set of fixed effects and time-trends, he identifies that the effect of the conflict on education is larger in the short-term than in the long-term but persistent, particularly if exposure to violence happened in the early years of life. León (2012) reports that individuals exposed to violence after having started their schooling are negatively affected in the short-term but are able to fully catch up in the long run with their peers who were not affected by the war. On the other hand, he finds that individuals who experienced violence in early childhood and preschool years only partially recover in the long run and that individuals exposed to conflicts while in utero suffer irreversible human capital losses. Quantitatively, León (2012) reveals that an average person experiencing the Peruvian civil war before starting school (while in utero, in early childhood or of preschool age) accumulated 0.31 years of schooling less upon reaching adulthood than untreated individuals.

Chamarbagwala and Morán (2011) further examine the impact of exposure to civil war on education in another Latin American country, namely Guatemala; and attempt to identify which demographic groups were most strongly affected by the war. Between 1960 and 1996, the country experienced a 36 year-long conflict separated into three periods: the initial period (1960-1978), the worst period (1979-1984), where the large majority of the war's human rights violations were committed, and the final period (1985-1996). Running a difference-in-differences model, the authors find that the war strongly reduced human capital accumulation of the most disadvantaged groups (rural Mayan males and females) and as such intensified gender, regional and ethnic disparities. They estimate that among rural Mayan males, those who were of school age during the three periods of war in high war intensity departments[70] completed respectively 0.27, 0.71 and 1.09 years of education less. For rural Mayan females, they equivalently find that exposure while of school age to high war intensity during the three periods of the conflict reduced completed years of schooling by 0.12, 0.57 and 1.17 years, respectively. The authors

[70]Conflict intensity is measured at the department level by the number of human rights violations (per 1000 population) or the number of victims (per 1000 population) in a given year. Based on these measures, departments are then categorized as *high war intensity* or *low war intensity* (treatment groups). Individuals' exposure to the conflict while of school age is determined based on their year of birth and department of birth.

argue that these decreases are mostly driven by war-induced lower likelihood of completing primary school grades, rather than higher school grades.

Brück, Di Maio and Miaari (2019) investigate the effect of the Israeli-Palestinian conflict in the West Bank between 2000 and 2006 (Second Intifada) on Palestinian students' test scores in the national high school exit exam. Their fixed-effects model exploits within-school variation over time in the number of Palestinian fatalities during an academic year[71] and shows that one standard deviation increase in conflict intensity reduces students' probability of passing the final exam by 1 pp, their total scores by 4 pp and their probability of admission to university by 0.01 pp. The authors also reveal that achievements of students in the upper tail of the scores' distribution are unaffected by conflict exposure. Moreover, the magnitude of the effects of violence on test scores varies with the type and timing of violent events. Brück, Di Maio and Miaari (2019) then consider several mechanisms underlying the estimated effects, such as the deterioration of learning conditions at school and a worsening of students' psychological well-being. They report that the conflict increases student density in classes, which in turn correlates with lower test scores; and that the negative effect of violent events happening very close in time to the exam date is stronger, which is consistent with a psychological channel.

Running several difference-in-differences models, Justino, Leone and Salardi (2014) study the short- and long-term human capital consequences of the 1999 wave of violence accompanying the withdrawal of Indonesian troops in Timor-Leste. In the short-term, the authors find that children from families who reported having been displaced during the 1999 wave of violence are 8.5 pp less likely to be enrolled in primary school in 2001, with a stronger effect for boys. Moreover, children from families reporting both damage to their dwelling and being displaced are 13.3 pp less likely to attend school in 2001. This effect is found to be larger for girls and younger children. In the longer-term, Justino, Leone and Salardi (2014) reveal that boys exposed to violence during their primary school years are 18.3 pp less likely to have completed primary school by 2007 than boys not exposed to violence. On the contrary, girls exposed to violence during primary school are 10.4 pp more likely to have completed primary school in 2007.[72] The authors hypothesize that the rapid recovery of girls' educational outcomes drew upon the major reconstruction effort after the 1999 violent episode.[73] They show supportive evidence that differing gender roles and heightened economic needs in the aftermath of the war likely induced boys to leave school for work. Justino, Leone and Salardi (2014) find that boys affected by the 1999 violence were 11 pp more likely to work in 2001 while girls were 3.6 pp less likely.

Not all of the relevant literature focuses on civil wars. Brown and Velásquez (2017), for instance, investigate the effect of the sudden, unanticipated[74] and geographically heterogeneous

[71] The number of Palestinian killed by Israeli Defense Forces at the locality level during an academic year is used as proxy to measure conflict intensity.

[72] Justino, Leone and Salardi (2014) separately study the effect on education of earlier episodes of high violence during the 25 year-long war, as well as the effect of the conflict duration as a whole. Doing so, they also find that in the long run, the education of girls was unaffected (or even improved) by the war while that of boys was significantly reduced.

[73] This effort was accompanied by a strong consideration of gender concerns (Justino, Leone and Salardi, 2014).

[74] Brown and Velásquez (2017) claim that the strengthening of the drug war's intensity in Mexico in the late 2000s is exogenous to the country's socioeconomic situation, as experts attribute it to the Mexican government's

surge in drug-related crime in Mexico in the late 2000s on young adults' educational attainments and employment behaviors. Using both individual fixed-effects regressions and a triple differences methodology, the authors find that an increase in the local homicide rate is associated with a decrease in completed years of education for men, but not for women. Precisely, a young male in an initially violence-free municipality subsequently experiencing the average homicide rate rise between 2005 and 2009 (approximately 15 per 100,000 people) completed on average about 0.3 years of schooling less than untreated men. Similarly, other educational outcomes (compulsory school completion, school attendance) of young women were unaffected by heightened violence while that of young men substantially deteriorated. Brown and Velásquez (2017) test several mechanisms possibly underlying the effects: fear of bodily harm, destruction of schooling supply and financial hardship/opportunity. They find no supportive evidence for the first two channels but do find for the last one. They observe that the educational attainments of individuals whose parents are self-employed[75] are more strongly affected by the increase in drug-related crime. They also report that young men exposed to local violence are more likely to work, especially if their parents are self-employed.

A last example of the literature on the effect of violent conflicts on human capital, still related to the illicit drug trade, is Monteiro and Rocha (2017). They take advantage of arguably exogenous variation in drug-related violence[76] over time and space in Rio de Janeiro's favelas to estimate the impact of these events on students' test scores in a national standardized math exam taken by fifth-graders. The authors build their own data set on drug gangs' gunfights based on thousands of anonymous reports made to a police hotline between 2003 and 2009 and match it with information on educational achievements of students in schools located in or near favelas. Running a fixed-effects model, Monteiro and Rocha (2017) find that drug gangs' conflicts within 250 meters from a school during the academic year are associated with a 0.054 standard deviation decrease in students' test scores. This effect increases with conflict intensity and proximity in time of the violent event to exam date. On the other hand, it decreases with distance between the school and the gunfight's location. Monteiro and Rocha (2017) identify loss of school supply as an important mechanism driving the results. They report that drug-related violence in Rio de Janeiro's favelas is associated with higher teacher absenteeism, principal turnover and temporary school shutdowns.

focus change in its battle against drug cartels from crop eradication to active search and capture of cartels' leadership.

[75]Economically, the self-employed were the ones most strongly impacted by violence (Brown and Velásquez, 2017).

[76]Monteiro and Rocha (2017) argue that conflicts between drug gangs in Rio de Janeiro are mostly unplanned and triggered by events such as betrayals, imprisonment/release of drug lords and revenge. As such, drug-related violent events are arguably exogenous to the local socioeconomic conditions of favelas.

C Appendix: Tables and Figures

Table 1: Descriptive Statistics

	Women			Men		
	(1) Control provinces	(2) Treated provinces	(3) All	(4) Control provinces	(5) Treated provinces	(6) All
Observations	1,455	26,177	27,632	443	10,159	10,602
Percent	3.56%	96.44%	100.00%	3.08%	96.92%	100.00%
Total years of education	1.51	1.15	1.17	3.66	4.13	4.12
	(3.43)	(3.11)	(3.12)	(4.81)	(4.93)	(4.92)
Literacy rate	0.17	0.14	0.14	0.44	0.49	0.49
	(0.38)	(0.34)	(0.35)	(0.50)	(0.50)	(0.50)
Primary school completion rate	0.14	0.10	0.10	0.29	0.37	0.36
	(0.35)	(0.30)	(0.30)	(0.46)	(0.48)	(0.48)
Labour market participation rate	0.02	0.14	0.13	0.96	0.97	0.97
	(0.13)	(0.35)	(0.34)	(0.19)	(0.17)	(0.17)
School-aged exposure	0.00	0.59	0.57	0.00	0.51	0.49
	(0.00)	(0.49)	(0.50)	(0.00)	(0.50)	(0.50)
Preschool-aged exposure	0.00	0.23	0.22	0.00	0.13	0.13
	(0.00)	(0.42)	(0.42)	(0.00)	(0.34)	(0.34)
Years of school-aged exposure	0.00	2.39	2.30	0.00	2.21	2.15
	(0.00)	(2.49)	(2.49)	(0.00)	(2.56)	(2.55)
Years of preschool-aged exposure	0.00	0.74	0.72	0.00	0.38	0.37
	(0.00)	(1.58)	(1.55)	(0.00)	(1.14)	(1.12)

Notes: The table reports the mean value and the standard deviation (in parenthesis) of each outcome variable. The statistics are produced with a sample from the 2015 Afghanistan DHS of ever-married women (respectively men) aged 20 to 49 years old. All statistics are weighted with sample weights. Control provinces refer to provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. All the other provinces are considered treated.

Table 2: Difference-in-Differences Results without Treatment Intensity

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation
Panel A: Women				
Taliban control x School-aged	0.00708	-0.00425	-0.00283	0.00784
	(0.373)	(0.0410)	(0.0332)	(0.0193)
Taliban control x Preschool-aged	-0.442	-0.0466	-0.0406	0.00711
	(0.367)	(0.0347)	(0.0379)	(0.0143)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579
R^2	0.348	0.342	0.292	0.313
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x School-aged	-0.579*	-0.0558	-0.0649**	0.0150
	(0.336)	(0.0411)	(0.0287)	(0.0134)
Taliban control x Preschool-aged	-0.316	0.0276	-0.0545	0.0159
	(0.489)	(0.0611)	(0.0397)	(0.0133)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.573	0.589	0.504	0.972
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of having been of compulsory school age (6-14) and of preschool age (0-5) under Taliban rule for at least one year on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *School-aged* is an indicator variable taking value one if the individual was of compulsory school age (6 to 14 years old) at the time of the Taliban rule and *Preschool-aged* is an indicator variable taking value one if the individual was in their early childhood (0 to 5 years old) at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression. Unweighted results are reported in the Appendix.

Table 3: Difference-in-Differences Results with Treatment Intensity

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation
Panel A: Women				
Taliban control x Years of school-aged exposure	-0.0807***	-0.0107***	-0.00564*	-0.000356
	(0.0247)	(0.00369)	(0.00282)	(0.00283)
Taliban control x Years of preschool-aged exposure	-0.485***	-0.0466***	-0.0443***	0.0146
	(0.130)	(0.0130)	(0.0120)	(0.0117)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579
R^2	0.354	0.346	0.298	0.313
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x Years of school-aged exposure	-0.0817	-0.0102	-0.00376	0.00634**
	(0.108)	(0.0105)	(0.00942)	(0.00301)
Taliban control x Years of preschool-aged exposure	-0.260	-0.0110	-0.0353**	0.00283
	(0.168)	(0.0199)	(0.0159)	(0.00809)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.573	0.589	0.504	0.972
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of one additional year of compulsory school-aged (6-14) and of preschool-aged (0-5) exposure to the Taliban rule on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *Years of school-aged exposure* is a variable ranging from 0 to 8 and indicating the number of years an individual was of compulsory school age (6 to 14 years old) at the time of the Taliban rule; and *Years of preschool-aged exposure* is a variable ranging from 0 to 6 and indicating the number of years an individual was of preschool age (0 to 5 years old) at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression. Unweighted results are reported in the Appendix.

Table 4: Treatment Effect Heterogeneity by Age at the Taliban Arrival

	Women				Men			
	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation	(5) Total years of schooling	(6) Literacy	(7) Primary school completion	(8) Labor force participation
0-	-3.510***	-0.364***	-0.326***	0.107	-1.961	-0.189**	-0.243*	0.0406
	(0.683)	(0.0713)	(0.0646)	(0.0789)	(1.266)	(0.0916)	(0.125)	(0.0598)
1	-3.228***	-0.305***	-0.295***	0.0216	-1.959**	-0.182***	-0.246***	0.0516
	(0.506)	(0.0502)	(0.0470)	(0.0514)	(0.869)	(0.0652)	(0.0753)	(0.0491)
2	-2.432***	-0.236***	-0.211***	-0.00223	-2.261***	-0.228***	-0.256***	0.0734*
	(0.468)	(0.0519)	(0.0469)	(0.0358)	(0.719)	(0.0646)	(0.0733)	(0.0375)
3	-1.950***	-0.193***	-0.174***	0.0241	-1.864**	-0.214**	-0.243***	0.0814**
	(0.261)	(0.0233)	(0.0283)	(0.0335)	(0.911)	(0.105)	(0.0885)	(0.0318)
4	-1.240***	-0.148***	-0.122***	-0.0434	-1.559***	-0.0670	-0.144**	0.0678**
	(0.241)	(0.0266)	(0.0260)	(0.0278)	(0.552)	(0.0742)	(0.0581)	(0.0303)
5	-0.941***	-0.101***	-0.0775***	0.0166	-0.892*	-0.0945	-0.104*	0.0666**
	(0.189)	(0.0268)	(0.0228)	(0.0243)	(0.515)	(0.0588)	(0.0599)	(0.0271)
6	-1.050***	-0.116***	-0.0966***	-0.0133	-1.080	-0.157	-0.0893	0.0739***
	(0.347)	(0.0353)	(0.0332)	(0.0282)	(0.673)	(0.0941)	(0.0630)	(0.0270)
7	-0.455	-0.0508	-0.0285	-0.0152	-0.790	-0.125**	-0.0637	0.0373
	(0.347)	(0.0340)	(0.0308)	(0.0284)	(0.739)	(0.0579)	(0.0801)	(0.0275)
8	-0.361	-0.0374	-0.0383*	0.0186	-0.0957	-0.0171	-0.0303	0.0275
	(0.220)	(0.0232)	(0.0214)	(0.0299)	(0.574)	(0.0463)	(0.0630)	(0.0240)
9	-0.159	-0.0221	-0.00685	-0.0142	-1.530*	-0.125**	-0.0877	0.0361*
	(0.374)	(0.0401)	(0.0325)	(0.0252)	(0.883)	(0.0595)	(0.0808)	(0.0192)
10	-0.0978	0.0101	-0.000821	0.0140	-0.374	-0.0494	-0.0183	0.0164
	(0.300)	(0.0295)	(0.0301)	(0.0278)	(0.455)	(0.0508)	(0.0584)	(0.0189)
11	-0.416	-0.0277	-0.0335	0.0210	-0.909*	-0.0537	-0.0696	0.0161
	(0.347)	(0.0329)	(0.0282)	(0.0484)	(0.484)	(0.0458)	(0.0576)	(0.0213)
12	-0.300	-0.00672	-0.0109	0.0176	-1.042*	-0.109*	-0.0933	0.0117
	(0.283)	(0.0244)	(0.0225)	(0.0244)	(0.610)	(0.0552)	(0.0749)	(0.0171)
13	0.124	0.00842	0.00541	-0.0102	-0.568	-0.0662	-0.0470	0.00595
	(0.211)	(0.0223)	(0.0197)	(0.0363)	(0.434)	(0.0480)	(0.0453)	(0.0222)
14	0.0526	0.0282	0.00898	0.0205	-0.316	-0.0393	-0.0534	0.00743
	(0.254)	(0.0287)	(0.0174)	(0.0358)	(0.451)	(0.0589)	(0.0445)	(0.0134)
16	-0.284	0.00368	-0.0157	-0.00215	-0.240	-0.00223	0.0159	0.00474
	(0.374)	(0.0338)	(0.0292)	(0.0288)	(0.508)	(0.0562)	(0.0510)	(0.0168)
17	-0.237	-0.00922	-0.0110	-0.0125	-0.202	-0.0559	-0.0304	-0.0117
	(0.324)	(0.0309)	(0.0277)	(0.0539)	(0.442)	(0.0570)	(0.0434)	(0.0182)
18	-0.120	0.0130	-0.00618	0.0220	0.770	0.0559	0.0172	-0.0275*
	(0.232)	(0.0242)	(0.0201)	(0.0274)	(0.615)	(0.0462)	(0.0597)	(0.0146)
19	-0.141	0.0202	0.00566	-0.0225	1.262*	0.0689	0.110*	0.00225
	(0.286)	(0.0232)	(0.0223)	(0.0439)	(0.629)	(0.0615)	(0.0636)	(0.0180)
20	-0.219	0.0150	-0.00586	-0.00773	-0.0774	-0.0520	-0.0643	-0.0291*
	(0.273)	(0.0265)	(0.0252)	(0.0390)	(0.572)	(0.0526)	(0.0616)	(0.0145)
21	-0.223	0.00614	-0.0106	-0.0150	0.165	-0.0601	0.00503	-0.00395
	(0.401)	(0.0458)	(0.0327)	(0.0287)	(0.623)	(0.0549)	(0.0739)	(0.0203)
22	-0.184	0.0151	-0.00204	0.00239	0.688	-0.0302	0.0318	-0.0280
	(0.294)	(0.0234)	(0.0272)	(0.0303)	(0.445)	(0.0409)	(0.0423)	(0.0182)
23	0.312	0.0493**	0.0387	-0.00506	0.387	0.0830**	0.0116	-0.0287*
	(0.257)	(0.0206)	(0.0266)	(0.0276)	(0.624)	(0.0399)	(0.0602)	(0.0160)
24	-0.114	0.0181	0.00147	-0.0159	1.215**	0.0289	0.114**	-0.0262
	(0.378)	(0.0294)	(0.0313)	(0.0622)	(0.494)	(0.0563)	(0.0548)	(0.0194)
25	-0.0967	0.0175	0.00846	-0.00997	0.971	0.0804	0.0735	-0.00351
	(0.395)	(0.0330)	(0.0343)	(0.0423)	(0.682)	(0.0674)	(0.0680)	(0.0155)
26	-0.0481	0.0119	0.00453	0.00533	-0.215	-0.0487	-0.0622	-0.0358
	(0.477)	(0.0541)	(0.0448)	(0.0523)	(0.848)	(0.0832)	(0.0907)	(0.0224)
27	0.205	0.0327	0.0315	0.0431	1.219*	0.153**	0.136*	0.0103
	(0.364)	(0.0357)	(0.0347)	(0.0736)	(0.600)	(0.0692)	(0.0691)	(0.0277)
28	0.154	0.0244	0.0161	0.0962	0.673	-0.00272	0.0780	-0.0572*
	(0.355)	(0.0396)	(0.0423)	(0.0894)	(0.520)	(0.0575)	(0.0579)	(0.0292)
29	0.261	0.0358	0.0245	0.108	0.603	0.0707	0.0922	0.0153
	(0.372)	(0.0424)	(0.0426)	(0.0963)	(0.596)	(0.0549)	(0.0627)	(0.0237)
30+	0.194	0.0590	0.0279	0.0553	0.766	0.0610	0.123*	0.00654
	(0.417)	(0.0459)	(0.0443)	(0.0851)	(0.806)	(0.0735)	(0.0704)	(0.0277)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579	10575	10560	10575	10591
R^2	0.356	0.348	0.300	0.317	0.577	0.593	0.508	0.972
Controls								

Notes: The table reports the average effect of exposure to the Taliban rule when this exposure starts at age j on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. Each dummy variable indicates whether an individual was the specified age at the arrival of the Taliban in their province of residence. 15 years old at Taliban arrival is used as base. Endpoints are binned, such that the dummy variable *0-* takes value one if the individual was a newborn or yet to be born at the Taliban's arrival and the variable *30+* takes value one if the individual was 30 years old or older at the Taliban's arrival. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regressions. Unweighted results are available in the Appendix.

Table 5: Results of the Analysis at the Start of Schooling

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor market participation
Panel A: Women				
Taliban control x Turned 6	-0.671**	-0.0756**	-0.0669**	-0.00347
	(0.255)	(0.0314)	(0.0288)	(0.0124)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	27561	27537	27561	27579
R^2	0.349	0.343	0.293	0.313
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x Turned 6	-0.426	-0.0330	-0.0473	0.0390*
	(0.450)	(0.0406)	(0.0400)	(0.0214)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.573	0.589	0.503	0.972
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of turning 6 years old under Taliban rule on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *Turned 6* is an indicator variable taking value one if an individual turned 6 years old and thus should begin school at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression. Unweighted results are reported in the Appendix.

Table 6: Results of the Placebo Regressions

Panel A: Fake treatment	Women				Men			
	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labour market participation	(5) Total years of schooling	(6) Literacy	(7) Primary school completion	(8) Labour market participation
Taliban control x 21-24 at Taliban arrival	0.140	0.0126	0.0133	0.0234	0.374	0.00946	0.0570	-0.00821
	(0.353)	(0.0349)	(0.0325)	(0.0322)	(0.512)	(0.0547)	(0.0758)	(0.0160)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5898	5898	5898	5902	2826	2822	2826	2832
R^2	0.285	0.267	0.232	0.357	0.550	0.580	0.497	0.975
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Irrelevant outcome variables	Women		Men	
	(1) tuberculosis	(2) cancer	(3) tuberculosis	(4) cancer
Taliban control x School-aged	0.00401	0.00257	0.0524	-0.00775
	(0.0131)	(0.00530)	(0.0320)	(0.0159)
Taliban control x Preschool-aged	-0.0118	0.00327	-0.00580	0.00728
	(0.0113)	(0.0107)	(0.0416)	(0.0176)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	20995	27535	7525	10573
R^2	0.155	0.078	0.146	0.115
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the results of the different placebo tests implemented. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. In Panel A, the estimation sample includes ever married women (respectively men) aged 37 to 49 in 2015 while in Panel B, it includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *School-aged* is an indicator variable taking value one if an individual was of compulsory school age (6 to 15 years old) at the time of the Taliban rule; *Preschool-aged* is an indicator variable taking value one if an individual was in their early childhood (0 to 5 years old) at the time of the Taliban rule and *21-24 at Taliban arrival* is an indicator variable taking value one if an individual was 21 to 24 years old at the time the Taliban seized their province of residence. *Tuberculosis* is an indicator variable taking value one if the respondent was ever told they had tuberculosis, and *Cancer* is an indicator variable taking value one if the individual was ever diagnosed with cancer. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Table 7: Robustness Tests of DiD Results without Treatment Intensity, for Women

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) School age: 6 to 20	(7) School age 7 to 15	(8) Province-age clustering
Panel A: Years of schooling								
Taliban control x School-aged	0.00708	0.149	0.198	0.171	-0.156	0.0132	0.267	0.00708
	(0.373)	(0.298)	(0.370)	(0.264)	(0.286)	(0.235)	(0.297)	(0.371)
Taliban control x Preschool-aged	-0.442	-0.528	-0.343	-0.298	-0.742***	-0.442	-0.670**	-0.442
	(0.367)	(0.429)	(0.374)	(0.323)	(0.264)	(0.376)	(0.270)	(0.343)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27561	27561	27556
R^2	0.348	0.360	0.364	0.348	0.349	0.348	0.349	0.257
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy								
Taliban control x School-aged	-0.00425	0.00486	0.0153	0.0163	-0.0281	0.00435	0.0104	-0.00425
	(0.0410)	(0.0354)	(0.0409)	(0.0270)	(0.0305)	(0.0253)	(0.0251)	(0.0426)
Taliban control x Preschool-aged	-0.0466	-0.0528	-0.0362	-0.0317	-0.0782***	-0.0467	-0.0756**	-0.0466
	(0.0347)	(0.0403)	(0.0354)	(0.0301)	(0.0241)	(0.0365)	(0.0320)	(0.0342)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27537	29361	22824	27537	27537	27537	27537	27532
R^2	0.342	0.353	0.359	0.342	0.343	0.342	0.343	0.235
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion								
Taliban control x School-aged	-0.00283	0.00971	0.0108	0.0179	-0.0152	0.00306	0.0194	-0.00283
	(0.0332)	(0.0265)	(0.0348)	(0.0243)	(0.0260)	(0.0193)	(0.0231)	(0.0346)
Taliban control x Preschool-aged	-0.0406	-0.0487	-0.0324	-0.0312	-0.0764***	-0.0407	-0.0669**	-0.0406
	(0.0379)	(0.0445)	(0.0391)	(0.0321)	(0.0270)	(0.0391)	(0.0299)	(0.0340)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27561	27561	27556
R^2	0.292	0.302	0.308	0.292	0.293	0.292	0.293	0.215
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labor market participation								
Taliban control x School-aged	0.00784	0.0106	0.000511	-0.00512	-0.00684	-0.00744	0.00295	0.00784
	(0.0193)	(0.0190)	(0.0162)	(0.0158)	(0.00780)	(0.0178)	(0.0204)	(0.0192)
Taliban control x Preschool-aged	0.00711	0.00347	-0.00428	0.0113	-0.0204	0.00735	-0.00346	0.00711
	(0.0143)	(0.0148)	(0.0153)	(0.0150)	(0.0140)	(0.0139)	(0.0124)	(0.0184)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27579	29406	22862	27579	27579	27579	27579	27574
R^2	0.313	0.308	0.317	0.313	0.313	0.313	0.313	0.207
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including women aged 15 to 49 and women aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact that the actual begin of the Taliban rule at the province level is not precisely known. Columns (6) and (7) define school-age differently: a more general definition, including higher schooling, is used in column (6) (ages 6 to 20) and and alternative age of compulsory schooling is used in column (7) (ages 7 to 15). Column (8) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Table 8: Robustness Tests of DiD Results without Treatment Intensity, for Men

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) School age: 6 to 20	(7) Schoo age: 7 to 15	(8) Province-age clustering
Panel A: Years of schooling								
Taliban control x School-aged	-0.579*	-0.617*	-0.512	-0.305	-0.657**	-0.207	-0.287	-0.579
	(0.336)	(0.359)	(0.341)	(0.378)	(0.281)	(0.330)	(0.346)	(0.344)
Taliban control x Preschool-aged	-0.316	-0.308	-0.228	-0.370	-0.824**	-0.314	-0.435	-0.316
	(0.489)	(0.487)	(0.476)	(0.465)	(0.380)	(0.488)	(0.450)	(0.459)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10575	10575	10571
R^2	0.573	0.576	0.596	0.573	0.573	0.573	0.573	0.274
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy								
Taliban control x School-aged	-0.0558	-0.0595	-0.0629	-0.0239	-0.0576*	0.00677	-0.0219	-0.0558
	(0.0411)	(0.0425)	(0.0451)	(0.0410)	(0.0303)	(0.0391)	(0.0361)	(0.0380)
Taliban control x Preschool-aged	0.0276	0.0261	0.0160	0.00990	-0.00520	0.0289	-0.0337	0.0276
	(0.0611)	(0.0613)	(0.0622)	(0.0565)	(0.0886)	(0.0625)	(0.0405)	(0.0767)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10560	10718	8181	10560	10560	10560	10560	10556
R^2	0.589	0.592	0.611	0.589	0.589	0.589	0.589	0.191
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion								
Taliban control x School-aged	-0.0649**	-0.0621**	-0.0574**	-0.0278	-0.0387	-0.0373	-0.0311	-0.0649**
	(0.0287)	(0.0272)	(0.0265)	(0.0358)	(0.0365)	(0.0373)	(0.0311)	(0.0248)
Taliban control x Preschool-aged	-0.0545	-0.0568	-0.0560	-0.0482	-0.0908***	-0.0550	-0.0483	-0.0545*
	(0.0397)	(0.0402)	(0.0381)	(0.0395)	(0.0319)	(0.0402)	(0.0397)	(0.0319)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10575	10575	10571
R^2	0.504	0.508	0.526	0.504	0.504	0.504	0.503	0.219
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labour market participation								
Taliban control x School-aged	0.0150	0.0158	0.0109	0.0229*	0.0124	-0.000958	0.01000	0.0150
	(0.0134)	(0.0143)	(0.0156)	(0.0109)	(0.0167)	(0.0119)	(0.0110)	(0.0127)
Taliban control x Preschool-aged	0.0159	0.0148	0.0117	0.0136	0.0115	0.0156	0.0394*	0.0159*
	(0.0133)	(0.0122)	(0.0145)	(0.0109)	(0.0157)	(0.0140)	(0.0213)	(0.00900)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10591	10749	8205	10591	10591	10591	10591	10587
R^2	0.972	0.971	0.973	0.972	0.972	0.972	0.972	0.088
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including men aged 15 to 49 and men aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact that the actual begin of the Taliban rule at the province level is not precisely known. Columns (6) and (7) define school-age differently: a more general definition, including higher schooling, is used in column (6) (ages 6 to 20) and and alternative age of compulsory schooling is used in column (7) (ages 7 to 15). Column (8) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Table 9: Robustness Tests of DiD Results with Treatment Intensity, for Women

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) School age: 6 to 20	(7) School age: 7 to 15	(8) Province-age clustering
Panel A: Years of schooling								
Taliban control x Years of school-aged exposure	-0.0807***	-0.0596**	-0.0820***	-0.00809	-0.0971***	-0.0329	-0.0273	-0.0807***
	(0.0247)	(0.0248)	(0.0222)	(0.0372)	(0.0342)	(0.0356)	(0.0302)	(0.0258)
Taliban control x Years of preschool-aged exposure	-0.485***	-0.491***	-0.485***	-0.264***	-0.566***	-0.481***	-0.433***	-0.485***
	(0.130)	(0.122)	(0.129)	(0.110)	(0.147)	(0.127)	(0.110)	(0.120)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27561	27561	27556
R^2	0.354	0.365	0.370	0.351	0.354	0.354	0.354	0.263
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy								
Taliban control x Years of school-aged exposure	-0.0107***	-0.00906**	-0.0105**	-0.00154	-0.0117**	-0.00564	-0.00551	-0.0107**
	(0.00369)	(0.00388)	(0.00404)	(0.00430)	(0.00465)	(0.00388)	(0.00377)	(0.00435)
Taliban control x Years of preschool-aged exposure	-0.0466***	-0.0468***	-0.0461***	-0.0249**	-0.0535***	-0.0468***	-0.0429***	-0.0466***
	(0.0130)	(0.0119)	(0.0122)	(0.0115)	(0.0148)	(0.0137)	(0.0110)	(0.0122)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27537	29361	22824	27537	27537	27537	27537	27532
R^2	0.346	0.357	0.363	0.344	0.346	0.346	0.346	0.240
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion								
Taliban control x Years of school-aged exposure	-0.00564*	-0.00360	-0.00636**	0.000734	-0.00739**	-0.00178	-0.000642	-0.00564**
	(0.00282)	(0.00286)	(0.00292)	(0.00333)	(0.00363)	(0.00269)	(0.00276)	(0.00257)
Taliban control x Years of preschool-aged exposure	-0.0443***	-0.0445***	-0.0446***	-0.0249**	-0.0516***	-0.0437***	-0.0399***	-0.0443***
	(0.0120)	(0.0119)	(0.0118)	(0.0103)	(0.0136)	(0.0121)	(0.0104)	(0.0111)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27561	27561	27556
R^2	0.298	0.308	0.314	0.295	0.298	0.297	0.298	0.221
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labor market participation								
Taliban control x Years of school-aged exposure	-0.000356	-0.000102	-0.00718	-0.00196	0.00203	0.00395	0.000536	-0.000356
	(0.00283)	(0.00254)	(0.00440)	(0.00149)	(0.00367)	(0.00458)	(0.00291)	(0.00312)
Taliban control x Years of preschool-aged exposure	0.0146	0.0116	0.00650	0.0102	0.0241	0.0171	0.0115	0.0146
	(0.0117)	(0.0110)	(0.00817)	(0.00822)	(0.0169)	(0.0134)	(0.00990)	(0.0108)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27579	29406	22862	27579	27579	27579	27579	27574
R^2	0.313	0.309	0.318	0.313	0.314	0.314	0.313	0.207
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

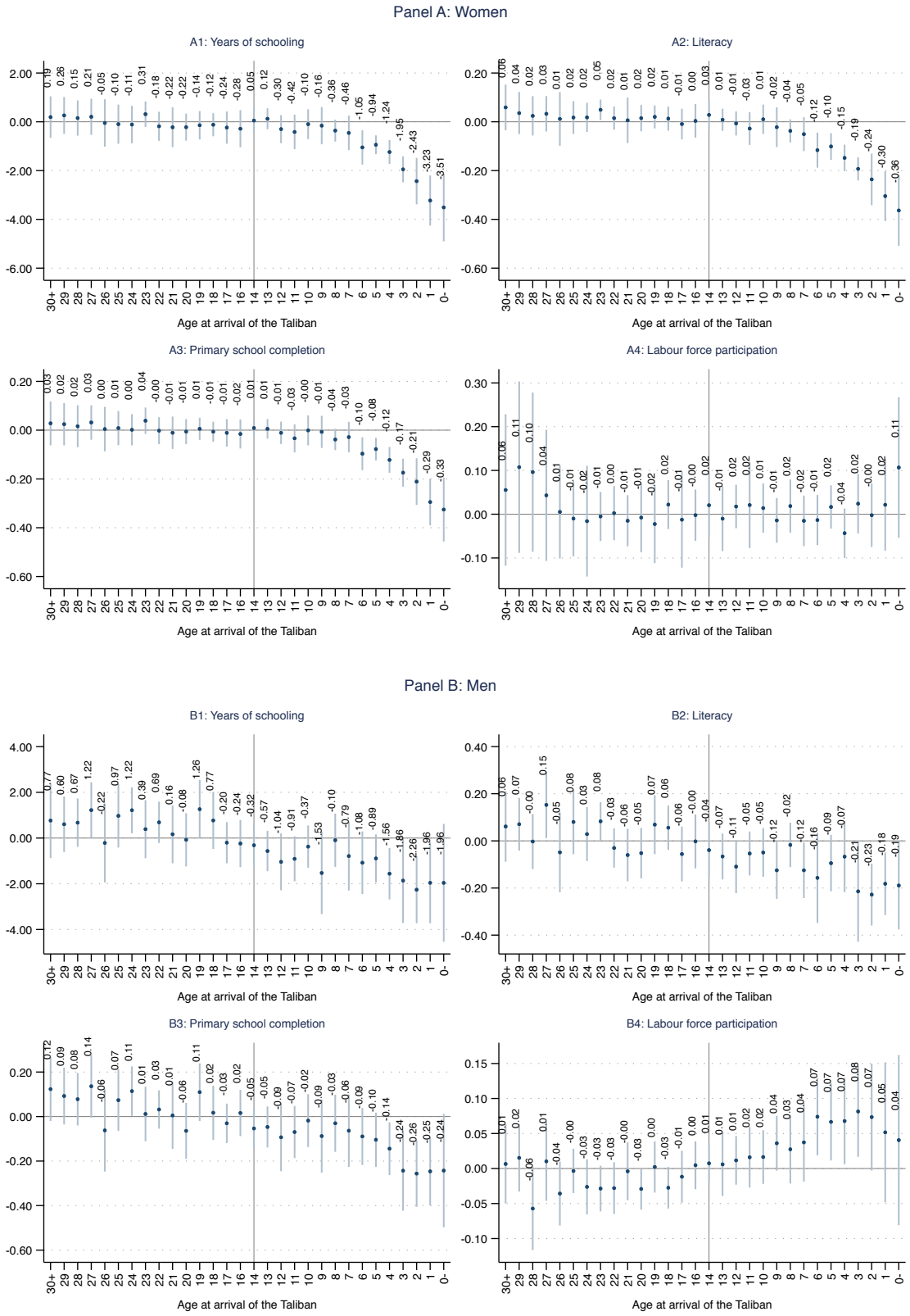
Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including women aged 15 to 49 and women aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact that the actual begin of the Taliban rule at the province level is not precisely known. Columns (6) and (7) define school-age differently: a more general definition, including higher schooling, is used in column (6) (ages 6 to 20) and and alternative age of compulsory schooling is used in column (7) (ages 7 to 15). Column (8) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Table 10: Robustness Tests of DiD Results with Treatment Intensity, for Men

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) School age 6 to 20	(7) School age: 7 to 15	(8) Province-age clustering
Panel A: Years of schooling								
Taliban control x Years of school-aged exposure	-0.0817	-0.0893	-0.0787	-0.0416	-0.0696	-0.0894	-0.0695	-0.0817
	(0.108)	(0.114)	(0.137)	(0.0858)	(0.108)	(0.0872)	(0.107)	(0.0908)
Taliban control x Years of preschool-aged exposure	-0.260	-0.204	-0.256	-0.190	-0.329*	-0.292	-0.248	-0.260
	(0.168)	(0.160)	(0.173)	(0.146)	(0.177)	(0.183)	(0.165)	(0.161)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10575	10575	10571
R^2	0.573	0.576	0.596	0.573	0.573	0.573	0.573	0.275
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy								
Taliban control x Years of school-aged exposure	-0.0102	-0.0112	-0.0188	-0.00220	-0.00615	-0.00130	-0.00892	-0.0102
	(0.0105)	(0.0107)	(0.0128)	(0.00863)	(0.0102)	(0.0107)	(0.0104)	(0.00920)
Taliban control x Years of preschool-aged exposure	-0.0110	-0.00759	-0.0222	-0.0124	-0.0249	-0.00952	-0.0131	-0.0110
	(0.0199)	(0.0185)	(0.0203)	(0.0151)	(0.0176)	(0.0219)	(0.0157)	(0.0261)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10560	10718	8181	10560	10560	10560	10560	10556
R^2	0.589	0.592	0.611	0.589	0.589	0.589	0.589	0.191
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion								
Taliban control x Years of school-aged exposure	-0.00376	-0.00386	-0.00465	-0.00125	-0.00304	-0.00201	-0.00262	-0.00376
	(0.00942)	(0.00926)	(0.0104)	(0.00760)	(0.00947)	(0.00905)	(0.00885)	(0.00733)
Taliban control x Years of preschool-aged exposure	-0.0353**	-0.0328**	-0.0376**	-0.0220	-0.0401**	-0.0356*	-0.0314**	-0.0353**
	(0.0159)	(0.0158)	(0.0165)	(0.0154)	(0.0179)	(0.0180)	(0.0154)	(0.0140)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10575	10575	10571
R^2	0.504	0.508	0.526	0.504	0.504	0.504	0.504	0.219
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labor market participation								
Taliban control x Years of school-aged exposure	0.00634**	0.00633*	0.00530	0.00655***	0.00665*	0.00484**	0.00446*	0.00634**
	(0.00301)	(0.00318)	(0.00427)	(0.00227)	(0.00332)	(0.00226)	(0.00256)	(0.00306)
Taliban control x Years of preschool-aged exposure	0.00283	0.00139	0.00147	0.00145	0.00120	0.00413	0.00730	0.00283
	(0.00809)	(0.00670)	(0.00955)	(0.00659)	(0.0103)	(0.00788)	(0.00684)	(0.00678)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10591	10749	8205	10591	10591	10591	10591	10587
R^2	0.972	0.971	0.973	0.972	0.972	0.972	0.972	0.088
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

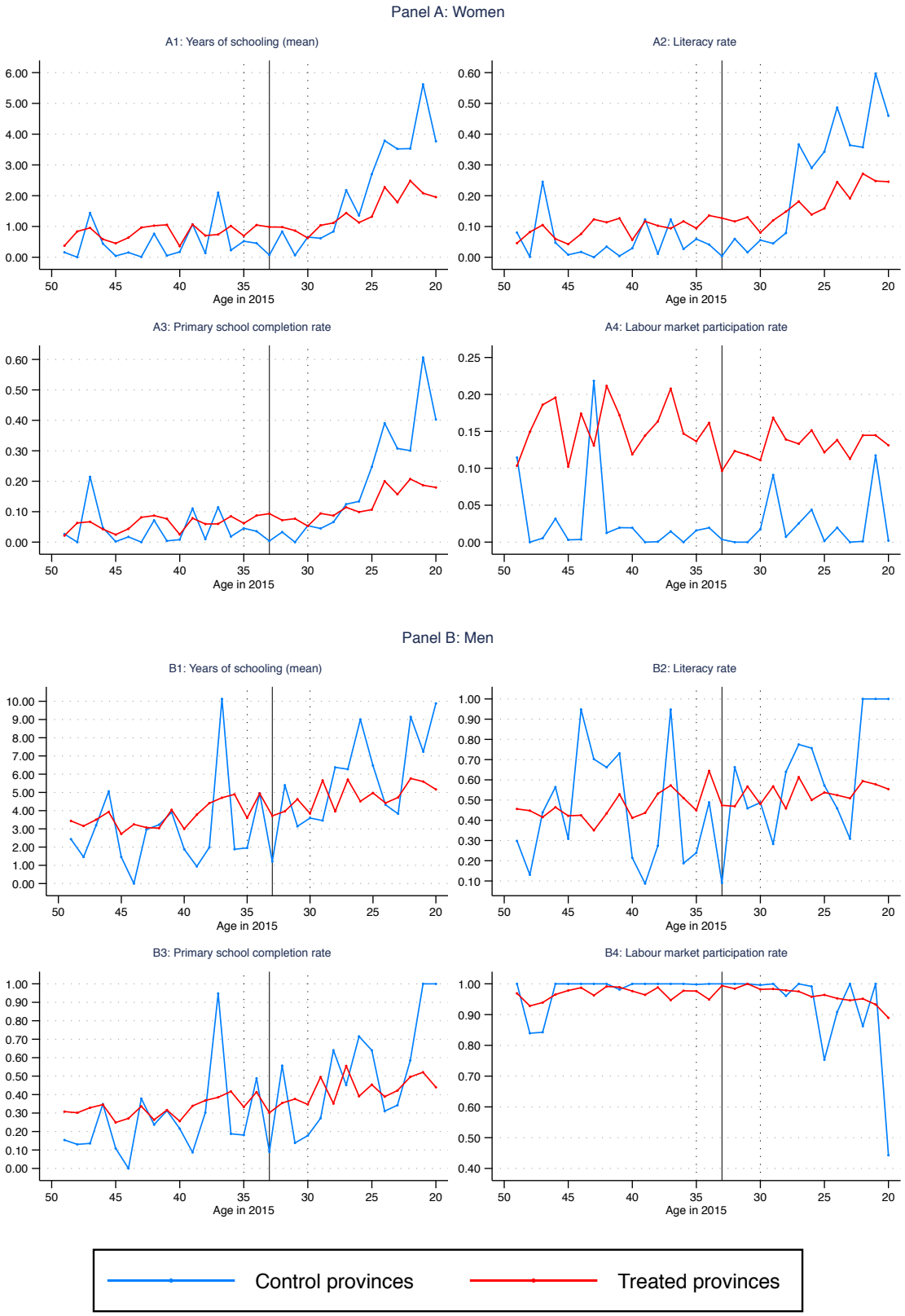
Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including men aged 15 to 49 and men aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact that the actual begin of the Taliban rule at the province level is not precisely known. Columns (6) and (7) define school-age differently: a more general definition, including higher schooling, is used in column (6) (ages 6 to 20) and and alternative age of compulsory schooling is used in column (7) (ages 7 to 15). Column (8) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Figure 3: Treatment Effect Heterogeneity by Age at the Taliban Arrival



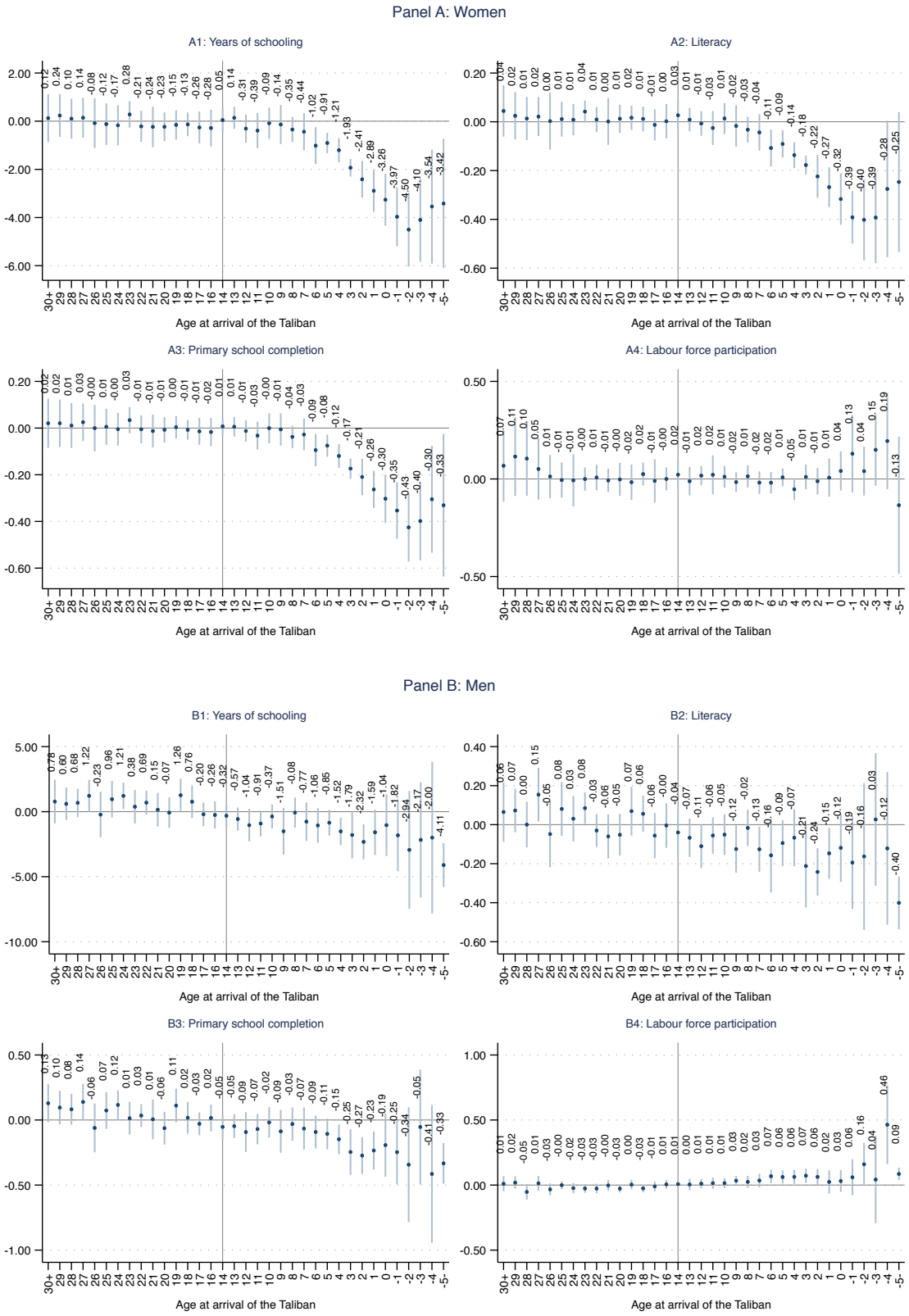
Notes: This figure reports the average effect of exposure to the Taliban rule when this exposure begins at age j. It plots the $AgeAtTalibanArrival_{pbj}$ leads/lags (also found in Table 4) for each outcome variable of interest. The first lag is used as reference point ($AgeAtTalibanArrival_{pb15}$) and thus dropped (normalized to zero). The light blue bars around the coefficients report their 95% confidence interval. The black vertical line indicates the "just treated" cohort, i.e., people who were in their last year of compulsory schooling at first Taliban exposure. Endpoints are binned, such that *0-* includes newborn and yet to be born individuals at first exposure and *30+* comprises individuals aged 30 years or more at first exposure. Standard errors are clustered at the province level. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates consists of ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sample weights are used in all regressions. Unweighted results are available in the appendix.

Figure 4: Graphical Support for the Parallel Trends Assumption



Notes: This figure reports the evolution of educational and labor market outcomes of both men and women across generations in Afghanistan, distinguishing between provinces that ever (treated) and never (control) were under Taliban occupation. The latter group includes Panjsher and Badakhshan. The plotted means are computed using sample weights. Assuming that the Taliban occupation started in 1996 in all affected provinces, all individuals aged 20 to 33 in 2015 and living in treated provinces were of compulsory school age for at least one year under Taliban rule, and as such treated; while older individuals were out of school already and thus not treated. This cutoff is depicted by a black solid vertical line. In reality, the timing of the Taliban occupation varies at the province level, such that depending on their province of residence, some individuals aged 31 to 35 in 2015 may or may not be treated. The treatment cutoff thus varies between ages 30 and 35. This is depicted by two dotted vertical lines. Individuals living in control provinces are untreated regardless of their age.

Figure 5: Treatment Effect Heterogeneity by Age at the Taliban Arrival: 15 to 49 Year-Olds



Notes: This figure reports the average effect of exposure to the Taliban rule when this exposure begins at age j. It plots the $AgeAtTalibanArrival_{pbj}$ leads/lags for each outcome variable of interest. The first lag is used as reference point ($AgeAtTalibanArrival_{pb15}$) and thus dropped (normalized to zero). The light blue bars around the coefficients report their 95% confidence interval. The black vertical line indicates the "just treated" cohort, i.e. people who were in their last year of compulsory schooling at first Taliban exposure. Endpoints are binned. Standard errors are clustered at the province level. The estimation sample includes ever married women (respectively men) aged 15 to 49 in 2015. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates consists of ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sample weights are used in all regressions. Unweighted results are available in Appendix.

Table A1: Number of Respondents of a Given Age in a Given Province

Current age	Kabul	Kapisa	Parwan	Wardak	Logar	Nangarhar	Laghman	Panjsher	Baghlan	Bamyan	Ghazni	Paktika	Paktya	Khost	Kunarha	Nooristan	Badakhshan	Takhar	Kunduz	Samangan	Balkh	Sar-e-pul	Ghor	Daykundi	Urozgan	Zabul	Kandahar	Jawzjan	Faryab	Helmand	Badghis	Herat	Farah	Nimroz	Total
Panel A: Women																																			
15	1	0	0	0	0	1	2	0	2	0	0	0	0	3	3	0	2	1	0	0	0	0	3	5	0	0	1	0	0	2	9	1	5	8	49
16	2	3	1	2	0	7	7	0	2	1	2	0	1	3	2	3	3	0	0	1	5	5	12	3	2	1	5	1	3	7	7	3	22	10	126
17	3	1	3	1	0	11	7	1	4	0	18	0	5	5	2	8	6	7	4	8	4	9	9	9	1	0	10	2	9	7	25	9	44	21	253
18	13	15	14	21	10	33	31	6	24	13	19	12	43	46	19	46	22	15	14	11	25	8	34	29	26	3	35	4	21	25	47	24	76	41	825
19	17	25	13	13	4	22	9	3	16	10	24	15	20	20	16	31	18	17	14	14	19	8	10	12	17	4	26	7	24	16	18	10	57	27	576
20	33	45	45	51	34	70	52	28	28	50	54	68	53	84	49	98	55	68	46	20	48	48	61	44	37	3	72	20	32	61	76	46	81	61	1,721
21	20	24	15	21	8	22	9	9	17	12	43	62	35	33	16	53	26	14	20	23	18	17	20	11	19	2	28	15	17	15	18	14	50	10	736
22	37	34	38	43	29	52	43	21	21	27	56	58	57	63	30	83	52	39	39	23	51	44	38	29	35	9	59	21	36	41	42	52	47	29	1,378
23	30	33	32	34	21	55	22	31	33	18	43	61	42	84	31	66	24	32	27	17	32	20	35	33	40	5	35	19	32	31	29	33	41	23	1,144
24	31	29	37	33	21	27	22	25	33	13	46	65	39	51	19	59	26	27	35	27	35	32	26	17	48	12	38	24	25	26	39	43	47	27	1,104
25	47	59	47	88	38	88	46	37	38	46	78	75	91	122	59	126	57	49	53	54	62	59	66	67	47	10	69	32	37	50	96	49	63	45	2,050
26	23	27	26	30	27	48	31	12	39	19	55	46	40	55	20	78	14	22	36	22	37	28	42	15	48	11	37	43	17	45	20	33	56	22	1,124
27	32	25	19	44	38	48	25	18	26	27	28	67	34	43	19	81	20	31	36	23	34	30	39	17	58	7	32	23	27	24	20	43	47	19	1,104
28	38	42	36	44	34	47	62	25	29	29	61	66	72	85	54	72	47	40	34	42	29	41	55	27	51	7	64	71	31	46	43	59	44	21	1,548
29	9	16	18	23	35	23	15	7	17	5	31	26	39	27	19	33	10	14	26	12	11	8	17	9	23	4	17	20	20	33	5	23	18	8	621
30	48	45	48	71	49	60	83	43	52	69	57	69	75	110	62	92	61	56	56	61	47	55	69	59	78	10	71	71	41	67	93	51	56	50	2,085
31	8	14	7	15	19	10	6	3	16	5	30	32	25	13	4	39	8	9	15	2	16	10	6	2	12	2	14	18	11	15	6	13	14	9	428
32	21	18	20	22	37	23	15	12	21	13	35	33	33	50	22	42	24	24	17	14	36	36	42	14	34	4	34	42	15	24	20	32	23	24	876
33	12	26	11	16	13	11	16	11	21	11	30	31	25	33	20	32	10	16	22	13	21	13	13	8	27	4	14	20	14	20	12	13	21	10	590
34	13	9	18	11	18	13	9	8	12	13	14	32	25	11	9	16	11	19	16	13	25	16	20	4	21	8	9	27	20	16	7	17	17	5	502
35	58	49	38	48	56	59	43	44	38	53	52	37	57	73	45	45	54	37	47	46	43	49	35	61	19	11	41	60	31	59	64	46	38	45	1,581
36	19	18	16	9	37	28	17	13	17	12	20	22	36	32	15	22	22	19	19	12	30	21	15	6	20	4	13	25	25	20	9	26	17	18	654
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38	31	39	26	22	62	40	25	39	20	17	48	23	51	51	35	29	33	34	24	31	38	33	30	22	12	8	39	36	24	25	22	48	26	25	1,068
39	14	14	15	11	25	17	11	16	15	5	33	11	22	9	6	14	11	10	9	15	12	10	9	7	8	2	10	17	15	22	4	13	7	8	427
40	59	43	33	47	59	36	44	80	42	60	41	51	55	73	45	33	62	60	34	46	50	50	46	58	33	13	47	56	25	27	56	44	46	36	1,590
41	12	17	8	4	21	4	4	6	10	9	12	13	19	5	5	12	7	10	16	4	5	6	4	2	6	2	6	7	8	13	3	8	8	2	278
42	14	30	14	19	41	18	15	13	24	10	27	10	25	19	14	23	30	22	20	20	18	18	21	10	10	1	17	10	21	15	9	21	15	7	601
43	10	20	16	18	19	9	8	9	14	3	26	9	13	12	6	13	4	11	23	12	13	12	9	2	5	5	11	12	15	10	6	9	16	4	384
44	9	14	13	20	15	6	12	7	5	7	13	13	10	9	8	12	10	7	5	6	15	11	7	7	12	3	6	20	16	7	7	10	14	2	338
45	30	33	26	44	32	36	35	47	31	50	45	22	42	37	32	35	39	34	44	34	28	28	30	41	11	5	38	28	27	22	33	42	31	29	1,121
46	9	20	19	9	20	15	13	13	7	7	18	15	19	16	7	10	12	12	19	10	18	16	22	3	13	0	10	19	24	12	2	21	14	2	446
47	15	16	20	11	21	24	12	8	11	8	20	12	12	9	9	23	10	13	13	14	18	16	10	4	6	3	8	25	13	12	6	19	21	4	446
48	16	28	24	12	36	27	26	35	22	10	26	19	25	27	12	34	18	11	18	14	22	28	9	17	17	0	16	33	19	5	8	56	25	9	704
49	6	21	15	6	15	12	18	30	6	6	20	19	6	5	7	17	12	17	17	6	22	7	3	3	3	1	8	15	22	3	7	37	12	5	409
Total	755	874	744	870	915	1,023	800	681	740	652	1,146	1,110	1,174	1,338	734	1,398	835	819	839	682	909	812	886	669	805	172	952	865	742	843	875	989	1,133	680	29,461
Panel B: Men																																			
15	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	0	2
16	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	1	0	2	0	0	0	4
17	0	0	0	1	0	0	0	0	0	0	0	0	1	1	1	0	0	0	0	1	1	0	0	0	0	0	0	0	1	2	0	0	4	1	14
18	3	1	1	4	1	2	4	0	0	0	2	1	4	3	0	1	1	0	0	0	0	2	6	0	2	0	1	0	1	5	3	1	14	1	64
19	2	1	1	0	1	2	3	2	1	1	7	4	6	4	2	0	2	0	0	0	0	0	8	0	2	0	2	0	2	3	1	1	12	4	74
20	5	3	4	10	3	7	12	0	5	1	24	7	12	14	1	9	2	0	1	3	3	0	13	6	4	0	9	2	1	4	12	1	22	12	212
21	2	1	1	3	5	2	4	1	2	3	10	9	9	10	4	4	3	1	4	2	9	0	9	2	2	0	6	1	5	4	1	5	15	7	146
22	6	4	9	15	6	10	14	4	5	7	17	12	20	17	8	5	5	3	8	5	8	2	16	5	10	0	20	3	7	12	8	9	20	9	309
23	3	9	5	11	6	12	8	5	4	2	18	20	7	24	4	14	8	4	2	5	9	1	13	4	6	1	17	6	9	7	11	11	22	6	294
24	1	11	7	13	10	10	9	6	4	4	17	26	18	27	8	10	8	4	8	5	8	3	18	3	14	1	6	7	8	7	8	15	29	8	341
25	11	14	16	36	16	23	22	20	11	8	31	33	32	43	10	39	10	18	10	7	20	16	16	8	16	4	21	12	13	21	21	25	22	18	643
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27	8	9	12	22	10	11	8	7	11	7	17	22	17	17	7	28	6	8	14	9	11	11	17	4	16	4	14	13	7	13	11	13	8	8	400
28	16	17	15	25	13	19	25	10	13	9	32	36	27	52	19	27	9	11	22	22	19	18	18	12	27	3	24	17	7	26	5	22	22	6	645
29	5	6	6	12	20	5	5	5	8	2	15	16	11	5	1	17	5	3	4	6	8	7	6	2	12	1	9	15	10	8	3	8	7	2	255
30	9	17	17	53	16	43	34	14	20	16	48	43	34	53	17	23	23	11	19	24	19	25	34	21	39	8	34	26	16	29	37	23	26	16	887
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37	9	3	6	7	12	7	7	2	9	8	9	13	16	14	6	12	4	5	10	6	13	8	5	1	4	2	4	11	5	11	7	8	7	6	257
38	10	8	10	10	30	15	10	9	8	5	26	13	21	18	11	23	6	9	16	13	10	15	15	6	7	4	16	17	10	13	8	23	7	7	429
39	2	4	6	4	11	7	3	4	3	6	14	4	9	8	3	6	4	2	5	1	6	5	4	7	2	2	5	8	3	8	1	4	10	3	174
40	9	17	13	33	34	35	22	25	19	19	35	17	32	47	10	17	16	14	11	20	15	15	14	11	18	6	24	28	6	25	29	19	21	11	687
41	1	6	3	1	11	4	1	2	8	3	11	9	10	3	1	5	6	3	6	4	5	12	5	2	2	0	7	6	4	5	1	11	7	5	170
42	7	8	9	8	22	9	6	7	5	3	9	8	10	12	4	11	8	12	10	11	13	12	7	3	2	4	12	10	4	10	6	6	8	4	280
43	11	7	2	3	9	3	6	6	5	5	8	4	6	3	5	8	0	6	7	9	3	7	9	0	3	2	9	9	4	2	5	6	5	0	177
44	5	5	9	8	15	4	5	1	6	4	6	7	4	2	4	8	1	7	8	3	8	4	6	2	1	0	0	8	3	3	6	1	9	6	169
45	12	16	16	21	13	18	16	11	14	16	26	9	24	27	9	15	22	10	19	20	16	13	15	10	14	4	26	8	15	14	23	23	14	17	546
46	4	10	5	8	9	2	5	4	2	2	14	3	9	9	6	3	6	3	8	2	10	5	12	0	6	1	7	8	6	4	2	9	6	0	190
47	5	9	1	16	10	6	5	3	10	2	8	6	4	7	4	11	9	2	3	11	4	5	11	5	3	2	4	8	3	7	1	10	7	2	204
48	8	21	16	13	18	11	11	9	7	4	26	12	8	13	3	15	9	5	10	11	13	10	13	6	14	1	10	10	12	3	11	17	18	7	375
49	2	8	12	3	22	5	14	13	2	5	23	7	4	4	2	3	15	15	8	10	7	11	5	3	3	2	14	16	6	4	6	18	7	1	280
Total	207	280	259	418	404	353	334	202	246	193	576	451	472	560	186	419	246	217	297	269	314	260	398	150	337	69	411	331	230	344	304	367	457	199	10,760

Notes: This table counts the number of respondents of a given age in each province.

Table A2: Number of Respondents of a Given Age in a Given Treatment Group

	Treatment from...													
	Women							Men						
Current age	Never	1994	1995	1996	1997	1998	1999	Never	1994	1995	1996	1997	1998	1999
15	2	6	22	16	1	2	0	0	0	2	0	0	0	0
16	3	11	62	28	17	4	1	0	0	1	2	1	0	0
17	7	20	119	56	32	12	7	0	0	10	1	2	1	0
18	28	93	351	189	88	48	28	1	3	42	13	4	0	1
19	21	59	216	113	100	40	27	4	4	50	10	3	2	1
20	83	156	654	378	261	98	91	2	19	122	46	9	9	5
21	35	60	311	138	105	52	35	4	10	86	17	17	7	5
22	73	132	513	287	225	71	77	9	35	153	51	27	17	17
23	55	113	448	233	168	68	59	13	28	145	52	38	11	7
24	51	115	424	197	172	73	72	14	24	188	46	41	13	15
25	94	193	765	462	298	138	100	30	49	293	126	93	26	26
26	26	111	451	220	174	80	62	11	48	205	61	51	22	14
27	38	114	426	225	170	76	55	13	38	164	73	59	27	26
28	72	149	587	316	254	100	70	19	66	279	111	89	44	37
29	17	53	280	104	89	34	44	10	24	110	36	49	16	10
30	104	218	724	438	315	182	104	37	102	375	163	114	60	36
31	11	30	191	73	78	23	22	9	22	96	33	35	16	15
32	36	86	355	143	171	48	37	13	41	166	66	68	15	25
33	21	53	225	103	110	45	33	9	20	113	42	44	17	20
34	19	42	186	67	116	38	34	5	30	80	42	52	21	15
35	98	132	546	314	269	137	85	33	67	272	126	91	62	32
36	35	43	252	110	138	41	35	10	34	112	53	57	21	20
37	36	38	201	79	133	53	34	6	11	108	48	45	23	16
38	72	81	411	182	204	68	50	15	33	183	77	69	26	26
39	27	27	170	66	78	35	24	8	16	79	22	28	10	11
40	142	151	525	273	284	148	67	41	59	288	122	95	58	24
41	13	16	109	40	53	23	24	8	11	78	13	36	15	9
42	43	38	220	93	119	54	34	15	21	104	43	59	19	19
43	13	23	145	52	83	29	39	6	14	55	38	36	19	9
44	17	28	120	54	83	18	18	2	3	67	32	35	13	17
45	86	95	376	201	178	115	70	33	54	203	93	78	50	35
46	25	26	168	56	109	24	38	10	14	83	22	42	6	13
47	18	21	151	89	101	33	33	12	14	88	32	31	23	4
48	53	50	249	123	141	46	42	18	31	148	59	71	22	26
49	42	15	131	67	104	18	32	28	22	98	32	63	17	20
Total	1,516	2,598	11,084	5,585	5,021	2,074	1,583	448	967	4,646	1,803	1,632	708	556

Notes: This table counts the number of respondents of a given age in a given treatment group.

Table A3: Number of Respondents of a Given Age at Taliban Arrival (All Provinces Combined)

	Women		Men	
Age at Taliban arrival	Sample: 15-49	Sample: 20-49	Sample: 15-49	Sample: 20-49
-6	6	0	0	0
-5	33	0	2	0
-4	98	0	1	0
-3	241	0	15	0
-2	485	0	48	0
-1	597	156	84	19
0	928	714	147	132
1	976	821	170	167
2	1,093	1,025	210	207
3	1,080	1,053	247	246
4	1,218	1,218	328	328
5	1,347	1,347	447	447
6	1,344	1,344	438	438
7	1,225	1,225	404	404
8	1,249	1,249	468	468
9	1,164	1,164	430	430
10	1,250	1,250	563	563
11	959	959	419	419
12	900	900	386	386
13	714	714	314	314
14	719	719	309	309
15	836	836	422	422
16	802	802	343	343
17	732	732	326	326
18	826	826	381	381
19	762	762	313	313
20	899	899	433	433
21	600	600	291	291
22	652	652	262	262
23	491	491	206	206
24	476	476	257	257
25	617	617	313	313
26	536	536	263	263
27	492	492	241	241
28	595	595	311	311
29	449	449	229	229
30	279	279	139	139
31	183	183	89	89
32	60	60	43	43
33	32	32	20	20

Notes: This table counts the number of respondents of a given age at the start of the Taliban occupation in their province of residence (all treated provinces combined).

Table A4: Difference-in-Differences Results without Treatment Intensity (Unweighted)

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation
Panel A: Women				
Taliban control x School-aged	-0.434	-0.0368	-0.0422	-0.00827
	(0.318)	(0.0305)	(0.0285)	(0.00830)
Taliban control x Preschool-aged	-0.714***	-0.0662***	-0.0644**	0.00571
	(0.235)	(0.0238)	(0.0260)	(0.00975)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579
R^2	0.311	0.303	0.263	0.454
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x School-aged	-0.761*	-0.0409	-0.0602*	0.0162
	(0.436)	(0.0340)	(0.0325)	(0.0103)
Taliban control x Preschool-aged	-0.446	-0.0453	-0.0540*	0.00259
	(0.333)	(0.0317)	(0.0302)	(0.0128)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.566	0.596	0.511	0.969
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of having been of compulsory school age (6-14) and of preschool age (0-5) under Taliban rule for at least one year on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *School-aged* is an indicator variable taking value one if an individual was of compulsory school age (6 to 14 years old) at the time of the Taliban rule and *Preschool-aged* is an indicator variable taking value one if an individual was in their early childhood (0 to 5 years old) at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsheer and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Table A5: Difference-in-Differences Results with Treatment Intensity (Unweighted)

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation
Panel A: Women				
Taliban control x Years of school-aged exposure	-0.162***	-0.0125***	-0.0129***	0.000288
	(0.0390)	(0.00432)	(0.00369)	(0.00299)
Taliban control x Years of preschool-aged exposure	-0.486***	-0.0456***	-0.0456***	0.00554
	(0.0974)	(0.0102)	(0.0106)	(0.00473)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579
R^2	0.317	0.307	0.269	0.454
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x Years of school-aged exposure	-0.150	-0.00742	-0.00749	0.00285
	(0.103)	(0.00734)	(0.00813)	(0.00310)
Taliban control x Years of preschool-aged exposure	-0.383**	-0.0265**	-0.0406***	-0.00404
	(0.146)	(0.0111)	(0.0134)	(0.00697)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.566	0.596	0.511	0.969
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of one additional year of compulsory school-aged (6-14) and of preschool-aged (0-5) exposure to the Taliban rule on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *Years of school-aged exposure* is a variable ranging from 0 to 8 and indicating the number of years an individual was of compulsory school age (6 to 14 years old) at the time of the Taliban rule; and *Years of preschool-aged exposure* is a variable ranging from 0 to 6 and indicating the number of years an individual was of preschool age (0 to 5 years old) at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsheer and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Table A6: Treatment Effect Heterogeneity by Age at the Taliban Arrival (Unweighted)

	Women				Men			
	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor force participation	(5) Total years of schooling	(6) Literacy	(7) Primary school completion	(8) Labor force participation
0-	-3.543***	-0.316***	-0.333***	0.0464	-3.208**	-0.193**	-0.308***	-0.0307
	(0.593)	(0.0681)	(0.0634)	(0.0396)	(1.195)	(0.0845)	(0.0963)	(0.0748)
1	-3.168***	-0.273***	-0.294***	0.00437	-2.606***	-0.146**	-0.236***	0.00638
	(0.420)	(0.0509)	(0.0486)	(0.0262)	(0.925)	(0.0671)	(0.0663)	(0.0560)
2	-2.539***	-0.212***	-0.233***	0.00329	-2.131***	-0.145**	-0.188***	0.0223
	(0.390)	(0.0484)	(0.0441)	(0.0230)	(0.768)	(0.0552)	(0.0497)	(0.0482)
3	-2.357***	-0.203***	-0.218***	0.0147	-1.885**	-0.118*	-0.181***	0.0233
	(0.306)	(0.0393)	(0.0366)	(0.0231)	(0.773)	(0.0659)	(0.0535)	(0.0414)
4	-1.650***	-0.132***	-0.155***	-0.00507	-1.754***	-0.118**	-0.140***	0.0239
	(0.275)	(0.0372)	(0.0332)	(0.0209)	(0.601)	(0.0502)	(0.0432)	(0.0366)
5	-1.340***	-0.0962**	-0.114***	0.00506	-1.009*	-0.0684	-0.0732*	0.0343
	(0.257)	(0.0352)	(0.0296)	(0.0213)	(0.562)	(0.0507)	(0.0422)	(0.0329)
6	-1.114***	-0.0778**	-0.0998***	-0.00200	-0.852	-0.0410	-0.0512	0.0351
	(0.245)	(0.0325)	(0.0257)	(0.0214)	(0.518)	(0.0435)	(0.0400)	(0.0286)
7	-0.898***	-0.0558*	-0.0801***	-0.00998	-0.738	-0.0418	-0.0273	0.0240
	(0.212)	(0.0285)	(0.0226)	(0.0198)	(0.556)	(0.0481)	(0.0494)	(0.0268)
8	-0.577***	-0.0329	-0.0506**	0.00933	-1.054*	-0.0249	-0.0671	0.0235
	(0.203)	(0.0260)	(0.0198)	(0.0170)	(0.526)	(0.0445)	(0.0447)	(0.0219)
9	-0.380*	-0.00564	-0.0286	0.00178	-1.068	-0.0837	-0.0511	0.0285
	(0.201)	(0.0242)	(0.0203)	(0.0165)	(0.643)	(0.0577)	(0.0604)	(0.0178)
10	-0.275*	-0.00436	-0.0199	0.000515	-0.723	-0.0464	-0.0259	0.0169
	(0.161)	(0.0199)	(0.0172)	(0.0166)	(0.511)	(0.0553)	(0.0503)	(0.0156)
11	-0.232	0.00108	-0.0196	0.0215	-1.009**	-0.0673	-0.0705	0.0200
	(0.178)	(0.0202)	(0.0184)	(0.0185)	(0.475)	(0.0529)	(0.0493)	(0.0158)
12	-0.277*	-0.0157	-0.0168	0.000821	-0.813	-0.0627	-0.0529	0.0251**
	(0.138)	(0.0181)	(0.0139)	(0.0211)	(0.492)	(0.0526)	(0.0523)	(0.0110)
13	-0.0734	0.00672	-0.00364	-0.0112	-0.764	-0.0359	-0.0563	0.0247*
	(0.139)	(0.0165)	(0.0141)	(0.0128)	(0.485)	(0.0485)	(0.0505)	(0.0144)
14	0.0446	0.0235	-0.000444	-0.00680	-0.130	-0.0129	-0.0243	0.0188
	(0.173)	(0.0184)	(0.0148)	(0.0148)	(0.389)	(0.0476)	(0.0355)	(0.0122)
16	0.0902	0.0360**	0.0123	0.00624	-0.00841	-0.0160	0.0135	0.0150
	(0.147)	(0.0169)	(0.0141)	(0.0161)	(0.497)	(0.0555)	(0.0484)	(0.0117)
17	-0.00315	0.00795	0.00195	0.0129	-0.184	-0.0182	-0.0295	0.00886
	(0.171)	(0.0191)	(0.0158)	(0.0162)	(0.404)	(0.0465)	(0.0331)	(0.0136)
18	0.0444	0.0190	0.00931	0.00421	0.746	0.0422	0.0455	-0.00751
	(0.157)	(0.0177)	(0.0151)	(0.0152)	(0.478)	(0.0467)	(0.0413)	(0.0121)
19	0.178	0.0361*	0.0208	-0.0121	1.065**	0.0798	0.0757*	0.0212
	(0.164)	(0.0196)	(0.0154)	(0.0162)	(0.467)	(0.0478)	(0.0391)	(0.0137)
20	0.287*	0.0493**	0.0346**	0.00140	0.442	-0.0234	0.00760	-0.00540
	(0.162)	(0.0228)	(0.0159)	(0.0225)	(0.445)	(0.0513)	(0.0384)	(0.0144)
21	0.451**	0.0680**	0.0442**	0.00885	0.475	-0.0259	0.0543	0.0181
	(0.211)	(0.0266)	(0.0196)	(0.0191)	(0.512)	(0.0449)	(0.0402)	(0.0189)
22	0.241	0.0540**	0.0331*	0.00720	1.164**	0.0198	0.0935**	-0.00354
	(0.183)	(0.0230)	(0.0169)	(0.0217)	(0.448)	(0.0523)	(0.0375)	(0.0181)
23	0.396***	0.0587***	0.0398***	-0.0126	0.815	0.0778	0.0643	-0.00677
	(0.139)	(0.0163)	(0.0143)	(0.0256)	(0.527)	(0.0504)	(0.0421)	(0.0175)
24	0.501***	0.0770***	0.0429**	-0.00771	1.681***	0.106*	0.136***	-0.00238
	(0.174)	(0.0202)	(0.0170)	(0.0287)	(0.513)	(0.0560)	(0.0378)	(0.0143)
25	0.617***	0.0851***	0.0657***	0.00650	1.571**	0.113	0.139**	0.0196
	(0.178)	(0.0200)	(0.0186)	(0.0265)	(0.595)	(0.0673)	(0.0517)	(0.0153)
26	0.669***	0.0951***	0.0654***	0.0175	0.969	0.0430	0.0657	-0.00224
	(0.200)	(0.0271)	(0.0212)	(0.0319)	(0.656)	(0.0613)	(0.0541)	(0.0165)
27	0.790***	0.106***	0.0824***	0.0244	1.629**	0.130**	0.152***	0.0367*
	(0.187)	(0.0223)	(0.0183)	(0.0299)	(0.616)	(0.0583)	(0.0470)	(0.0196)
28	0.758***	0.0959***	0.0738***	0.0276	1.268*	0.0690	0.128***	-0.0182
	(0.177)	(0.0237)	(0.0187)	(0.0296)	(0.634)	(0.0576)	(0.0447)	(0.0206)
29	0.730***	0.0887***	0.0665***	0.0255	1.488*	0.0953	0.132***	0.0348
	(0.174)	(0.0234)	(0.0194)	(0.0421)	(0.746)	(0.0635)	(0.0474)	(0.0216)
30+	0.756***	0.109***	0.0731***	0.0295	1.297	0.0806	0.135**	0.0229
	(0.230)	(0.0281)	(0.0246)	(0.0358)	(0.969)	(0.0649)	(0.0619)	(0.0290)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27561	27537	27561	27579	10575	10560	10575	10591
R^2	0.318	0.308	0.270	0.455	0.568	0.597	0.512	0.970
Controls								

Notes: The table reports the average effect of exposure to the Taliban rule when this exposure starts at age j on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. Each dummy variable indicates whether an individual was the specified age at the arrival of the Taliban in their province of residence. 15 years old at Taliban arrival is used as base. Endpoints are binned, such that the dummy variable *0-* takes value one if the individual was a newborn or yet to be born at Taliban arrival and the variable *30+* takes value one if the individual was 30 years old or older at Taliban arrival. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsheer and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Table A7: Results of the Analysis at the Start of Schooling (Unweighted)

	(1) Total years of schooling	(2) Literacy	(3) Primary school completion	(4) Labor market participation
Panel A: Women				
Taliban control x Turned 6	-0.730**	-0.0668**	-0.0668**	0.00189
	(0.307)	(0.0310)	(0.0327)	(0.00877)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	27561	27537	27561	27579
R^2	0.310	0.302	0.263	0.454
Controls	Yes	Yes	Yes	Yes
Panel B: Men				
Taliban control x Turned 6	-0.217	-0.0198	-0.0321	0.0113
	(0.329)	(0.0254)	(0.0290)	(0.0139)
Age dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
N	10575	10560	10575	10591
R^2	0.565	0.595	0.510	0.969
Controls	Yes	Yes	Yes	Yes

Notes: The table reports the average effect of turning 6 years old under Taliban rule on the four outcome variables considered. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are clustered at the province level and reported in parentheses. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. *Taliban control* is an indicator variable taking value one if the individual's province of residence ever was under Taliban rule; *Turned 6* is an indicator variable taking value one if an individual turned 6 years old and thus should begin school at the time of the Taliban rule. The (never treated) control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsheer and Badakhshan. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Table A8: Robustness Tests for Heterogeneous Effects, Dep. Var. = Years of Schooling

	Women					Men				
	(1) Baseline results	(2) Ages 15-49	(3) Ages 20-40	(4) Alternative control group	(5) Clustering	(6) Baseline results	(7) Ages 15-49	(8) Ages 20-40	(9) Alternative control group	(10) Clustering
0-	-3.510***	-3.528***	-3.431***	-1.867**	-3.510***	-1.961	-1.258	-3.251**	-0.811	-1.961
	(0.683)	(0.541)	(0.531)	(0.695)	(0.679)	(1.266)	(1.002)	(1.208)	(1.205)	(1.330)
1	-3.228***	-2.831***	-3.145***	-1.779***	-3.228***	-1.959**	-1.570*	-2.859***	-0.946	-1.959**
	(0.506)	(0.424)	(0.368)	(0.579)	(0.497)	(0.869)	(0.860)	(0.723)	(0.890)	(0.866)
2	-2.432***	-2.387***	-2.387***	-1.171*	-2.432***	-2.261***	-2.283***	-3.125***	-1.424*	-2.261***
	(0.468)	(0.384)	(0.392)	(0.588)	(0.617)	(0.719)	(0.685)	(0.574)	(0.718)	(0.579)
3	-1.950***	-1.909***	-1.909***	-0.777*	-1.950***	-1.864**	-1.781*	-2.653***	-1.092	-1.864**
	(0.261)	(0.204)	(0.198)	(0.437)	(0.269)	(0.911)	(0.883)	(0.817)	(0.941)	(0.835)
4	-1.240***	-1.189***	-1.202***	-0.334	-1.240***	-1.559***	-1.499***	-2.293***	-0.898	-1.559***
	(0.241)	(0.271)	(0.264)	(0.448)	(0.380)	(0.552)	(0.538)	(0.448)	(0.616)	(0.538)
5	-0.941***	-0.892***	-0.897***	-0.133	-0.941***	-0.892*	-0.832	-1.523***	-0.452	-0.892*
	(0.189)	(0.236)	(0.246)	(0.392)	(0.186)	(0.515)	(0.502)	(0.358)	(0.615)	(0.466)
6	-1.050***	-1.007**	-1.020**	-0.492	-1.050***	-1.080	-1.049	-1.746***	-0.675	-1.080*
	(0.347)	(0.393)	(0.392)	(0.499)	(0.362)	(0.673)	(0.662)	(0.603)	(0.762)	(0.539)
7	-0.455	-0.431	-0.446	-0.0371	-0.455	-0.790	-0.753	-1.330*	-0.238	-0.790
	(0.347)	(0.391)	(0.398)	(0.484)	(0.351)	(0.739)	(0.738)	(0.695)	(0.846)	(0.632)
8	-0.361	-0.343	-0.338	-0.0596	-0.361*	-0.0957	-0.0676	-0.542	0.572	-0.0957
	(0.220)	(0.265)	(0.272)	(0.298)	(0.209)	(0.574)	(0.580)	(0.551)	(0.686)	(0.534)
9	-0.159	-0.134	-0.135	0.0402	-0.159	-1.530*	-1.500	-1.951**	-0.954	-1.530*
	(0.374)	(0.404)	(0.419)	(0.425)	(0.360)	(0.883)	(0.889)	(0.894)	(0.962)	(0.855)
10	-0.0978	-0.0924	-0.101	0.0490	-0.0978	-0.374	-0.362	-0.725	0.0253	-0.374
	(0.300)	(0.323)	(0.332)	(0.321)	(0.279)	(0.455)	(0.463)	(0.471)	(0.552)	(0.391)
11	-0.416	-0.396	-0.390	-0.489	-0.416	-0.909*	-0.910*	-1.164**	-0.481	-0.909**
	(0.347)	(0.364)	(0.370)	(0.360)	(0.328)	(0.484)	(0.490)	(0.487)	(0.545)	(0.403)
12	-0.300	-0.319	-0.300	-0.333	-0.300	-1.042*	-1.038	-1.252**	-0.765	-1.042
	(0.283)	(0.286)	(0.294)	(0.295)	(0.262)	(0.610)	(0.615)	(0.612)	(0.666)	(0.627)
13	0.124	0.130	0.156	0.141	0.124	-0.568	-0.571	-0.645	-0.325	-0.568
	(0.211)	(0.226)	(0.235)	(0.213)	(0.225)	(0.434)	(0.436)	(0.472)	(0.535)	(0.389)
14	0.0526	0.0443	0.0854	-0.0355	0.0526	-0.316	-0.317	-0.416	-0.215	-0.316
	(0.254)	(0.263)	(0.266)	(0.254)	(0.290)	(0.451)	(0.462)	(0.440)	(0.442)	(0.416)
16	-0.284	-0.287	-0.267	-0.521	-0.284	-0.240	-0.262	-0.120	-0.143	-0.240
	(0.374)	(0.376)	(0.391)	(0.387)	(0.424)	(0.508)	(0.508)	(0.511)	(0.529)	(0.490)
17	-0.237	-0.263	-0.250	-0.388	-0.237	-0.202	-0.205	-0.0693	-0.420	-0.202
	(0.324)	(0.327)	(0.334)	(0.381)	(0.373)	(0.442)	(0.442)	(0.453)	(0.412)	(0.496)
18	-0.120	-0.139	-0.154	-0.408**	-0.120	0.770	0.756	0.998	0.431	0.770
	(0.232)	(0.241)	(0.257)	(0.198)	(0.230)	(0.615)	(0.615)	(0.647)	(0.595)	(0.587)
19	-0.141	-0.157	-0.162	-0.387	-0.141	1.262*	1.256*	1.625**	0.857	1.262*
	(0.286)	(0.302)	(0.340)	(0.248)	(0.325)	(0.629)	(0.626)	(0.633)	(0.601)	(0.629)
20	-0.219	-0.245	-0.268	-0.523**	-0.219	-0.0774	-0.0807	0.373	-0.401	-0.0774
	(0.273)	(0.297)	(0.347)	(0.212)	(0.250)	(0.572)	(0.573)	(0.547)	(0.583)	(0.579)
21	-0.223	-0.250	-0.329	-0.597	-0.223	0.165	0.139	0.621	-0.362	0.165
	(0.401)	(0.422)	(0.480)	(0.407)	(0.380)	(0.623)	(0.632)	(0.664)	(0.589)	(0.551)
22	-0.184	-0.235	-0.224	-0.558*	-0.184	0.688	0.682	1.015	0.154	0.688
	(0.294)	(0.323)	(0.388)	(0.277)	(0.276)	(0.445)	(0.447)	(0.673)	(0.445)	(0.480)
23 / 23+	0.312	0.260	0.290	0.104	0.312	0.387	0.367	2.102***	-0.124	0.387
	(0.257)	(0.280)	(0.346)	(0.287)	(0.309)	(0.624)	(0.632)	(0.634)	(0.741)	(0.669)
24	-0.114	-0.189		-0.426	-0.114	1.215**	1.205**		0.493	1.215**
	(0.378)	(0.418)		(0.293)	(0.440)	(0.494)	(0.498)		(0.419)	(0.455)
25	-0.0967	-0.148		-0.502	-0.0967	0.971	0.944		0.147	0.971*
	(0.395)	(0.433)		(0.331)	(0.366)	(0.682)	(0.687)		(0.596)	(0.540)
26	-0.0481	-0.112		-0.603	-0.0481	-0.215	-0.242		-1.362*	-0.215
	(0.477)	(0.521)		(0.441)	(0.435)	(0.848)	(0.859)		(0.736)	(0.688)
27	0.205	0.106		-0.424	0.205	1.219*	1.201**		0.585	1.219**
	(0.364)	(0.419)		(0.311)	(0.344)	(0.600)	(0.587)		(0.519)	(0.577)
28	0.154	0.0584		-0.543***	0.154	0.673	0.659		-0.280	0.673*
	(0.355)	(0.418)		(0.196)	(0.356)	(0.520)	(0.516)		(0.462)	(0.378)
29	0.261	0.183		-0.429	0.261	0.603	0.574		-0.274	0.603
	(0.372)	(0.455)		(0.286)	(0.372)	(0.596)	(0.591)		(0.472)	(0.623)
30+	0.194	0.0591		-0.712**	0.194	0.766	0.748		-0.191	0.766
	(0.417)	(0.510)		(0.320)	(0.391)	(0.806)	(0.811)		(0.688)	(0.595)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27561	29386	22846	27561	27556	10575	10733	8195	10575	10571
R^2	0.356	0.368	0.373	0.354	0.266	0.577	0.580	0.600	0.577	0.281
Controls										

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including individuals aged 15 to 49 and individuals aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. Column (5) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regressions.

Table A9: Robustness Tests for Heterogeneous Effects, Dep. Var. = Literacy

	Women					Men				
	(1) Baseline results	(2) Ages 15-49	(3) Ages 20-40	(4) Alternative control group	(5) Clustering	(6) Baseline results	(7) Ages 15-49	(8) Ages 20-40	(9) Alternative control group	(10) Clustering
0-	-0.364***	-0.341***	-0.361***	-0.180**	-0.364***	-0.189**	-0.139*	-0.369***	-0.112	-0.189**
	(0.0713)	(0.0536)	(0.0569)	(0.0782)	(0.0711)	(0.0916)	(0.0779)	(0.0790)	(0.0866)	(0.0815)
1	-0.305***	-0.262***	-0.300***	-0.135**	-0.305***	-0.182***	-0.148**	-0.336***	-0.125**	-0.182***
	(0.0502)	(0.0395)	(0.0385)	(0.0658)	(0.0468)	(0.0652)	(0.0643)	(0.0514)	(0.0596)	(0.0660)
2	-0.236***	-0.221***	-0.236***	-0.0904	-0.236***	-0.228***	-0.242***	-0.370***	-0.188***	-0.228***
	(0.0519)	(0.0436)	(0.0478)	(0.0708)	(0.0646)	(0.0646)	(0.0608)	(0.0527)	(0.0551)	(0.0457)
3	-0.193***	-0.175***	-0.192***	-0.0554	-0.193***	-0.214**	-0.213**	-0.341***	-0.175	-0.214**
	(0.0233)	(0.0217)	(0.0215)	(0.0494)	(0.0237)	(0.105)	(0.104)	(0.105)	(0.110)	(0.0861)
4	-0.148***	-0.135***	-0.147***	-0.0382	-0.148***	-0.0670	-0.0674	-0.192**	-0.0190	-0.0670
	(0.0266)	(0.0284)	(0.0283)	(0.0482)	(0.0371)	(0.0742)	(0.0733)	(0.0761)	(0.0811)	(0.0776)
5	-0.101***	-0.0892***	-0.0995***	-0.00111	-0.101***	-0.0945	-0.0949	-0.200***	-0.0614	-0.0945*
	(0.0268)	(0.0299)	(0.0301)	(0.0470)	(0.0242)	(0.0588)	(0.0571)	(0.0474)	(0.0591)	(0.0521)
6	-0.116***	-0.106***	-0.115***	-0.0410	-0.116***	-0.157	-0.158	-0.251**	-0.133	-0.157*
	(0.0353)	(0.0385)	(0.0384)	(0.0512)	(0.0319)	(0.0941)	(0.0934)	(0.0951)	(0.106)	(0.0909)
7	-0.0508	-0.0424	-0.0517	0.00829	-0.0508*	-0.125**	-0.126**	-0.207***	-0.0819	-0.125***
	(0.0340)	(0.0377)	(0.0378)	(0.0483)	(0.0267)	(0.0579)	(0.0568)	(0.0528)	(0.0685)	(0.0452)
8	-0.0374	-0.0318	-0.0370	0.0146	-0.0374	-0.0171	-0.0172	-0.0835*	0.0336	-0.0171
	(0.0232)	(0.0269)	(0.0282)	(0.0326)	(0.0231)	(0.0463)	(0.0455)	(0.0426)	(0.0497)	(0.0363)
9	-0.0221	-0.0160	-0.0202	0.0109	-0.0221	-0.125**	-0.125**	-0.187***	-0.0755	-0.125**
	(0.0401)	(0.0423)	(0.0432)	(0.0476)	(0.0368)	(0.0595)	(0.0597)	(0.0595)	(0.0646)	(0.0501)
10	0.0101	0.0134	0.00816	0.0373	0.0101	-0.0494	-0.0512	-0.107**	-0.0195	-0.0494
	(0.0295)	(0.0315)	(0.0322)	(0.0330)	(0.0262)	(0.0508)	(0.0511)	(0.0499)	(0.0581)	(0.0439)
11	-0.0277	-0.0250	-0.0252	-0.0289	-0.0277	-0.0537	-0.0558	-0.0877*	-0.0247	-0.0537
	(0.0329)	(0.0349)	(0.0342)	(0.0355)	(0.0289)	(0.0458)	(0.0461)	(0.0460)	(0.0505)	(0.0403)
12	-0.00672	-0.00735	-0.00741	0.00198	-0.00672	-0.109*	-0.110*	-0.142**	-0.0929	-0.109**
	(0.0244)	(0.0250)	(0.0256)	(0.0278)	(0.0211)	(0.0552)	(0.0556)	(0.0549)	(0.0584)	(0.0466)
13	0.00842	0.00845	0.0128	0.0111	0.00842	-0.0662	-0.0670	-0.0775	-0.0521	-0.0662
	(0.0223)	(0.0239)	(0.0240)	(0.0242)	(0.0230)	(0.0480)	(0.0482)	(0.0523)	(0.0523)	(0.0420)
14	0.0282	0.0269	0.0324	0.0188	0.0282	-0.0393	-0.0398	-0.0521	-0.0309	-0.0393
	(0.0287)	(0.0301)	(0.0300)	(0.0289)	(0.0255)	(0.0589)	(0.0598)	(0.0588)	(0.0607)	(0.0513)
16	0.00368	0.00247	0.00769	-0.0223	0.00368	-0.00223	-0.00418	0.0205	0.00473	-0.00223
	(0.0338)	(0.0349)	(0.0356)	(0.0337)	(0.0410)	(0.0562)	(0.0561)	(0.0570)	(0.0586)	(0.0438)
17	-0.00922	-0.0125	-0.00730	-0.0262	-0.00922	-0.0559	-0.0559	-0.0320	-0.0733	-0.0559
	(0.0309)	(0.0315)	(0.0322)	(0.0361)	(0.0318)	(0.0570)	(0.0569)	(0.0563)	(0.0571)	(0.0493)
18	0.0130	0.0116	0.0134	-0.0257	0.0130	0.0559	0.0558	0.0954*	0.0306	0.0559
	(0.0242)	(0.0250)	(0.0259)	(0.0209)	(0.0222)	(0.0462)	(0.0459)	(0.0479)	(0.0476)	(0.0486)
19	0.0202	0.0167	0.0248	-0.00732	0.0202	0.0689	0.0694	0.122*	0.0267	0.0689
	(0.0232)	(0.0247)	(0.0285)	(0.0206)	(0.0264)	(0.0615)	(0.0613)	(0.0631)	(0.0633)	(0.0667)
20	0.0150	0.0117	0.0169	-0.0203	0.0150	-0.0520	-0.0518	0.0261	-0.0848	-0.0520
	(0.0265)	(0.0286)	(0.0328)	(0.0237)	(0.0233)	(0.0526)	(0.0527)	(0.0539)	(0.0569)	(0.0503)
21	0.00614	0.000900	-0.00341	-0.0341	0.00614	-0.0601	-0.0605	0.00952	-0.120**	-0.0601
	(0.0458)	(0.0476)	(0.0471)	(0.0497)	(0.0434)	(0.0549)	(0.0554)	(0.0610)	(0.0582)	(0.0431)
22	0.0151	0.00804	0.0250	-0.0270	0.0151	-0.0302	-0.0298	0.0561	-0.0946*	-0.0302
	(0.0234)	(0.0257)	(0.0303)	(0.0242)	(0.0214)	(0.0409)	(0.0414)	(0.0622)	(0.0482)	(0.0456)
23 / 23+	0.0493**	0.0408*	0.0752**	0.0188	0.0493*	0.0830**	0.0850**	0.232***	0.0401	0.0830*
	(0.0206)	(0.0225)	(0.0293)	(0.0222)	(0.0270)	(0.0399)	(0.0397)	(0.0609)	(0.0469)	(0.0413)
24	0.0181	0.00770		-0.0163	0.0181	0.0289	0.0309		-0.0802	0.0289
	(0.0294)	(0.0316)		(0.0258)	(0.0322)	(0.0563)	(0.0572)		(0.0560)	(0.0534)
25	0.0175	0.00923		-0.0204	0.0175	0.0804	0.0806		-0.0288	0.0804
	(0.0330)	(0.0357)		(0.0299)	(0.0290)	(0.0674)	(0.0680)		(0.0690)	(0.0478)
26	0.0119	0.00102		-0.0475	0.0119	-0.0487	-0.0481		-0.197**	-0.0487
	(0.0541)	(0.0584)		(0.0525)	(0.0512)	(0.0832)	(0.0843)		(0.0770)	(0.0657)
27	0.0327	0.0191		-0.0352	0.0327	0.153**	0.154**		0.0762	0.153**
	(0.0357)	(0.0398)		(0.0231)	(0.0310)	(0.0692)	(0.0675)		(0.0716)	(0.0677)
28	0.0244	0.0104		-0.0491**	0.0244	-0.00272	0.000301		-0.140**	-0.00272
	(0.0396)	(0.0438)		(0.0195)	(0.0361)	(0.0575)	(0.0573)		(0.0609)	(0.0488)
29	0.0358	0.0213		-0.0376	0.0358	0.0707	0.0727		-0.0453	0.0707
	(0.0424)	(0.0486)		(0.0300)	(0.0439)	(0.0549)	(0.0538)		(0.0583)	(0.0659)
30+	0.0590	0.0404		-0.0328	0.0590	0.0610	0.0651		-0.0755	0.0610
	(0.0459)	(0.0530)		(0.0283)	(0.0445)	(0.0735)	(0.0744)		(0.0709)	(0.0462)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27537	29361	22824	27537	27532	10560	10718	8181	10560	10556
R^2	0.348	0.359	0.365	0.346	0.242	0.593	0.596	0.615	0.594	0.199
Controls										

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including individuals aged 15 to 49 and individuals aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. Column (5) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regressions.

Table A10: Robustness Tests for Heterogeneous Effects, Dep. Var = Primary School Completion

	Women					Men				
	(1) Baseline results	(2) Ages 15-49	(3) Ages 20-40	(4) Alternative control group	(5) Clustering	(6) Baseline results	(7) Ages 15-49	(8) Ages 20-40	(9) Alternative control group	(10) Clustering
0-	-0.326***	-0.324***	-0.338***	-0.172**	-0.326***	-0.243*	-0.209*	-0.324***	-0.0941	-0.243**
	(0.0646)	(0.0547)	(0.0507)	(0.0682)	(0.0633)	(0.125)	(0.107)	(0.118)	(0.124)	(0.112)
1	-0.295***	-0.257***	-0.305***	-0.160***	-0.295***	-0.246***	-0.235***	-0.294***	-0.111	-0.246***
	(0.0470)	(0.0419)	(0.0347)	(0.0561)	(0.0443)	(0.0753)	(0.0729)	(0.0587)	(0.0824)	(0.0644)
2	-0.211***	-0.207***	-0.223***	-0.0902	-0.211***	-0.256***	-0.271***	-0.304***	-0.144*	-0.256***
	(0.0469)	(0.0412)	(0.0423)	(0.0629)	(0.0587)	(0.0733)	(0.0715)	(0.0597)	(0.0738)	(0.0569)
3	-0.174***	-0.172***	-0.186***	-0.0639	-0.174***	-0.243***	-0.246***	-0.283***	-0.152*	-0.243***
	(0.0283)	(0.0260)	(0.0227)	(0.0457)	(0.0268)	(0.0885)	(0.0857)	(0.0771)	(0.0881)	(0.0736)
4	-0.122***	-0.118***	-0.132***	-0.0384	-0.122***	-0.144**	-0.148**	-0.184***	-0.0723	-0.144**
	(0.0260)	(0.0291)	(0.0241)	(0.0408)	(0.0331)	(0.0581)	(0.0576)	(0.0475)	(0.0558)	(0.0589)
5	-0.0775***	-0.0738***	-0.0858***	0.000688	-0.0775***	-0.104*	-0.106*	-0.141***	-0.0541	-0.104*
	(0.0228)	(0.0269)	(0.0230)	(0.0380)	(0.0233)	(0.0599)	(0.0590)	(0.0504)	(0.0601)	(0.0568)
6	-0.0966***	-0.0936**	-0.105***	-0.0403	-0.0966**	-0.0893	-0.0921	-0.123**	-0.0518	-0.0893
	(0.0332)	(0.0362)	(0.0347)	(0.0461)	(0.0355)	(0.0630)	(0.0618)	(0.0550)	(0.0633)	(0.0562)
7	-0.0285	-0.0270	-0.0381	0.0196	-0.0285	-0.0637	-0.0652	-0.0883	-0.0107	-0.0637
	(0.0308)	(0.0345)	(0.0341)	(0.0439)	(0.0302)	(0.0801)	(0.0796)	(0.0744)	(0.0854)	(0.0720)
8	-0.0383*	-0.0378	-0.0448*	-0.00645	-0.0383*	-0.0303	-0.0306	-0.0505	0.0288	-0.0303
	(0.0214)	(0.0248)	(0.0242)	(0.0296)	(0.0222)	(0.0630)	(0.0632)	(0.0616)	(0.0671)	(0.0614)
9	-0.00685	-0.00557	-0.0122	0.0172	-0.00685	-0.0877	-0.0873	-0.104	-0.0323	-0.0877
	(0.0325)	(0.0347)	(0.0359)	(0.0390)	(0.0336)	(0.0808)	(0.0808)	(0.0797)	(0.0838)	(0.0814)
10	-0.000821	-0.00100	-0.00785	0.0173	-0.000821	-0.0183	-0.0182	-0.0363	0.0151	-0.0183
	(0.0301)	(0.0320)	(0.0325)	(0.0335)	(0.0310)	(0.0584)	(0.0584)	(0.0604)	(0.0610)	(0.0540)
11	-0.0335	-0.0331	-0.0364	-0.0326	-0.0335	-0.0696	-0.0699	-0.0807	-0.0387	-0.0696
	(0.0282)	(0.0299)	(0.0302)	(0.0306)	(0.0276)	(0.0576)	(0.0575)	(0.0587)	(0.0596)	(0.0553)
12	-0.0109	-0.0137	-0.0145	-0.00742	-0.0109	-0.0933	-0.0926	-0.0997	-0.0763	-0.0933
	(0.0225)	(0.0226)	(0.0231)	(0.0240)	(0.0231)	(0.0749)	(0.0749)	(0.0750)	(0.0788)	(0.0824)
13	0.00541	0.00466	0.00647	0.00855	0.00541	-0.0470	-0.0471	-0.0431	-0.0313	-0.0470
	(0.0197)	(0.0208)	(0.0218)	(0.0205)	(0.0202)	(0.0453)	(0.0452)	(0.0491)	(0.0503)	(0.0526)
14	0.00898	0.00681	0.0118	0.00402	0.00898	-0.0534	-0.0530	-0.0594	-0.0458	-0.0534
	(0.0174)	(0.0185)	(0.0188)	(0.0167)	(0.0294)	(0.0445)	(0.0451)	(0.0423)	(0.0445)	(0.0451)
16	-0.0157	-0.0174	-0.0116	-0.0377	-0.0157	0.0159	0.0150	0.0263	0.0244	0.0159
	(0.0292)	(0.0297)	(0.0312)	(0.0297)	(0.0363)	(0.0510)	(0.0509)	(0.0508)	(0.0534)	(0.0437)
17	-0.0110	-0.0145	-0.00816	-0.0254	-0.0110	-0.0304	-0.0293	-0.0234	-0.0579	-0.0304
	(0.0277)	(0.0285)	(0.0281)	(0.0333)	(0.0326)	(0.0434)	(0.0434)	(0.0453)	(0.0445)	(0.0533)
18	-0.00618	-0.00832	-0.00427	-0.0361**	-0.00618	0.0172	0.0173	0.0306	-0.0192	0.0172
	(0.0201)	(0.0205)	(0.0222)	(0.0171)	(0.0201)	(0.0597)	(0.0601)	(0.0640)	(0.0629)	(0.0624)
19	0.00566	0.00317	0.0117	-0.0196	0.00566	0.110*	0.111*	0.132*	0.0761	0.110
	(0.0223)	(0.0235)	(0.0274)	(0.0186)	(0.0266)	(0.0636)	(0.0631)	(0.0664)	(0.0666)	(0.0665)
20	-0.00586	-0.00857	-0.00130	-0.0397*	-0.00586	-0.0643	-0.0629	-0.0368	-0.0980	-0.0643
	(0.0252)	(0.0269)	(0.0320)	(0.0203)	(0.0251)	(0.0616)	(0.0619)	(0.0625)	(0.0655)	(0.0646)
21	-0.0106	-0.0143	-0.00670	-0.0477	-0.0106	0.00503	0.00477	0.0311	-0.0619	0.00503
	(0.0327)	(0.0349)	(0.0402)	(0.0334)	(0.0323)	(0.0739)	(0.0751)	(0.0770)	(0.0747)	(0.0700)
22	-0.00204	-0.00771	0.0169	-0.0403	-0.00204	0.0318	0.0341	0.0437	-0.0287	0.0318
	(0.0272)	(0.0296)	(0.0323)	(0.0269)	(0.0276)	(0.0423)	(0.0428)	(0.0677)	(0.0440)	(0.0524)
23 / 23+	0.0387	0.0317	0.0576*	0.0163	0.0387	0.0116	0.0132	0.136**	-0.0487	0.0116
	(0.0266)	(0.0272)	(0.0302)	(0.0298)	(0.0318)	(0.0602)	(0.0620)	(0.0622)	(0.0718)	(0.0614)
24	0.00147	-0.00664		-0.0301	0.00147	0.114**	0.116**		0.0557	0.114*
	(0.0313)	(0.0342)		(0.0225)	(0.0377)	(0.0548)	(0.0550)		(0.0537)	(0.0649)
25	0.00846	0.00285		-0.0306	0.00846	0.0735	0.0729		0.000327	0.0735
	(0.0343)	(0.0378)		(0.0294)	(0.0335)	(0.0680)	(0.0690)		(0.0588)	(0.0620)
26	0.00453	-0.00368		-0.0494	0.00453	-0.0622	-0.0616		-0.162*	-0.0622
	(0.0448)	(0.0491)		(0.0419)	(0.0434)	(0.0907)	(0.0926)		(0.0831)	(0.0825)
27	0.0315	0.0214		-0.0317*	0.0315	0.136*	0.138*		0.0776	0.136*
	(0.0347)	(0.0389)		(0.0187)	(0.0339)	(0.0691)	(0.0686)		(0.0727)	(0.0727)
28	0.0161	0.00599		-0.0525**	0.0161	0.0780	0.0814		-0.0131	0.0780
	(0.0423)	(0.0465)		(0.0226)	(0.0437)	(0.0579)	(0.0577)		(0.0630)	(0.0567)
29	0.0245	0.0151		-0.0428	0.0245	0.0922	0.0945		0.00344	0.0922
	(0.0426)	(0.0499)		(0.0293)	(0.0467)	(0.0627)	(0.0616)		(0.0656)	(0.0741)
30+	0.0279	0.0140		-0.0555*	0.0279	0.123*	0.128*		0.0352	0.123**
	(0.0443)	(0.0520)		(0.0308)	(0.0457)	(0.0704)	(0.0717)		(0.0707)	(0.0545)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27561	29386	22846	27561	27556	10575	10733	8195	10575	10571
R^2	0.300	0.310	0.317	0.298	0.224	0.508	0.512	0.529	0.508	0.226
Controls										

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including individuals aged 15 to 49 and individuals aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. Column (5) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regressions.

Table A11: Robustness Tests for Heterogeneous Effects, Dep. Var. = Labor Market Participation

	Women					Men				
	(1) Baseline results	(2) Ages 15-49	(3) Ages 20-40	(4) Alternative control group	(5) Clustering	(6) Baseline results	(7) Ages 15-49	(8) Ages 20-40	(9) Alternative control group	(10) Clustering
0-	0.107	0.0623	0.0572	0.103	0.107	0.0406	0.0387	0.0905	-0.00578	0.0406
	(0.0789)	(0.0582)	(0.0552)	(0.0716)	(0.0757)	(0.0598)	(0.0427)	(0.0578)	(0.0544)	(0.0427)
1	0.0216	0.00111	-0.0224	0.0148	0.0216	0.0516	0.0228	0.0924*	0.00102	0.0516
	(0.0514)	(0.0428)	(0.0384)	(0.0450)	(0.0596)	(0.0491)	(0.0454)	(0.0457)	(0.0483)	(0.0389)
2	-0.00223	-0.0146	-0.0392	-0.00783	-0.00223	0.0734*	0.0598*	0.114***	0.0439	0.0734**
	(0.0358)	(0.0289)	(0.0276)	(0.0271)	(0.0462)	(0.0375)	(0.0326)	(0.0320)	(0.0315)	(0.0267)
3	0.0241	0.00731	-0.0101	0.0272	0.0241	0.0814**	0.0707**	0.119***	0.0568**	0.0814***
	(0.0335)	(0.0292)	(0.0326)	(0.0291)	(0.0318)	(0.0318)	(0.0286)	(0.0243)	(0.0221)	(0.0234)
4	-0.0434	-0.0558**	-0.0738**	-0.0493*	-0.0434	0.0678**	0.0616**	0.104***	0.0388*	0.0678***
	(0.0278)	(0.0256)	(0.0301)	(0.0255)	(0.0293)	(0.0303)	(0.0285)	(0.0241)	(0.0221)	(0.0220)
5	0.0166	0.00642	-0.0116	0.0177	0.0166	0.0666**	0.0605**	0.0966***	0.0434**	0.0666***
	(0.0243)	(0.0206)	(0.0228)	(0.0220)	(0.0245)	(0.0271)	(0.0264)	(0.0205)	(0.0201)	(0.0241)
6	-0.0133	-0.0213	-0.0370	-0.0186	-0.0133	0.0739***	0.0672**	0.101***	0.0566***	0.0739***
	(0.0282)	(0.0260)	(0.0297)	(0.0290)	(0.0270)	(0.0270)	(0.0266)	(0.0218)	(0.0194)	(0.0190)
7	-0.0152	-0.0202	-0.0328	-0.0173	-0.0152	0.0373	0.0336	0.0644**	0.0144	0.0373
	(0.0284)	(0.0269)	(0.0272)	(0.0290)	(0.0277)	(0.0275)	(0.0270)	(0.0253)	(0.0255)	(0.0282)
8	0.0186	0.0127	0.00223	0.0169	0.0186	0.0275	0.0239	0.0506**	0.00909	0.0275
	(0.0299)	(0.0274)	(0.0263)	(0.0318)	(0.0301)	(0.0240)	(0.0236)	(0.0226)	(0.0236)	(0.0247)
9	-0.0142	-0.0179	-0.0294	-0.0226	-0.0142	0.0361*	0.0332*	0.0593***	0.0285	0.0361*
	(0.0252)	(0.0238)	(0.0227)	(0.0267)	(0.0250)	(0.0192)	(0.0192)	(0.0188)	(0.0188)	(0.0190)
10	0.0140	0.0114	0.00642	0.00739	0.0140	0.0164	0.0143	0.0325*	0.0105	0.0164
	(0.0278)	(0.0273)	(0.0273)	(0.0295)	(0.0243)	(0.0189)	(0.0191)	(0.0186)	(0.0198)	(0.0168)
11	0.0210	0.0200	0.0139	0.00857	0.0210	0.0161	0.0147	0.0323	0.0257	0.0161
	(0.0484)	(0.0480)	(0.0458)	(0.0544)	(0.0532)	(0.0213)	(0.0216)	(0.0215)	(0.0201)	(0.0187)
12	0.0176	0.0173	0.0120	0.00894	0.0176	0.0117	0.0106	0.0206	0.00688	0.0117
	(0.0244)	(0.0243)	(0.0241)	(0.0277)	(0.0195)	(0.0171)	(0.0173)	(0.0169)	(0.0172)	(0.0153)
13	-0.0102	-0.0121	-0.0184	-0.0251	-0.0102	0.00595	0.00490	0.0138	0.000169	0.00595
	(0.0363)	(0.0358)	(0.0337)	(0.0413)	(0.0413)	(0.0222)	(0.0222)	(0.0229)	(0.0232)	(0.0228)
14	0.0205	0.0209	0.0146	0.0176	0.0205	0.00743	0.00750	0.00900	0.00726	0.00743
	(0.0358)	(0.0358)	(0.0336)	(0.0388)	(0.0350)	(0.0134)	(0.0131)	(0.0144)	(0.0143)	(0.00988)
16	-0.00215	-0.00113	-0.00510	0.00259	-0.00215	0.00474	0.00592	0.00400	0.00375	0.00474
	(0.0288)	(0.0286)	(0.0280)	(0.0344)	(0.0312)	(0.0168)	(0.0165)	(0.0183)	(0.0178)	(0.0144)
17	-0.0125	-0.0107	-0.00934	-0.00933	-0.0125	-0.0117	-0.0102	-0.0170	-0.00918	-0.0117
	(0.0539)	(0.0550)	(0.0555)	(0.0611)	(0.0524)	(0.0182)	(0.0183)	(0.0183)	(0.0196)	(0.0168)
18	0.0220	0.0251	0.0313	0.0351	0.0220	-0.0275*	-0.0250*	-0.0369**	-0.00853	-0.0275*
	(0.0274)	(0.0296)	(0.0321)	(0.0291)	(0.0227)	(0.0146)	(0.0146)	(0.0144)	(0.0143)	(0.0151)
19	-0.0225	-0.0174	-0.0117	-0.0165	-0.0225	0.00225	0.00397	-0.0113	0.00775	0.00225
	(0.0439)	(0.0453)	(0.0455)	(0.0456)	(0.0428)	(0.0180)	(0.0176)	(0.0196)	(0.0206)	(0.0183)
20	-0.00773	-0.00128	0.0110	-0.00290	-0.00773	-0.0291*	-0.0265*	-0.0487***	-0.0245	-0.0291**
	(0.0390)	(0.0422)	(0.0466)	(0.0380)	(0.0348)	(0.0145)	(0.0141)	(0.0146)	(0.0172)	(0.0134)
21	-0.0150	-0.00779	0.0185	-0.0107	-0.0150	-0.00395	-0.00163	-0.0237	-0.00123	-0.00395
	(0.0287)	(0.0301)	(0.0338)	(0.0278)	(0.0322)	(0.0203)	(0.0202)	(0.0283)	(0.0220)	(0.0241)
22	0.00239	0.00862	0.0179	0.0104	0.00239	-0.0280	-0.0258	-0.0670***	-0.0223	-0.0280
	(0.0303)	(0.0327)	(0.0472)	(0.0270)	(0.0288)	(0.0182)	(0.0178)	(0.0192)	(0.0207)	(0.0191)
23 / 23+	-0.00506	0.000284	0.0121	0.0108	-0.00506	-0.0287*	-0.0255	-0.0743***	-0.00916	-0.0287
	(0.0276)	(0.0300)	(0.0447)	(0.0234)	(0.0353)	(0.0160)	(0.0158)	(0.0214)	(0.0199)	(0.0181)
24	-0.0159	-0.00698		-0.0161	-0.0159	-0.0262	-0.0229		-0.0200	-0.0262
	(0.0622)	(0.0668)		(0.0634)	(0.0609)	(0.0194)	(0.0190)		(0.0205)	(0.0193)
25	-0.00997	-0.00379		-0.0217	-0.00997	-0.00351	0.000282		-0.00505	-0.00351
	(0.0423)	(0.0460)		(0.0359)	(0.0456)	(0.0155)	(0.0143)		(0.0174)	(0.0186)
26	0.00533	0.0136		-0.0126	0.00533	-0.0358	-0.0322		-0.0409	-0.0358
	(0.0523)	(0.0563)		(0.0465)	(0.0574)	(0.0224)	(0.0216)		(0.0265)	(0.0237)
27	0.0431	0.0532		0.0274	0.0431	0.0103	0.0151		-0.00116	0.0103
	(0.0736)	(0.0797)		(0.0698)	(0.0739)	(0.0277)	(0.0262)		(0.0290)	(0.0333)
28	0.0962	0.107		0.0702	0.0962	-0.0572*	-0.0516*		-0.0772**	-0.0572**
	(0.0894)	(0.0971)		(0.0797)	(0.0884)	(0.0292)	(0.0288)		(0.0324)	(0.0254)
29	0.108	0.118		0.0804	0.108	0.0153	0.0207		0.00515	0.0153
	(0.0963)	(0.103)		(0.0809)	(0.0946)	(0.0237)	(0.0227)		(0.0226)	(0.0407)
30+	0.0553	0.0715		0.00914	0.0553	0.00654	0.0125		-0.00910	0.00654
	(0.0851)	(0.0953)		(0.0602)	(0.0836)	(0.0277)	(0.0270)		(0.0276)	(0.0325)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27579	29406	22862	27579	27574	10591	10749	8205	10591	10587
R^2	0.317	0.312	0.321	0.317	0.212	0.972	0.971	0.973	0.972	0.095
Controls										

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including individuals aged 15 to 49 and individuals aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. Column (5) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regressions.

Table A12: Robustness Tests for the Analysis at the Start of Schooling, for Women

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) Province-age clustering
Panel A: Years of schooling						
Taliban control x Turned 6	-0.671**	-0.591**	-0.575**	-0.409	-0.442	-0.671**
	(0.255)	(0.221)	(0.244)	(0.245)	(0.373)	(0.253)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27556
R^2	0.349	0.360	0.365	0.348	0.348	0.258
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy						
Taliban control x Turned 6	-0.0756**	-0.0699**	-0.0650**	-0.0467	-0.0467	-0.0756**
	(0.0314)	(0.0296)	(0.0303)	(0.0289)	(0.0360)	(0.0288)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	27537	29361	22824	27537	27537	27532
R^2	0.343	0.353	0.360	0.342	0.342	0.236
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion						
Taliban control x Turned 6	-0.0669**	-0.0597**	-0.0591**	-0.0438*	-0.0407	-0.0669**
	(0.0288)	(0.0262)	(0.0283)	(0.0240)	(0.0388)	(0.0286)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	27561	29386	22846	27561	27561	27556
R^2	0.293	0.303	0.309	0.292	0.292	0.216
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labour market participation						
Taliban control x Turned 6	-0.00347	-0.00386	-0.0165	-0.000899	0.00746	-0.00347
	(0.0124)	(0.0122)	(0.0153)	(0.0135)	(0.0141)	(0.0156)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	27579	29406	22862	27579	27579	27574
R^2	0.313	0.308	0.317	0.313	0.313	0.207
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including women aged 15 to 49 and women aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact the actual begin of the Taliban rule at the province level is not precisely known. Column (6) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Table A13: Robustness Tests for the Analysis at the Start of Schooling, for Men

	(1) Reported baseline	(2) Ages 15 to 49	(3) Ages 20 to 40	(4) Alternative Control group	(5) Shifted Exposure	(6) Province-age clustering
Panel A: Years of schooling						
Taliban control x Turned 6	-0.426	-0.460	-0.421	-0.601	-0.306	-0.426
	(0.450)	(0.449)	(0.448)	(0.408)	(0.490)	(0.371)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10571
R^2	0.573	0.576	0.596	0.573	0.573	0.274
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Literacy						
Taliban control x Turned 6	-0.0330	-0.0366	-0.0455	-0.0520	0.0286	-0.0330
	(0.0406)	(0.0408)	(0.0410)	(0.0383)	(0.0617)	(0.0245)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	10560	10718	8181	10560	10560	10556
R^2	0.589	0.592	0.611	0.589	0.589	0.191
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Primary school completion						
Taliban control x Turned 6	-0.0473	-0.0470	-0.0525	-0.0602*	-0.0534	-0.0473
	(0.0400)	(0.0391)	(0.0376)	(0.0352)	(0.0404)	(0.0301)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	10575	10733	8195	10575	10575	10571
R^2	0.503	0.507	0.526	0.504	0.503	0.219
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Labour market participation						
Taliban control x Turned 6	0.0390*	0.0384*	0.0328	0.0394**	0.0157	0.0390*
	(0.0214)	(0.0213)	(0.0233)	(0.0181)	(0.0139)	(0.0202)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	10591	10749	8205	10591	10591	10587
R^2	0.972	0.971	0.973	0.972	0.972	0.089
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. Standard errors are reported in parentheses. The table presents the results of several robustness tests. Column (1) reports the baseline results. Columns (2) and (3) run the specification with an alternative sample, i.e. respectively including men aged 15 to 49 and men aged 20 to 40. Column (4) uses an alternative (never treated) control group including both provinces that never were under Taliban rule between 1994 and 2001 and provinces that were only partially under Taliban control: Panjsher, Badakhshan, Kapisa, Laghman, Kunhara, Nooristan, Baghlan and Takhar provinces. In column (5), the begin of the Taliban rule is shifted 1 year in the future to account for the fact the actual begin of the Taliban rule at the province level is not precisely known. Column (6) clusters at the province-age level instead of the province level. The set of potential covariates includes ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area. Sampling weights are used in all regression.

Figure A1: Age at First Taliban Exposure and Age at the Time of the Survey Correspondence

	Age in 2015 of people in provinces seized in …					
Age at Taliban arrival	**1994**	**1995**	**1996**	**1997**	**1998**	**1999**
-6	15	14	13	12	11	10
-5	16	15	14	13	12	11
-4	17	16	15	14	13	12
-3	18	17	16	15	14	13
-2	19	18	17	16	15	14
-1	20	19	18	17	16	15
0	21	20	19	18	17	16
1	22	21	20	19	18	17
2	23	22	21	20	19	18
3	24	23	22	21	20	19
4	25	24	23	22	21	20
5	26	25	24	23	22	21
6	27	26	25	24	23	22
7	28	27	26	25	24	23
8	29	28	27	26	25	24
9	30	29	28	27	26	25
10	31	30	29	28	27	26
11	32	31	30	29	28	27
12	33	32	31	30	29	28
13	34	33	32	31	30	29
14	35	34	33	32	31	30
15	36	35	34	33	32	31
16	37	36	35	34	33	32
17	38	37	36	35	34	33
18	39	38	37	36	35	34
19	40	39	38	37	36	35
20	41	40	39	38	37	36
21	42	41	40	39	38	37
22	43	42	41	40	39	38
23	44	43	42	41	40	39
24	45	44	43	42	41	40
25	46	45	44	43	42	41
26	47	46	45	44	43	42
27	48	47	46	45	44	43
28	49	48	47	46	45	44
29	50	49	48	47	46	45
30	51	50	49	48	47	46
31	52	51	50	49	48	47
32	53	52	51	50	49	48
33	54	53	52	51	50	49

Notes: This table illustrates how an individual's age at Taliban arrival is computed based on their age in 2015 and the timing of the Taliban occupation in their province of residence.

Figure A2: Years of Taliban Exposure by Timing of the Taliban Occupation at the Province Level

								Province occupied from … to 2001:											
								1994		1995		1996		1997		1998		1999	
Age in 2015	Born in	5 in …	6 in …	7 in …	14 in …	15 in …	20 in …	0 to 5	6 to 14	0 to 5	6 to 14	0 to 5	6 to 14	0 to 5	6 to 14	0 to 5	6 to 14	0 to 5	6 t 14
15	2000	2005	2006	2007	2014	2015	2020	2	0	2	0	2	0	2	0	2	0	2	0
16	1999	2004	2005	2006	2013	2014	2019	3	0	3	0	3	0	3	0	3	0	3	0
17	1998	2003	2004	2005	2012	2013	2018	4	0	4	0	4	0	4	0	4	0	3	0
18	1997	2002	2003	2004	2011	2012	2017	5	0	5	0	5	0	5	0	4	0	3	0
19	1996	2001	2002	2003	2010	2011	2016	6	0	6	0	6	0	5	0	4	0	3	0
20	1995	2000	2001	2002	2009	2010	2015	6	1	6	1	5	1	4	1	3	1	2	1
21	1994	1999	2000	2001	2008	2009	2014	6	2	5	2	4	2	3	2	2	2	1	2
22	1993	1998	1999	2000	2007	2008	2013	5	3	4	3	3	3	2	3	1	3	0	3
23	1992	1997	1998	1999	2006	2007	2012	4	4	3	4	2	4	1	4	0	4	0	3
24	1991	1996	1997	1998	2005	2006	2011	3	5	2	5	1	5	0	5	0	4	0	3
25	1990	1995	1996	1997	2004	2005	2010	2	6	1	6	0	6	0	5	0	4	0	3
26	1989	1994	1995	1996	2003	2004	2009	1	7	0	7	0	6	0	5	0	4	0	3
27	1988	1993	1994	1995	2002	2003	2008	0	8	0	7	0	6	0	5	0	4	0	3
28	1987	1992	1993	1994	2001	2002	2007	0	8	0	7	0	6	0	5	0	4	0	3
29	1986	1991	1992	1993	2000	2001	2006	0	7	0	6	0	5	0	4	0	3	0	2
30	1985	1990	1991	1992	1999	2000	2005	0	6	0	5	0	4	0	3	0	2	0	1
31	1984	1989	1990	1991	1998	1999	2004	0	5	0	4	0	3	0	2	0	1	0	0
32	1983	1988	1989	1990	1997	1998	2003	0	4	0	3	0	2	0	1	0	0	0	0
33	1982	1987	1988	1989	1996	1997	2002	0	3	0	2	0	1	0	0	0	0	0	0
34	1981	1986	1987	1988	1995	1996	2001	0	2	0	1	0	0	0	0	0	0	0	0
35	1980	1985	1986	1987	1994	1995	2000	0	1	0	0	0	0	0	0	0	0	0	0
36	1979	1984	1985	1986	1993	1994	1999	0	0	0	0	0	0	0	0	0	0	0	0
37	1978	1983	1984	1985	1992	1993	1998	0	0	0	0	0	0	0	0	0	0	0	0
38	1977	1982	1983	1984	1991	1992	1997	0	0	0	0	0	0	0	0	0	0	0	0
39	1976	1981	1982	1983	1990	1991	1996	0	0	0	0	0	0	0	0	0	0	0	0
40	1975	1980	1981	1982	1989	1990	1995	0	0	0	0	0	0	0	0	0	0	0	0
41	1974	1979	1980	1981	1988	1989	1994	0	0	0	0	0	0	0	0	0	0	0	0
42	1973	1978	1979	1980	1987	1988	1993	0	0	0	0	0	0	0	0	0	0	0	0
43	1972	1977	1978	1979	1986	1987	1992	0	0	0	0	0	0	0	0	0	0	0	0
44	1971	1976	1977	1978	1985	1986	1991	0	0	0	0	0	0	0	0	0	0	0	0
45	1970	1975	1976	1977	1984	1985	1990	0	0	0	0	0	0	0	0	0	0	0	0
46	1969	1974	1975	1976	1983	1984	1989	0	0	0	0	0	0	0	0	0	0	0	0
47	1968	1973	1974	1975	1982	1983	1988	0	0	0	0	0	0	0	0	0	0	0	0
48	1967	1972	1973	1974	1981	1982	1987	0	0	0	0	0	0	0	0	0	0	0	0
49	1966	1971	1972	1973	1980	1981	1986	0	0	0	0	0	0	0	0	0	0	0	0

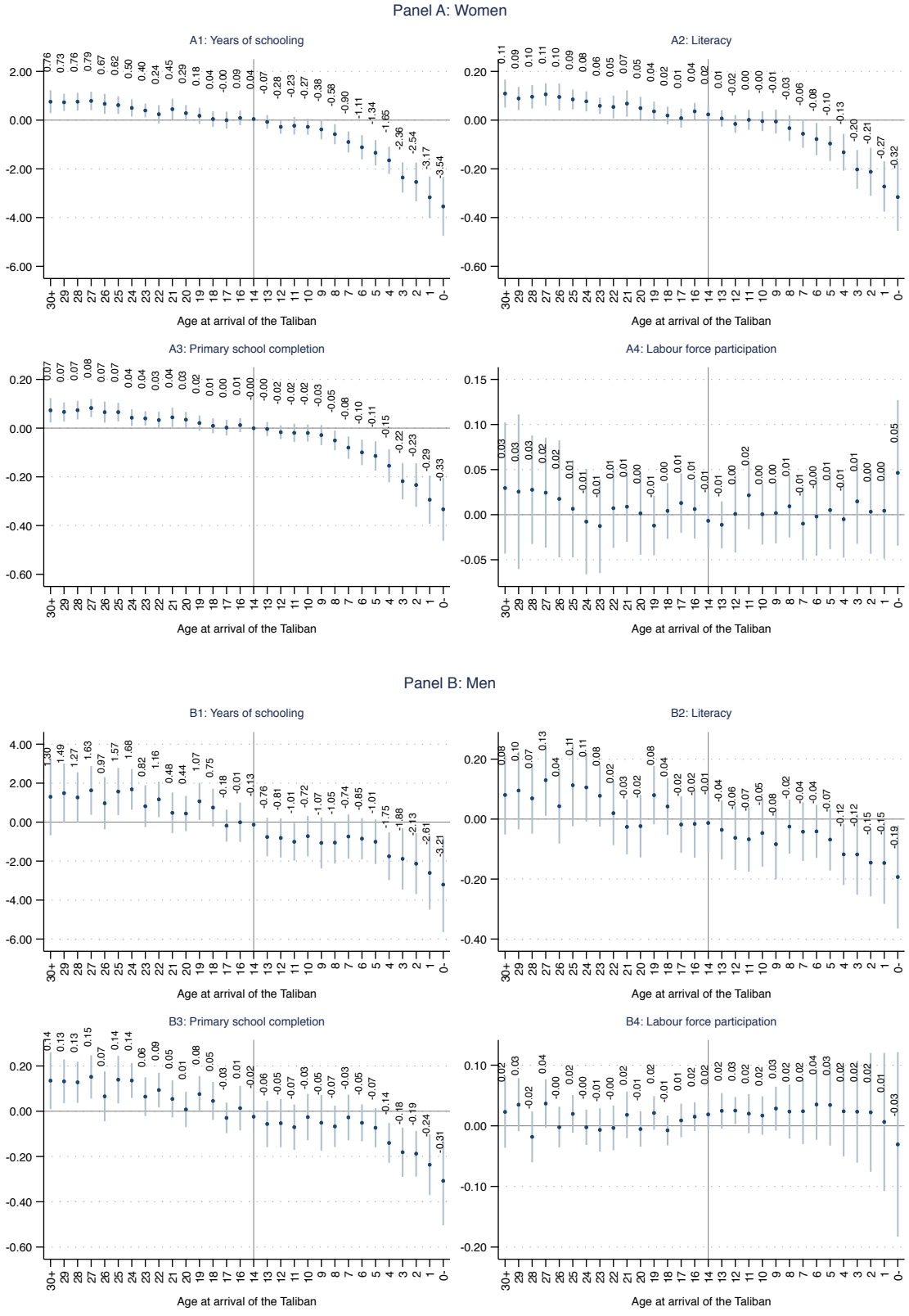
Notes: This table illustrates how the number of years of exposure to the Taliban regime while of preschool and compulsory school age are computed in the baseline analysis.

Figure A3: Years of Taliban Exposure by Timing of the Taliban Occupation at the Province Level (for Robustness Tests)

								Province occupied from … to 2001:												Province occupied from … to 2001:											
								1994		1995		1996		1997		1998		1999		1994		1995		1996		1997		1998		1999	
Age in 2015	Born in	5 in …	6 in …	7 in …	14 in …	15 in …	20 in …	0 to 6	7 to 15	0 to 6	7 to 15	0 to 6	7 to 15	0 to 6	7 to 15	0 to 6	7 to 15	0to 6	7 to 15	0 to 5	6 to 20	0 to 5	6 to 20	0 to 5	6 to 20	0 to 5	6 to 20	0 to 5	6 to 20	0 to 5	6 to 20
15	2000	2005	2006	2007	2014	2015	2020	2	0	2	0	2	0	2	0	2	0	2	0	2	0	2	0	2	0	2	0	2	0	2	0
16	1999	2004	2005	2006	2013	2014	2019	3	0	3	0	3	0	3	0	3	0	3	0	3	0	3	0	3	0	3	0	3	0	3	0
17	1998	2003	2004	2005	2012	2013	2018	4	0	4	0	4	0	4	0	4	0	3	0	4	0	4	0	4	0	4	0	4	0	3	0
18	1997	2002	2003	2004	2011	2012	2017	5	0	5	0	5	0	5	0	4	0	3	0	5	0	5	0	5	0	5	0	4	0	3	0
19	1996	2001	2002	2003	2010	2011	2016	6	0	6	0	6	0	5	0	4	0	3	0	6	0	6	0	6	0	5	0	4	0	3	0
20	1995	2000	2001	2002	2009	2010	2015	7	0	7	0	6	0	5	0	4	0	3	0	6	1	6	1	5	1	4	1	3	1	2	1
21	1994	1999	2000	2001	2008	2009	2014	7	1	6	1	5	1	4	1	3	1	2	1	6	2	5	2	4	2	3	2	2	2	1	2
22	1993	1998	1999	2000	2007	2008	2013	6	2	5	2	4	2	3	2	2	2	1	2	5	3	4	3	3	3	2	3	1	3	0	3
23	1992	1997	1998	1999	2006	2007	2012	5	3	4	3	3	3	2	3	1	3	0	3	4	4	3	4	2	4	1	4	0	4	0	3
24	1991	1996	1997	1998	2005	2006	2011	4	4	3	4	2	4	1	4	0	4	0	3	3	5	2	5	1	5	0	5	0	4	0	3
25	1990	1995	1996	1997	2004	2005	2010	3	5	2	5	1	5	0	5	0	4	0	3	2	6	1	6	0	6	0	5	0	4	0	3
26	1989	1994	1995	1996	2003	2004	2009	2	6	1	6	0	6	0	5	0	4	0	3	1	7	0	7	0	6	0	5	0	4	0	3
27	1988	1993	1994	1995	2002	2003	2008	1	7	0	7	0	6	0	5	0	4	0	3	0	8	0	7	0	6	0	5	0	4	0	3
28	1987	1992	1993	1994	2001	2002	2007	0	8	0	7	0	6	0	5	0	4	0	3	0	8	0	7	0	6	0	5	0	4	0	3
29	1986	1991	1992	1993	2000	2001	2006	0	8	0	7	0	6	0	5	0	4	0	3	0	8	0	7	0	6	0	5	0	4	0	3
30	1985	1990	1991	1992	1999	2000	2005	0	7	0	6	0	5	0	4	0	3	0	2	0	8	0	7	0	6	0	5	0	4	0	3
31	1984	1989	1990	1991	1998	1999	2004	0	6	0	5	0	4	0	3	0	2	0	1	0	8	0	7	0	6	0	5	0	4	0	3
32	1983	1988	1989	1990	1997	1998	2003	0	5	0	4	0	3	0	2	0	1	0	0	0	8	0	7	0	6	0	5	0	4	0	3
33	1982	1987	1988	1989	1996	1997	2002	0	4	0	3	0	2	0	1	0	0	0	0	0	8	0	7	0	6	0	5	0	4	0	3
34	1981	1986	1987	1988	1995	1996	2001	0	3	0	2	0	1	0	0	0	0	0	0	0	8	0	7	0	6	0	5	0	4	0	3
35	1980	1985	1986	1987	1994	1995	2000	0	2	0	1	0	0	0	0	0	0	0	0	0	7	0	6	0	5	0	4	0	3	0	2
36	1979	1984	1985	1986	1993	1994	1999	0	1	0	0	0	0	0	0	0	0	0	0	0	6	0	5	0	4	0	3	0	2	0	1
37	1978	1983	1984	1985	1992	1993	1998	0	0	0	0	0	0	0	0	0	0	0	0	0	5	0	4	0	3	0	2	0	1	0	0
38	1977	1982	1983	1984	1991	1992	1997	0	0	0	0	0	0	0	0	0	0	0	0	0	4	0	3	0	2	0	1	0	0	0	0
39	1976	1981	1982	1983	1990	1991	1996	0	0	0	0	0	0	0	0	0	0	0	0	0	3	0	2	0	1	0	0	0	0	0	0
40	1975	1980	1981	1982	1989	1990	1995	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	1	0	0	0	0	0	0	0	0
41	1974	1979	1980	1981	1988	1989	1994	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0
42	1973	1978	1979	1980	1987	1988	1993	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
43	1972	1977	1978	1979	1986	1987	1992	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
44	1971	1976	1977	1978	1985	1986	1991	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
45	1970	1975	1976	1977	1984	1985	1990	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
46	1969	1974	1975	1976	1983	1984	1989	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
47	1968	1973	1974	1975	1982	1983	1988	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
48	1967	1972	1973	1974	1981	1982	1987	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
49	1966	1971	1972	1973	1980	1981	1986	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

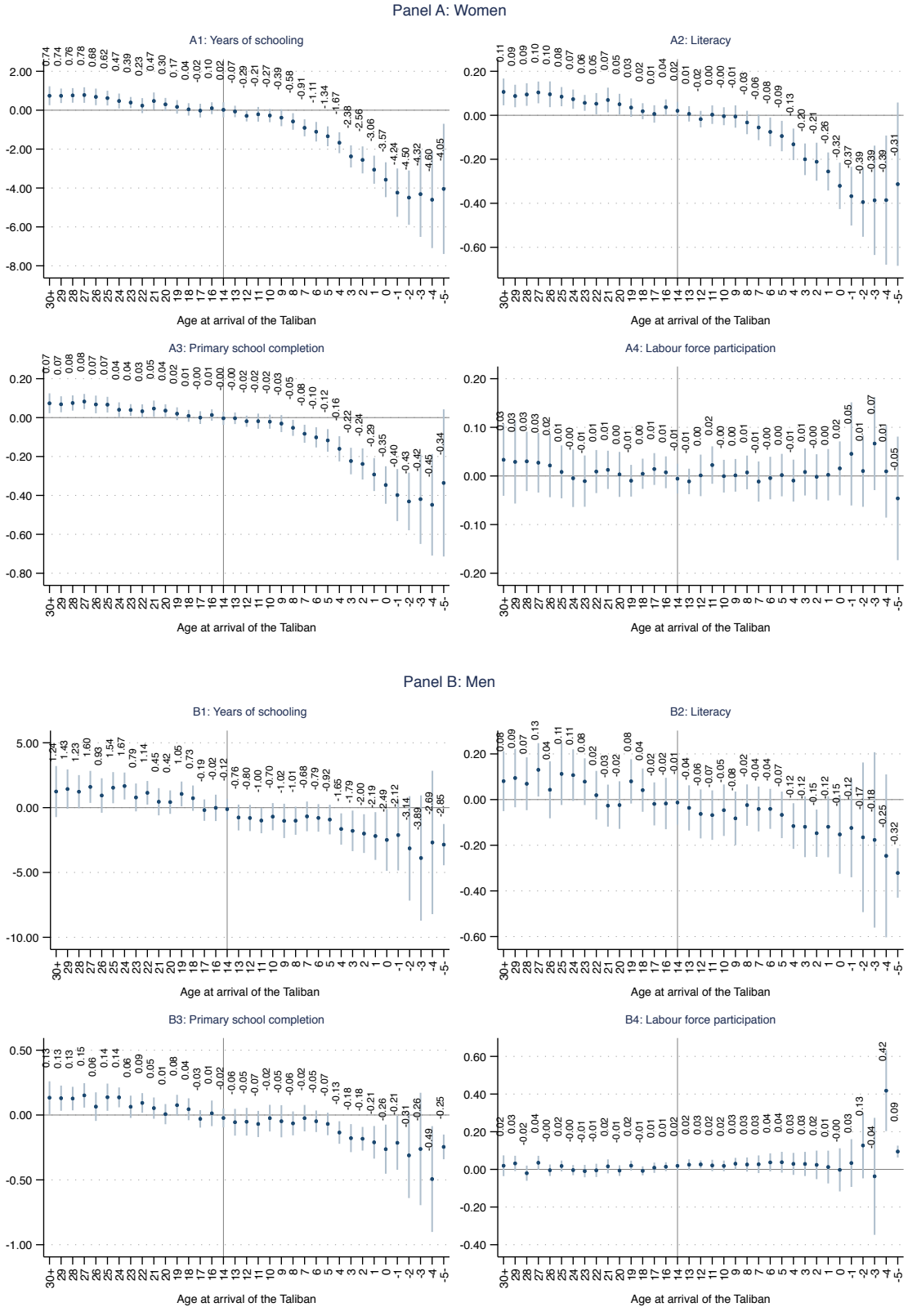
Notes: This table illustrates how the number of years of exposure to the Taliban regime while of preschool and compulsory school age are computed. This concerns the robustness tests varying the school age definition.

Figure A4: Treatment Effect Heterogeneity by Age at the Taliban Arrival: 15 to 49 Year-Olds (Unweighted)



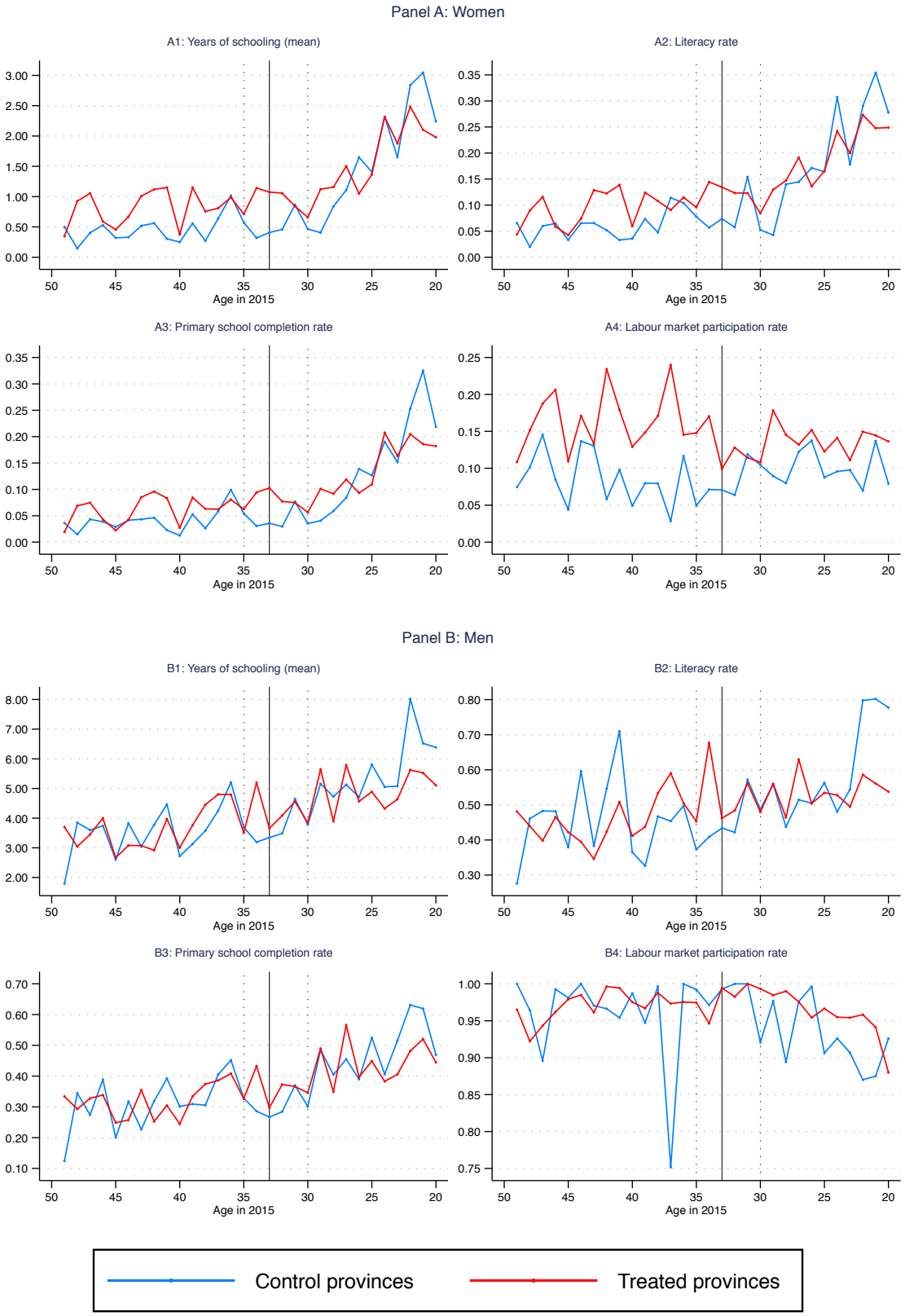
Notes: This figure reports the average effect of exposure to the Taliban rule when this exposure begins at age j. It plots the $AgeAtTalibanArrival_{pbj}$ leads/lags for each outcome variable of interest. The first lag is used as reference point ($AgeAtTalibanArrival_{pb15}$) and thus dropped (normalized to zero). The light blue bars around the coefficients report their 95% confidence interval. The black vertical line indicates the "just treated" cohort. Endpoints are binned. Standard errors are clustered at the province level. The estimation sample includes ever married women (respectively men) aged 20 to 49 in 2015. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates consists of ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Figure A5: Treatment Effect Heterogeneity by Age at the Taliban Arrival: Whole Sample (Unweighted)



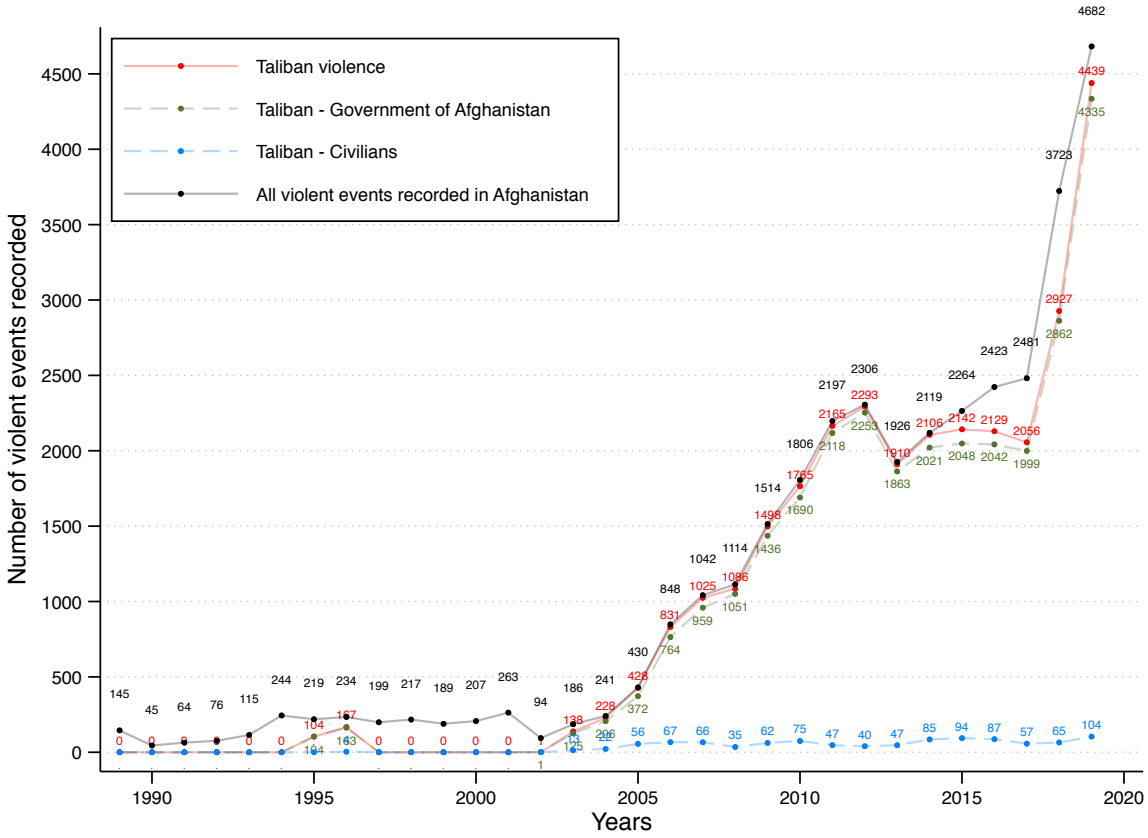
Notes: This figure reports the average effect of exposure to the Taliban rule when this exposure begins at age j. It plots the $AgeAtTalibanArrival_{pbj}$ leads/lags for each outcome variable of interest. The first lag is used as reference point ($AgeAtTalibanArrival_{pb15}$) and thus dropped (normalized to zero). The light blue bars around the coefficients report their 95% confidence interval. The black vertical line indicates the "just treated" cohort. Endpoints are binned. Standard errors are clustered at the province level. The estimation sample includes ever married women (respectively men) aged 15 to 49 in 2015. The control group is composed of the provinces that never were under Taliban rule between 1994 and 2001: Panjsher and Badakhshan. The set of potential covariates consists of ethnicity and language dummies, an index of wealth and an indicator variable equal to one if the respondent is living in a rural area.

Figure A6: Graphical Support for the Parallel Trends Assumption: Alternative Control Group



Notes: This figure reports the evolution of educational and labor market outcomes of both men and women across generations in Afghanistan, distinguishing between treated and control provinces. The latter group includes provinces that never were and only partially were under Taliban control: Panjsher, Badakhshan, Takhar, Baghlan, Nooristan, Kunhara, Kapisa and Laghman. The plotted means are computed using sample weights. Assuming that the Taliban occupation started in 1996 in all affected provinces, all individuals aged 20 to 33 in 2015 and living in treated provinces were of compulsory school age for at least one year under Taliban rule, and as such treated; while older individuals were out of school already and thus not treated. This cutoff is depicted by a black solid vertical line. In reality, the timing of the Taliban occupation varies at the province level, such that depending on their province of residence, some individuals aged 31 to 35 in 2015 may or may not be treated. The treatment cutoff thus varies between ages 30 and 35. This is depicted by two dotted vertical lines. Individuals living in control provinces are untreated regardless of their age.

Figure A7: Number of Violent Events Recorded per Year in Afghanistan



Notes: This figure shows the number of recorded violent events in Afghanistan between 1989 and 2019. The data come from the Uppsala Conflict Data Program (UCDP). UCDP records incidents "where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Högbladh, 2021). The blue line reports violent events committed by the Taliban against civilians; the green line counts events involving the Afghan government and the Taliban; the red line is the sum of the blue and green lines and the black line indicates all violent incidents recorded in Afghanistan, regardless of the parties involved. Clearly, the vast majority of recorded incidents can be linked to the Taliban. Before the Taliban's emergence in 1994, no Taliban violence is recorded. Similarly, once the Taliban rule was well established in 1996, no more Taliban violence is recorded until 2003, when the radical religious militia regrouped to form an insurgency movement.