EDUCATION AND CIVIC OUTCOMES IN ITALY

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Abstract

This paper examines the effect of education on civic outcomes in Italy. The analysis uses standard regression techniques such as OLS and ordered probit as well as IV and two-stage ordered probit methods in an attempt to account for the possibility that education is endogenous. In line with previous studies, our results show that the omission of unobserved factors makes OLS and ordered probit estimates on education to be biased. However, we find that the direction of this bias varies across different measures of civic outcomes. This result reconciles findings from previous studies (see, Dee, 2004; Milligan et al., 2004 and Gibson, 2001) showing mixed results about the causal effect of education on civic outcomes.

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1. Introduction

A vast number of studies in the political science and economic literature have analysed the relationship between education and civic outcomes (for an extensive literature review, see Campbell, 2006). However, one of the main problems in the estimation of this relationship lies in the potential endogeneity bias associated with education. It is quite possible that there are unobserved individual and family characteristics which simultaneously affect education and civic outcomes. Suppose that some parents encourage their children to place great value on education. If these same parents teach their children about the importance of being civically responsible, then the empirical association between education and civic outcomes would be spurious. This implies that if a method designed to deal with "selection on unobservables" is not employed, education will pick up the effect of omitted variables on civic outcomes.

Although several empirical studies have examined the impact of education on civic outcomes accounting for the bias associated with the endogeneity of education, results appear to be mixed. On the one hand, Gibson (2001) finds that the positive effect of schooling on the probability of doing volunteer work is reversed once one controls for the omitted-variables bias. On the other hand, Dee (2004) shows that increases in educational attainment have large and statistically significant effects on voter participation and support for free speech in the US and this result holds even when an instrumental variable (IV) strategy is used in an attempt to address the problem of the endogeneity of education. This finding is also consistent with Milligan et al. (2004) who, using a similar methodology, find a positive relationship between education and voting behavior in the US. Additionally, both Dee (2004) and Milligan et al. (2004) conclude that the omission of unobserved factors makes OLS estimates on education to be biased *downward* relative to the corresponding IV estimates.

In this paper, we attempt to reconcile these mixed results by examining the causal effect of education on five different measures of civic outcomes. Our intuition is that, as civic outcomes can be proxied by a vast array of measures, it is quite possible that those unobservables that make an individual develop a taste for education may have a different impact across these measures. For instance, consider individuals possessing unobserved qualities such as “determination” and “drive”. On the one hand, these characteristics are clearly correlated with educational attainment. On the other hand, however, their effect on voter turnout may be opposite to that on doing volunteer work. Possessing more “determination” and “drive” may discourage individuals from participating in political elections as they are mindful that their vote has a minuscule probability of being determinant, but at the same time it may make individuals more conscientious about their responsibility in helping less advantaged people.

In addition to using several measures of civic outcomes, this paper extends previous research in other two main aspects. First, in an attempt to account for the possibility that civic outcomes are correlated with unobserved factors that also influence educational attainment, we apply both standard IV and two-stage ordered probit estimation techniques. Second, our attention is focused on Italy. Works on the US and the UK tend dominate the empirical literature in this area and, whilst these studies are instructive, it would be rather hazardous to make inference exclusively based on them.

The remainder of the paper is as follows. Section 2 reviews the literature on the effect of education on civic and social engagement. Section 3 describes the data used in this study. Section 4 presents and discusses the empirical results. Section 5 concludes.

2. Education and civic outcomes

There are several mechanisms whereby education may promote civic engagement. To begin with, education *in general* can enhance cognitive proficiency and analytic ability. This is important as, for instance, it allows more educated individuals to have a greater capacity for absorbing and organising complex political information. Similarly, greater cognitive proficiency and analytic ability may help people to deal with bureaucratic procedures. For example, Wolfinger and Rosenstone (1980) find that in the US education is likely to lead to a higher voter turnout because it facilitates voter registration. Additionally, education may also help individuals to understand their own rights as well as the civil rights of others.

However, a number of researchers have also formulated the hypothesis that the relationship between education and civic outcomes could be negative. Higher educated individuals may face a higher opportunity cost of time and this, in turn, may make them spend less time and attention on civic activities. Although this consideration is likely to be especially relevant for those civic activities that are particularly time-consuming (e.g. volunteer work), Dee (2004) argues that it may also hold for voter participation. As outlined in the introduction, higher educated individuals may be discouraged from participating in political elections as they are aware that their vote has an infinitesimally small probability of influencing actual policy.

A direct channel through which education is likely to impact on civic outcomes is represented by civic courses. Although several studies show a positive relationship between civics instruction and civic outcomes (Niemi and Junn, 1998; Torney-Purta, 2002), other researchers have questioned this finding. For instance, Langton and Jennings (1968) conclude that classroom instruction in democratic education has virtually no impact on political and civic outcomes. This result is consistent with that obtained by Miller (1985) who, using longitudinal data, concludes that there little or no relationship between civic education in secondary schools and the kind or amount of political information of adults. Three main reasons have been offered to explain the ineffectiveness of civic courses. First, they may provide students with information that is mainly redundant. This occurs not only as some of the content of the civic courses is covered by lectures in other disciplines, but also because students may obtain the same information from other sources such as mass media and/or their parents. US students cite TV news as the most important source of political information. Second, civics instruction is supposed to be almost universal as it is received by students at a time when education is compulsory. Thus if each student receives the same amount of instruction in civics, then civics instruction will be unable to predict differences in civic engagement. The third explanation may lie in poor quality teaching. There is the possibility that civics is not taught using appropriate methods and/or is taught by individuals who have a lack of interest in this subject. The crucial role of teachers in the development of civic attitudes is highlighted by Ehman (1980) and Leming (1985).

Finally, several commentators argue that one of the reasons why schooling has a positive impact on civic outcomes is because a class represents an environment which is conducive to learning civic skills. Classroom activities include participation in debates over political issues as well as participation in workshops where students learn to think critically and to make decisions democratically. Additionally, extracurricular activities may also be beneficial. Using data from the US National Education Longitudinal Study, Smith (1999) finds that extracurricular activities in the eight, tenth and twelfth grades are positively related to political participation two year after high school graduation.

Nevertheless, the hypothesis that participation in high school groups may have a significant impact on subsequent political participation has been called into question by Campbell (2006). He argues that both actions may be driven by unobserved characteristics. More specifically, those unobservables which make an individual more likely to join high school groups may also exert a positive influence on the probability that he/she will participate in political elections.

3. Data

The main data used in this study come from the Survey of the Household Income and Wealth (SHIW) carried out by the Central Bank of Italy. The 2004 wave of the SHIW contains a special section that includes questions directly related to civic outcomes. This section was only addressed to household heads born in uneven years.

In this study we consider five civic-related questions. The first one is about political engagement as respondents were asked to indicate *"how interested they are in politics"*. Answers to this question ranges from 1 to 4, which correspond to "very", "fairly", "not very" and "not at all", respectively. In the next three questions respondents were asked to indicate the extent to which the following behaviors were justified: "*not paying the ticket on a public transport vehicle*", "*keeping money you obtained by accident when it would be possible to return it to the rightful owner*" and "*not leaving your name for the owner of a car you accidentally scraped*". Responses to these questions are given on an ordered scale from 1 for "never justifiable to 10 for "always justifiable". Finally, in the last question respondents were asked to *"rate the importance of the problem of tax evasion in relation to all the problems faced by the government"*. The answer to this question is coded on 1-5 scale, with 1 being "very serious", 2 as "serious", 3 as "the same as any other", 4 as "marginal" and 5 as "non-existent". In an attempt to simplify the interpretation of the empirical results, although the original scale of the responses to these five questions has been kept, the order is reversed so high score indicates a higher sense of civic duty.

While political engagement is a well-established proxy for civic outcomes, the rationale for using the other indicators is that civic knowledge is expected to help citizens to learn those skills needed to work with others towards goods that can only be created through collective actions. Thus, every citizen has a moral responsibility to contribute to sustaining the public institutions and processes on which the community is based, and from which all its members benefit. Additionally, one should note that the use of civic attitudes towards issues of public concern (e.g. corruption, tax evasion) finds increasing support within the existing socio-economic literature. A recent example includes the work by Algan and Cahuc (2006) in which they examine the determinants of civic attitudes towards government benefits.

The survey reports only the highest educational attainment of the individual and not the number of years he/she has spent at school. Following the approach employed in similar studies (see, for instance, Vieira, 1999; Brunello and Miniaci, 1999) we compute a continuous measure of education.[1] Years of education are hence calculated by imputing the number of years typically required to complete the highest level of educational attainment reported by the individual. More precisely, the following procedure is used. The number of statutory years required to obtain a primary school certificate and a lower secondary school diploma are 5 and 8 years, respectively. As regards upper secondary schools, the number of years depends on the type of school attended. While general schools (*licei*) and technical schools (*instituti tecnici e professionali*) comprise a five-year curriculum, teaching schools (*istituti magistrali*), which are specifically targeted to train

[1] One of the main advantages of this approach is that it increases comparability across studies. On the other hand, in constructing a continuous measure of years of education we assume that returns to education are linear.

primary school teachers, are based on a four-year programme. The number of statutory years required to complete university education varies according to the subject studied. It ranges from 4 years for people who study humanities and social sciences to 6 years for those who read medicine. Finally, most postgraduate courses tend to last one year in Italy.

In addition to schooling, a number of other explanatory factors are included in the model. These are: gender, age and its square, marital status, employment status, household income, number of children in the household, parental education and occupation, area of residence and urban location.

In an attempt to narrow down the potential sources of variation that may lead to a bias in the estimates of the effect of education on civic outcomes, we limit our analysis to men. Several studies (see, for instance, Schlozman, et al. 1994) show that there are significant gender differences in civic participation, with, for example, men more likely to be engaged in politics than women. As this result is also likely to reflect differences in unobservables, it is quite possible that the endogeneity bias of education in civic outcomes equations may vary across gender. Summary statistics for the final sample are presented in Table 1. There are 1,914 individuals in the sample[2], with an average level of 9.5 years of education.

Insert Table 1 near here

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[2] 3,798 household heads responded to the civic-related questions examined in this study. After focusing on men and dropping from the sample those individuals with missing relevant explanatory variables, we are left with 1,914 observations.

4. Empirical results

OLS and ordered probit

Table 2 presents the OLS and ordered probit estimates of the effect of education on our five measures of civic outcomes. The coefficient on education is statistically significant and has a positive sign in all the regressions shown in Table 2. This finding show that we are able to reproduce the standard result in the literature- there is a positive and statistically significant relationship in the data between education and civic outcomes.

Insert Table 2 near here

The results on the other explanatory variables are only briefly discussed here. The coefficient on age is statistically significant in most OLS and ordered probit regressions. Older individuals are likely to exhibit a higher concern for civic values relative to younger individuals. This result is in line with that of Algan and Cahuc (2006) who, using data on a number of OECD countries, find that the probability of considering it unjustifiable to cheat on government benefits increases with age. Additionally, there is some evidence that individuals living in the North tend to have a higher sense of civic duty relative to those living in the South. The importance of geographical area as a determinant of degree of ‘civicness’ in Italy has been emphasized by Ichino and Maggi (2000). Employing data on a large Italian bank, they find that employees born in the North are significantly less likely to shirk than those born in the South. There are also significant effects associated with family income. Higher family income tends to lead to a higher degree of ‘civicness’. The positive association between income and civic outcomes is not an unusual result in the socio-economic literature. For instance, Wolfinger and Rosenstone (1980) conclude that greater wealth and higher income increase the likelihood of participating in elections.

There are two main potential sources of bias in the estimates reported in Table 2. First, although we are aware that there is likely to be a measurement error in our indicators for civic outcomes, our primary concern is on how this varies across educational levels. Second, our estimates do not account for unobserved factors that affect both education and civic outcomes. In what follows we attempt to tackle both these issues.

The issue of attitude-behaviour inconsistency

A first concern lies in the measurement error associated with our measures for civic outcomes. As pointed out by Silver et al. (1986), it is possible that factors such as pressure and guilt about civic responsibilities may make individuals overstate their sense of civic duty. If misreporting is random across respondents, this affects the accuracy of our estimates but it is not a source of bias. On the other hand, if misreporting is systematically related to educational achievement, our estimates of the education coefficient are biased.

Unfortunately, unlike other studies (see, for instance, Milligan et al., 2004) we are unable to test whether misreporting is randomly distributed across educational levels. This is because we cannot validate the answers to any of the civic questions included in the 2004 wave of the SHIW. However, in an attempt to provide some evidence on the scale of this problem in Italy, we analyse data from another survey where, for at least one of our civic measures (i.e. political engagement), there is the possibility to check whether civic attitudes are connected with a specific action. This survey is the 2005 wave of the "Multipurpose Survey. Aspects of Daily Life" (MSADL) carried out by the Italian National Statistical Institute (ISTAT). It shares two important characteristics with the SHIW: 1) in both surveys the final sample is representative of the Italian population; 2) both surveys were conducted almost at the same time, i.e. the SHIW in 2004 and the MSADL in 2005. Additionally, in order to make the results from the MSADL comparable with those obtained from the SHIW, we select in the former only those male individuals who are household heads and were born in uneven years in 2004.

From the 2005 wave of the MSADL we focus our attention on two questions on political engagement. The first question asks individuals to indicate: *"how often do they talk about politics"*. Answers to this question are on a six-point scale, as follows: "every day", "sometimes a week", "once a week", "sometimes a month (less than four)", "sometimes a year" and "never". The second question asks the respondents to indicate whether they "*participated in the political election that took place in Italy in 2001*".[3] Our strategy is to

[3] Although self-reported measures of voting could be reasonably expected to contain some errors, several studies argue that they can still provide meaningful indicators for research purposes. For instance, Sigelman (1982, p. 53) concludes that "it seems safe to say that researchers who fit models of voting using self-

combine answers from these two questions to identify those individuals who, though they show a positive attitude towards political matters, this is not accompanied by a congruent action. More specifically, we create an *attitude-behaviour inconsistency* dummy that takes the value 1 if the individual reports talking about politics "every day", "sometimes a week" or "once a week" but claims not to have voted in 2001, and 0 otherwise.

We first use the responses to the two questions above as dependent variables[4] in regressions with approximately the same explanatory factors included in the model presented in Table 2.[5] The estimates, which are depicted in the first and second columns of Table 3, show that more educated individuals are more likely to talk about politics and are also more likely to have voted, respectively. Thus it appears that education exerts a significant positive influence not only on attitude towards politics but also on political behavior. Next we use the *attitude- behaviour inconsistency* dummy as a dependent variable in a probit model whose results are presented in the third column of Table 3. Education is not systematically correlated with the probability of talking frequently about politics but not having voted in 2001. This result may be interpreted as adding some credibility to our empirical findings because it supports the hypothesis that the discrepancy between civic opinions and congruent civic actions is randomly distributed across educational levels.[6]

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reported rather than validated voting data would not be led far astray in terms of what they conclude about the overall extent to which voting is related to demographic and political characteristics".

[4] For the first question, the order is reversed so high score indicates a higher political involvement.

[5] Whilst we replace household income with a set of dummies for the individual's occupation, number of children in the household is replaced with a dummy indicating the presence of at least once child in the household. Unfortunately there is no information on parental background or on urban location.

[6] Some further support for our approach is also provided by Milligan et al. (2004). Using officially validated voting data they find that the bias associated with self-reported voting data is not a function of education, after controlling for a number of individual characteristics.

Insert Table 3 near here

Exclusion restriction

The estimates depicted in Table 2 may be biased because of the endogeneity of education. In an attempt to address this problem, we use an IV method as well as a two-stage ordered probit model. These methods typically require an instrument or an exclusion restriction, which is a variable that affects education but has no impact on civic outcomes. Following the approach of Flabbi (1999), we use two reforms of the Italian educational system as an instrument for education.[7] Both reforms can be viewed as exogenous events that have contributed to remove barriers to participation in higher educational opportunities, thereby raising the average educational levels of the Italian population. The first reform (Law 1859, 31 December 1962) changed the organizational structure of lower secondary school in an attempt to increase participation in upper secondary education. Specifically, it established a single compulsory type of lower secondary school that gives to the students who have successfully completed it the right to enrol at any type of upper secondary school.[8] The second educational reform (Law 910, 11 December 1969) yielded an increase in participation in higher education by opening universities also to those students successfully completing non-general upper secondary schools.[9]

[7] A large number of studies use institutional features of the schooling system as an instrumental variable for education (for a review, see Card, 2001).
[8] Before the reform there were two types of lower secondary school and one of them (i.e. *scuola di avviamento professionale*) had a strong vocational nature. Individuals who attended the latter were unlikely to continue their studies given that they could not enrol at general upper secondary schools and they had to pass an exam to be able to enrol at non-general upper secondary schools.
[9] Before 1969 only those individuals successfully completing general upper secondary schools had automatic access to university.

As suggested by Flabbi (1999), considering that in Italy individuals usually begin lower secondary school at the age of 12 and that the expected age of completion of upper secondary school is, in general, 19 years, both the 1962 and the 1969 reforms are likely to have affected the educational attainment of people born after 1950. This means that the effect of these reforms can be captured through a single dummy variable that takes the value 1 if the individual was born in 1951 or after, and 0 otherwise.[10]

The validity of the two-stage estimation strategy rests on the assumption that our instrumental variable is not related to civic outcomes. This means that the 1962 and 1969 educational reforms should not be associated with other factors that, in turn, may affect civic attitudes. At least one consideration supports this assumption. Civic courses, which are found to be an important determinant of civic outcomes by several studies, were introduced in Italy before these reforms. Specifically, they started in 1958 and since then they have been compulsory and taught in lower secondary schools (*scuole medie*) as part of the curriculum of classes in history (Sani, 2004).

In an attempt to analyze the effect of 1962 and 1969 reforms on educational attainment, Figure 1 displays the proportion of individuals with at least an upper secondary school diploma[11] by year of birth. From this Figure it emerges that, on top of a general positive trend in educational attainment, educational levels jumped for individuals born during the

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[10] The same dummy variable is used as an instrument by Brunello and Miniaci (1999) and Brunello et al. (2000). Additionally, this dummy is also employed by Checchi and Brunello (2005) as an explanatory factor in an educational attainment model.

[11] We focus on these individuals as one would expect these reforms to increase the proportion of people who have either an upper secondary school diploma or a tertiary education degree.

beginning of the 1940s as well as for those born around 1950. The latter result may possibly reflect the impact of these reforms.

Insert Figure 1 near here

Following the approach of Dee (2004), we employ a more formal method to test that our instrument is “relevant” (i.e., correlated with education). Specifically, we estimate the effects of the educational reforms on different levels of educational attainment using a specification that includes a full set of controls. The OLS estimates, which are reported in Table 4, provide support for the use of our instrument. In line with our expectations, these estimates indicate that the effects associated with the educational reforms were practically exclusively concentrated at the upper secondary/tertiary levels.

Insert Table 4 near here

Following the idea advanced by Card (1995) that accessibility matters more for individuals on the margin of continuing their education, it is very likely that the 1962 and 1969 educational reforms had a differential effect, with larger increases in educational attainment among people from less advantaged family backgrounds.[12] Table 5 provides some evidence supporting this hypothesis. It shows that increases in educational attainment within the cohort born in 1951 or after are larger among those individuals who

[12] A further consideration reinforces this conclusion. In Italy individuals from less advantaged family backgrounds tend to send their children to vocational/non-general secondary schools (Checchi and Flabbi, 2006). Before the 1962 and 1969 reforms this educational choice was significantly limiting their possibility to continue with further education.

have less-educated parents. For instance, whilst the proportion of individuals who, despite having a father with no formal educational attainment, have at least an upper secondary school diploma is 9.42 percent within the cohort born before 1951, the corresponding figure is three times higher (i.e. 29.37 percent)within the cohort born in 1951 or after. This suggests that a possible specification of the first stage of the IV and two-stage ordered probit models may include interactions of the reform dummy with family background variables as instruments for schooling, in addition to the reform dummy that is used as a direct control variable.[13]

Insert Table 5 near here

Two-stage estimates

This Section presents two-stage estimates in an attempt to account for the possibility that education is endogenous to civic outcomes. Specifically, we use two different two-stage procedures which only differ in the second stage estimation procedure. The first common stage consists of an OLS regression where education is regressed on our instruments and other covariates. In the second stage fitted values from this regression are then included in the civic outcome equations instead of education. In line with the approach of Dee (2004) and Milligan et al. (2004), we first estimate the second stage equation using OLS following a standard IV process. However, to test the robustness of our findings and

[13] Numerous studies (see, for instance, Card 1995) have used interactions between an institutional feature of the educational system and family background variables as instruments for schooling.

given the categorical nature of the dependent variables, this specification is also estimated using an ordered probit model.[14]

Table 6 reports the estimates for two alternative specifications of the first stage. The first specification relies on a more parsimonious set of instruments that comprises the reform dummy and interactions of the reform dummy with parental education. In the second specification we add interactions of the reform dummy with parental occupation to our set of instruments. This approach allows an examination of whether or not the estimates are sensitive to the instruments employed to identify the model.

Insert Table 6 near here

These estimates show that those affected by the 1962 and 1969 educational reforms have significantly higher educational attainment, conditioning on control variables such as (among others) age[15], gender, parental education and occupation. Specifically, the findings indicate that these educational reforms increased years of schooling by a statistically significant 2.61 years and 2.68 years in the first and specification specifications, respectively.[16] The coefficients on parental education and occupation

[14] Given that years of education can be considered as a categorical variable, one may argue that other estimation techniques (e.g. ordered probit) than OLS would be more appropriate in the first stage. Nevertheless, Heckman (1978, p. 947) suggests that there is no loss in using OLS. In the light of this, several studies (see, for instance, Meer et al., 2003; Chun and Oh, 2002) employ an OLS regression in the first stage of a two-stage probit model.

[15] Controlling for linear age and age squared variables is especially important as this ensures against the possibility that our reform dummy is simply picking up an upward drift in educational attainment.

[16] If the reform dummy is included as the only instrument, the value of its coefficient (i.e. 0.91) is quite similar to that found by Brunello et al. (2000). However, following the approach of Denny and Harmon (2000), we do not report IV and two-stage ordered probit estimates based only on this instrument. When the

suggest that individuals from less advantaged backgrounds have lower education relative to those from more advantaged backgrounds, other things being held constant. However, in line with our expectations, the 1962 and 1969 reforms had the effect of reducing this education inequality. For instance, whilst individuals whose father is a manager or a professional have about 2.24 years more education than the default case of individuals whose father has another occupation, after the reforms this disparity is reduced to approximately 0.54 years.[17]

Next we check the explanatory power of our instrumental variables. Bound et al. (1995) show that a weak correlation between the instruments and the endogenous variable may make the two-stage estimation technique not to be superior of a simple regression where there is no attempt to control for the endogeneity bias. Thus the correlation between our set of instruments and education is checked using the F-statistic suggested by Staiger and Stock (1997). This test, whose results are shown in Table 6, confirms that our instruments are relevant as the F-statistic on the excluded instruments in the first stage is very high in both specifications, thereby indicating that our identifying variables make a significant contribution to explaining educational attainment.

Table 7 depicts the estimates of the second stage of both the IV and the two-stage ordered probit models. As regards the latter, consistent standard errors are computed using the

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sample size is insufficiently large asymptotically efficiency is gained through the use of more instruments (Davidson and McKinnon, 1993).

[17] A similar result is obtained by Denny and Harmon (2000).

procedures discussed in Murphy and Topel (1985). Its implementation in Stata is outlined by Hole (2006).[18]

Insert Table 7 near here

These estimates suggest that both OLS and ordered probit estimates of the impact of education on civic outcomes are likely to be biased. Correcting for the endogeneity bias associated with education leads to a significant increase in the size of education coefficient's standard error. This is a common outcome when using a two-stage procedure (Wooldridge, 2002), where one faces a trade off between inconsistent estimators that have relatively small standard errors (e.g. OLS and ordered probit) and consistent but imprecise estimators (IV and IV ordered probit). The most important result emerging from Table 7 is that the effect of schooling varies across different measures of civic outcomes. Whilst education turns out to still be a positive and significant predictor of interest in politics, this factor appears not to have any statistically significant effect on the other measures of civic outcomes.[19] These findings are consistent across IV and two-

[18] The Murphy Topel estimate of variance for a two step model is given by:

$$\hat{V}_2 + \hat{V}_2(\hat{C}\hat{V}_1\hat{C}' - \hat{R}\hat{V}_1\hat{C}' - \hat{C}\hat{V}_1\hat{R}')\hat{V}_2$$

where $\hat{V}_1$ (qxq) and $\hat{V}_2$ (pxp) are the estimated covariance matrices for model 1 (education equation estimated by OLS) and model 2 (civic outcome equation estimated by ordered probit), respectively. The correction considers sampling error in the two stage procedure linking both models 1 and 2 by the definition of matrices $\hat{C}$ (pxq) and $\hat{R}$ (pxq),

$$\hat{C} = \left[\sum_{i=1}^{n} \left(\frac{\partial \ln f_{i2}}{\partial \hat{\theta}_2} \right) \left(\frac{\partial \ln f_{i2}}{\partial \hat{\theta}_1'} \right) \right] \quad \text{and} \quad \hat{R} = \left[\sum_{i=1}^{n} \left(\frac{\partial \ln f_{i2}}{\partial \hat{\theta}_2} \right) \left(\frac{\partial \ln f_{i1}}{\partial \hat{\theta}_1'} \right) \right]$$

where $\ln f_{i1}$ and $\ln f_{i2}$ are the log likelihood (for observation *i*) of models 1 and 2, respectively. Thus, the correction of the standard errors depends on: a) the precision of the first stage estimates and b) the correlation between the derivatives and the explanatory variables used in the second stage of the regression.

[19] In one case the effect is even negative. As regards the civic measure "*keeping money you obtained by accident when it would be possible to return it to the rightful owner*", using the full set of instruments the education coefficient has a negative sign and it is statistically significant at 10% in both the IV and two-stage ordered probit models .

stage ordered probit estimates and are also consistent irrespective of the set of instruments used. However, the specification with the full set of instruments delivers a slightly more precise estimate of education (i.e. the estimated standard error are smaller). Additionally, in line with the results obtained by Dee (2004) and Milligan et al. (2004), our OLS and ordered probit estimates indicate that the omission of unobserved factors leads to a *downward* bias in the estimated effect of education on political engagement.

However, the results depicted in Table 7 can be considered to be superior than those reported in Table 2 only if education is found to be endogenous. In order to test for this, we include the residual obtained from the first stage regression as an additional regressor in the civic outcomes models whose results are presented in Table 2. If the coefficient on the residual turns out to be statistically significant, this implies that education is endogenous. Our findings from both the IV and two-stage ordered probit methods indicate that, using the full set of instruments, the large majority of our measures of civic outcomes are endogenous.[20] Additionally, we use the Sargan misspecification test to check the validity of our instruments, i.e. they should be orthogonal to the error term of the civic outcomes equations. The results suggest that both our sets of instruments are valid for all the civic outcomes IV models.[21]

Our findings imply the endogeneity bias associated with education can be either positive or negative. On the one hand, since unobserved factors driving education are likely to be

[20] In two cases education is found to be exogenous. The first one is with respect to "*not paying the ticket on a public transport vehicle*" in the IV method. The second one is with respect to "*not leaving your name for the owner of a car you accidentally scraped*" in the two-stage ordered probit model.
[21] To save space, the results of the Hausman and Sargan tests are not reported.

negatively correlated with interest in politics, this causes a downward bias in the estimated effect of schooling on this civic measure. On the other hand, since unobservables that make individuals develop a taste for education are likely to be positively correlated with the other measures of civic outcomes, the effect of education tends to disappear once one accounts for the endogeneity bias.

Robustness checks

Two tests are performed to check the robustness of our two-stage estimates. First, we re-estimate the civic outcome equations with some modifications in control variables (for instance, it may be appropriate to drop martial status and family income as both these variables are potentially endogenous). New IV and two-stage ordered probit estimates confirm the robustness of our findings as the magnitude of the coefficients on education is close to that reported in Table 7.[22] Second, we conduct estimations on a subsample which consists of individuals aged 33-73 in 2004. These individuals are selected in an attempt to eliminate the effect of other factors potentially causing changes in the educational attainment besides the 1962 and 1969 educational reforms. Thus, on the one hand, we exclude those individuals whose educational attainment might have been negatively affected by the WWII, i.e. individuals who were beyond compulsory schooling age during or before the WWII. On the other hand, we also drop from the sample those individuals who were 19 or younger in 1990 as their educational attainment might have been positively influenced by the significant expansion of university supply (Bratti et al., 2007). For this specific sub-sample, results are broadly consistent with those

[22] Results are available from the authors upon request.

depicted in Table 7. Thus, the only civic measure affected by education once one controls for the endogeneity bias is interest in politics, thought the size of the education coefficients are slightly bigger than those in reported in Table 7 for both the IV and two-stage ordered probit estimates.[23]

5. Conclusions

This paper has examined the casual effect of education on civic outcome in Italy. We have attempted to extend previous research in this area by investigating the effect of education on several measures of civic outcomes. Thus, in addition to political engagement, which is a well-established proxy for civic outcomes, we have considered other indicators capturing citizens' opinion on a number of civic issues. We have employed standard regression techniques such as OLS and ordered probit as well as IV and two-stage ordered probit methods in an attempt to account for the endogeneity of education.

[23] Using the procedure employed by Altonji et al (2005), it is possible to compute the potential bias due to the use of a contaminated set of instruments with respect to the IV estimates of the effect of education on interest in politics. This bias results from the possibility that for a specific sub-sample our instruments are correlated with the residuals. To check whether this may be a problem in our analysis, the following equation is estimated for individuals aged either over 73 or under 33:

$$Y_i = X_i^{'}\alpha + (Z\hat{\beta}_{FS})\delta + \varepsilon_i$$

where Y is interest in politics, X is a vector of explanatory factors, Z is vector of instruments and $\hat{\beta}_{FS}$ is the estimated impact of the instruments in the first stage. Results from this regression show that the estimators of δ are not statistically significant, giving further support the results reported in Table 7 for the full sample.

In line with previous studies, our results show that the omission of unobserved factors makes OLS and ordered probit estimates on education to be biased. However, we conclude that the direction of this bias varies across different measures of civic outcomes. On the one hand, we find that unobserved factors making individuals develop a taste for education are likely to be negatively correlated with interest in politics. This implies that the effect of education on interest in politics is likely to be underestimated if one does not account for the endogeneity bias associated with education. On the other hand, since unaccounted factors driving education are likely to be positively correlated with the other civic measures examined in this study, the effect of education tends to disappear once one controls for the endogeneity bias.

These results helps to reconcile various findings from previous studies (see, Dee, 2004 and Milligan et al., 2004 on the one hand and Gibson, 2001 on the other hand) showing mixed results about the causal effect of education on civic outcomes.

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Table 1
Descriptive statistics

Variables	Mean	Standard Deviation
Education (years of schooling)	9.50	4.23
Civic outcomes		
Interest in politics	2.04	0.92
Not paying the ticket on a public transport vehicle	8.78	1.96
Keeping money and not return it to the rightful owner	8.84	1.96
Not leaving your name for the owner of a car you scraped	8.96	1.88
Importance of the problem of tax evasion	4.12	0.80
Household income (euro)	35078.39	38001.83
Age (years)	55.54	14.62
Age squared (years)	3298.16	1624.95
Father's education (years of schooling)	5.03	4.09
Mother's education (years of schooling)	4.10	3.57
Married=1	0.81	0.39
Employed=1	0.53	0.50
Number of children in the household	0.86	0.98
Area of residence		
North=1	0.47	0.50
South=1	0.29	0.45
Centre=1	0.24	0.43
Urbanization		
Small town (below 20,000 inhabitants)=1	0.31	0.46
Medium-sized town (between 20,000 and 40,000 inhabitants)=1	0.20	0.40
Big town (between 40,000 and 500,000 inhabitants)=1	0.40	0.49
Very big town (above 500,000 inhabitants)=1	0.08	0.28
Mother's occupation		
Manager or professional	0.01	0.09
Self-employed	0.07	0.26
Not working	0.71	0.45
Others	0.21	0.41
Father's occupation		
Manager or professional	0.05	0.22
Self-employed	0.21	0.41
Not working	0.04	0.20
Others	0.69	0.46
Reform dummy =1	0.45	0.50
Number of observations	1,914	

Table 2
OLS and Ordered Probit estimates (weighted)

Independent variables	OLS					Ordered Probit				
	Interest in politics	Not paying the ticket on a public vehicle	Keeping money and not return it to the owner	Not leaving your name for the owner of a car you scraped	Importance of the problem of tax evasion	Interest in politics	Not paying the ticket on a public vehicle	Keeping money and not return it to the owner	Not leaving your name for the owner of a car you scraped	Importance of the problem of tax evasion
Age	0.031*	0.070*	0.017	0.029	0.032*	0.041*	0.043*	0.016	0.027**	0.046*
	(0.010)	(0.021)	(0.021)	(0.020)	(0.009)	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)
Age squared	-0.000*	-0.000**	-0.000	-0.000	-0.000*	-0.000*	-0.000**	-0.000	-0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.071*	0.052*	0.026***	0.035*	0.026*	0.087*	0.048*	0.026*	0.036*	0.037*
	(0.006)	(0.014)	(0.014)	(0.013)	(0.006)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)
father's education	-0.022**	-0.021	0.022	0.021	-0.003	-0.027**	-0.017	0.023***	0.009	-0.004
	(0.009)	(0.019)	(0.019)	(0.017)	(0.008)	(0.011)	(0.012)	(0.012)	(0.012)	(0.011)
mother's education	0.012	0.019	0.010	-0.003	0.017**	0.016	0.020	-0.000	0.006	0.026**
	(0.009)	(0.020)	(0.020)	(0.019)	(0.008)	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)
married	0.053	-0.127	-0.167	-0.356*	0.115**	0.065	-0.054	-0.038	-0.205*	0.170**
	(0.056)	(0.121)	(0.121)	(0.112)	(0.049)	(0.071)	(0.075)	(0.077)	(0.079)	(0.071)
employed	0.021	0.120	-0.095	-0.001	-0.017	0.021	0.052	-0.141	-0.080	-0.029
	(0.063)	(0.136)	(0.136)	(0.126)	(0.055)	(0.080)	(0.085)	(0.087)	(0.088)	(0.081)
Number of children in the household	-0.093*	0.008	0.087	0.122**	-0.014	-0.119*	0.001	0.006	0.062***	-0.015
	(0.025)	(0.054)	(0.054)	(0.050)	(0.022)	(0.032)	(0.034)	(0.035)	(0.035)	(0.032)
household income	0.000002*	0.000003***	0.000004*	0.000002***	0.00000	0.000002**	0.000003**	0.000005*	0.000002***	0.00000
	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)	(0.000001)

Table 2- continued

Area of residence- Reference category is South										
North	-0.017	0.693*	0.631*	0.820*	0.060	-0.016	0.330*	0.314*	0.447*	0.079
	(0.050)	(0.107)	(0.107)	(0.099)	(0.044)	(0.063)	(0.067)	(0.068)	(0.068)	(0.064)
Centre	-0.067	0.538*	0.189	0.441*	0.172*	-0.076	0.189**	0.012	0.214*	0.265*
	(0.061)	(0.132)	(0.132)	(0.122)	(0.054)	(0.078)	(0.082)	(0.082)	(0.083)	(0.079)
Urbanization- Reference category is very big town										
small town	-0.078	-0.025	0.096	0.184	0.115***	-0.098	-0.023	0.130	0.118	0.177**
	(0.067)	(0.144)	(0.144)	(0.134)	(0.059)	(0.084)	(0.091)	(0.091)	(0.093)	(0.085)
medium-sized town	-0.148***	0.191	0.188	0.177	0.162**	-0.188***	0.175	0.195***	0.136	0.236**
	(0.078)	(0.168)	(0.168)	(0.156)	(0.069)	(0.099)	(0.107)	(0.107)	(0.109)	(0.100)
big town	0.045	-0.190	-0.143	-0.032	0.189*	0.055	-0.128	-0.050	-0.035	0.280*
	(0.070)	(0.150)	(0.150)	(0.139)	(0.061)	(0.087)	(0.094)	(0.094)	(0.096)	(0.089)
Mother's occupation- Reference category is others										
manager or prof	0.241	-0.730	0.063	-0.485	-0.243	0.253	-0.542***	-0.227	-0.652**	-0.385
	(0.220)	(0.472)	(0.471)	(0.437)	(0.192)	(0.272)	(0.286)	(0.291)	(0.284)	(0.276)
self-employed	0.038	0.463**	0.417**	0.206	-0.046	0.030	0.260**	0.227***	0.045	-0.059
	(0.095)	(0.204)	(0.204)	(0.189)	(0.083)	(0.120)	(0.131)	(0.131)	(0.132)	(0.122)
not working	-0.099***	0.072	0.356*	0.023	-0.097**	-0.136**	0.024	0.177*	-0.036	-0.134**
	(0.051)	(0.109)	(0.109)	(0.101)	(0.044)	(0.064)	(0.067)	(0.068)	(0.070)	(0.065)
Father's occupation- Reference category is others										
manager or prof	0.080	0.074	-0.090	-0.042	-0.216**	0.097	0.060	0.002	0.045	-0.304**
	(0.100)	(0.214)	(0.214)	(0.198)	(0.087)	(0.124)	(0.139)	(0.142)	(0.144)	(0.126)
self-employed	-0.049	0.064	0.174	0.117	-0.043	-0.048	-0.004	0.081	0.010	-0.065
	(0.053)	(0.115)	(0.114)	(0.106)	(0.047)	(0.068)	(0.072)	(0.073)	(0.073)	(0.068)
not working	0.025	0.100	-0.059	-0.024	-0.083	0.045	0.015	-0.089	0.020	-0.131
	(0.107)	(0.230)	(0.230)	(0.213)	(0.094)	(0.136)	(0.143)	(0.145)	(0.151)	(0.135)
Constant	0.609**	5.370*	7.103*	6.976*	2.592*					
	(0.271)	(0.582)	(0.581)	(0.538)	(0.237)					
R-squared adjusted (Pseudo R-squared)	0.109	0.059	0.044	0.060	0.052	0.049	0.029	0.025	0.028	0.028
Log-likelihood	-2432.156	-3896.323	-3893.251	-3748.274	-2174.589	-2279.982	-2689.822	-2547.569	-2399.648	-2086.864
Number of obs	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914

*denotes significance at one percent, **denotes significance at five percent and ***denotes significance at ten percent. (Standard errors are given in parentheses).

Table 3

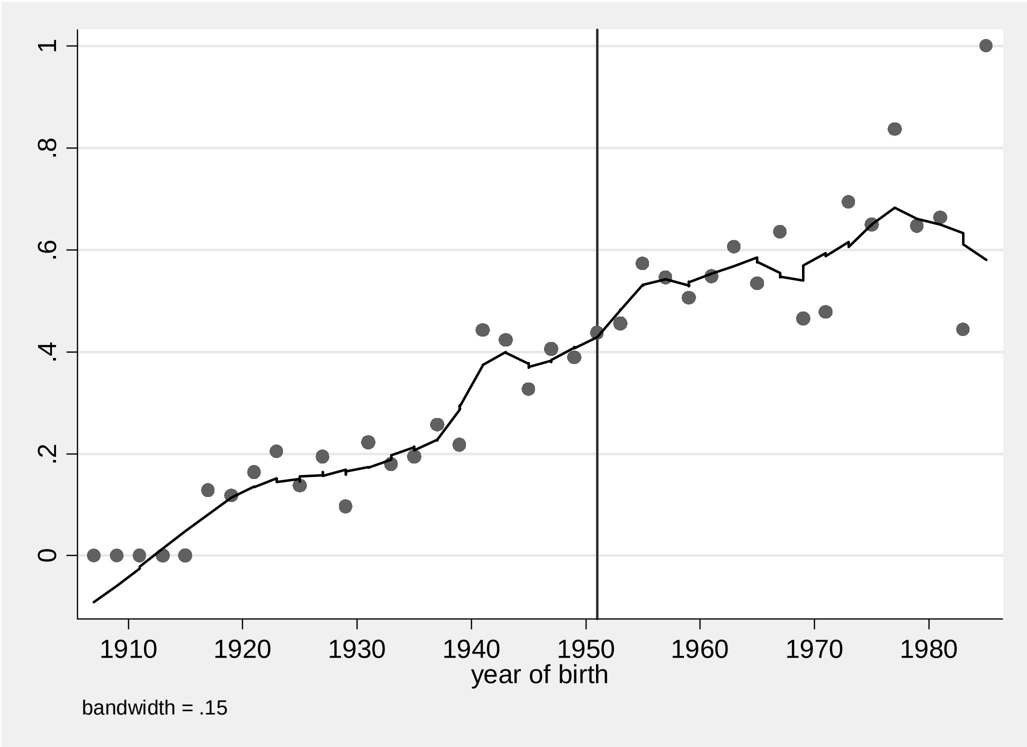
Estimates of the effect of education on civic attitudes and behaviors

Independent variables	Dependent variables		
	Talk about politics (OLS)	Voted in 2001 elections (Probit)	Attitude behavior -inconsistency (Probit)
	Coefficient	Coefficient	Coefficient
Education	0.122* (0.006)	0.044* (0.007)	-0.001 (0.011)
Age	0.090* (0.010)	0.107* (0.010)	-0.031*** (0.016)
Age squared	-0.001* (0.000)	-0.001* (0.000)	0.0002 (0.000)
Female	0.155** (0.062)	0.230* (0.066)	-0.204** (0.103)
Married	0.063 (0.062)	-0.005 (0.076)	0.003 (0.118)
Employed	0.122* (0.006)	0.044* (0.007)	-0.001 (0.011)
Area of residence- Reference category is South			
North	0.099** (0.046)	-0.147* (0.055)	0.154*** (0.088)
Center	0.046 (0.058)	-0.097 (0.070)	0.082 (0.112)
Occupation-Reference category is others			
manager, director or freelance	0.092 (0.070)	-0.042 (0.089)	0.198*** (0.117)
entrepreneur	0.164 (0.100)	0.124 (0.125)	-0.017 (0.188)
at least one child in the household	0.098*** (0.053)	0.067 (0.061)	-0.089 (0.097)
Constant	-0.504*** (0.272)	-1.970* (0.281)	-1.043** (0.449)
Ajusted R squared (Pseudo R squared)	0.105	0.067	0.025
Log-likelihood	-12433.516	-1571.560	-558.753
Number of obs	6,464	6,464	6,464

* denotes significance at one percent, ** denotes significance at five percent and *** denotes significance at ten percent.

(Robust standard errors are given in parentheses).

Figure 1: Fraction of individuals with at least an upper secondary school diploma



Note: The y axis represents a weighted mean of individuals who have either an upper secondary school diploma or a tertiary education degree.

Table 4
OLS estimates (weighted) of the effect of educational reforms on measures of educational attainment

Dependent variable	Coefficient
No educational attainment	0.005(0.013)
Primary school	-0.122*(0.030)
Lower secondary school	0.024(0.034)
Upper secondary school/tertiary education	0.093**(0.037)

The sample size is 1,914. The models comprise age, age squared, parental education, binary indicators for area of residence (2) and parental occupation (6).
Standard errors are given in parentheses.
*denotes significance at one percent, **denotes significance at five percent and ***denotes significance at ten percent.

Table 5
Proportion of individuals with at least an upper secondary school diploma by parental education and by cohort of birth (percentage)

	Before 1951	In 1951 or after
Father's education		
No formal educational attainment	9.42	29.37
Primary school certificate	32.72	47.94
Lower secondary school diploma	77.92	72.99
At least upper secondary school diploma	94.12	87.31
Mother's education		
No formal educational attainment	13.16	38.30
Primary school certificate	40.46	52.34
Lower secondary school diploma	83.67	71.05
At least upper secondary school diploma	96.15	92.00

Table 6
First stage estimates (OLS –weighted) for education. Basic and full specification

	Basic specification		Full specification	
	Coefficient	Std err	Coefficient	Std err
Age	0.161*	0.035	0.165*	0.035
Age squared	-0.002*	0.000	-0.002*	0.000
father's education	0.622*	0.046	0.600*	0.047
mother's education	0.157*	0.054	0.166*	0.054
Area of residence- Reference category is South				
North	-0.253	0.177	-0.266	0.178
Centre	-0.154	0.220	-0.150	0.220
Mother's occupation- Reference category is others				
manager or professional	-0.158	0.803	-1.328	1.339
self-employed	-0.344	0.348	-0.568	0.505
not working	0.093	0.187	0.214	0.289
Father's occupation- Reference category is others				
manager or professional	1.110*	0.364	2.243*	0.621
self-employed	0.429**	0.195	0.522**	0.263
not working	-0.687***	0.392	-1.032**	0.462
Reform dummy	2.607*	0.341	2.677*	0.469
reform dummy*father's education	-0.294*	0.060	-0.259*	0.061
reform dummy*mother's education	-0.023	0.068	-0.034	0.069
reform dummy*father's occupation is manager or prof			-1.702**	0.767
reform dummy*father's occupation is self-employment			-0.231	0.392
reform dummy*father's occupation is not working			1.348	0.883
reform dummy*mother’s occupation is manager or prof			1.658	1.675
reform dummy*mother's occupation is self-employment			0.491	0.700
reform dummy*mother's occupation is not working			-0.220	0.376
Constant	1.589	1.071	1.444	1.095
F- stat for exclusion of instruments (P-value)	25.07 (0.000)		9.41 (0.000)	
Weak identification: Cragg-Donald F-stat	25.07>13.91 (5% bias)		9.41 < 11.46 (10% bias)	
Adjusted R squared	0.408		0.409	
Log-likelihood	-4921.112		-4916.385	
Number of observations	1,914		1,914	

The basic specification passed the weak identification test since 25.07 > 13.91 (critical value for 5 percent maximal IV relative bias). However, the full specification has between 10 percent and 20 percent of IV relative bias.
* denotes significance at one percent, ** denotes significance at five percent and *** denotes significance at ten percent.

Table 7
Second stage estimates of the effect of education on civic outcomes. IV and Ordered probit IV (weighted)

Dependent variable	IV (OLS)		IV (Ordered Probit)	
	Coefficient	Std err	Coefficient	Std err
Basic first stage specification				
interest in politics	0.111*	0.035	0.136*	0.042
not paying the ticket on a public transport vehicle	-0.036	0.072	-0.039	0.046
keeping money and not return it to the rightful owner	-0.118	0.075	-0.074	0.049
not leaving your name for the owner of a car you scraped	-0.061	0.067	-0.027	0.047
importance of the problem of tax evasion	-0.027	0.030	-0.038	0.043
Full first stage specification				
interest in politics	0.121*	0.034	0.148*	0.041
not paying the ticket on a public transport vehicle	-0.038	0.068	-0.039	0.044
keeping money and not return it to the rightful owner	-0.136***	0.073	-0.083***	0.048
not leaving your name for the owner of a car you scraped	-0.073	0.064	-0.026	0.045
importance of the problem of tax evasion	-0.028	0.028	-0.041	0.041

The sample size is 1,914. The models comprise age, age squared, parental education, household income, number of children in the household, binary indicators for employment status (1), marital status (1) area of residence (2), urban location (3) and parental occupation (6).
* denotes significance at one percent, ** denotes significance at five percent and *** denotes significance at ten percent.