



# When does education matter? The protective effect of education for cohorts graduating in bad times



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## ABSTRACT

Using Eurobarometer data, we document large variation across European countries in education gradients in income, self-reported health, life satisfaction, obesity, smoking and drinking. While this variation has been documented previously, the reasons why the effect of education on income, health and health behaviors varies is not well understood. We build on previous literature documenting that cohorts graduating in bad times have lower wages and poorer health for many years after graduation, compared to those graduating in good times. We investigate whether more educated individuals suffer smaller income and health losses as a result of poor labor market conditions upon labor market entry. We confirm that a higher unemployment rate at graduation is associated with lower income, lower life satisfaction, greater obesity, more smoking and drinking later in life. Further, education plays a protective role for these outcomes, especially when unemployment rates are high: the losses associated with poor labor market outcomes are substantially lower for more educated individuals. Variation in unemployment rates upon graduation can potentially explain a large fraction of the variance in gradients across different countries.

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## 1. Introduction

A long line of literature has examined the impact of education on health and health behaviors (Cutler and Lleras-Muney, 2008, 2010). Almost all observational studies find that education is positively associated with health, but two points are worth noting. First, there is substantial variation across countries and cohorts in the extent to which education predicts better health. For example Kunst and Mackenbach (1994) document that education has a small effect on mortality in the Netherlands, Sweden, Denmark, and Norway but its effect is about two times as large in the United States, France, or Italy. And this association is larger for more recent cohorts: education is a larger predictor of mortality today than in the past (Meara et al., 2008). Education gradients in health behaviors also vary substantially: for instance Cutler and Lleras-Muney (2014) document that the effect of a year of education on smoking for women ranges from 0.02 to −0.06 depending on the country. Second not all studies find that education is protective.

Most notably, recent attempts to estimate causal impacts of education using compulsory schooling legislation find different results across countries. For instance, compulsory schooling reduced mortality in the United States (Lleras-Muney, 2005) but not in England (Clark and Royer, 2013), or France (Albouy and Lequien, 2009), and the effects for Sweden are ambiguous (Meghir et al., 2013; Fischer et al., 2013). A recent study looking at the impact of compulsory school on mortality for several European countries confirms this variability, finding large effects for example in Belgium but not in Spain (Gathmann et al., forthcoming).

In this paper, we explore one explanation for this variability: that the effect of education is larger for cohorts who started their career in bad economic times. Previous literature has documented that individuals graduating in recessions have worse labor market outcomes and health outcomes for many years thereafter (Oreopoulos et al., 2012; Genda et al., 2010; Kahn, 2010; Kondo, 2007; Kwon et al., 2010; Oyer, 2006, 2008; Schoar and Zuo, 2011; Maclean, 2013). Because education is thought to affect health in part through its effect on income and resources, we hypothesize that the health benefits of education will be lower for individuals for whom education has a smaller return in the labor market. If lifetime incomes increase the demand for health (Grossman, 1972),

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and recessions increase the gap in lifetime incomes across education groups, then recessions will also increase health gaps across education groups. Whether recessions have larger or smaller effects on the lifetime incomes of low educated is not clear: in Canada, Oreopoulos et al. (2012) find larger effects of recessions on incomes for those with lower quality of education (college and mayor) but they only study college graduates. Genda et al. (2010) find larger effects on employment for the uneducated in both Japan and the US, but larger earnings effects for the highly educated in the US—consistent with Hershbein (2012) and Oyer (2006, 2008).

Education has been hypothesized to increase one's ability to cope with negative shocks and uncertainty (Schultz, 1975; Huang and Zhou, 2013). This provides a different reason why the deleterious health effects of recessions differ across education groups. During recessions individuals are more likely to suffer from depression and stress (Cooper, 2011) and suicides are higher (Stuckler et al., 2011; Reeves et al., 2012). Unhealthy behaviors also appear to respond to recessions in the short run: both smoking and BMI fall in recessions (Ruhm, 2004 but see Jónsdóttir and Ásgeirsdóttir, 2013), although the relationship with alcohol is unclear (e.g. Ettner, 1997; Ruhm, 1995, 2003; Gerdtham and Ruhm, 2006; Dávalos et al., 2012; Frijters et al., 2013; Montgomery et al., 1998). Education is associated with overall better mental health and higher rates of health-promoting behaviors (Cutler and Lleras-Muney, 2010): we hypothesize this association is larger for those graduating in bad times.

We investigate this theory by examining how education affects the income and health returns to early life labor market conditions. Our analysis is based on Eurobarometer data for 31 countries over the past 50 years. We document that the deleterious effect of recessions is substantially smaller for those with high education. In other words, individuals who graduated in a bad economy are more likely to smoke and drink later in life, but this is less true among the better educated. We observe that education is protective for all of the outcomes we study, although not all estimates are statistically significant. Simulations suggest that a sizeable portion of the cross-country variation in the education gradient can be attributed to differences in unemployment rates.

The returns to schooling in the labor market also vary substantially. Psacharopoulos and Patrinos (2004) document that the wage returns to education across countries range from 3% to more than 20% per year of school. Similar to the findings for health,

studies investigating the effects of compulsory schooling on wages find a large range of estimates (Card, 2001). Economics has emphasized the same explanation for these findings: they strongly suggest that the returns to schooling are heterogeneous (Card, 1999, 2012). However the specific sources of heterogeneity are not well-understood. We find that the labor market conditions under which individuals graduate can explain a substantial amount of the observed heterogeneity in returns to education in many domains. Although we cannot estimate causal effects of education – we only document associations, these results suggest that the returns to education vary depending on economic conditions over the lifetime.

Our paper is also part of a larger literature documenting the unequal impacts of recessions across demographic groups (Engemann and Wall, 2010; Borges-Mendez et al., 2013). The Great Recession's impact on youth in the United States and Europe has been particularly large, with unemployment rates among individuals aged 16–25 reaching a peak of about 20% in the US and rising even higher in Europe. Not only are these rates historically high, they are much higher than the unemployment rates among older adults (Bell and Blanchflower, 2011). Our findings suggest that the impact of the Great Recession on current cohorts is likely to be large and will generate large disparities by education in both health and income as these cohorts age.

## 2. Methods

### 2.1. Data

We use the Standard and Special Topic Eurobarometer Series, the longest running regular cross-national and cross-temporal opinion poll program in Europe. Starting in 1997 and up to 2012, 31 countries in Europe conducted biannual face-to-face interviews (<http://zacat.gesis.org/webview/>). We restrict analyses to individuals aged between 25 and 55 to minimize measurement error in education due to recall, and to minimize the effect of selective mortality by education, most of which will occur after age 55. Our outcomes of interest are not collected consistently every year – for each outcome we include all possible observations to maximize sample size. Details of the years covered for each outcome are in Table 1.

**Table 1**  
Summary statistics.

Sample	(1) Income	(2) Health	(3) Life dissatisfaction	(4) Obesity	(5) Smoke	(6) Drink
<i>Panel A: Dependent variable</i>						
Mean	2.23	0.18	0.21	0.11	0.38	0.08
Standard deviation	(1.05)	(0.38)	(0.41)	(0.31)	(0.49)	(0.27)
Interquartile range across countries	0.18	0.12	0.21	0.11	0.08	0.06
<i>Panel B: Years of education</i>						
Mean	12.10	12.32	12.53	12.24	12.48	12.64
Standard deviation	(4.25)	(4.09)	(4.10)	(4.17)	(4.04)	(4.03)
Interquartile range across countries	1.91	2.09	2.08	1.75	2.32	2.28
<i>Panel C: Unemployment rates</i>						
Mean	0.058	0.070	0.080	0.070	0.076	0.075
Standard deviation	(0.042)	(0.045)	(0.050)	(0.045)	(0.048)	(0.045)
Standard deviation of residual UR	(0.039)	(0.040)	(0.045)	(0.041)	(0.043)	(0.040)
Observations	50,590	28,411	87,407	17,765	47,818	19,632
Years of Eurobarometer	1997–2003	2002, 2006	2005, 2006, 2009, 2011, 2012	2003, 2005	2002, 2005, 2006, 2009, 2011, 2012	2006, 2009
Eurobarometer data source	47.2, 49.0, 52.1, 53.0, 55.0, 56.1, 57.0, 58.2 59.0	58.2, 64.3, 66.2	63.4, 64.2, 66.3, 71.2, 76.3, 77.3, 77.4, 78.1	59.0, 64.3	58.2, 64.1, 66.2, 72.3, 77.1	66.2, 72.3

Note: Standard deviations in parentheses. The residual unemployment rates are from regressing unemployment rates when graduating on indicators for gender, marital status, graduation cohorts, years since graduation, UR sources, countries, survey years, country interacting with survey years and linear time trends for specific country.

### 2.1.1. Measures of income and health

We investigate six outcomes. The first is income. The respondents are asked country-specific questions about their household income and the answers are categorized into eleven categories in different currency unit. The Eurobarometer principal investigators recode these into four income quartiles so that comparisons can be made consistently across the European countries. The variable we use ranges between 1 (top quartile) and 4 (bottom quartile).

Our second and third measures capture overall well-being. Life dissatisfaction is equal to one if the respondent feels unsatisfied or very unsatisfied with his/her life and zero if they report being satisfied or very satisfied. Self-rated satisfaction scores have been shown to be valid and reliable proxies for utility (Lepper, 1998; Sandvik et al., 1993).

Our measure of self-reported health is based on the question “How is your health in general?”, for which the answers are (1) very good, (2) good, (3) not bad and not good, (4) bad and, (5) very bad. Poor health is equal to one if the answer is 4 or 5, and zero otherwise. This subjective measure of health is highly predictive of mortality and other objective measures of health (Idler and Benyamini, 1997; Latham and Peek, 2013).

Finally, we have three measures of health behaviors. We use self-reported height and weight to calculate BMI. Obesity is equal to one for those with a BMI greater than 30 (excluding outliers with BMI above 60, about 0.1 percent of the sample). Although self-reported measures of height and weight suffer from well-known biases, there is no evidence that this bias is systematically correlated with education (Burkhauser and Cawley, 2008).

We construct an indicator of current smoking and an indicator for whether the individual drinks every day. The latter is an imperfect measure of excessive alcohol consumption: daily but moderate drinking is associated with lower mortality (Di Castelnuovo et al., 2006) and higher incomes (Cook and Moore, 2002). Thus increases in this measure are more difficult to interpret as detrimental.

### 2.1.2. Education

The Eurobarometer does not ask years of schooling. Instead, the survey asks individuals their age at graduation from their highest degree. We compute years of schooling as the age at graduation minus seven. We drop those with more than 25 years of schooling (1.5 percent of the sample) because people who finished school at later ages likely took some break in their education. The variation in education is large: 27 percent of the population has fewer than 9 years of education, and 37 percent have more than 12 years of education.

The benefit of this imputation is that we can measure years of schooling across all countries consistently. But this measure ignores the variation in age when people starting going to school. More importantly if individuals delay graduation in bad times, this behavior will bias our results. To assess the quality of our measure, we compare our measure of years of school to reports of years of education from the Survey of Health, Ageing and Retirement in Europe (SHARE). The correlation between the two measures is 0.90. Fig. A1a shows that education trends are almost identical in both data sets. Fig. A1b plots average years of education by gender, country and graduation year in the SHARE data against average imputed years in the Eurobarometer. The points are distributed almost symmetrically along the 45-degree line, suggesting no systematic bias in our education measure. We nevertheless conduct several robustness checks to assess if measurement error in education biases our results.

### 2.1.3. Unemployment rates

The earliest year at which a person finished their education in our sample was 1948 (a person aged 55 in 1997 who finished their

education at age 7 – that is they had no schooling). Thus, we need to measure country-specific unemployment from 1948 on. There is no single source of unemployment data covering that period for all countries. We combine data from four sources. The more recent unemployment rate data are from World Development Indicators (WDI) published by the World Bank. These data are supplemented by historical unemployment rates from the OECD (See website: <http://stats.oecd.org/>). The unemployment rates in earlier times are supplemented by Mitchell (1998) and Layard et al. (1991), where we use Layard et al. (1991) as a priority. Only total unemployment rates are available for early years—consistent series unemployment rates broken down by gender or smaller geographic regions do not exist. Appendix Table A1 reports the availability of the unemployment rates for different periods in specific countries. Using the overlapping years across sources we compute that the correlation coefficient between WDI and OECD is 0.99, and that between Mitchell's and Layard's series is 0.95, indicating a high degree of consistency in unemployment rates across different sources. To capture the systematic differences across different sources, dummy variables for different data sources are included throughout the analysis. Fig. A2 plots the annual unemployment rate by country, showing a large variation across different countries and time periods.

Unemployment rates are not available for all countries and all years covered by the Eurobarometer. For example, many countries in the former Soviet Union only have unemployment data for the 1990s. In addition not all waves collect all outcomes of interest. We are able to match individuals to unemployment rates at graduation for 94, 77, 76, 81, 78 and 74 percent of the samples with income, health, life satisfaction, obesity, smoking, and drinking respectively. Appendix Table A2 reports the country availability for different outcome variables, as well as the weighted means for the whole and matched samples, which are very similar.

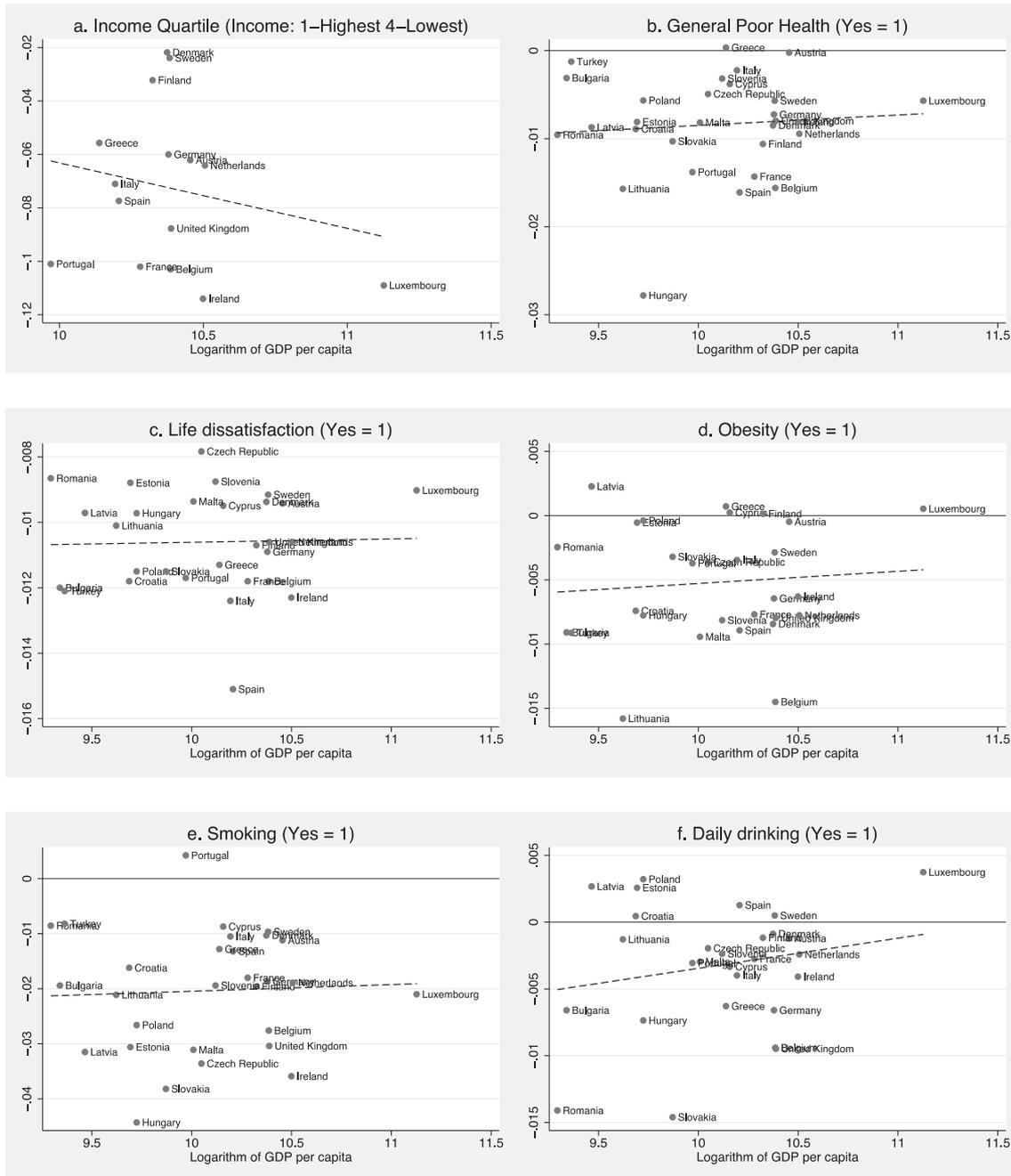
We assume that the country of current residence is the same as the country where the respondent was living at the time of graduation. There is very information in the Eurobarometer data on migration but it shows that 93.1 percent of people where born in the same country as they are surveyed. This is low, and lower than in the US where the 5-year cross-state migration rate is 8.9 percent and the lifetime cross-state migration rate is 32 percent (Molloy et al., 2011).

To alleviate concerns over the timing of graduation and remove some measurement error in the unemployment rate, we use the three-year average moving unemployment rate in our primary analysis – the average of unemployment rates in the year of graduation, one year before and one year after.

To document the variation in unemployment rates we use, Table 1 reports summary statistics for the unemployment rate and the residual unemployment rate, net of controls. The mean unemployment rate ranges from 6 to 8 percent across our different samples, with a standard deviation of 4.2–5 percent. If we regress unemployment rates on all controls, then we obtain the residual variation that identifies the effect of unemployment in our study. The standard deviation in these residuals is about 4 percent showing that there is a lot of variation in unemployment rates net of country-specific trends and cohort effects.

## 2.2. Empirical approach

Following Oreopoulos et al. (2012), our main estimation strategy relates outcomes to personal characteristics, including education, unemployment rate at the time of finishing school, and the interaction between education and finishing school unemployment rates. It is this interaction term that indicates how education affects the return to a good or bad economy.



Note: Each dot represents the coefficient of education from a country-level individual regression of outcome on education and basic covariates.

Fig. 1. Education gradients across countries by outcomes.

Clearly, estimating such a model needs controls for other factors that influence outcomes. Our first controls are for differences in cohorts over time. Graduation cohort effects ( $\delta_g$ ) and experience effects ( $\delta_e$ ) pick up differences across cohorts dated by when they entered the labor market and labor market experience. Because the graduation cohort effects may vary over time and space, we allow for country-specific linear graduation time trends ( $T_c$ ). We also control for education, the interaction between education and experience, and the impact

of education and experience squared. This set of variables traces out average differences in outcomes by education over the lifecycle.

To control for current unemployment or other conditions across countries that may influence outcomes, we include country-survey year fixed effects ( $\delta_{ct}$ ). Effectively, our identification is based on periods where unemployment is higher or lower than is typical for that country, controlling for the fact that cohorts differ on average and over time. The estimating equation is:

**Table 2**  
Education, unemployment rate and outcomes in Eurobarometer.

Dependent variables	(1) Income quartile (1:highest–4:lowest)	(2) General poor health (Yes = 1)	(3) Life dissatisfaction (Yes = 1)	(4) Obesity (Yes = 1)	(5) Smoker (Yes = 1)	(6) Daily drinker (Yes = 1)
<b>Panel A: Mean Unemployment rate around year of graduation (3 years)</b>						
UR when graduating (3 years average)	1.313** (0.372) [0.727]	0.052 (0.134) [0.184]	0.122* (0.072) [0.078]	0.089 (0.128) [0.156]	0.411** (0.120) [0.156]	0.203* (0.123) [0.103]
(Education – 9) * UR	–0.102* (0.053) [0.120]	–0.033** (0.016) [0.018]	–0.047** (0.010) [0.015]	–0.047** (0.019) [0.013]	–0.041** (0.017) [0.030]	–0.028 (0.018) [0.014]
Observations	50,590	28,440	87,450	17,734	48,367	19,656
R-squared	0.259	0.062	0.169	0.036	0.073	0.065
<b>Panel B: Unemployment rate in year of graduation</b>						
UR when graduating	1.205** (0.357) [0.682]	0.045 (0.122) [0.173]	0.090 (0.067) [0.074]	0.038 (0.120) [0.135]	0.269** (0.116) [0.160]	0.195* (0.111) [0.097]
(Education – 9) * UR	–0.095* (0.052) [0.113]	–0.033** (0.016) [0.015]	–0.044** (0.009) [0.014]	–0.043** (0.019) [0.012]	–0.038** (0.016) [0.028]	–0.027 (0.016) [0.013]
Observations	50,590	28,411	87,407	17,734	47,818	19,632
R-squared	0.259	0.063	0.169	0.036	0.073	0.065
<i>F-tests for whether the reported coefficients jointly significant (P-values reported)</i>						
Panel A	0.000	0.067	0.000	0.016	0.002	0.158
Panel B	0.000	0.139	0.000	0.011	0.005	0.070

Note: Standard errors in parentheses are clustered at country-graduation year level, and those in brackets are clustered at country level. Weights in Eurobarometer are applied. Respondents are aged between 25 and 55. The unit of experience is in 10 years. The covariates include years of education, interaction of education with years since graduation and its square term, indicators for gender, marital status, graduation cohorts, years since graduation, UR sources, countries, survey years, country interacting with survey years and linear time trends for specific country.

\*\* $p < 0.05$ , \* $p < 0.1$ .

$$Y_i = \beta_0 + \beta_1 UR_{cg} + \beta_2 Edu_i + \beta_3 UR_{cg} * (Edu_i - 9) + \beta_4 Edu_i * Exp_i + \beta_5 Edu_i * Exp_i^2 + X_i \alpha + T_c + \delta_e + \delta_g + \delta_{ct} + \delta_s + \varepsilon_i \quad (1)$$

where  $Y_i$  is the outcome for individual  $i$ ,  $UR_{cg}$  is the (three-year average) unemployment rate of graduation cohort  $g$  in country  $c$ ,  $Edu_i$  is years of education, and  $Exp_i$  is years since graduation.  $X_i$  is a set of control variables, including gender and marital status.  $\delta_s$  is a set of dummies for different source of unemployment data. Since unemployment rates are at the country-year level, we report standard errors clustered two ways, at the country-graduation cohort level, and at the country level. Country clustering is more conservative, but we have fewer country clusters than is recommended for standard cluster analysis (Angrist and Pischke, 2008).

In this model,  $\beta_1$  shows how unemployment for a cohort with 9 years of education affects outcomes, and  $\beta_3$  shows the differential response for the better educated. Recall that all of our outcomes are defined so that worse outcomes are a higher numerical value; thus, we expect  $\beta_1 > 0$  (unemployment worsens outcomes) and  $\beta_3 < 0$  (a smaller effect for the better educated). That said, the specific value of  $\beta_1$  – the effect of unemployment on the less educated – is dependent on the value we choose for less educated (9 years, in this case). To illustrate the full range of effects, we plot the impact of unemployment on outcomes at different levels of education. Although several outcomes of interest are dichotomous, we report OLS results for simplicity – marginal effects from probit or logit specifications are very similar (Appendix Table A3).

### 3. Empirical results

#### 3.1. Sample description and gradients in education

Panels A and B of Table 1 show summary statistics across countries for each estimation sample. On average, 18 percent of the

respondents reported poor health and 21 percent were dissatisfied with their lives. Eleven percent of the population is obese. Smoking is also popular, with 38 percent of respondent reporting smoking currently. Across the samples, the average years of education are stable, ranging from 12.1 to 12.6. There is also large variation in both education and outcome variables across countries. For example, the interquartile range across countries is 0.12 (67 percent of the mean value) for poor health, and about one sixth relative to the mean for education.

To document the variation in the effect of education across settings, we estimate the education gradient for each country by regressing the outcome of interest on education controlling for gender, marital status, age, and age-squared. Fig. 1 plots these country-specific education gradients against the logarithm of GDP per capita. For example, Fig. 1b plots education-health gradients and shows negative coefficients, indicating that higher education is associated with better health. The average gradients for all the other outcomes are also as expected. Education is positively associated with income (recall that higher income is a lower value), and negatively associated with life dissatisfaction.

Even with these means, there is significant variation across countries. For example, the mean education gradient in smoking is  $-0.018$ , but the interquartile range is 0.017, almost the same magnitude. This variation is not particularly related to average current income, with the exception of income quartile (higher income countries have steeper gradients) and daily drinking (higher income countries have smaller gradients).

#### 3.2. Impact of unemployment rates in early adulthood

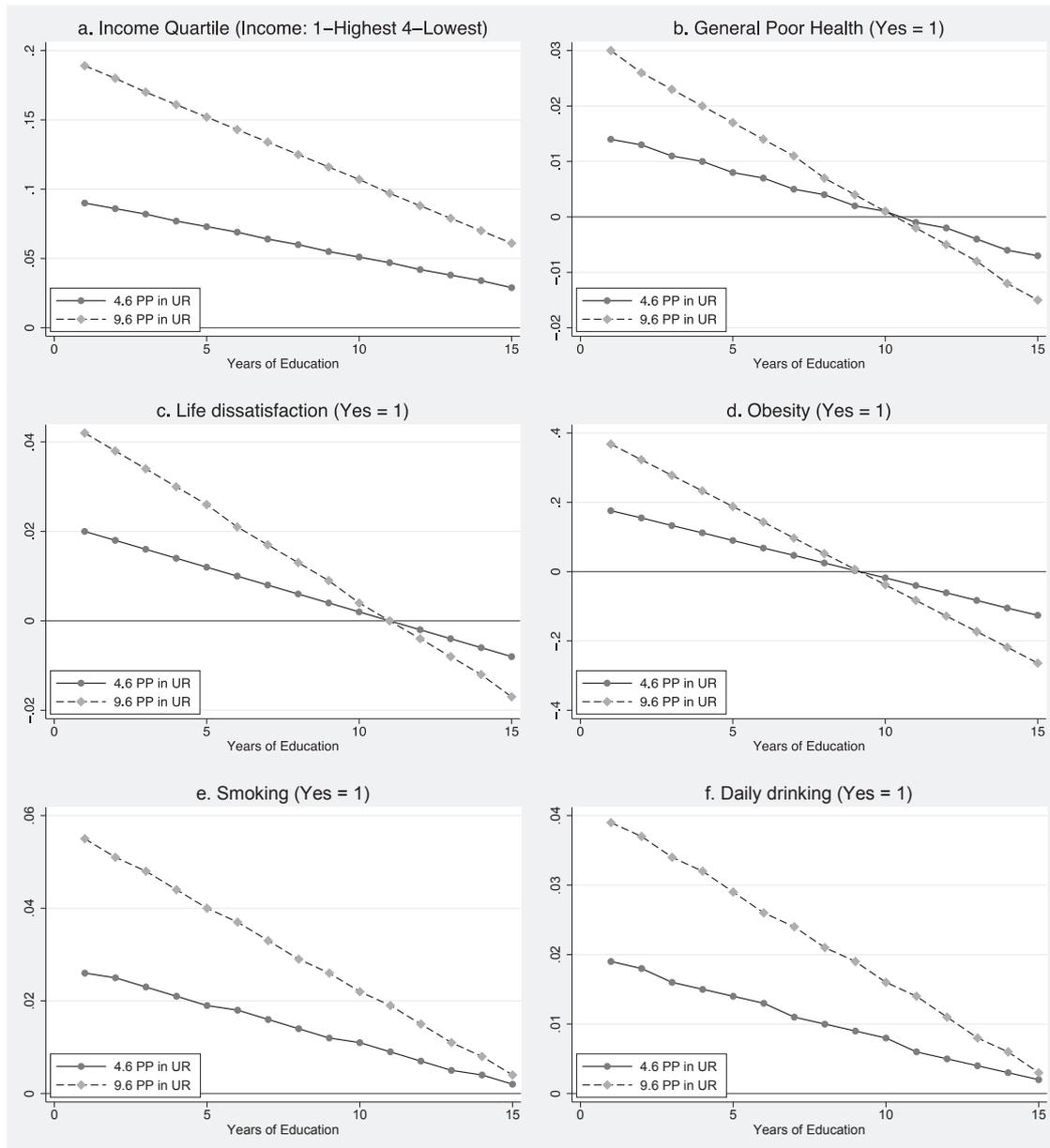
Table 2 reports the OLS estimates of equation (1). We report the coefficients on unemployment at graduation ( $\beta_1$ ) and its interaction with education ( $\beta_3$ ) in the first two rows. The standard errors in parentheses are clustered at country-graduation cohort year level and those in brackets are clustered at country level. Panel A shows the results using three-year average unemployment rates around

the time of graduation (the entire set of coefficients is displayed in the Appendix Table A4). Panel B shows results using the unemployment rate in the year of graduation.

The coefficients in the first row indicate that higher unemployment when graduating is associated with lower socioeconomic status and worse health behaviors, consistent with Bell and Blanchflower (2011) and Maclean (2013). The coefficients are large; for those with exactly 9-years of education, a 5 percentage point increase in unemployment is associated with a 3 percent greater likelihood of being unsatisfied with life relative to the mean ( $0.05 * 0.122/0.21$ ), 5 percent greater likelihood of smoking ( $0.05 * 0.411/0.38$ ), and a 12 percent greater likelihood of daily drinking ( $0.05 * 0.203/0.08$ ). The two exceptions are poor health

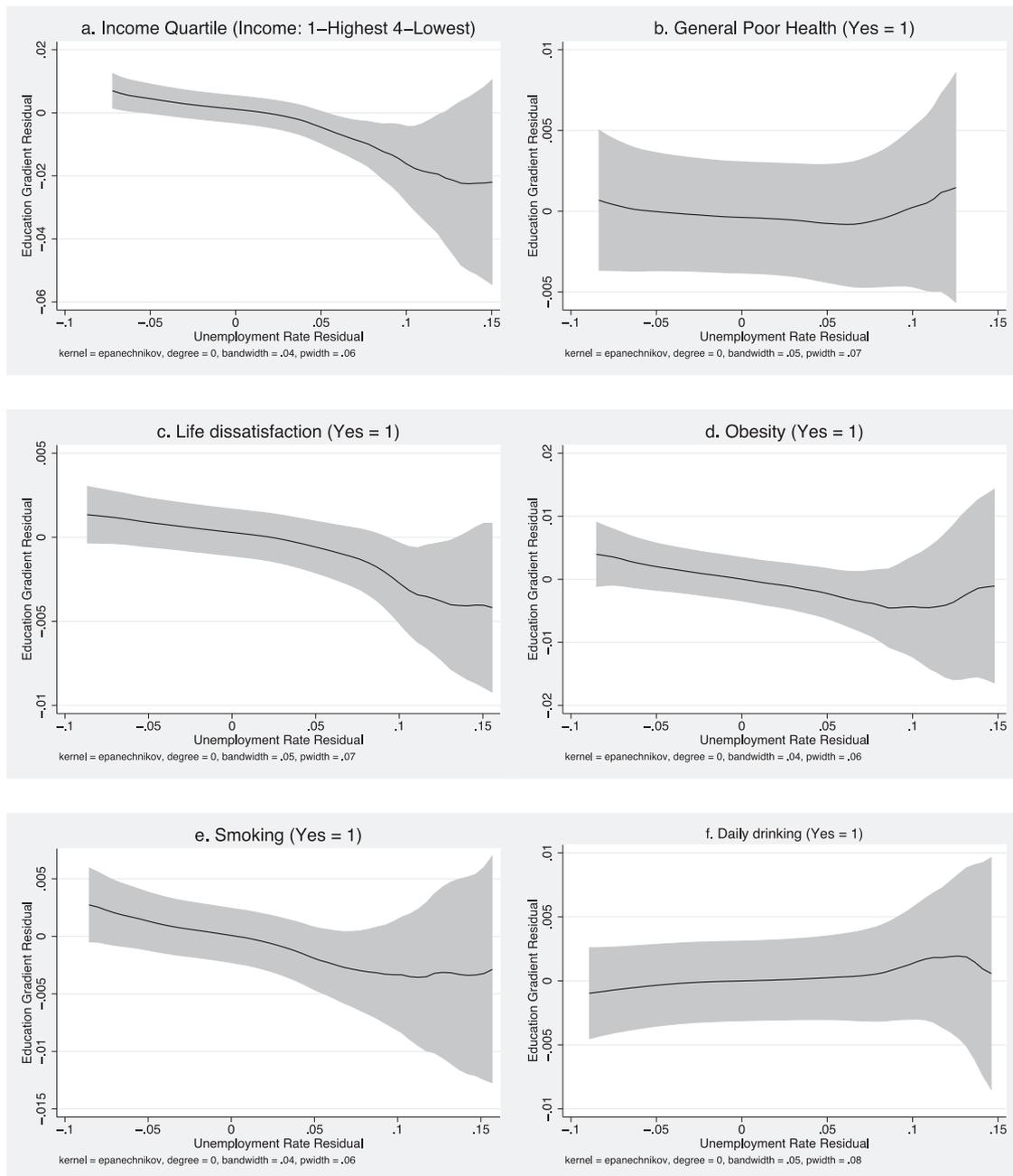
status and obesity: these estimates are also positive but not statistically significant. Panel B reports the results using unemployment rates in the exact year of graduation—the coefficients are similar but somewhat smaller, consistent with greater measurement error. In general unemployment and its interactions are jointly significant at the 10 percent level or higher (*p*-values for joint test at bottom of table).

The sign on the interaction between schooling and unemployment rates is opposite to that on the unemployment rate: education plays a protective role when unemployment is high. For most outcomes, the coefficients are significant at 5 percent level. Fig. 2 plots the relationship between education and outcomes when unemployment is 4.6 and 9.6 percent, roughly corresponding to the



Note: Solid lines and dashed lines are predicted outcomes based on the coefficients in Panel A of Table 2.

Fig. 2. Impact of education on outcomes, for different levels of unemployment.



Note: Residuals in the y-axis are from regressing gradients for each graduation cohort and country, on linear and square terms of graduation years. The residuals of x-axis are residuals from regressing unemployment rates on linear and square terms of graduation year.

Fig. 3. Non-parametric estimation – residuals of education gradients against residuals of unemployment rates.

annual unemployment rates in US before and during the Great Recession, or to the 30th and 70th percentile of the unemployment rates distribution. Overall, the relationship between education and health is much steeper in bad times. For example, in Fig. 2e, we see that for those without formal schooling, a 4.6 percent unemployment rate is associated with a 2.5 percent probability of smoking, while a 9.6 percent unemployment rate is associated with 5.5 percent probability of smoking. For those with 15 years of

education, the effect of greater unemployment is very small, less than 1 percent.

Importantly, high unemployment raises the share of the less educated who are in poor health. Table 2 showed a non-significant impact of unemployment on self-reported poor health of those with 9 years of education. At education levels below that (about one-quarter of the sample), the impact of unemployment is markedly higher.

**Table 3**  
Education, unemployment rates when aged 18 and outcomes.

Dependent variables	(1) Income quartile (1: highest–4: lowest)	(2) General poor health (Yes = 1)	(3) Life dissatisfaction (Yes = 1)	(4) Obesity (Yes = 1)	(5) Smoker (Yes = 1)	(6) Daily drinker (Yes = 1)
<b>Panel A: Mean UR at age 17–19</b>						
Mean UR at age 17–19	1.314** (0.352) [0.890]	0.235* (0.130) [0.160]	0.285** (0.071) [0.120]	0.244* (0.128) [0.099]	0.305** (0.126) [0.203]	0.178* (0.103) [0.107]
(Education – 9) * UR at 18	–0.395** (0.054) [0.154]	–0.047** (0.015) [0.018]	–0.059** (0.009) [0.017]	–0.035** (0.016) [0.012]	–0.069** (0.016) [0.034]	–0.013 (0.015) [0.017]
Observations	50,291	27,927	85,773	17,451	47,621	19,290
R-squared	0.261	0.063	0.170	0.035	0.073	0.066
<b>Panel B: UR at age 18</b>						
UR at age 18	1.307** (0.327) [0.794]	0.147 (0.120) [0.156]	0.226** (0.069) [0.108]	0.249** (0.120) [0.101]	0.267** (0.116) [0.185]	0.147 (0.094) [0.091]
(Education – 9) * UR at 18	–0.384** (0.051) [0.142]	–0.041** (0.015) [0.017]	–0.054** (0.009) [0.016]	–0.031** (0.015) [0.012]	–0.064** (0.015) [0.032]	–0.009 (0.015) [0.015]
Observations	50,215	27,723	85,125	17,334	47,317	19,127
R-squared	0.261	0.063	0.171	0.036	0.073	0.066

Note: see Table 2 notes for sample and specification details.

\*\* $p < 0.05$ , \* $p < 0.1$ .

Previous literature documents gender differences in the effects of recessions (Hershbein, 2012; Novo et al., 2001). Appendix Table A5 reports shows that the effect of unemployment is larger for lower education groups for both men and women. However except for life dissatisfaction, we cannot reject equality the coefficients across genders.

To investigate the relative short- and long-term impact of early life unemployment rates, we include the unemployment rate and its interaction with education with age group dummies (“younger” is a dummy for being aged 40 or less, and “older” is greater than 40). Appendix Tables A6 and A7 show that the “scarring” effect of high unemployment when graduating (and the protective effects of education) is driven primarily by younger cohorts, consistent with Oreopoulos et al. (2012). However the effects of unemployment on drinking and smoking are very similar across ages, consistent with high addictiveness. Hessel and Avendano (2013) find that economic recessions at the time of leaving school are associated with better health status in the later life. Our findings likely differ because we focus on working age individuals (up to age 55) whereas they look at those 50 and older.

### 3.3. Functional form

In Appendix Table A10 we use education categories instead of linear years of schooling. Panel A reproduces our main results for reference. In panel B we show that the results are very similar if we use dummies for education categories rather than continuous years of education.

To further investigate non-linearities we estimate a non-parametric model. We do this in several steps. First, we estimate the education gradient in outcomes for each graduation cohort and country, with basic controls including marital status, gender and survey year dummies. We then regress the resulting gradients on linear and square terms of graduation years; the residuals are the gradient in education adjusted for a general time trend.

Fig. 3 plots these adjusted residuals against unemployment residuals (from regressing unemployment rates when graduating on linear and square terms of graduation year). We estimate the relationship between the two using a kernel-weighted local polynomial. Generally, the patterns are consistent with our conclusions,

but suggest the protective effects of education are largest when unemployment rates are high.

### 3.4. Endogenous education

If individuals choose to remain in school or leave school because of business-cycle associated job prospects then our estimates might be biased. Oreopoulos et al. (2012) argue that the bias this behavior induces is small because the effect of unemployment on graduation timing is empirically small. We also find no statistically significant relationship between years of schooling and unemployment rates in our data (first column of Appendix Table A8).

To further assess this extent of this bias, we follow the intuition in Kahn (2010) and Maclean (2013) and use unemployment at a fixed age. We chose unemployment at age 18, the median age at school completion. If this unemployment rate precedes the decision on when to graduate, it is independent of individual education's choices. Table 3 presents estimates using this alternative unemployment rate. Again we find a large effect of unemployment at age 18 on outcomes, that is smaller among the more educated (Panel A), and attenuated if we do not average over three years (Panel B).

Alternatively we relate outcomes to the unemployment rates prevailing the first year individuals were allowed to drop out of school according to compulsory schooling laws—this is the last unemployment rate individuals observe before they can alter their education choices. The data on compulsory schooling laws across several (but not all) European countries and years can be found in Gathmann et al. (forthcoming).

Panel A of Table 4 first reports baseline results for the subset of country and cohorts for whom compulsory schooling laws are available. The coefficients on unemployment and its interaction with education are very similar to those in Table 2, although a bit larger in magnitude. Panel B uses the unemployment rate at the “school-leaving age” instead. The magnitude and significance of the coefficients is very similar in both panels, although again slightly larger when we use the compulsory school law unemployment rates. Overall Tables 5 and 6 suggest that the bias resulting from endogenous timing of graduation is negligible. However note that unemployment at age 18 or at compulsory schooling ages is likely a better proxy for those with less education than for those with more

**Table 4**  
Unemployment rates at age required by compulsory schooling laws (CSLs) and outcomes.

Dependent variables	(1) Income quartile (1:highest–4:lowest)	(2) General poor health (Yes = 1)	(3) Life dissatisfaction (Yes = 1)	(4) Obesity (Yes = 1)	(5) Smoker (Yes = 1)	(6) Daily drinker (Yes = 1)
<b>Panel A: Unemployment rate in year of graduation</b>						
Unemployment rate	1.849** (0.411) [0.670]	0.286* (0.168) [0.254]	0.233** (0.101) [0.082]	0.341** (0.168) [0.203]	0.687** (0.150) [0.239]	0.056 (0.182) [0.166]
(Education – 9) * UR	–0.254** (0.063) [0.123]	–0.060** (0.022) [0.026]	–0.057** (0.012) [0.016]	–0.042* (0.024) [0.022]	–0.086** (0.023) [0.043]	–0.028 (0.027) [0.021]
Observations	31,622	14,498	36,871	9348	23,391	9167
R-squared	0.271	0.064	0.070	0.042	0.078	0.071
<b>Panel B: Unemployment rate at the least graduation age required by CSLs</b>						
Unemployment rate	1.534** (0.401) [0.820]	0.327** (0.162) [0.183]	0.182** (0.091) [0.112]	0.297* (0.169) [0.210]	0.067 (0.174) [0.294]	0.038 (0.185) [0.207]
(Education – 9) * UR	–0.436** (0.061) [0.158]	–0.063** (0.019) [0.023]	–0.065** (0.011) [0.010]	–0.046** (0.021) [0.027]	–0.138** (0.021) [0.053]	–0.013 (0.020) [0.013]
Observations	31,622	14,498	36,871	9348	23,391	9167
R-squared	0.273	0.064	0.071	0.042	0.079	0.071

Note: The countries in this table include Austria, Belgium, Denmark, France, Ireland, Netherlands, Spain, Sweden and United Kingdom. See Table 2 notes for specification details.

\*\* $p < 0.05$ , \* $p < 0.1$ .

education, leading to possible measurement error for those with higher education and attenuation bias.

### 3.5. The effect of contemporary recessions and recessions at the time of graduation

In Table 5 we investigate how recessions at graduation differ from current recessions. We drop country-year survey dummies and instead control for the current unemployment rate and its interaction with education. Overall the direction and magnitude of the effects of graduating in a recession for low and high education groups remain the same (relative to Panel A of Table 2)—the only important exception is life satisfaction, for which the effects of graduating in a recession become insignificant. Instead current unemployment rates lower satisfaction for all and more so for the uneducated.

The other coefficients generally confirm previous findings that recessions are good for (current) health. Current unemployment is associated with better general health, less smoking, and less drinking, though the interactions with education suggest this improvement is *smaller* among the more educated. But self-reported health and risky behaviors are worse for those graduating in recessions, so the pattern reverses over time. These findings suggest that individuals, particularly the low educated, cut down on “luxury expenses” (smoking, alcohol, deserts) in the short run, but this short term deprivation leads to long-term increases in consumption of these goods. Without panel data, we cannot investigate the dynamics of consumption. But clearly the short and long term effects of recessions are substantially different.

### 3.6. The overall impact of early life labor markets

To understand how much of the variation in the education gradients across countries could result from this early life heterogeneity, we perform a simulation. We start by estimating the impact of a year of education on each outcome. We first predict the average outcome using the actual X's for each individual (which we term  $\hat{Y}_{edu}$ ), and again after increasing each individual's education by one year (termed  $\hat{Y}_{edu+1}$ ). The difference between these predictions,  $\Delta\hat{Y} = \hat{Y}_{edu+1} - \hat{Y}_{edu}$ , is the education gradient, holding

demographics constant. We average this gradient for each country. We then set the unemployment rate for each cohort and country equal to the sample average, and predict again the average outcome for each individual using their actual education ( $\hat{Y}_{edu}$ ) and after increasing education by one year ( $\hat{Y}_{edu+1}$ ). The difference between these,  $\Delta\hat{Y} = \hat{Y}_{edu+1} - \hat{Y}_{edu}$ , is the education gradient that would have obtained in the country if the unemployment rate were not different from the average.

Table 6 reports the differences of gradients across countries under the above two settings. Column 1 shows the difference between the 90th percentile of the gradient distribution across countries and the 10th percentile under actual unemployment rates. Column 2 reports that difference under mean unemployment rates. Column 3 shows the ratio, which is consistently smaller than one, ranging from 0.59 to 0.92. The smaller the ratio is, the larger portion can be explained by unemployment rates.

The results indicate that a sizeable portion of the cross-country variation in the education gradient can be attributed to differences in unemployment rates. This effect is largest for life satisfaction and income, with the ratio being 0.31 and 0.59, but it is also large for drinking and smoking with the ratios being 0.77 and 0.72, respectively. The overall effects on health and obesity are more modest, with ratios around 0.85. Since we cannot estimate causal effects of education, these results are only suggestive, but they point to differences in labor market conditions as a potentially important explanation for the heterogeneity in the returns to school.

## 4. Discussion

Using consistent measures across a large set of countries and cohorts, we document that education gradients in income, health, life satisfaction, obesity, smoking, and drinking vary substantially across countries. We then examine whether differences in unemployment at the time individuals first enter the labor market explain some of the variation in these education gradients. We find that higher unemployment at graduation is associated with lower household income, poorer general health, lower life satisfaction, and higher probability of obesity, smoking and everyday drinking later in life. Furthermore these negative effects are substantially

**Table 5**  
Education, unemployment rates and outcomes in Eurobarometer, with current unemployment rates included.

Dependent variables	(1) Income quartile (1: highest–4: lowest)	(2) General poor health (Yes = 1)	(3) Life dissatisfaction (Yes = 1)	(4) Obesity (Yes = 1)	(5) Smoker (Yes = 1)	(6) Daily drinker (Yes = 1)
UR when graduating (3 years average)	0.737* (0.382)	0.053 (0.133)	−0.034 (0.077)	0.103 (0.131)	0.582** (0.142)	0.240* (0.124)
(Education − 9)* UR	−0.125** (0.059)	−0.039** (0.017)	−0.007 (0.012)	−0.057** (0.022)	−0.079** (0.019)	−0.035* (0.019)
Current UR	0.853 (0.590)	−1.256** (0.354)	1.537** (0.086)	−0.218 (0.463)	−0.109 (0.212)	−0.437** (0.149)
(Education − 9)* Current UR	−0.003 (0.062)	0.042 (0.028)	−0.069** (0.010)	0.050* (0.030)	0.104** (0.027)	0.030 (0.021)
Observations	50,590	28,537	86,620	17,697	38,132	19,699
R-squared	0.244	0.054	0.158	0.103	0.056	0.061

Note: see Table 2 notes for sample and specification details.

\*\* $p < 0.05$ , \* $p < 0.1$ .

**Table 6**  
Variation in gradients and unemployment rates.

Outcomes	(1)	(2)	(3)
	90th percentile–10th percentile		Ratio (2)/(1)
	Actual unemployment rates	Mean unemployment rates	
Income	0.0228	0.0135	0.59
Health	0.0125	0.0105	0.84
Life dissatisfaction	0.0079	0.0025	0.31
Obesity	0.0133	0.0113	0.85
Smoke	0.0061	0.0044	0.72
Drink	0.0042	0.0032	0.77

smaller for those with more education. Our estimates suggest that 15 to 70 percent (depending on the outcome) of the cross-country variation in the gradient is explained by economic conditions at the time of graduation.

There are some limitations in our analysis. First, the relationship of education with outcomes is an association. We document that the effect of unemployment on outcomes varies significantly by education—whether education itself “causes” individuals to respond differently to unemployment rates remains an open question.

These results imply that education levels can be used for targeting programs aimed at helping those most affected by adverse economic conditions. They do not however explain why recessions have a differential impact by education. Unemployment at the time of graduation differentially influences incomes—which likely translate into higher health and life satisfaction. Unemployment might differentially affect the probability of marrying—we find this not to be the case however (see Appendix Table A8) and the results are almost the same without controlling for marital status (Appendix Table A9). Alternatively unemployment influences outcomes by increasing stress, which is unavailable in the Eurobarometer. Finally as argued in Genda et al. (2010), differences across countries in labor market institutions might mediate the impact of recessions by education.

We investigate a possible reason why education gradients vary across time and place. Of course, there are other explanations for these patterns. Differential adoption of health innovations over time by the more educated is another potential explanation. For example the Surgeon General’s report in 1964 resulted in larger effects of maternal education on infant health (Aizer and Stroud, 2010). Individual and family differences might also matter: the return to education could be higher or lower for those from wealthy families or with lower cognitive ability (Kane, 1994).

Nevertheless our findings have some interesting implications. First, dynamic life-cycle considerations are important in understanding socioeconomic differences in health: the effects of contemporary shocks not only differ by education but emerge fully only after a few years, consistent with Case et al. (2005) and Cunha and Heckman (2007). We also find that although recessions improve self-reported health, obesity, smoking and drinking in the short run, they are deleterious in the long run, particularly for those with low education.

Second, policies that target youth unemployment might have particularly large payoffs over the long term. The extent to which job training and other programs improve the labor market success of young uneducated individuals is hotly debated, but it appears to be modest (Card et al., 2011); our results suggest that evaluations of these programs should include health and health behaviors as outcomes. They also suggest that non-labor market programs could help youth in bad economic times by, for instance, improving mental health and preventing the development of poor health habits.

Future research should replicate our results using objective measures of health such as mortality, document the specific mechanisms by which the more educated are able to buffer themselves from recessions, and the extent to which policy can improve the outcomes of uneducated youth. This agenda is particularly important today, given that the Great Recession had a large and disproportionate impact on youth.

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## Appendix A. Supplementary data

Supplementary data related to this article can be found at <http://dx.doi.org/10.1016/j.socscimed.2014.07.056>.

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