Does Schooling Give Us Rational Expectations?

Ajay Shenoy*
University of California, Santa Cruz

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Abstract

I assess whether schooling causes people to form more accurate expectations. Using a panel of Thai households I compare each respondent's prediction of household income with actual income and test whether schooling improves the accuracy of these predictions. I exploit a regression discontinuity created by an abrupt change in the enforcement of compulsory schooling. I find strong evidence that schooling reduces the noise of expectations. Households with noisier expectations borrow more and more often. I find evidence that the mechanism may be higher literacy, which makes it easier for households to learn the most recent crop prices from newspapers.

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1 Introduction

It is unpleasant to accept that some expectations are more rational than others. Even the most elegant model may become intractable when there is heterogeneity in how agents think about the future. As a result, most applied theory either assumes all expectations are equally rational or sticks with one of two exceptions: anyone can pay a cost to become better informed, or a select few have inside information. But suppose two people have access to the same sources of information but one can glean a more rational prediction—not because he pays a cost but because some trait makes him better able to absorb information. If this trait were widespread but not universal, society would divide into those with more rational and less rational expectations.

This paper tests whether schooling creates such division. I use a panel survey of households from rural Thailand that in each year asks the respondent to predict what her household income will be next year. Since the survey is a panel I observe actual income next year. The gap between prediction and reality is the forecast error. For each respondent I compute the bias and noise of this error, as well as combining the two to get the overall prediction error. The major advantage of studying income rather than, say, inflation or unemployment is that income has an immediate effect on the household. Even if the respondent ignores statistics on inflation or unemployment, she certainly keeps track of income and should be able to form a meaningful prediction. Likewise, a test of whether some of these predictions are better than others has clear consequences for household welfare.

The test exploits an abrupt change in Thailand's education policy. In effect the new policy raised compulsory schooling from four years to six. It took immediate effect in 1978, meaning a student who might otherwise have dropped out after four years had to stay for six. Compared to a student who finished fourth grade in 1977, a student who finished fourth grade in 1978 was discontinuously more likely to stay the extra two years. I use the discontinuity to test whether primary schooling makes expectations more accurate.

Such a test is invalid if schooling makes income more predictable, as would happen if educated households switch from unpredictable farming to predictable salaried jobs. By studying rural Thailand I ensure changes in the source of in-

come are small. Salaried jobs are uncommon in Thai villages, and schooling someone for 4 rather than 6 years would not drastically raise his chance to get such a job. I show that there is no statistically significant change at the discontinuity in either the mean or standard deviation of income. I find no evidence of a change in any of several measures of how the household earns its revenue, and no evidence of selective attrition, as would happen if the better schooled migrated to the city.

I reject that the schooled and unschooled have equally rational expectations. Better schooled respondents form expectations with less noise and lower prediction error. By contrast I find only weak evidence that their expectations are less biased. Consistent with a simple model of consumption and savings under noisy expectations, I find that better schooled households—households with less noisy expectations—take out fewer loans and have less debt. Using the panel I show that households take out more debt in years when their income falls short of their expectations. Though accurate expectations are only one possible channel from schooling to borrowing, taken together the results are suggestive.

I then explore the mechanism through which schooling improves expectations. When calibrated to match the reduced-form estimates, the model suggests giving someone 6 versus 4 years of primary schooling drastically raises the odds she is an informed agent. In the data I find a corresponding rise in the level of literacy, and the regression discontinuity confirms the pattern is causal. A channel from schooling to literacy to expectations is consistent with Carroll (2003), who proposes that households learn what to expect by reading professional forecasts printed in newspapers. Since I find no evidence that the better schooled are more likely to read a newspaper, the channel may be that schooling helps them understand what they read. Finally, I test one possible means by which knowing the news might improve expectations: knowledge of the international rice price. I show that the better schooled have expectations more similar to a forecast that uses the most recent price.

This paper is hardly the first to ask whether behavior or beliefs are irrational. Much of the empirical literature studies what factors are correlated with rational actions, such as financial literacy (Agarwal et al., 2009) or violations of choice theory (Choi et al., 2014). Other work uses experiments to test whether

information or peer effects improve decisions (e.g. Duflo and Saez, 2003; Cai, de Janvry, and Sadoulet, 2011; Jensen, 2010; Dizon-Ross, 2014). By contrast, I directly examine expectations, and I measure the causal effect of schooling, which is hard to vary in an experiment. Another branch of literature evaluates whether beliefs are accurate and corrects the standard model when they are not (e.g. Smith, Taylor, and Sloan, 2001; Carroll, 2003; Wang, 2014). I extend this literature by linking inaccurate beliefs to a trait that varies widely between and within societies. My research design is similar to that used by Oreopoulos (2006), who used an abrupt change in compulsory schooling to estimate returns to schooling in Britain, but I study a different and unexplored consequence of schooling.

Though most applied theory assumes all agents are fully rational, a few models require some agents to be "less rational" than others. Galí, López-Salido, and Vallés (2007) study the effect on fiscal policy of making some fraction of consumers follow a "rule-of-thumb." Meanwhile, "noise traders" are used in models of finance to explain empirical results that are hard to explain with models in which everyone is rational (De Long et al., 1990; Shleifer and Summers, 1990; Campbell and Kyle, 1993; Shleifer and Vishny, 1990). Taken broadly, my results justify such models and give one reason why less rational agents might live alongside more rational agents.

My results do not necessarily contradict the rational expectations hypothesis. As Kantor (1979) writes, "The implication of rational expectations" is not that people make no errors, but "rather that the forecast errors are not correlated with anything that could profitably be known when the forecast is made." But what does it mean to know something? And by whom is it known? Suppose two farmers have newspapers that print predictions of the price of their crops. One farmer has enough schooling to read the whole prediction with ease while the other has too little schooling to read even a sentence. Both farmers have access to the same information—neither has an inside tip he can use to beat the market—but it is hard to argue the information is known to both.

Instead my results raise doubts about whether the rational expectations hypothesis is applied correctly. Even assuming all agents optimally use what they know, it is not clear they are equally good at acquiring knowledge. Much applied theory either assumes no gap in knowledge or assumes the gap is between the

public and a few insiders. My results suggest such gaps are more widespread. Even rich countries contain both high school dropouts and PhDs. Given these gaps in schooling, my results might imply that gaps in the quality of predictions are not the exception but the rule. And given that schooling is endogenous, my results might imply a person's ability to make predictions depends on the very outcomes an economic model is meant to explain.

2 Model

Though simple, the model I build in this section will outline a mechanism by which schooling affects expectations and expectations affect borrowing. Once calibrated, the model will help interpret the reduced-form estimates of the effect of primary schooling on expectations.

2.1 Information and Expectations

Suppose a rice farmer (or a shopkeeper whose customers are rice farmers) must predict his income next year y_{t+1} given his information set $\mathcal{I}_t$. Income is a function of two variables X and u:

$$y_{t+1} = \alpha_0 X_{t+1} + \alpha_1 u_t$$

For example, X might be the price of rice just after the harvest while u is the price before planting—that is, u_t is the price just before the $t+1$ crop is planted and X_{t+1} is the price after that crop is harvested. Both may matter for income because a high post-harvest price next year earns the farmer more profit for his crop, whereas a high pre-planting price might drive the government to cut farm subsidies.

Suppose the post-harvest price follows a random walk

$$X_{t+1} = X_t + \eta_{t+1}$$

For simplicity, suppose the error term η and the pre-planting price u are independent and normally distributed with have mean 0 and variance σ_η^2 and σ_u^2. (Think of u_t as all new information in the pre-planting price that cannot

be gleaned from the post-harvest price X_t.)

Everyone knows this year's post-harvest price ($X_t \in \mathcal{I}_t$), perhaps because people have talked about it all year. The question is whether the farmer knows the pre-planting price ($u_t \in \mathcal{I}_t$). If the farmer is informed he believes

$$\tilde{y}^I_{t+1} \sim N[\alpha_0 X_t + \alpha_1 u_t, \alpha_0^2 \sigma_\eta^2]$$

whereas if uninformed he believes

$$\tilde{y}^U_{t+1} \sim N[\alpha_0 X_t, \alpha_0^2 \sigma_\eta^2 + \alpha_1^2 \sigma_u^2]$$

Not surprisingly, the uninformed have noisier beliefs that make it harder to predict the future.

If this were the model of Grossman and Stiglitz (1980), the farmer might pay a cost to learn the pre-planting price. Instead I follow Carroll (2003) and assume the farmer can learn the price by reading the newspaper. But though everyone reads the newspaper, not everyone finds the article that quotes the price and understands what is written. The conjecture I test is whether some people have more rational expectations not because of any action they take, but because they had more primary schooling. Those with more schooling are more literate, and higher literacy makes it more likely the reader gleans information from what is read. Though everyone is handed the same text, some are better able to make use of it.

Suppose a fraction λ^+ of schooled and λ^- of unschooled become informed. Call $\varepsilon_t = y_{t+1} - \mathbb{E}[\tilde{y}_{t+1} \mid \mathcal{I}_t]$ the error of the expectation formed in year t about income next year. In this model $\mathbb{E}[\varepsilon_t \mid \mathcal{I}_t] = 0$ whether the farmer is informed or uninformed, meaning no one has biased expectations (though I will measure bias when I turn to the data). But on average, the expectations of schooled farmers should have a lower standard deviation. The average noise of the schooled is

$$\mathbb{E}[SDev(\varepsilon_t \mid S)] = \lambda^+ \sqrt{\alpha_0^2 \sigma_\eta^2} + (1 - \lambda^+) \sqrt{\alpha_0^2 \sigma_\eta^2 + \alpha_1^2 \sigma_u^2}$$

with a similar expression for the noise of the unschooled. Define $r = |\alpha_1 \sigma_u| / |\alpha_0 \sigma_\eta|$, which represents the value of being informed. The difference in the noise of the

schooled and unschooled is

$$\mathbb{E}[SDev(\varepsilon_t \mid S)] - \mathbb{E}[SDev(\varepsilon_t \mid U)] = (\lambda^- - \lambda^+)\left(\sqrt{1+r^2} - 1\right)|\alpha_0 \sigma_\eta| \tag{1}$$

Assuming $r > 0$ (the pre-planting price contains information), this difference is negative if and only if $\lambda^+ > \lambda^-$.

A statistically significant difference is clearly inconsistent with a model in which everyone's expectations are equally good. But even the model of Grossman and Stiglitz (1980) predicts the difference is zero. In their model the cost of being informed is the same for everyone. In equilibrium the returns to being informed fall until everyone is equally indifferent to being informed, and thus both schooled and unschooled become informed at the same rate. The null hypothesis implied by both equal expectations and Grossman and Stiglitz (1980) is that $\lambda^+ = \lambda^-$. If I reject the null I reject the idea that everyone's ability to form expectations is equal.

2.2 Noisy Expectations and Borrowing

I embed the model of expectations into the simplest of economic problems: an endowment economy in which the farmer receives an uncertain income and decides how much to consume and save. My setup differs from the usual endowment economy in two ways. First, I assume there is a wedge between the savings rate and the borrowing rate. Though a departure from the standard model, this assumption seems reasonable. The median interest rate on loans taken by my sample in 2005 is 6 percent, whereas in the same year the interest rate on savings accounts offered by Bank of Bangkok ranged from 0 to 2 percent.[1]

Second, I assume the farmer must choose his consumption for next year at the end of this year—that is, before he knows his income. This means either that the farmer does not fully pay for what he consumes between harvests until after the harvest, or that he must commit to a level of consumption that continues through the harvest. For example, his child's school fees might be due

[1] Most households can borrow some money at 0 interest, but usually these loans come from friends and neighbors. Given the social cost of appearing poor, it is reasonable to assume the household will avoid this if possible.

at harvest, and given that commitment he must decide how much to spend on upgrading the house. If he expects big profits and spends accordingly, when profits disappoint he must borrow to keep the child in school. This is an inelegant but simple way to capture the intuition that people with more accurate expectations can more easily live within their means.

The farmer chooses consumption c_{t+1} under the information set $\mathcal{I}_t$. If he is informed he can condition his choice on both X_t and u_t; otherwise, he can only use X_t. He solves

$$\max_{\{c_{t+j},b_{t+j}\}} \mathbb{E}\left[\sum_{j=1}^{\infty} \beta^j u(c_{t+j}) \middle| \mathcal{I}_t\right]$$

subject to:

$$y_{t+j} + (1+r)b_{t+j-1} = c_{t+j} + b_{t+j}$$

To make the math simple, suppose the farmer is risk-neutral and that the savings rate $1 + r^S = 1/\beta - \tau < 1/\beta + \tau = 1 + r^B$, the borrowing rate, for some $\tau > 0$. I show in Appendix 1.1 that the results hold for other utility functions and other rates of saving and borrowing.

Under these assumptions the farmer will consume the amount he expects will leave him without savings or debt. That is, he consumes his expected income plus all of his assets. Let $r(b) = r^s$ if $b > 0$ and r^b otherwise. Then

$$\begin{aligned} b_{t+1} &= y_{t+1} + [1 + r(b_t)]b_t - c_{t+1} \\ &= y_{t+1} + [1 + r(b_t)]b_t - \mathbb{E}_{t-1}[\tilde{y}_t] - [1 + r(b_t)]b_t \\ &= \begin{cases} -\alpha_0 \eta_{t+1} & \text{if informed} \\ -(\alpha_0 \eta_{t+1} + \alpha_1 u_t) & \text{if uninformed} \end{cases} \end{aligned} \tag{2}$$

which implies

$$SDev(b_{t+1}) = \begin{cases} \sqrt{\alpha_0^2 \sigma_\eta^2} & \text{if informed} \\ \sqrt{\alpha_0^2 \sigma_\eta^2 + \alpha_1^2 \sigma_u^2} & \text{if uninformed} \end{cases} \tag{3}$$

The standard deviation of the asset position is higher for the uninformed because they have a harder time setting their consumption to match their income.

The dataset I use in my empirical work does not record savings or actual

payments on interest, making it impossible to calculate b_{t+1}. Consider instead $b_{t+1} \mid b_{t+1} \leq 0$, the farmer's asset position conditional on his being in debt. Since b_{t+1} is normally distributed, it is easy to show that

$$\mathbb{E}[b_{t+1} \mid b_{t+1} \leq 0] = -\sqrt{\frac{2}{\pi}} \cdot SDev(b_{t+1}) \tag{4}$$

Taken together, Equations 3 and 4 imply the better informed, and thus the better schooled, should have less debt than the unschooled. Moreover, the model predicts that borrowing should be high when actual income falls short of expectations, that is when $\mathbb{E}[\tilde{y}_{t+1}] - y_{t+1}$ is high. I show in Section 6 that both predictions hold.

3 Data

I build my sample using an annual survey of households collected by the Townsend Thai Project. In May of 1997 the Project surveyed over two thousand rural households in four provinces. The Project followed the households from one-third of the original districts up through 2010 (Townsend et al., 1997). Every year one member of the household reports every other member's age, highest education completed, and whether they were schooled under the old versus new system.

The interviewer then asks the respondent a battery of questions about every source of revenue, both from wages and businesses, and every business expense. These measures of expenses do not include interest paid on loans, which is why a rise in debt does not alter the target the household is asked to predict. The interviewer calculates the household's "net profit" and checks the number with the household. Then the household is asked to make a prediction about "net profit" next year. Though the ordering of these questions may prime the respondent to over-weight current income in her prediction, it also ensures the household knows exactly what components of income to include in her prediction.

Crucially, the survey records the relation between the respondent and the head of household. I can uniquely identify the respondent if she is either the head or the head's spouse. (Since the survey does not record the unique ID of the respondent, I cannot identify any other household members.) I can then

link a respondent's prediction of household income to her birth year and education.

Since the survey is a panel I can compare the prediction of income next year to actual income next year. I compute for each respondent three measures of irrational expectations: the bias, the noise, and the prediction error. (I define each in Section 4.) Since the measures make no sense when calculated using only one prediction—it is hard to tell if someone makes noisy predictions if they have made only one—I discard all respondents for whom I have only a single observation. After trimming, the median number of observations per respondent is 5.[2] To these respondent-level variables I add several household-level variables calculated using all of the data observed for the household. These are the mean and standard deviation of income, the fraction of revenue from agriculture and salaried work, the number of loans, and total debt.[3]

Table 1 gives the mean and standard deviation of the most important variables in my dataset. Given that respondents are either heads of household or their spouses, it is not surprising that only 8 percent of the sample was 10 years old or younger in 1978, the year of the policy. It is also not surprising that the average respondent has fewer than 5 years of schooling—as I describe in Section 5, the laws on compulsory schooling were poorly enforced before 1978. Since the policy is most likely to affect how long a student stays in primary school, my main regressor of interest is the years of primary schooling rather than the years of total schooling. As I describe in Section 5, the policy changes the definition of primary schooling. To stay consistent I use the definition in force before the policy: the first seven years of school. Before the policy it was common for students to drop out after lower primary school, which ended after 4 years. Thus it is not surprising that the average respondent has barely more than 4 years of primary schooling. The average respondent lived in a household with an

[2]Controlling for the number of observations in my regressions makes little difference—not surprising, given that I show the number does not change at the discontinuity.

[3]Since I cannot match household members across time, I effectively assume the head and the head's spouse do not change. How accurate is this assumption over the span of the survey? It is difficult to tell precisely because the respondents, as in all developing countries, often cannot perfectly recall their age, meaning the same person may report a slightly different age one year versus the next. The same issue applies to year of birth, as I calculate this from the age. One check is to take a large change in the year of birth—greater than five years—as a sign of a new head or spouse. These respondents account for less than 15 percent of the total. Dropping them does not change the main results of Section 6 (see Appendix 2.1).

Table 1
Sample Descriptive Statistics: Main Sample

Measures of Irrationality:		*Schooling:*	
Bias	-0.15	Years of primary	4.31
	(0.49)		(1.80)
Noise	0.72	Total years	4.86
	(0.65)		(2.94)
Prediction error	0.72	*Fraction with:*	
	(0.65)	At least 6 years schooling	0.28
		Schooled under new system	0.27
Turned 10 after policy	0.08		
Average Number of Loans	1.73	Mean of household income	126146.62
	(1.12)		(111784.13)
Average Debt	145135.25	Sdev of household income	76343.09
	(207766.22)		(94195.81)
Respondents	1873	Cohorts	70
Households	1319		

income of about 3153 dollars, somewhat less than the national average.[4] The average within-household standard deviation of income is also relatively high. Neither is surprising given that many of these respondents are poor farmers.

The Townsend Thai survey asks no questions about literacy or the use of media, both of which are mechanisms through which schooling might help someone make a good prediction. For such questions I turn to the 1987 Demographic and Health Survey, which asked Thai women of childbearing age

[4] At 2005 exchange rates.

Table 2
Sample Descriptive Statistics: DHS Sample

Years of Primary Schooling	4.02	Reads Well	0.53
	(1.66)	Listens to Radio	0.46
Turned 10 after policy	0.04	Reads Newspaper	0.43
Respondents	3751	Cohorts	36

some of the right questions. For each woman I compile the completed years of primary schooling (defined exactly as in the main sample) and the year of birth.[5] I measure literacy using the woman's response to whether she can "read a letter or newspaper easily, with difficulty, or not at all." I code that she "Reads Well" if she says she reads easily. To these I add her responses to whether she reads a newspaper or magazine at least once per week, and whether she listens to the radio every week. The woman is asked whether she reads a newspaper only if she says she can read, meaning regressions that use this measure have a smaller sample. To keep the samples comparable I include only women from rural areas.

Table 2 presents descriptive statistics of my sample from the Demographic and Health Survey. Given that the survey was taken in 1987, it is not surprising that only 4 percent of the respondents turned 10 after the policy changed, which may be why this sample has even less primary schooling than my main sample. The lack of schooling may explain why barely half the sample reads well (though few are wholly illiterate).

4 Defining and Measuring Rational Expectations

Though Section 2 gives expressions for the noise of an unbiased expectation, here I define more general measures. Let $\tilde{y}_{t+1}$ be the respondent's belief about income next year—that is, her subjective probability distribution. Her expectation in year t of income next year is

$$\mathbb{E}_t[\tilde{y}_{t+1}] = y_{t+1} + \varepsilon_t$$

The forecast error ε_t and its moments govern the rationality of these expectations. Suppose that for each respondent ε_t has mean B for bias and standard deviation N for noise. In the model of Section 2, $B = 0$ for both the informed and uninformed whereas N was higher for the uninformed (and thus the unschooled). If B is not zero, meaning the expectations are biased, then it is natural to introduce a third measure: the root mean-squared error M. As with

[5] To avoid measurement error in the running variable I exclude women whose birth year was imputed.

a biased statistical estimator, the root mean-squared error combines bias and noise to give the overall accuracy of the prediction. For unbiased expectations, $M = N$.

Suppose I observe the prediction of income next year $\mathbb{E}_t[\tilde{y}_{t+1}]$ and actual income next year y_{t+1}. I compute the year t forecast error $\varepsilon_t = \mathbb{E}_t[\tilde{y}_{t+1}] - y_{t+1}$. Given T errors observed for the respondent I can estimate the bias, noise, and prediction error as

$$
\begin{aligned}
b &= \sum_{j=1}^{T} \frac{\varepsilon_t}{T} \Big/ \sum_{t=1}^{T} \frac{y_t}{T} \\
n &= \sqrt{\sum_{t=1}^{T} \frac{\left[\varepsilon_t - (\sum_{j=1}^{T} \varepsilon_t)/T\right]^2}{T}} \Big/ \sum_{t=1}^{T} \frac{y_t}{T} \\
m &= \sqrt{\sum_{t=1}^{T} \frac{\varepsilon_t^2}{T}} \Big/ \sum_{t=1}^{T} \frac{y_t}{T}
\end{aligned}
$$

where in all cases I scale the measure by average income (thus $b = B/\bar{y}$). After scaling, a bias of $b = .5$ would mean the person on average overestimates her income by one-half.

The key input to all three measures is the subjective expectation $\mathbb{E}_t[\tilde{y}_{t+1}]$. The ideal way to measure it would be to first record the entire subjective distribution $\tilde{y}_{t+1}$, and then to take the expectation of this distribution. Unfortunately the survey does not ask the questions needed to estimate the full distribution. Instead the survey asks

> What is your best guess about what the household's net profit will be next year?

Delavande, Giné, and McKenzie (2011) call the response to this question the "simple expectation." They show that although well correlated with the mean of the subjective distribution, the simple expectation is often closer to the mode or the median. If respondents interpret "best guess" to be the median instead of the mean it will distort my measures of irrational expectations. On average households will seem biased because the question measures the wrong mo-

Table 3
Expectations Contain More Information than Current Income

	(1) Levels, OLS b/se	(2) Logs, OLS b/se	(3) Levels, FE b/se	(4) Logs, FE b/se
Predicted Income	0.439***	0.564***	0.193**	0.269***
	(0.09)	(0.02)	(0.08)	(0.02)
Current Income	0.192***	0.125***	0.007	0.002
	(0.07)	(0.02)	(0.05)	(0.02)
Households	1319	1319	1319	1319
Observations	12226	12226	12226	12226

Note: The outcome in all four regressions is next year's income. The first two columns use OLS while the second two control for household fixed-effects. The regressions marked as "log" have both the outcome and all regressors in logs. I restrict the sample to the households included in the regressions of the paper's main results.

ment. It is thus doubly important I confirm the true distribution of household income does not change at the discontinuity, as this might raise the gap between the simple expectation and the true subjective expectation.

But since the simple expectation is well-correlated with the true expectation, it still makes sense to compare the measures of irrationality across farmers despite the distortion. There is no reason to expect the distortion is better or worse for households born into one cohort versus the next. Thus the regression discontinuity helps handle measurement error as well as selection bias.

Despite its flaws the survey question does capture information about future income. Table 3 reports regressions of next year's income on both this year's income and the prediction of next year's income. Columns 1 and 2 run ordinary least squares, and Columns 3 and 4 use fixed-effects. In all cases, the prediction is significant and its coefficient is larger than that on current income. In the fixed-effects regressions current income is insignificant after controlling for the prediction. The prediction not only contains information, it contains more information than current income.

Finally, Table 4 shows that the household's actions change with its predictions. I report fixed-effects regressions of borrowing on expected income. Column 1 shows that, controlling for current income, households that predict higher income take out more loans. Column 4, which uses the log of expected income, suggests households that expect income to be 10 percent above average take

Table 4
Households that Expect High Income Take Out Loans

	(1) Loans b/se	(2) Debt b/se	(3) Log Debt b/se	(4) Loans b/se	(5) Debt b/se	(6) Log Debt b/se
Predicted Income	0.450***	0.138	0.164			
	(0.17)	(0.09)	(0.21)			
Current Income	0.026	0.101*	0.733***			
	(0.13)	(0.05)	(0.18)			
Log Pred. Income				0.142***	18788.413***	0.109***
				(0.03)	(4298.62)	(0.03)
Log Income				-0.012	12698.624***	0.123***
				(0.02)	(2994.67)	(0.02)
Households	1319	1319	1226	1319	1319	1226
Observations	13895	13895	10891	13895	13895	10891

Note: All regressions control for household fixed-effects and cluster by household. The coefficients in Columns 1 and 3 are scaled down by 10^6. I restrict the sample to the households included in the regressions of the paper's main results.

out an extra 1.4 loans. Columns 5 and 6 show similar results for total debt. A ten percent rise in expectations predicts a 1 percent rise in debt. The relation is noisier when using the level rather than the log of expectations (Columns 2 and 3), but this may simply reflect that the levels of these variables are themselves noisy.

Households that expect high income borrow against it, exactly as both theory and common sense dictate. Of course, these regressions do not prove higher expectations cause higher borrowing. A farmer who borrows to buy a tractor probably expects the tractor will raise his yields, in which case higher borrowing might cause higher expectations. But regardless of which causes which, the pattern shows that these expectations are not pure noise.

5 Research Design

5.1 Education in Thailand: The Policy

Like most reforms, Thailand's National Education Development Scheme of 1977 was one law on paper and another in practice. Passed in 1977, the law came into force in most places the next year. (Indeed, Hawley, 2004, simply calls it

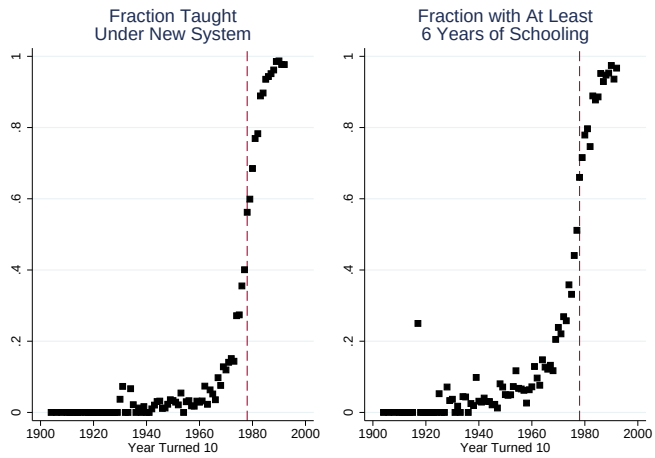
the 1978 educational reform.) On paper the law cut compulsory schooling from 7 years to 6, though to compensate each school year became longer. The stated aim was to switch away from a system that split primary school into two parts. Under the old system students had to pass four years of lower primary before taking three years of upper primary. The new system dropped the upper-lower split and shortened primary school to six years (Watson, 1980).

But in practice the change let the government extend compulsory schooling. Under the old system it was hard to enforce the full seven years, particularly in rural provinces. According to the survey, many students stopped going to school after lower primary. By making primary school shorter and undivided, the government could more easily enforce the law. Sangnapaboworn (2007) writes that "In a sense, the compulsory education was extended from 4 to 6 years."

Consider two children in 1978, one eleven years old and the other ten years old. Assuming both students started school at age six (as is standard), the eleven-year-old finished lower-primary school in 1977. Like many students in the countryside he dropped out after lower primary. But the ten-year-old began her fourth year under the new system. Instead of starting the last year of lower primary she started the fourth year of a new and undivided course. She would then be expected to complete not only the fourth year, but also the fifth and sixth. Thus my regression discontinuity compares the cohort that turned 10 in 1978 to that which turned 10 in 1977.

This logic assumes that students who started under the old system were forced into the new system partway through their schooling. The data make a compelling argument that the assumption holds. Figure 1 graphs the fraction of people in each cohort schooled under the new system, using the entire household roster recorded in the 1997 survey. The jump among cohorts that turned 10 in 1978 suggests many students were indeed forced to change systems partway through their schooling. As a result, the fraction of people with at least 6 years of schooling also jumps. The rise in exposure to the new system and the fraction who get at least 6 years of schooling begins even before 1978. This may be because some schools started implementing the new regime before they were required to, or it may be because turning 10 in 1978 is not a perfect predictor of starting the fourth year in 1978. The measurement error is

Figure 1
The Jump in Exposure to the 6-Year System



Note: Based on the responses of all people on the 1997 household roster.

only a problem if the amount of error jumps at the discontinuity, which seems unlikely.

But the regression discontinuity is a valid instrument for schooling only if there is no other sharp change between the 1977 and 1978 cohorts. The policy was hardly the only event in 1978. But since the 1977 cohort was also alive in 1978, there is no reason to expect an event unrelated to the policy would differ sharply in its effect on either cohort. The bigger concern is whether the policy had other effects. For example, it introduced a slightly different curriculum. But the cohort that turned 10 in 1978 spent four years learning the old curriculum, just as the cohort that turned 9 had three. That said, I cannot definitively rule out that other parts of the policy partly explain my result. If so the parameter I estimate no longer measures the effect of an additional year of schooling on the rationality of expectations; it is a combination of more schooling and the new cirriculum. Even so, the discontinuity will let me run the test derived in Section 2.1, which asks only if education has any effect on the accuracy of expectations.

5.2 First-Stage: The Regression Discontinuity

To exploit the discontinuity I estimate

$$
\begin{aligned}
[Schooling]_i = \pi_{00} + \pi_{11}[Cohort] + \pi_{12}[Cohort]^2 \\
+ [10\ by\ 1978] \cdot (\pi_{21}[Cohort] + \pi_{22}[Cohort]^2) \\
+ \beta[10\ by\ 1978] + [Controls] + \epsilon_i
\end{aligned} \tag{5}
$$

where $[10\ by\ 1978]$ is a dummy for whether the respondent turned 10 by 1978. The coefficient β measures the extra schooling receved by the cohort that turned 10 in 1978. When I run two-stage least squares I control for the true mean and standard deviation of household income for all the years I observe the household.[6]

A typical regression discontinuity design discards observations outside a narrow window around the discontinuity. But since my sample has only 70 cohorts, any such window would throw away many clusters. For my main analysis I use all observations. But I show in Appendix 2.1 that the reduced-form coefficients—the change at the discontinuity in each measure of rational expectations—move little if I shrink the window.

Figure 2 graphs the predicted values and 90 percent confidence intervals. The quadratic polynomial in cohort seems to do well in capturing the relation between schooling and cohort. (I confirm in Appendix 2.1 that the reduced form coefficients do not change much when I raise the order of the polynomial.) Table 5 reports estimates of the change in several measures of schooling at the discontinuity. I cluster all standard errors by cohort of birth. Column 1 suggests a child who was 10 years old in 1978 got one-third of a year more primary schooling than a child who was 11 years old, and Column 5 suggests this amount rises slightly when I control for the moments of household income. The estimate of the effect on total schooling in Column 2 is nearly the same as the effect on primary schooling, suggesting the policy did not keep many stu-

[6]One might worry that the volatility of income will differ for different respondents (e.g. if the head dies and his son takes over in a time when rice prices are less volatile). I can instead calculate the mean and standard deviation using only the years of income during which the respondent answered the survey. The results are unchanged.

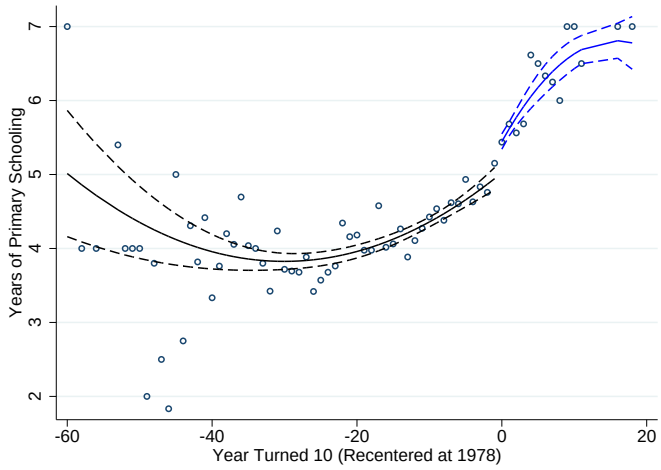
Table 5

First-Stage: The Regression Discontinuity in Schooling

	(1) Years of Primary b/se	(2) Total Schooling b/se	(3) Finished 6 Years Ed. b/se	(4) New System b/se	(5) Years of Primary b/se	(6) Total Schooling b/se	(7) Finished 6 Years Ed. b/se	(8) New System b/se
Age 10- in 1978	0.362***	0.372*	0.212***	0.427***	0.423***	0.529**	0.227***	0.437***
	(0.11)	(0.20)	(0.07)	(0.07)	(0.12)	(0.23)	(0.07)	(0.07)
SD[Income]					-0.000***	-0.000***	-0.000***	-0.000**
					(0.00)	(0.00)	(0.00)	(0.00)
E[Income]					0.000***	0.000***	0.000***	0.000***
					(0.00)	(0.00)	(0.00)	(0.00)
Cohorts	70	70	70	70	70	70	70	70
Respondents	1873	1873	1873	1873	1873	1873	1873	1873
F-Stat	11.509	3.623	10.327	38.626	12.360	5.452	12.069	34.856

Note: All regressions use second-order polynomial in cohort as the control function. I allow the coefficients of the polynomial to change at the discontinuity. I cluster all standard errors by cohort. The "F-Stat" is from the test for whether the coefficient "Age 10 or younger in 1978" is different from zero—that is, whether the excluded instrument for the second stage is signficant.

Figure 2
Primary Schooling Around the Discontinuity



Note: The dots are cohort averages. The dashed lines give 90 percent confidence intervals.

dents in school long after primary. But the estimates are noisier because some students did stay long past the two years they were forced to attend.

Since my running variable is the year of birth, could the jump in Figure 2 be an artifact of treating cohort, a discrete variable, as continuous? Table 6 estimates (5) assuming the policy changed 10, 20, and 30 years before it actually changed. The fake discontinuities produce no statistically significant changes.

5.3 Does Schooling Change the Income Distribution?

Does schooling help respondents make better predictions, or does it only earn them paychecks that are easier to predict? Better schooled students might be better able to get jobs with predictable salaries. The noise of their predictions would be low even though they are no better at acquiring or using information than the unschooled.

The policy is unlikely to have made such dramatic changes to the income of the household. The direct effect of the policy was to force students to stay in school for 6 rather than 4 years. Employers are unlikely to value those two

Table 6
Placebo Tests: Schooling Does Not Jump at Fake Discontinuities

	(1)	(2)	(3)	(4)
	1978	1968	1958	1948
	b/se	b/se	b/se	b/se
Age 10- in...	0.362***	0.051	0.147	-0.430
	(0.11)	(0.11)	(0.21)	(0.27)
Cohorts	70	70	70	70
Respondents	1873	1873	1873	1873

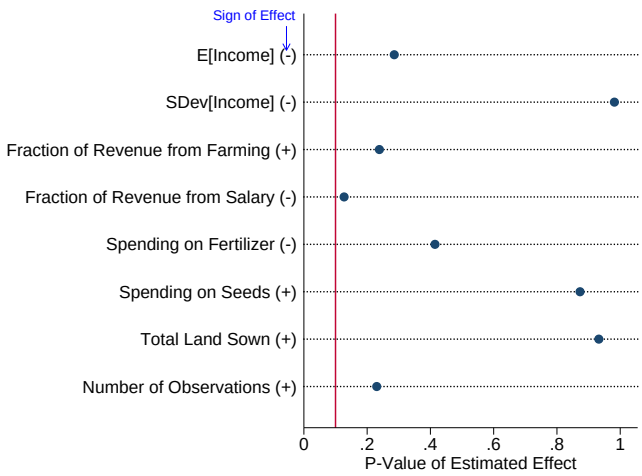
Note: I estimate (5) assuming the indicated year is the year of the policy change.

years enough to switch a new hire into an entirely different field of work. The cohort first exposed to the policy came of age in the late 1980s and early 1990s, when jobs in industry "were mostly assembly and other labour-intensive jobs that required keen eyesight and good manual skills" rather than formal education (Baker and Phongpaichit, 2009, p. 210). Regardless, most of these jobs were in factories around Bangkok, not in the rural areas where my sample lives.

The other concern stems straight from my results: what if people with more accurate expectations make riskier and more profitable investments? (This is precisely what the market selection hypothesis predicts.) Again, the concern is less grave in rural Thailand. Few of the households in my sample buy shares on the stock market. More importantly, these effects would work against my finding any effect. If the better schooled households have better expectations, it would drive them to accept riskier income streams. These would be harder for them to predict, not easier.

If risky equities are not an issue, what about debt? If, as I show in Section 6, those with noisier expectations are more indebted, would this not cost them more in interest? The wording of the survey actually solves this problem. When households compute their "net profit," they do not deduct interest payments on loans they have taken out (the survey covers borrowing in a different section). Since the respondent has just answered a battery of questions to compute this year's net income, he knows exactly what to include in his prediction of next year's income. Thus it is reasonable to assume interest payments will not bias

Figure 3
No Evidence of a Change in the Income Distribution

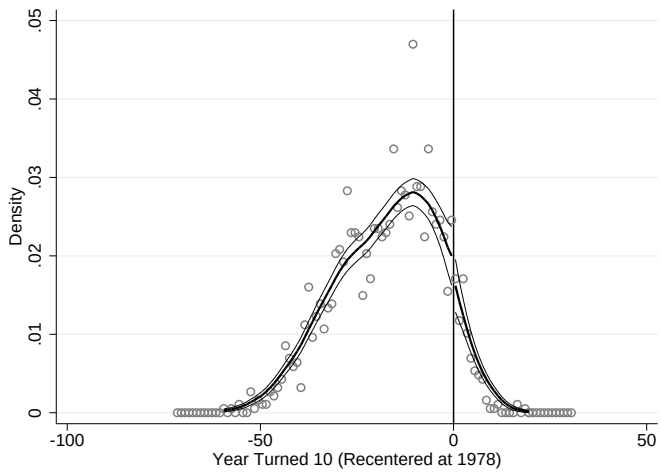


the predictions.

Figure 3, which shows the p-values on tests for whether several measures of income and jobs jump at the discontinuity, shows that there is no significant change in the distribution. The mean and standard deviation of income, the most direct measures, show no statistically significant change. It is not surprising that controlling for them in Section 6 does not change the results. I also study two measures of the nature of the household's job: the fraction of revenue from agriculture and salaries. These measure how much of the household's economic activity comes from risky agriculture versus stable salaries. Neither measure changes at the discontinuity. Indeed, the measure that comes closest to statistical significance, the fraction of revenue earned from salaries, actually has the wrong sign in the regression; if anything, households on the educated side of the discontinuity earn less money from predictable salaries.

Another concern might be that better schooled farmers switch to a farming technology that has more predictable outcomes even as it yields equally variable income. If such technologies exist they would probably require more expensive fertilizer or seeds. I test for changes in spending on fertilizer and seeds per unit of land and find no significant effect on either. A household that

Figure 4
No Evidence of a Change in the Density



Note:
This figure gives a graphical representation of a McCrary test for a jump in the density function at the discontinuity. The p-value of the test is 0.45.

sows more land might mechanically have less variable income, but total land sown also does not change. Finally, I test whether better schooled households are more likely to respond to the survey, giving me more observations and thus more precise estimates of their expectations. Again, I find no statistically significant change.

One last concern is that better schooled people might move to the city, making respondents on the lefthand side of the discontinuity a flawed counterfactual for those on the right. This seems unlikely for the same reason that the policy ought not have big effects on income. Households migrate to get jobs in industry, and industrial firms do not much care about the extra two years of schooling. But if it is a problem it would appear as a statistically significant decrease in the fraction of respondents in the affected cohort. Figure 4 shows a McCrary test of a change in the density at the discontinuity. The p-value of the test is .45, far from statistically signficant.[7]

According to these tests, the most obvious factors that would make the in-

[7]The test and figure were generated using the DCdensity Stata command written by Justin McCrary and Brian Novak.

come of the better-schooled easier to predict—the variance of income, the type of work, the quality of inputs—do not change at the discontinuity. I can never completely rule out that the better-schooled take some action that makes their income easier to predict. But given there is no systematic change in the most important actions, it is unlikely any residual differences can completely explain the results I find in Section 6

6 Regression Results

6.1 Schooling and Rationality

I use the predicted values of Equation 5 to run the second stage regression

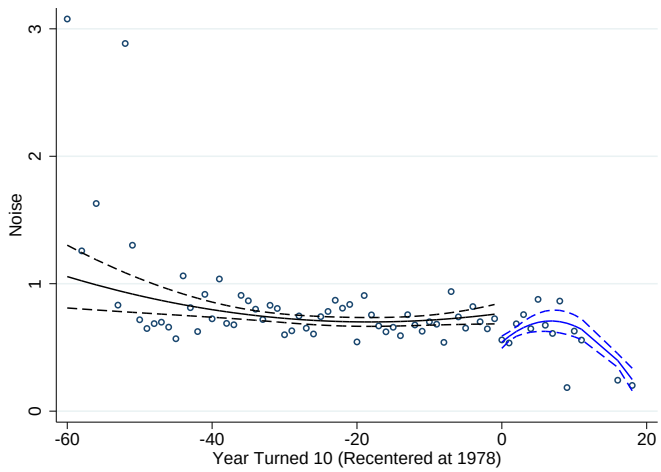
$$\begin{aligned}[Measure]_i = \omega_{00} + \omega_{11}[Cohort] + \omega_{12}[Cohort]^2 \\ + [10\ by\ 1978] \cdot (\omega_{21}[Cohort] + \omega_{22}[Cohort]^2) \\ + \phi[\widehat{Schooling}] + [Controls] + v_i \end{aligned} \tag{6}$$

where the measures of rationality are the bias, noise, and root mean-squared error as defined in Section 4 and the controls are the mean and standard deviation of household income.

Table 7 reports the two-stage least squares regression of each measure of rational expectations on schooling. Columns 4 through 6 control for the mean and standard deviation of household income while Columns 1 through 3 do not. Adding the controls barely changes the results. In all cases an extra year of primary schooling improves each measure of rational expectations. Households that turned 10 before 1978 on average underestimate their income by 18 percent (as reported in the row labeled "Control Mean"). According to Column 1 an extra year of schooling eliminates the negative bias, though the estimate is only marginally significant.

The far bigger effect is on the noise of expectations. Columns 2 and 5 suggest an extra year of schooling lowers the noise from 73 percent of average income to 19 percent. This decrease is not driven by those who use their educations to get stable jobs with predictable salaries—Column 5 confirms that controlling

Figure 5
Noise Around the Discontinuity



Note: The dots are cohort averages. The dashed lines give 90 percent confidence intervals.

Table 7
Second-Stage: The Effect of Schooling on Rational Expectations

	(1) Bias b/se	(2) Noise b/se	(3) Pred. Error b/se	(4) Bias b/se	(5) Noise b/se	(6) Pred. Error b/se
Years Prim.	0.202*	-0.552***	-0.560***	0.194**	-0.537***	-0.543***
	(0.11)	(0.20)	(0.19)	(0.10)	(0.19)	(0.18)
SDev(Income)				-0.000	0.000**	0.000**
				(0.00)	(0.00)	(0.00)
Avg(income)				-0.000	0.000	0.000
				(0.00)	(0.00)	(0.00)
Control Mean	-0.15	0.73	0.73	-0.15	0.73	0.73
Cohorts	70	70	70	70	70	70
Respondents	1873	1873	1873	1873	1873	1873

Note: Columns 1-3 use Column 1 of Table 5 as the first-stage. Columns 4-6 use Column 5 of Table 5 as the first-stage. I cluster all standard errors by cohort. The "control mean" is the mean value of the dependent variable taken over all respondents in cohorts that turned 10 before 1978.

for the standard deviation of income has little effect on the estimate. Columns 3 and 6 confirm that the total effect of reducing bias and noise is a large reduction in prediction error. Figure 5 shows the effect graphically—again, the quadratic polynomial does well in capturing the relation between cohort and noise. Nevertheless in Appendix 2.1 I confirm that even when I vary the order of the polynomial control function and restrict the regressions to cohorts close to the discontinuity, the estimates remain large and significant.[8]

Indeed, the estimates seem too large. Given that my measure of noise is itself noisy, one may worry that these estimates are driven by outliers. Figure 6 reports the results of a rather extreme test. For each measure I discard the top and bottom P percent of the distribution and rerun two-stage least squares. I do this for all P from 0 to 20. In all cases the estimates shrink; given that the bias and prediction error are bounded below by zero but unbounded above, the size of those estimates is likely to shrink even if outliers do not drive the result. Nevertheless, the effect never reaches zero. The effect of a year of primary school on noise settles around -0.2, still large and significant. The effect on the prediction error just barely loses significance when I trim the top and bottom 20 percent, likely because losing almost half the sample cuts my power. By contrast, the effect of schooling on bias loses significance quickly, which might suggest it is driven by outliers.

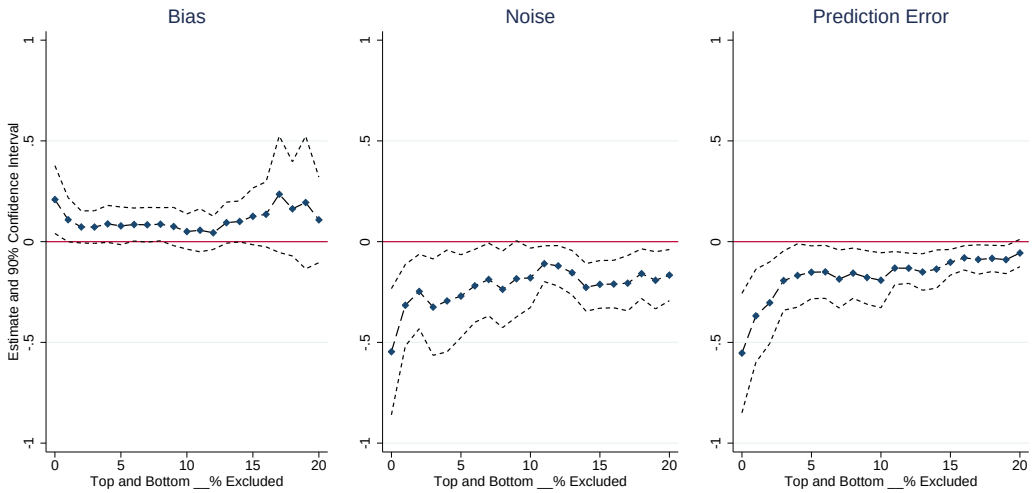
Given that the effect of primary schooling on the noise of expectations is probably nonlinear, it would be unwise to extrapolate these numbers. In Section 7 I calibrate the model of expectations to better interpret their meaning.

6.2 Borrowing

Given that a better schooled respondent has less noisy expectations, the model of Section 2.2 predicts she should also borrow less. For each household I compute the average number of loans, the average outstanding debt, and the average difference between what is owed and what was borrowed—that is, the average expected total interest. (It is possible the household defaults on these expected payments.) I rescale debt and expected interest by their standard de-

[8]The odd hump among cohorts that turn 10 in the early to mid 1980s may be the result of early life exposure to a communist insurgency, which I discuss in more detail in Online Appendix 2.2.

Figure 6
Robustness: Trimming Outliers



Note: Each point is an estimate of the IV regression of the indicated measure of rational expectations after trimming some fraction of the sample. For example, the estimates furthest to the right give the IV estimate after trimming the top and bottom 20 percent of the sample. The dashed lines are 90 percent confidence intervals.

viations to make them easier to interpret.

Note two points about these estimates. First, though my running variable is defined for each respondent, I observe borrowing only by household. Thus I estimate the effect on household debt of giving one of the main decision-makers more primary schooling, and I adjust the standard errors to cluster by both cohort and household. I show in Appendix 2.1 that if instead I keep only the head of household or the spouse if the head is not in the dataset, the results are similar. Second, the model makes predictions about the net asset position rather than borrowing per se. Since I do not observe total savings I cannot measure the household's net asset position.[9] But the intuition of the model—that households with noisier expectations are more likely to consume too much, and thus need more loans—still holds.

Columns 1 through 3 of Table 8 report two-stage least squares estimates of the effect of a year of schooling on each measure of borrowing. An extra year of primary schooling causes a households to take out one fewer loan and causes its average debt and expected interest payments both to fall by roughly 0.6 standard deviations. These results are consistent with the idea that households better able to predict their income do not need to make as many corrections to their budgets. If correcting each error requires getting a new loan, those who make fewer errors need fewer loans.

I test this idea by measuring how a shortfall in expected income affects outcomes in the household-year panel. I define the shortfall as

$$[Shortfall] = \log(Income\ expected) - \log(Income\ received)$$

or roughly the percent gap between how much income the household expected it would receive this year and how much income it actually received. I run fixed-effects regressions similar to those of Section 4. Columns 4 through 6 of Table 8 show that, controlling for current expectations and current income, a shortfall 10 percent larger than average predicts an extra 0.4 loans and a 1 percent rise in debt.

This evidence is only suggestive. I have shown that schooling helps people form better expectations, but I cannot claim it has no other effects. Even con-

[9]The interviewers never asked how much the household saved in cash or jewelry, likely because households fear revealing this will make them targets for burglary.

Table 8

Consequences: People with Less Noisy Expectations Take Out Fewer Loans

	Cross-Section (Reg. Disc.)			Panel (Fixed-Effects)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Loans	Debt	Interest	Loans	Debt	Log Debt
	b/se	b/se	b/se	b/se	b/se	b/se
Years of Primary	-0.992***	-0.626**	-0.664*			
	(0.28)	(0.27)	(0.36)			
Income Shortfall				0.042**	0.038***	0.094***
				(0.02)	(0.01)	(0.02)
Expected Income				0.132***	0.045***	0.058**
				(0.04)	(0.02)	(0.03)
Current Income				0.005	0.089***	0.219***
				(0.04)	(0.02)	(0.03)
Cohorts	70	70	70			
Households	1319	1319	1319	1319	1319	1210
Observations	1873	1873	1873	12226	12226	9671

Note: Columns 1-3 use the respondent-level dataset and cluster standard errors by cohort and household. Columns 4-6 use the household panel and cluster standard errors by household. The dependent variable in Column 1 is the average number of loans across all years, and in Column 4 it is the number of loans for that year. Likewise, Column 2 uses average debt while Column 5 uses debt for that year, both in standard deviation units.

trolling for the accuracy of expectations, those with more schooling might be better able to compute how big a loan they need or have friends willing to give them bigger loans. In short, though I have shown the extra two years of schooling do not change the distribution of income, I cannot prove they change no other aspect of the respondent's life. Still, these results do match the predictions of the model.

Given the effect on borrowing, it may seem puzzling that I do not find a change in income. People who are more certain about the future should be more willing to invest. Even if Thai villagers cannot buy stocks, they can invest in their farms and businesses. But this logic could break down for several reasons. The uninformed might become informed before they make their decision, as would happen if they are uncertain about what wages they will be offered next year. Farmers may be too poor to get more land or labor, leaving the informed unable to exploit their knowledge. The uninformed may believe their expectations are less noisy than they are in truth, making them unaware of how big a risk they take. Finally, it is possible that schooling makes people more cautious, balancing out the effect of having less noisy expectations. I show in

Appendix 2.3 that there is a statistically significant rise in risk aversion at the discontinuity. I discuss these explanations in greater depth in the appendix.

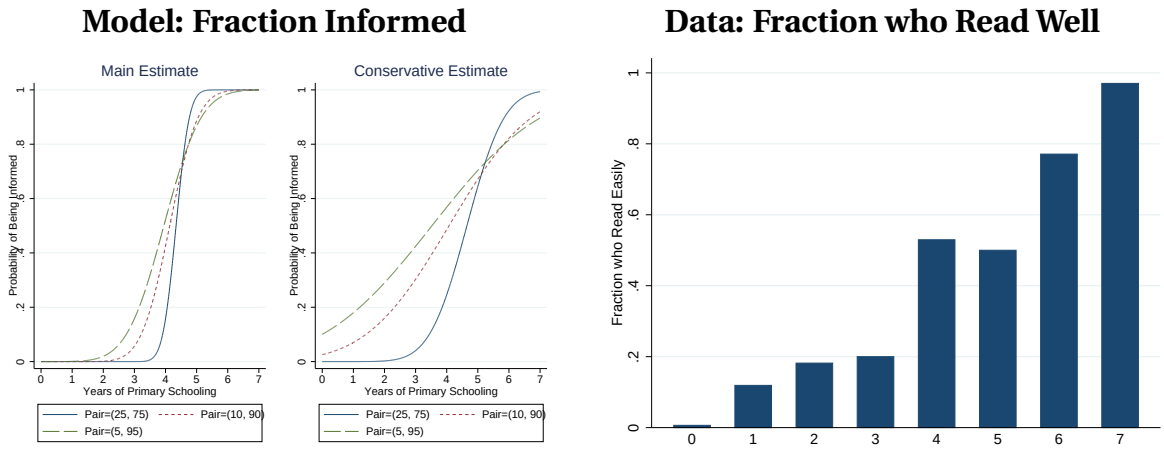
7 Mechanism: Mapping the Results to the Model

Though the model of Section 2 is a gross simplification, taking it literally may help explain why schooling has such large effects on the accuracy of expectations. For example, if the true effect is nonlinear then any extrapolation of the regression results might be misleading. The model can account for nonlinearity and also point to mechanisms for why schooling affects expectations. I first calibrate the model to back out λ, the probability of being informed as a function of primary schooling. I then return to the data to explore whether, as the model suggests, literacy and knowledge of the rice price may be what sets apart the schooled from the unschooled.

To adapt the model I must impose a functional form on λ. For simplicity I assume it follows a probit form $\lambda(S) = \Phi(\gamma_0 + \gamma_1 S)$ where Φ is the standard normal distribution function. To apply this function to Equation 1 I must have values for the noise of the expectations of informed and uninformed people. Given that this distinction is more metaphor than fact, any choice I make is arbitrary. Rather than take a stand, I redo the calibration with three sets of values, each a pair of percentiles from the distribution of noise in my respondent-level dataset. I use percentiles 25 and 75, 10 and 90, and 5 and 95. Finally, I must feed into Equation 1 an estimate of the reduced form change in noise at the discontinuity. Given that my main estimate of -.54 is much bigger than my conservative estimate of -.2, I do the calibration with each and compare the results. Computing the parameters is straightforward; I detail each step in Online Appendix 1.2.

The first panel of Figure 7 shows the function $\lambda(S)$ implied by different combinations of percentiles and reduced-form estimates. Though there are clear differences in these curves, all of them suggest that people with no primary schooling have almost no chance of being informed, whereas those who finish primary school are almost certain to be informed. (Note that being informed does not imply zero noise but noise equal to the lower of the pair of percentiles.)

Figure 7
The Importance of Years 4 to 6 in Theory and Data



Note: The left panel shows the probability of being informed as a function of schooling given different calibrations of the model from Section 2.1. Each curve assumes a different level of noise for the informed versus uninformed farmers. For example, the curve labeled (25,75) assumes the informed have noise equal to the 25th percentile while the uninformed are at the 75th percentile. The first set of curves assumes the effect of schooling on noise using the entire sample is the truth while the second set uses the conservative estimate. The right panel shows the fraction of women who say they read well in the Demographic and Health Survey as a function of their years of primary schooling.

Many of the curves, especially those that use the main estimate, suggest a dramatic improvement in the chance of being informed between from years 4 to 6. What could drive such a dramatic change?

To answer this I turn to a sample of rural women drawn from the 1987 Demographic and Health Survey. As I explain in Section 3, I must use a new sample because the Townsend Thai Annual survey asks no questions about literacy or the use of media. The righthand panel of Figure 7 shows the fraction of women who say they can read a newspaper or magazine with ease. The fraction is nearly zero for women with no schooling. It rises slightly for those with some schooling but jumps dramatically after the fourth year of schooling, then jumps again at the sixth year of schooling. Though only suggestive, the pattern is remarkably similar to the model's prediction about the probability of being informed.

To confirm that schooling causes the increase in literacy I exploit the discontinuity. Column 1 of Table 9 confirms that switching samples does not change the effect of the discontinuity on primary schooling. Compared to women a

Table 9
Mechanism: Schooling Improves Literacy

	(1) Years of Primary b/se	(2) Reads Well b/se	(3) Reads Newspaper b/se	(4) Listens to Radio b/se
Born after 1968	0.449***			
	(0.11)			
Years of Primary		0.150**	0.096	0.059
		(0.07)	(0.09)	(0.10)
F-Stat	17.08			
Cohorts	36	36	35	36
Respondents	3751	3751	3312	3750

Note: I cluster all standard errors by cohort. Women are asked if they read a newspaper only if they say they can read. Thus the number of respondents is lower for the regression reported in Column 3. I can also define an unconditional indicator for whether the woman reads the newspaper. Using the unconditional indicator produces nearly identical results.

year older than them, women who turned 10 in 1978 completed an extra .45 years of primary school—not much different from the extra .42 years gained by the respondents in my main sample. Since across samples the policy had a similar effect on schooling, it is more likely to have had a similar effect on literacy.

Column 2 of Table 9 suggests schooling has made the sample better able to read. Taking the discontinuity as an instrument for years of primary school, I find that an extra year causes a 15 percent increase in the chance a woman reads well—a big increase over the sample mean of 53 percent. By contrast I find no evidence that the exposed cohort is more likely to read a newspaper or listen to the radio.

This evidence suggests the effect of schooling on expectations may be through literacy. Based on statistical significance, it appears schooling does not make a woman in my sample more likely to read a newspaper, but it does increase the chance she understands what she reads or reads the entire paper. If the article relevant to her income is buried in the back pages or written in dense prose, being able to read well may raise the odds she learns anything of use.

But does the story of the model—that farmers read about prices and use them to inform their predictions—find any support in the data? Thai newspapers do publish articles about farming and rice prices. For example, one arti-

cle published in 2012 in the *Bangkok Post* ran under the headline "World rice supply soars, prices to fall?" If even the *Bangkok Post*, which serves English-speaking urbanites, publishes articles about the price of rice, it is reasonable to assume the same of newspapers that serve farmers. Thus it is reasonable to assume that those who read well are more likely to know the current price of rice. Though not everyone in my sample is a farmer, rice farming is so central to the village economy that a high rice price is like an aggregate productivity shock. When farmers have more money to spend it helps the shopkeepers and laborers who sell to them.

I take the model of Section 2 literally and assume the sole difference between the informed and uninformed is whether they know the latest price of rice. Since the survey was fielded in May, I take the price of rice in May as u_t. For X_t I take the average price from January through April. This average represents a noisy awareness of prices since the end of the last harvest—by controlling for it I pick up the additional information in the latest rice price.

In the panel I regress future income on the two prices:

$$y_{t+1} = \bar{\alpha} + \alpha_0 X_t + \alpha_1 u_t + \eta_{t+1} \tag{7}$$

I then compute the predicted values from this regression. For each respondent I compute the correlation coefficient between this "optimal" prediction and the reported expectation of future income. I do the same for the suboptimal prediction from a regression of future income on just X_t. Table 10 shows the change in each correlation at the discontinuity. Both the reduced-form and two-stage least squares estimates suggest that schooling makes the respondent's prediction more similar to the optimal prediction, but not the suboptimal prediction.[10]

This evidence is only suggestive, and it is worth noting that computing these correlations by household pushes my data to its limits. Still, the results offer one mechanism by which schooling, through better literacy, might improve expec-

[10]The coefficients on the prices in Equation 7 are 111 with a t-statistic of 4.62 and -29 with a t-statistic of -1.95. The coefficient on the most recent price may be negative because a sudden rise in the rice price just before planting inspires farmers to plant more, depressing local prices at harvest. Another possibility is that a rise in prices causes the government to reduce its rice subsidies.

Table 10
Mechanism: Predictions of Better Schooled Are More Similar to Optimal Forecasts Based on Rice Prices

	(1) Optimal Forecast b/se	(2) Optimal Forecast b/se	(3) Suboptimal Forecast b/se	(4) Suboptimal Forecast b/se
Age 10- in 1978	0.139**		0.082	
	(0.06)		(0.05)	
Years Prim.		0.329*		0.194
		(0.18)		(0.16)
Cohorts	70	70	70	70
Respondents	1873	1873	1873	1873

tations. Most likely a newspaper or almanac or trade magazine will contain far more information than just the rice price. Taken together, the extra information gleaned by the schooled and literate may explain why their predictions are more accurate than those of the unschooled.

8 Discussion

To assess what these results mean for the rational expectations hypothesis, one must first define it. But a look at the literature suggests different authors have different standards for what it means to have rational expectations. In his statement of the hypothesis, Muth (1961) takes the strongest view: that the predictions of firms are the "same as the prediction of the theory." Agents not only know all the relevant information, but they know how to use it because they know the model and they know the parameters that govern it.[11] This view of rational expectations also underpins the Lucas Critique (1976). For agents to know how a new policy will change the world they must know the model and all the information fed into it.

In contrast to this "eductive" view of rational expectations, other theorists have taken what Binmore (1987) calls the "evolutive" view. In the spirit of Fried-

[11]To be precise, Muth assumed the average expectation across all agents equaled the true expectation. The standard assumption today, on the other hand, is to assume the expectations of each agent meet this standard.

man's "as if" argument (1953), agents are not all-knowing but have learned how different variables move together. The farmer may not know anything about the elasticity of supply, but over many years he has learned that a drought in China will with fifty-fifty chance raise the price of rice by at least 10 percent.[12]

My results are inconsistent with both views. Assuming the world is in equilibrium, both forms of rational expectations predict everyone should have correct expectations, which implies everyone should have equally good expectations. But my results suggest the unschooled neither know everything nor behave as if they did.[13]

Other theorists have taken seriously that rational expectations is just "*the application of the principle of rational behavior to the acquisition and processing of information and to the formation of expectations*" (Maddock and Carter, 1982, emphasis in original). Agents in the model of Grossman and Stiglitz (1980) can pay a cost to learn more about the returns to an asset. In equilibrium some agents choose to remain uninformed because the gains from acquiring information equal the costs, a prediction supported by Ippolito's comparison of net returns from actively versus passively managed mutual funds (1989). Mankiw and Reis (2002) and Reis (2006a,b) build macroeconomic models of inattentive producers and consumers. They show that these models make more accurate predictions than models that assume fully rational expectations.[14] Meanwhile, Feige and Pearce (1976) and Carroll (2003) both derive tests that reject fully rational expectations in favor of models of imperfectly informed expectations.

Whereas these models assume the agents with better predictions have paid some cost or hold an elite position (e.g. at a professional forecasting firm), my results show that even a few years of schooling can improve expectations. Though schooling does carry a cost, it is a cost often paid by parents rather than students, and compared to the effort of learning about an asset it is a cost more likely to be unaffordable. If so, the poor are disproportionately likely to have unreliable expectations, making expectations a new channel through which in-

[12]Examples of macroeconomic models that take this approach include Friedman (1979); Taylor (1975); Cyert and DeGroot (1974). Such learning does not, however, guarantee that the world lands in a rational expectations equilibrium (see Blume and Easley (1982) for an example).

[13]On the other hand, I cannot rule out that agents simply have not yet finished learning the distribution, as in Friedman (1979), especially if schooling lets agents learn more quickly.

[14]See Woodford (2003) and Coibion and Gorodnichenko (2012) for more work in this vein.

equality may persist. Since schooling varies widely within and between countries, my results suggest that differences in the quality of expectations are too widespread to ignore.

Section 6.2 shows that these differences have consequences. Households with noisier expectations have more debt. Recent theoretical work suggests that in other contexts the consequences could be more dire. The market selection hypothesis suggests agents with inaccurate beliefs should be driven from the market (e.g. Sandroni, 2000). How have such agents survived to appear in my data? First, recent work by Borovička (2013) shows that biased beliefs need not go extinct in a model with a more general (and more realistic) utility function. Second, as I have said, stocks are not a big part of the net worth of Thai villagers, limiting the stock market's ability to select. Finally, "extinction" in the market need not imply extinction in truth. Households with less accurate beliefs may survive even as they enjoy less of society's wealth. In this light my results are consistent with Piketty's (2014) finding that since World War 2 the wealth held by the poorest households in rich countries—countries where stock markets do matter—has shrunk.

Finally, the results may have empirical as well as theoretical implications. Epper (2010) shows that a revision in expectations may cause households to revise their plans as though they have time inconsistent preferences. If the unschooled revise their expectations more often, in the data it will appear as though they have less consistent preferences. A researcher who observes the data might mistake inaccurate predictions for imperfect self-control.

9 Summary

Economists have many models to explain how schooling affects income. But if schooling also affects the accuracy of these models—in particular, the accuracy of how we model a person's ability to form expectations—then the accuracy of our models may be endogenous.

This paper provides evidence that schooling does improve the accuracy of expectations. But these results are hardly the final word on schooling and expectations. The type of prediction I study—a person's prediction of her own

income—is important but basic. It is the one statistic a person reliably observes each year. This may explain why my calibrated model implies even a few years of primary schooling drastically reduce the noise of expectations. Other statistics—the rate of inflation, the interest rate, the level of unemployment—may only rarely come to the attention of most people, making them harder to predict even with schooling.

The logical next step is to measure how a year of schooling improves the prediction of these statistics. Future research must also look beyond schooling to other factors—upbringing, nutrition, and the role of social networks—that might improve the accuracy of expectations. Finally, research in applied theory must devise tractable models in which agents are not equally good at gleaning the information they need to form accurate expectations.

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A Technical Appendix (For Online Publication Only)

1.1 Borrowing and Information

1.1.1 Risk Neutral Preferences with General Wedges

Suppose the savings rate and borrowing rate are $1+r^s = \tau^s/\beta$ and $1+r^b = \tau^b/\beta$, with $\tau^s < 1 < \tau^b$. Let p_b be the probability the farmer must borrow next year given his commitment to consume. To satisfy the Euler equation the farmer sets

$$\begin{aligned} \frac{1}{\beta} &= \mathbb{E}[1 + r_{t+1} \mid \mathcal{I}_t] \\ &= p^b \frac{\tau^b}{\beta} + (1-p^b)\frac{\tau^s}{\beta} \\ \Rightarrow p^b &= \frac{1-\tau^s}{\tau^b - \tau^s} \equiv \omega \end{aligned} \tag{8}$$

If $\tau^s/\beta = 1/\beta - \tau, \tau^b/\beta = 1/\beta + \tau$ as in the main text, $\omega = 1/2$.
Given a choice of consumption,

$$\begin{aligned} p^b &= Pr\{y_{t+1} + (1+r(b_t))b_t - C_{t+1} < 0\} \\ &= \begin{cases} \Phi\left[\frac{C_{t+1}-(1+r(b_t))b_t - \alpha_0 X_t}{\sqrt{\alpha_1^2\sigma_u^2 + \alpha_0^2\sigma_\eta^2}}\right] & \text{if uninformed} \\ \Phi\left[\frac{C_{t+1}-(1+r(b_t))b_t - \alpha_0 X_t - \alpha_1 u_t}{\sqrt{\alpha_0^2\sigma_\eta^2}}\right] & \text{if informed} \end{cases} \end{aligned} \tag{9}$$

Combining (8) and (9) gives

$$C_{t+1} = \begin{cases} (1+r_t)b_t + \alpha_0 X_t + \sqrt{\alpha_1^2\sigma_u^2 + \alpha_0^2\sigma_\eta^2} \cdot \Phi^{-1}(\omega) & \text{if uninformed} \\ (1+r_t)b_t + \alpha_0 X_t + \alpha_1 u_t' + \sqrt{\alpha_0^2\sigma_\eta^2} \cdot \Phi^{-1}(\omega) & \text{if informed} \end{cases}$$

which implies

$$b_{t+1} = \begin{cases} -\left[\alpha_1 u_t + \alpha_0 \eta_{t+1} + \sqrt{\alpha_1^2 \sigma_u^2 + \alpha_0^2 \sigma_\eta^2} \cdot \Phi^{-1}(\omega)\right] & \text{if uninformed} \\ -\left[\alpha_0 \eta_{t+1} + \sqrt{\alpha_0^2 \sigma_\eta^2} \cdot \Phi^{-1}(\omega)\right] & \text{if informed} \end{cases}$$

This is the same expression as in the main text except for the precautionary savings/borrowing term, which is proportional to $\Phi^{-1}(\omega)$. When borrowing and saving have symmetric wedges the argument $\omega = 1/2$ and $\Phi^{-1}(\omega)$ vanishes. Then the farmer's borrowing is just his forecast error.

When $\omega \neq 1/2$ precautionary savings may be positive or negative. The level of borrowing has a normal distribution, though the mean is shifted by the level of precautionary saving. Define $\Gamma(z) = 1 - \frac{2}{\sqrt{\pi}} \int_0^z e^{-t^2} dt$. Then

$$\mathbb{E}[b_{t+1} \mid b_{t+1} < 0] = \begin{cases} -\frac{\alpha_1 e^{-\left[\Gamma^{-1}(2\omega)\right]^2} \left(2\sqrt{\pi}(1-\omega) e^{-\left[\Gamma^{-1}(2\omega)\right]^2} \left[\Gamma^{-1}(2\omega)\right] + 1\right) \sqrt{\sigma_\eta^2 + \sigma_u^2}}{(1-\omega)\sqrt{2\pi}} & \text{if uninformed} \\ -\frac{\alpha_1 e^{-\left[\Gamma^{-1}(2\omega)\right]^2} \left(2\sqrt{\pi}(1-\omega) e^{-\left[\Gamma^{-1}(2\omega)\right]^2} \left[\Gamma^{-1}(2\omega)\right] + 1\right) \sqrt{\sigma_\eta^2}}{(1-\omega)\sqrt{2\pi}} & \text{if informed} \end{cases} \tag{10}$$

Then

$$\frac{\mathbb{E}[b_{t+1} \mid b_{t+1} < 0, \text{informed}]}{\mathbb{E}[b_{t+1} \mid b_{t+1} < 0, \text{uninformed}]} = \sqrt{\frac{\alpha_0^2 \sigma_\eta^2}{\alpha_0^2 \sigma_\eta^2 + \alpha_1^2 \sigma_u^2}} < 1$$

meaning the informed borrow less than the uninformed even though the uninformed have a higher level of precautionary savings.

1.1.2 Risk-Averse Preferences

The farmer has two types of borrowing: planned borrowing, which she uses to smooth consumption, and unplanned borrowing, which she is forced into because she overestimates her income. A risk-neutral farmer gets no benefit from smoothing consumption. Since the interest on a loan exceeds her rate of patience, she will never willingly save or borrow.

A risk-averse farmer wants to smooth consumption and may not simply consume her current income. For the uninformed farmer the issue does not

arise; she believes

$$\mathbb{E}_t[\tilde{y}_{t+1}] = \mathbb{E}_t[\tilde{y}_{t+j}] = \alpha_0 X_t \qquad \text{for all } j \geq 1.$$

Since next year's income equals permanent income, there is no need to borrow to smooth consumption. But the informed farmer believes

$$\mathbb{E}_t[\tilde{y}_{t+j}] = \begin{cases} \alpha_0 X_t + \alpha_1 u_t & \text{for } j = 1. \\ \alpha_0 X_t & \text{for } j > 1. \end{cases}$$

meaning she assumes u_t is a one-time shock to her income. She would prefer to save (or borrow) it away. But given the wedge between the savings and borrowing rate, she will never smooth it away entirely because the gains from saving it are lower than her rate of discounting. Unlike a risk-neutral farmer, the risk-averse farmer may not simply consume her expected income, but her consumption will be closer to her expected current income than it would be if there were no extra cost to outside funds. Since her expectations are closer to actual income than those of the uninformed agent, she will on average have less unplanned borrowing and saving.

1.2 Details of the Calibration

This appendix shows each step of the calibration. Let $[Noise]_I$ and $[Noise]_U$ be the levels of noise I choose for the informed and uninformed (for example, the 25th and 75th percentiles of the distribution). Let $\Delta[School]$ be the first-stage coefficient I estimate and $\Delta[Noise]$ be the second-stage coefficient, which is either my main estimate $-.54$ or the conservative estimate $-.2$. Let $[School]_C$ be the average level of schooling and $[Noise]_C$ the average level of noise among those who turned 10 before the policy.

First I must find λ^+ and λ^-. From (1),

$$\frac{\Delta[Noise] * \Delta[School]}{\left(\sqrt{1+r^2} - 1\right) |\alpha_0 \sigma_\eta|} = \lambda^- - \lambda^+ \tag{11}$$

Table 11
Results of Calibration

Informed		Uninformed		Main Estimate		Conservative Estimate	
Percentile	Noise	Percentile	Noise	$\lambda(0)$	$\lambda(6)$	$\lambda(0)$	$\lambda(6)$
25	0.39	75	0.88	0	1	0	.92
10	0.23	90	1.27	0	1	.03	.82
5	0.15	95	1.57	0	.99	.1	.82

where

$$r^2 = \frac{\alpha_1^2 \sigma_u^2}{\alpha_0^2 \sigma_\eta^2} = \frac{[Noise]_U^2 - [Noise]_I^2}{[Noise]_I^2} \tag{12}$$

and $|\alpha_0 \sigma_\eta| = [Noise]_I$.

Next I calculate

$$\lambda^- = \frac{[Noise]_U - [Noise]_C}{[Noise]_U - [Noise]_I} \tag{13}$$

and $\lambda^+ = \lambda^- - (\lambda^- - \lambda^+)$.

Given that $\lambda(S) = \Phi[m + \gamma S]$ I have

$$\lambda^- = \lambda([School]_C)$$
$$\lambda^+ = \lambda([School]_C + \Delta[School])$$

which implies

$$\gamma = \frac{\Phi^{-1}(\lambda^+) - \Phi^{-1}(\lambda^-)}{\Delta[School]}$$
$$m = \Phi^{-1}(\lambda^-) - \gamma[School]_C$$

Table 11 gives the percentiles fed into the calibration and exact numbers for the end points.

B Empirical Appendix (For Online Publication Only)

Table 12
Household-Level Borrowing Regressions

	(1) Number of Loans b/se	(2) Average Debt b/se	(3) Average Interest Due b/se
Years of Primary	-0.758**	-0.459**	-0.443**
	(0.31)	(0.20)	(0.21)
Cohorts	70	70	70
Households	1319	1319	1319
Observations	1319	1319	1319

Note: I cluster all standard errors by cohort.

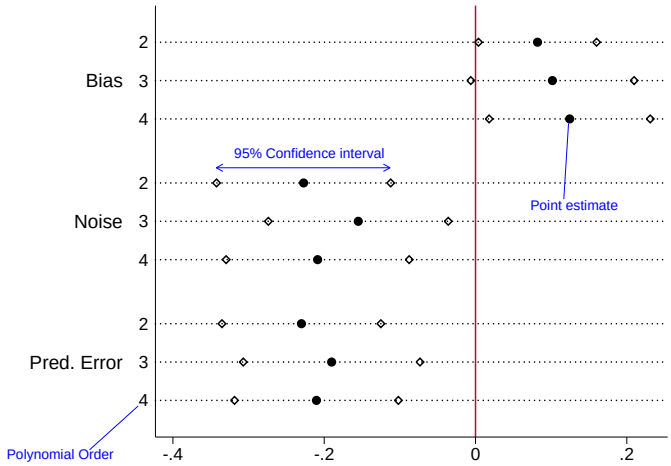
2.1 Additional Specifications

This appendix shows robustness checks referenced in the main text. Table 12 shows the two-stage least squares estimates of the effect of schooling on borrowing, where I keep only a single respondent per household. For each household, I keep the head if available and take the spouse if the head is not in the dataset. In all cases the estimates are statistically significant and within roughly one standard error of the coefficients in Table 8.

Figure 8 checks that the results are not driven by my choice of the polynomial control function. Using higher-order polynomials in the schooling regression inflates the standard errors and lowers the F-statistic, making any second-stage coefficients hard to interpret. Instead I look at the robustness of the reduced-form coefficient—the change in each measure of rationality at the discontinuity. For each measure, Figure 8 reports the point estimate and 95 percent confidence interval of the reduced-form regression in which the order of the polynomial in cohort has degree 2, 3, and 4. In all specifications, the reduction in noise and prediction error is signficant and large.

Figure 9 checks that the results do not change when I restrict the regressions to cohorts within some radius of the discontinuity. In the main regressions I use the entire sample for the sake of power and to avoid the problems that comes with drawing inference from too few clusters. But Figure 8 shows that the point estimates for the effect on the noise and prediction error do not change much for windows restricted to cohorts less than 40, 20, and 5 years from the discon-

Figure 8
Robustness: Changing the Order of the Polynomial



Note: Each dot plot gives the reduced form point estimate and 95 percent confidence interval for a regression that uses a different order for the polynomial control function. All regressions control for the mean and standard deviation of household income, and all standard errors are clustered by cohort.

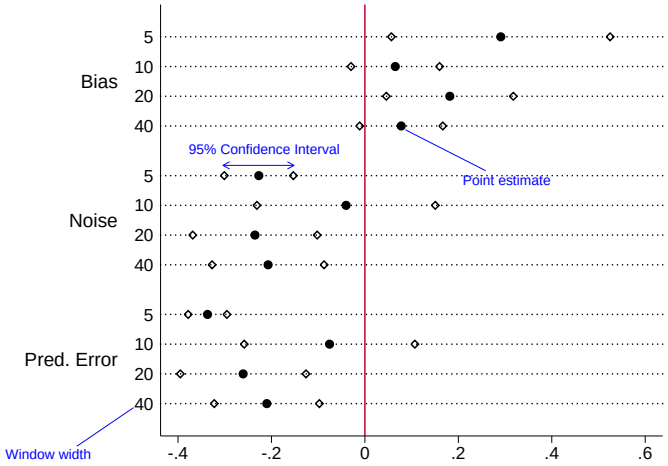
tinuity. The coefficients shrink to insignificance for a window of 10, but it is not surprising that the estimates bounce around as the number of cohorts shrinks. The estimates of the effect on the bias are less consistent, reinforcing the need to interpret them with caution.

Tables 13 and 14 redo the first- and second-stage imposing a "stable age condition." This condition is that the max absolute deviation across rounds of the survey in the computed year of birth and its mode must be no greater than 5 years. The effect of schooling on bias is no longer significant, which reinforces the statement in the main text that this effect is not robust. The effect on noise and prediction error remain within 2 standard errors of those reported in the main text.

2.2 Additional Graphs

This appendix discusses the shape of the polynomial in more detail. The most unusual feature is the hump in noise among cohorts a few years above the

Figure 9
Robustness: Changing the Window



Note: Each dot plot gives the reduced form point estimate and 95 percent confidence interval for a regression that restricts the sample to cohorts within the given radius of the discontinuity. All regressions control for the mean and standard deviation of household income, and all standard errors are clustered by cohort.

Table 13
Robustness: First Stage with Stable Age Condition

	(1) Years of Primary b/se	(2) Total Schooling b/se	(3) Finished 6 Years Ed. b/se	(4) New System b/se	(5) Years of Primary b/se	(6) Total Schooling b/se	(7) Finished 6 Years Ed. b/se	(8) New System b/se
Age 10- in 1978	0.511***	0.584***	0.193**	0.395***	0.532***	0.632***	0.198***	0.399***
	(0.16)	(0.21)	(0.07)	(0.07)	(0.15)	(0.21)	(0.07)	(0.07)
SD[Income]					-0.000***	-0.000***	-0.000***	-0.000***
					(0.00)	(0.00)	(0.00)	(0.00)
E[Income]					0.000***	0.000***	0.000***	0.000***
					(0.00)	(0.00)	(0.00)	(0.00)
Cohorts	64	64	64	64	64	64	64	64
Respondents	1600	1600	1600	1600	1600	1600	1600	1600
F-Stat	10.766	7.551	6.948	36.553	12.051	9.431	7.857	30.814

Note: All regressions use second-order polynomial in cohort as the control function. I allow the coefficients of the polynomial to change at the discontinuity. I cluster all standard errors by cohort. The "F-Stat" is from the test for whether the coefficient "Age 10 or younger in 1978" is different from zero—that is, whether the excluded instrument for the second stage is signficant.

Table 14
Robustness: Second Stage with Stable Age Condition

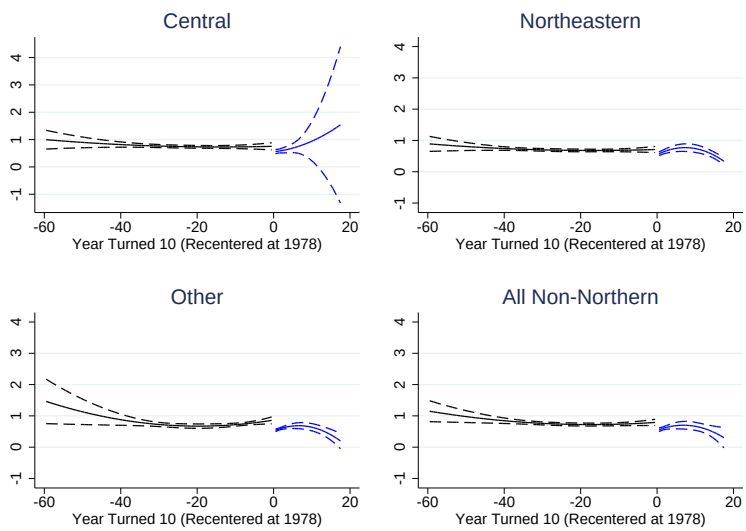
	(1) Bias b/se	(2) Noise b/se	(3) Pred. Error b/se	(4) Bias b/se	(5) Noise b/se	(6) Pred. Error b/se
Years Prim.	0.110	-0.341**	-0.364**	0.101	-0.311**	-0.333**
	(0.08)	(0.15)	(0.15)	(0.08)	(0.16)	(0.14)
SDev(Income)				-0.000	0.000***	0.000***
				(0.00)	(0.00)	(0.00)
Avg(income)				0.000	-0.000	-0.000
				(0.00)	(0.00)	(0.00)
Control Mean	-0.15	0.72	0.72	-0.15	0.72	0.72
Cohorts	64	64	64	64	64	64
Respondents	1600	1600	1600	1600	1600	1600

Note: Columns 1-3 use Column 1 of Table 5 as the first-stage. Columns 4-6 use Column 5 of Table 5 as the first-stage. I cluster all standard errors by cohort. The "control mean" is the mean value of the dependent variable taken over all respondents in cohorts that turned 10 before 1978.

discontinuity—that is, cohorts born between 1970 and 1975. These cohorts are unusual in that they were born during the most violent period of a Communist insurgency in the countryside. During those years Thai troops garrisoned villages, often raping and looting the locals (Baker and Phongpaichit, 2009, p. 183-184). Children born during those years or still in the womb might have been affected.

To test this I break out the regressions by provinces. The insurgency was concentrated in the northeast provinces. The central provinces should have suffered little damage, and the other provinces somewhat less than the northeast. This is exactly what Figure 10 shows. There is no evidence of a hump in the central provinces, and only a mild one in the "other" provinces. By contrast, there is a large hump in the northeastern provinces. But in all cases, there is a clear decrease in noise at the discontinuity. The decrease has similar magnitude across all provinces. This suggests the discontinuity is crucial to the identification.

Figure 10
Noise at the Discontinuity, By Province



Note: Regressions control for average income and the standard deviation of income.

2.3 Expectations and Income

Suppose for simplicity that the farmer chooses a level of investment I and earns an uncertain per-unit return X. His subjective distribution for X is normal with mean $\bar{X}$ and noise N. To make my point clear I assume he has constant absolute risk-aversion. He solves

$$\max \mathbb{E}[-\exp(-\gamma[XI - I])]$$

Since X is normal, $\exp(-\gamma(X-1)I)$ is lognormal and thus

$$\mathbb{E}[-\exp(-\gamma(X-1)I)] = -\exp(-\gamma(\bar{X}-1)I + \frac{\gamma^2}{2}N^2I^2)$$

The first-order condition implies

$$I = \frac{\bar{X}-1}{\gamma N^2} \tag{14}$$

In other words, a farmer with less noisy expectations invests more. Since he

is more certain about X, the gamble seems less risky and he is more willing to take it.

Why, then, do I find that households with more schooling, and thus less noisy expectations, have the same average income as the less well-schooled?

One possibility is that the investment happens at a time when everyone is equally well-informed. For example, if all farmers choose I before learning the latest rice price (and thus before answering the survey) then schooling will not matter for expectations. This possibility is unlikely, as the survey is taken just before planting. More likely is that I is chosen later on when even the less educated have learned the price. In particular, if X is actually the wage paid to farm workers and I the hours they work then at the time they choose to work the noise of their expectations will be zero. (Note the argument about borrowing still holds as long as they choose consumption before they start working.)

Another possibility is that the less informed do not realize their predictions are noisier. If everyone believes the noise of their prediction is equally low, then the uninformed will on average keep up with the informed as long as their predictions are unbiased. This "fools rush in" explanation suggests schooling not only improves our abilities but helps us grasp their limits.

A third possibility is that households are constrained. I have already argued they are constrained in their choice of jobs—two extra years of schooling does not get them salaried jobs. But they may also be constrained in their investment. If

$$I \leq \bar{I} \leq \frac{\bar{X} - 1}{\gamma(N^U)^2} < \frac{\bar{X} - 1}{\gamma(N^I)^2}$$

where superscript U and I stand for uninformed and informed, then everyone will choose the same level of investment. In Shenoy (2014) I reject tests that markets for land, labor, and capital are perfect. These imperfections cause misallocation (though not much), and a credit intervention reduces the amount of misallocation.

One last possibility is that schooling changes preferences. For example, if the better schooled are more risk-averse (they have a higher γ) then greater certainty may be balanced by lower tolerance for any uncertainty. Unlike the other explanations, this one suggests a positive test, namely checking whether risk

aversion changes at the discontinuity. I use a question that asks whether the respondent would be willing to take hypothetical jobs that yield riskier but on average higher returns. I define two measures of risk-aversion: an indicator for whether the respondent chose the most risk-averse options, and a rough calculation of their coefficient of relative risk-aversion.[15] The respondent answers the question every year after 2003, so I take the average.

I also run three negative tests. Differentiate (14) with respect to expected returns:

$$\frac{\partial I}{\partial \bar{X}} = \frac{1}{\gamma N^2}$$

With no frictions a farmer with less noisy expectations should make a bigger response to a change in expected returns. But if any of the four explanations given above holds true, the difference in responses will be small or even zero.

For each farm input examined in Figure 3, I compute the correlation between the respondent's forecast of next year's income and the amount actually used next year. The correlation is undefined for many households because they either spend no money on fertilizer or seeds, have no land, or (in the case of land) never change the amount of land they farm. Likewise, I cannot compute a coefficient of relative risk aversion for some respondents because they dropped out of the survey before it started asking about risk-aversion. Nevertheless, Table 15 shows that, as all four explanations predict, there is no change in how strongly households respond to expectations at the discontinuity. Consistent with the last explanation, I find a rise in risk-aversion at the discontinuity.

C Data Appendix (For Online Publication Only)

I construct each variable from the Townsend Thai survey as follows:

- **Income:** I start with question net profit, which the enumerator calculates in the Income section. The enumerator records how much revenue the household takes from each source, the farm and business expenses used

[15]The survey asks two questions. Based on the responses I can compute a range on the coefficient of relative risk-aversion. For each respondent I take the lower bound as the coefficient, assuming 0 to be the bound on the lowest option.

Table 15
Risk-Aversion and Reactions to Forecasts

	(1) Fertilizer b/se	(2) Seeds b/se	(3) Land b/se	(4) Risk Averse? b/se	(5) CRRA Coef. b/se
Age 10- in 1978	0.068	-0.020	0.050	0.068*	0.134**
	(0.06)	(0.06)	(0.07)	(0.04)	(0.07)
Cohorts	65	65	65	70	70
Respondents	1402	1402	1403	1702	1702

Note: I cluster all standard errors by cohort.

to generate that revenue, and computes the difference. The respondent is then asked if the number seems accurate, and if not the respondent and enumerator work to revise the estimate. I use the revised number if one is given, and if not the original number. I deflate the value to 2005 baht using the Consumer Price Index. I discard a single household-year observation in which income is reported to be over 100 million baht, a likely coding error. I take the mean and standard deviation of a household's income for the all years that I observe it.

- **Expected income:** I start with a question in the Income section that asks "What is your best guess about what the households net profit will be next year?" I deflate the value to 2005 baht assuming the household reported a value using current prices.

- **Loans:** From the borrowing section of the survey I take the household's response to whether it owes money or goods to anyone; the indicator for "yes" is the variable I call "Any Loans." If the household does owe loans it is asked how many, which I take as my variable "Number of Loans."

- **Debt:** For each loan the household is asked both how much was borrowed and how much must be repaid. I define debt as the total amount to be repaid for all loans. I define (expected) interest as the difference between amount to be repaid and amount borrowed.

- **Farm Inputs:** The household is asked about how much it spent on fertilizer and seeds conditional on spending any. I assume the amount is

zero if the household used land for agriculture but did not report positive spending. I scale expenditure by the total area of farmland (in rai). For regressions using the regression discontinuity I average the expenditure per rai over all years I observe the household.

- **Risk Preferences:** In 2003 the survey started posing to households a hypothetical choice between staying at their current income forever and taking a job that with 50-50 chance pays either double or two-thirds their current income. If they choose their current job the interviewer gives them the same choice except the alternate job now has a 50-50 chance of paying either double or 80 percent of their current income. If the respondent chose his current job for both questions I marked it as "Risk Averse" (in fact, this measures being highly risk averse.) The questions let me compute bounds on the coefficient of relative risk aversion. I take the lower bound as a rough measure of the true coefficient. For each respondent I average both the indicator for "Risk Averse" and the coefficient of relative risk aversion across all rounds I observe them.

- **Individual variables:** I link the respondent ID from the Household ID section to the household roster from the Household Composition section. Since the respondent ID only reports the relation between the respondent and the head of household I can only link the head and the head's spouse.

  - **Year of birth:** Each respondent reports his or her age in each round of the survey. I use the age and the date the survey was collected to calculate the year of birth. This year of birth sometimes changes for a given person because there are errors in the recorded age. I take the earliest report available for a respondent as the true age.[16]

  - **Schooling:** Each respondent reports his or her highest grade completed in each round of the survey. I compute the total years of schooling and the number of years of primary schooling under the old definition (first seven years) to keep the variable consistent. I also create an indicator for whether the respondent had at least six years of school. Finally, the respondent was asked under which system he or

[16]The main results remain unchanged if instead I use the modal year of birth.

she was schooled. I make an indicator for whether they say they were schooled under the new system. If a respondent reported different levels of education in different rounds I took the max (in case the respondent is still in school).[17]

[17]The main results remain unchanged if instead I use the modal years of schooling.