Crime, health and wellbeing – Longitudinal evidence from Mexico

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Abstract:

This paper uses variation in victimization probabilities between individuals living in the same community to shed new light on the costs of crime. I use panel data from the Mexican Family Life Survey for 2002 and 2005 and look at the impact of within-community differences in victimization risk on changes in self-rated and mental health. My results from fixed effects and instrumental variable estimations point towards substantial negative health effects of actual victimization, which might help to explain the existence of compensating differentials in wages or house prices found in earlier studies.

Keywords: cost of crime; victimization; health

JEL Classification: H40, I10, I12, K00, K42, R23

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All analyses used Stata 11.2. Do-Files are available from the author on request. All analyses and opinions expressed in this paper as well as any possible errors are under the sole responsibility of the author.

1. Introduction

The medical literature has long identified a negative correlation between crime and victimization risk on the one side and measures of health and mental well-being on the other side (e.g., Miller et al., 1993; Chandola, 2001; Stafford et al., 2007; Jackson and Stafford, 2009). In economics, numerous papers have been concerned with estimating the cost of crime for society. Examples include willingness to pay studies for the avoidance of victimization (Ludwig and Cook, 2001; Cohen et al., 2004) or the calculation of compensating differentials for regional crime rates in either wages or house prices as predicted by models by Roback (1982, 1988), who also provides some evidence. Other examples in the latter group include, inter alia, Gerking and Neirick (1983), Blomquist et al. (1988), Smith (2005), Schmidt and Courant (2006) and Braakmann (2009) for wages and Bowes and Ihlanfeldt (2001), Lynch and Rasmussen (2001) and Gibbons (2004) for house prices.

The central assumption underlying the papers looking at compensating differentials is that crime or victimization risks lower the utility of affected individuals, thus leading to the need of monetary compensation. While this assumption has intuitive appeal – after all, it is easy to imagine that most people do not like being mugged – there have been only a few studies that look at where and why these utility losses actually occur. Two papers look at behavioral changes caused by crime and fear of crime: Hamermesh (1999) finds that victimization risk changes working time patterns in the US. Using the same dataset and econometric approach as this paper, Braakmann (2012) looks at measures individuals take to protect themselves, such as stopping to go out, changing routes or modes of transportation or starting to carrying a weapon, and their property, such as barring windows or getting an electronic alarm system. The logic underlying these papers is that crime might lead individuals away from their preferred choices, such as going out at a certain frequency or working at certain times, which leads to utility losses and then ultimately to a compensating differential.

Another possible reason for why we might observe compensating wage differentials for crime is health. Intuitively, a link between health and crime (or victimization) is rather plausible: Some crimes, such as bodily harm, manslaughter or murder, are essentially defined by some loss of (physical) health on the side of the victim. In terms of mental health, it is easy to imagine that victimization can lead to traumata or that states of prolonged fear (of crime) might affect mental health or well-being. Finally, victimization risks might also lead to stress, which might than affect the risk of illnesses through its impact on the body's immune system. As long as individuals loose utility through worsening states of health, this link might also help to explain the existence of compensating wage differentials for crime. A contribution from economists in spirit of the previously mentioned medical papers is Powdthavee (2005) who uses South African data and finds that victimization lowers the victim's life satisfaction, which can be seen as a measure of mental health.

In this paper, I use household data from Mexico, specifically the 2002 and 2005 waves of the Mexican Family Life Survey, a panel survey of 40,000 individuals from roughly 8,400 households throughout Mexico. One particular feature of this survey is that it contains individual measures of crime and victimization risk. Furthermore it also contains a large number of health outcomes, which allows for a thorough investigation of the link between victimization and health. In terms of victimization risks, I look (a) actual past victimization and (b) subjective assessments of future victimization. Specifically, I use information on whether an individual has ever been assaulted or robbed in its life and on a subjective assessment of the probability of falling victim to a crime within the next year. Looking at both of these measures seems sensible as that there is a large literature examining the consequences of objective risk and subjective assessments of that risk being different (see, e.g., Sloan and Pratt, 2011, for a recent study). Furthermore, the correlation between actual victimization and perceived victimization risk is only 0.17, which highlights that fear of crime affects a greater number of people than actual victimization.

I start by estimating individual fixed effects regressions while also controlling for community-time-effects, in other words by looking at the effects of changes in individual victimization rates or probabilities relative to changes in these characteristics in the respective community. Using individual victimization data has a number of advantages over the classical approach of using regional crime rates as a proxy for victimization risk (see Braakmann, 2012). First, individual victimization risks, even for individuals living in the same region, are likely to differ from and be more heterogeneous than regional crime rates, which can be seen as the average victimization risk in a region. An obvious example is the risk of being raped, which will likely differ at least by gender. Similarly, the risk of being assaulted in the street might well differ by appearance, behavior, age and the likely ability to fend off an attacker.

Second, using differences in victimization risks between individuals living in the same region allows for a flexible control of regional factors influencing both the respective outcome and crime rates by regional fixed effects or regional-time fixed effects in the case of longitudinal data as used in this paper. Unobserved regional factors, such as negative economic shocks, influencing both the outcome, such as wages or rents, and crime rates are usually a major concern in studies of the economic consequences of crime as the canonical economic model of crime (Becker, 1968) predicts a negative relationship between economic opportunities in legal employment and an individual's propensity to engage in crime (see, for example, Gould et al., 2002, for recent evidence and Piehl, 1998, and Freeman, 1999, for surveys). Exploiting within-region differences in victimization risks combined with regional-time fixed effects flexibly controls for these factors without the need for extensive regional controls as in Bowes and Ihlanfeldt (2001) or Braakmann (2009), natural experiments as in Smith (2005) or instrumental variables as in Gibbons (2004).

The flipside of these advantages, however, is a higher risk of simultaneity bias. While regional crime rates are generally more or less uninfluenced by the behavior of every single person in the data and regional shocks are the main worry, individual crime risks may well be influenced by changes in individual health or well-being. Consider for example the case where the fixed effects estimates indicate a non-significant effect of victimization on some health outcome. A possible explanation could be that crime simply does not influence health. However, an equally valid explanation would be that individuals whose health worsened are less likely to go out in the evenings, say to nightclubs, which might reduce their victimization risks.

To address these concerns I rely on the same instrumental variable strategy I used in Braakmann (2012): Individual victimization risks are instrumented by the share of individuals in the respective community – excluding the respective individual – who have been victimized or consider victimization to be likely. The logic underlying these instruments is rather simple: Individuals living in the same community face the same regional developments in crime rates, which implies that individual victimization risks should be correlated with regional aggregations of these risks. The regional aggregates, however, should be uncorrelated with individual changes in behavior as long as the regional characteristics are calculated without the individual under question. The crucial assumption here is that regional (average) victimization risks have no direct influence on an individual's health or mental state once individual risks are controlled for, which seems plausible. Note that this strategy only works due to the availability of measures of both individual and aggregate victimization risks. Without the individuals measures one would be in the typical situation where regional measures of victimization are used as a proxy for individual risk. These typical proxy-regressions can be seen as the reduced form of my estimates.

An important question is whether subjective beliefs about victimization risks may differ from some "true", objective risk of victimization, i.e., whether individuals are actually able to judge their victimization risks accurately. Here, it is important to note two things: First, while individuals might indeed misjudge their victimization risk, they will also not know their "true", objective victimization risk and consequently have no choice but rely on their subjective judgment. Second, given the usual level of underreporting of criminal activities and the risk that the resulting measurement error varies regionally, it is not clear whether the alternative of using reported crime rates is any better than using subjective beliefs. In fact, Hamermesh (1999, p. 315) points out that (subjective) fear of crime is more relevant to individual behavior than (objective) regional crime rates and should consequently be preferred.

My estimates indicate that, first, actual victimization appears to have large, statistically significant and negative effects on the victims' health, be it self-rated, actual medical conditions or indicators of mental health. Second, the subjective likelihood of victimization has a much smaller and often insignificant effect on health. In other words, the negative health effects of crime and victimization seem to be confined to the actual victims. Third, standard fixed effects estimates seem to suffer from considerable bias towards zero, as can be seen from the fact that the IV estimates generally tend to be much larger. Note that these results can be explained by good health having a positive effect on crime risks (for example, as sick people are more likely to stay at home where victimization risks tend to be relatively low), while at the same time crime (risks) having a negative effect on health.

The rest of the paper is organized as follows: Section 2 describes the data used.

Section 3 lays describes the estimation approach. Results can be found in section 4. Section 5 concludes.

2. Data

I use data from the 2002 and 2005 waves of the Mexican Family Life Survey.[1] The survey was conducted by researchers of the University Iberoamericana (UIA), the Center of Economic Research and Teaching (CIDE: *Centro de Investigación y Docencia Económicas*), the National Institute of Public Health (INSP: *Instituto Nacional de Salud Pública*), and the University of California, Los Angeles (UCLA). The data cover approximately 40,000 individuals from roughly 8,400 households throughout Mexico in each wave. Crucially it also contains more than one household per community and year, which enables me to use community-time fixed effects. Communities range from small villages to cities and cover both urban and rural areas.

In this paper I focus on adult respondents – defined as being 14 years of age or older – as several key variables, including victimization risks, are missing for children. The final sample used in the estimations consists of 11,736 observations for 5,883 men and 16,756 observations for 8,393 women from 150 communities. Communities contain between 19 and 508 individuals with an average of 95. Major reductions in the sample size from the original roughly 40,000 individuals occur due to the restriction to adult individuals and the requirement of individuals being observed in both waves.

Victimization risks are measured by two dummy variables as in Braakmann (2012). The first indicates whether an individual considers it likely or very likely to be robbed or

[1] Data and documentation are available at http://www.ennvih-mxfls.org/.

assaulted within the next year. In other words, it captures an individual's expectation regarding its victimization risk. The second measure is a dummy variable indicating whether an individual has ever been assaulted, robbed or attacked in the past. This second variable is similar to the one used by Powdthavee (2005) in his study of the life-satisfaction effects of victimization in South Africa. 9% of the individuals in the sample have been victimized with 2.3% experiencing more than one assault (up to a maximum of 10).

The health variables can be split into two groups. The first are measures of self-rated health. These include a dummy for having bad self-rated health, a dummy for having worse health than a year ago, a dummy for expecting health to be worse next week and a dummy for stating that ones health is worse than that of people of comparable age and gender. The second groups of variables relates to mental health. It consists of a set of dummies for having suffered from stress, having experienced sleeping problems, frequent feelings of fear, frequent feelings of pessimism and frequent wishes to die during the last 4 weeks prior to the interview and, finally, the individual's usual hours of sleep per night.

From the data I also take a number of standard socio-economic controls on age and education. I do not control for changes in labor force status and income as these might themselves be influenced by health consequences of to victimization and crime risks, if, e.g., someone has to give up work due to injuries received when assaulted (see Angrist and Pischke, 2009, ch. 3.2.3 for a textbook treatment on bad controls in causal inference).

(TABLE 1 ABOUT HERE.)

Table 1 contains descriptive statistics for all variables. Note that there is a considerable number of individuals who consider it likely to be victimized or have been victimized in the past: Around 21% of all individuals in the sample consider it likely to become victim of a crime within the next year and between 7% and 12% have become a victim in the past. The fact that more people consider victimization likely than actually experience it is not unusual and often found in the literature (e.g., Dominitz and Manski, 1997).

3. Estimation strategy

Following Braakmann (2012), I estimate regressions of the form

<u>FE I:</u>

$$y_{ict} = X_{it}'\beta + \tau^{*}v_{ict} + \eta_{ct} + \alpha_i + \varepsilon_{ict}, \qquad (1)$$

where y_{ict} is the respective outcome for individual *i* in community *c* observed in year *t*, v_{ict} are the measures of victimization risk for that individual, X_{it} contains the socio-demographic characteristics described above, α_i is an individual fixed effect, η_{ct} is a community-year fixed effect and ε_{ic} is a standard error term. Standard errors are adjusted for clustering on both the individual and the community level. The coefficients of interest are in τ, which measure the impact of victimization (risk) on the respective health outcome. All regressions are run separately for men and women. Note that the fact that y_{ict} might contain dummies is relatively innocuous as all variables and in particular the measures of victimization risk are essentially dummies, which prevents the usual issues with using a linear probability model on discrete outcomes (see Angrist, 2001, in the context of IV estimation).

The individual fixed effects capture any baseline differences between individuals such as lifestyles or general physical appearance. Their presence implies that the effects of victimization are identified through changes in individual victimization risks. The community-year effects capture all changes that occur on the regional level, including overall changes in crime rates as well as changes in the economic situation. Their inclusion also means that all effects are identified using within-community-within-year differences in victimization risk. In an alternative specification I replace the community-year effects η_{ct} with separate fixed effects for communities (μ_c) and (ϕ_t) years and add some time-varying community characteristics W_{ct}, specifically the local population, a dummy that is "1" if income opportunities have improved during the last year and three dummies indicating whether prices for corn, health care and other foods increased during the last year. This results in the specification FE II:

<u>FE II:</u>

$$y_{ict} = X_{it}'\beta + W_{ct}'\delta + \tau^* v_{ict} + \mu_c + \phi_t + \alpha_i + \varepsilon_{ict}, \quad (2)$$

As in Braakmann (2012) the results from these two specifications usually do not differ in any meaningful way.

The estimates based on equation (1) or (2) may still suffer from reverse causality or omitted variable bias through the omission of time-varying variables. Consider first the case of reverse causality: A decline in health might force an individual to stay at home more often (as opposed to, say, going out clubbing in the evening), which in turn might very well reduce its risk of falling victim to a crime. A similar case can be made for time-varying omitted variables. Say, an individual decides to adopt a healthier lifestyle, where lifestyle is unobserved. This might induce all sorts of behavioral changes, for instance, again staying at home more often instead of going out drinking, which might affect both victimization risks and health. Note that while the direction of the bias in the preceding two examples was rather clear, it would be equally possible to find examples that would lead to biases in other directions.

A further problem could be measurement error: If individuals are not very good at judging their victimization risk in a consistent way over the years, a good part of the within-individual variation in victimization risk could be noise. While this is probably less of a problem for actually experienced past victimization – which does not occur that regularly in the life of each individual and should be a rather memorable event – it might very well be a bigger problem when it comes to the subjective victimization probability. Note that the resulting bias in this case would be towards zero.

To attenuate these concerns I rely on the same instrumental variable strategy I used elsewhere (Braakmann, 2012). In a first step, I calculate for each individual the averages of the victimization measures using all other individuals living in the same community, denoted by $\bar{v}_{-ict}$. Effectively, these averages are simple the shares of individuals in the respective community, excluding the respective individual, who consider it likely to be victimized or have been victimized in the past. It is important to stress that there is enough variation in these measures within communities to make this approach sensible: The within-community standard deviation (over time) of the share of individuals who consider victimization to be likely is 0.066 (mean 0.197), which is more than half the between-community standard deviation of 0.119, while the corresponding value for actual victimization is 0.031 for the within-community standard deviation (mean 0.085) and 0.096 for the between-community standard deviation.

In a second step, I use these averages as instruments for the individual measures of victimization risks, leading to a first stage

$$v_{ict} = X_{it}'\pi + W_{ct}'\xi + \rho^* \bar{v}_{-ict} + \mu_c + \phi_t + \alpha_i + \upsilon_{ict}. \qquad (3)$$

As already stated in the introduction, the logic underlying these instruments is fairly simple: Individuals living in the same community face the same changes in regional factors that might influence victimization risks, like changes in the presence of gangs or the police or economic downturns. Consequently, we would expect changes in aggregate measures of victimization to be correlated with changes in the corresponding individual measures. As the regional measures are calculated without the respective individual, there is no possibility of reverse causality. Similarly, any changes in individual life-styles or other variables that could lead to omitted variable bias should not influence the aggregate victimization measures. It is important to stress the difference between this estimation strategy and the use of regional crime rates as proxies for individual risk. While both my IV approach and the proxy approach use regional variation in crime risks, the IV approach still distinguishes between individuals whose *individual* risk changes as a result of the changes in regional risk and those whose risk does not change, whereas the proxy approach effectively assumes that all individuals in the same region face the same change in risk.

As the instruments effectively vary only on the community-year-level, it is necessary to replace the community-year fixed effects with separate community and year fixed effects as well as some time-varying controls on the community level. This change might lead to the familiar concern with unobserved regionals shocks that also arises when using regional crime rates as a proxy for individual victimization risk. One way to test for potential biases arising from this change is to compare standard fixed effects estimates based on equations (1) and (2). As already mentioned earlier, results from these two specifications are almost identical.

Despite this result, it is important to be aware that estimates based on FE I are conceptually somewhat different from those based on FE II and the IV estimates. The former effectively looks at how individuals behave whose victimization risk relative to their community in a given year changes, that is, all victimization risks in that specification are relative risks. FE II and the IV estimates also use variation in victimization risk that arise through changes in victimization risk within communities over time. The IV and FE 2 estimates are then again somewhat different as the former identify local average treatment effects, that is, effects for those individuals whose victimization risk changes because the average risk in the community changes, whereas the latter also contain changes in victimization risk due to changes in personal circumstances.

(TABLE 2 ABOUT HERE.)

Table 2 presents first stage results on the relationship of aggregate and individual measures of victimization. As we can see the results show that the instruments are correlated fairly strongly with the individual measures of victimization risk. All first stage F-values furthermore indicate the absence of any weak instrument problem.

4. RESULTS

Table 3 presents the first set of results related to self-rated health. Note first that both fixed effects specifications, FE I and FE II, are always very similar, which suggests that the included regional control variables capture all important regional time-varying confounders. In general, the results indicate no or almost no relationship between the subjective probability of victimization and self-rated health with the exception of the expectation of worsening health for women. Actual victimization on the other hand is often associated with a significant worsening of the respective health measures: Individuals who have been victimized are more likely to report a decrease in health relative to one year ago. They are also more likely to expect a further worsening of their health and – at least when they are women – are more likely to report having bad health relative to people of the same age and gender. It should be stressed that the IV estimates are generally (much) larger than the corresponding FE estimates, indicating that the already mentioned biases are relevant and on balance negative. The fact that the IV estimates are more likely to be insignificant can easily be explained by their well-known lower statistical efficiency. A note of caution, however, is probably in order when it comes to the female results for the decrease in health relative to one year ago. Here, the IV estimates appear to be unrealistically large indicating an approximately 106% increase in the likelihood to report decreased health.

(TABLE 3 ABOUT HERE.)

Finally, table 4 presents results regarding mental health. The main result emerging from the table is that crime victims are much more likely to state that they are suffering from sleeping problems and also sleep between 2 and 3 hours less per night than individuals who have not been victimized. For the remaining outcomes both the FE and the IV estimates are generally insignificant and also not particularly large in magnitude when looking at men. For women, the picture is somewhat different: Some FE estimates indicate that victimization might increase the risk of feeling pessimistic and experiencing death wishes. The corresponding IV estimates tend to be insignificant, but are usually not small, which means that imprecise estimation might rather than true non-effects might be responsible for this result. Finally, there are also some hints that the subjective likelihood of victimization may lead to frequent feelings of fear or pessimism and to sleeping problems for women. For other outcomes and men, the subjective risk of victimization does not seem to have any effect, which is similar to the results for the other outcome groups.

(TABLE 4 ABOUT HERE.)

To summarize the main results: First, actual victimization appears to have large, statistically significant and negative effects on the victims' health, be it self-rated or indicators of mental health. These results suggest that some of the compensating differentials related to crime that were found in earlier studies might arise because of negative health effects. Second, the subjective likelihood of victimization has a much smaller and often insignificant effect on health. In other words, the negative health effects of crime and victimization seem to be confined to the actual victims. Third, standard fixed effects estimates seem to suffer from considerable negative bias, as can be seen from the fact that the IV estimates generally tend to be much larger.

5. Conclusion

This paper provided evidence on some of the non-monetary costs of crime using data from Mexico, a country with a relatively severe crime problem. I exploited within-community differences in changes in individual victimization risks and used a combination of fixed effects and instrumental variable estimation. I also considered the effects of both subjective believes about victimization risks and past victimization to shed light on the question whether the costs of crime are borne only by the actual victims or crime affects other individual in the same community.

The results indicate substantial negative health effects of actual victimization, which might help to explain both the large willingness to pay for crime reduction and the existence of compensating differentials in wages or house prices found in earlier studies. The results also indicate that these negative effects are found only for actual crime victims, but not for those just expecting to be victimized. This latter result is different from studies looking at behavioral changes such as Hamermesh (1999) and Braakmann (2012) that generally find effects when looking at measures such as fear of crime. On a political level, the results also suggests that a successful battle against crime might also be good for public health.

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Table 1:

Descriptive statistics

	Men		Women	
	Mean	Std. dev.	Mean	Std. dev.
Considers victimization in next year likely	0.210	0.407	0.218	0.413
Ever victimized	0.124	0.330	0.069	0.253
Share of individuals in community who consider victimization to be likely	0.211	0.134	0.214	0.134
Share of victimized people in community	0.089	0.094	0.092	0.096
Has bad self-rated health	0.041	0.198	0.058	0.233
Health has decreased compared with 1 year ago	0.090	0.286	0.131	0.338
Expects health to be worse next week	0.063	0.242	0.070	0.255
Bad health relative to people of same age and sex	0.054	0.225	0.085	0.279
Suffered from stress in the last 4 weeks	0.000	0.021	0.001	0.030
Hours of sleep per day	7.612	1.323	7.813	1.304
Had problems sleeping in last 4 weeks	0.035	0.183	0.057	0.231
Felt frequently pessimistic in last 4 weeks	0.025	0.155	0.047	0.212
Felt fear frequently in last 4 weeks	0.018	0.133	0.032	0.177
Frequently wished to die during last 4 weeks	0.011	0.104	0.020	0.141
Age (years)	41.044	17.556	39.546	16.221
Elementary schooling	0.406	0.491	0.434	0.496
Completed Jr. high school	0.248	0.432	0.252	0.434
Completed high school	0.140	0.347	0.122	0.327
College graduate	0.103	0.304	0.073	0.260
Community variables (based on interview with community official)				
Local income opportunities improved during the last year	0.185	0.388	0.188	0.391
Local population	223,406	429,524	225,909	432,545
Price of health care increased	0.588	0.492	0.576	0.494
Price of corn increased	0.383	0.486	0.378	0.485
Price of other food increased	0.457	0.498	0.448	0.497
Observations	11,736		16,756	

All variables based on individual survey responses except for community variables at the bottom of the table, which are based on an interview with a community official.

Table 2:

First stage results

	Men		Women	
Outcome	Considers victimization likely (1 = yes)	Ever victimized (1 = yes)	Considers victimization likely (1 = yes)	Ever victimized (1 = yes)
Share of individuals in community who consider victimization to be likely (0 to 1)	0.7191***	-0.0270	0.7732***	0.0746***
	(0.0764)	(0.0556)	(0.0724)	(0.0231)
Share of victimized people in community (0 to 1)	0.1804	0.9698***	-0.0145	0.3711***
	(0.1151)	(0.1100)	(0.1334)	(0.0619)
Individual fixed effects	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Regional controls	Yes	Yes	Yes	Yes
F-value excluded instruments	47.10	49.40	86.81	25.51
F-value for Angrist-Pischke test of excluded instruments	90.34	68.84	73.02	36.25
Kleibergen-Paap Wald rk F statistic	36.78		20.82	
Observations	11,736		16,756	

Coefficients, standard errors adjusted for clustering on the individual and the community level in parentheses. */**/*** denote statistical significance on the 10%, 5% and 1% level respectively. Control variables in all specifications are age and age squared and dummies for having completed elementary school, Jr. High School, High School and College with no schooling being the base alternative. Regional controls are the local population, a dummy indicating whether income opportunities have improved compared to last year, and dummies indicating whether the prices of corn, other food and health care have risen respectively.

Table 3:

Victimization risk and self-rated health

	Men			Women		
	FE I	FE II	IV	FE I	FE II	IV
	Bad self-rated health					
Considers victimization in next year likely	-0.0097*	-0.0089	0.0183	0.0002	-0.0002	0.0122
	(0.0055)	(0.0055)	(0.0463)	(0.0054)	(0.0053)	(0.0538)
Ever victimized	0.0024	0.0026	-0.0057	0.0168	0.0166	0.0321
	(0.0084)	(0.0084)	(0.0924)	(0.0116)	(0.0113)	(0.1808)
	Health has decreased compared with 1 year ago					
Considers victimization in	-0.0068	-0.0053	0.0347	0.0069	0.0080	-0.0969
next year likely	(0.0090)	(0.0091)	(0.0981)	(0.0094)	(0.0091)	(0.0922)
Ever victimized	0.0095	0.0208	0.3846***	0.0350*	0.0469**	1.0577***
	(0.0166)	(0.0158)	(0.1142)	(0.0181)	(0.0188)	(0.3657)
	Expects health to be worse next week					
Considers victimization in	-0.0059	-0.0042	0.0298	0.0140**	0.0157***	0.0004
next year likely	(0.0071)	(0.0069)	(0.0635)	(0.0057)	(0.0055)	(0.0642)
Ever victimized	0.0231*	0.0265**	0.1230	0.0147	0.0166	0.2805
	(0.0118)	(0.0114)	(0.0922)	(0.0131)	(0.0132)	(0.2062)
	Bad health relative to people of same age and sex					
Considers victimization in	0.0094	0.0094	-0.0234	0.0066	0.0038	-0.1480**
next year likely	(0.0067)	(0.0067)	(0.0514)	(0.0077)	(0.0078)	(0.0642)
Ever victimized	0.0032	0.0051	0.0945	0.0225*	0.0283**	0.5568***
	(0.0116)	(0.0112)	(0.0869)	(0.0123)	(0.0126)	(0.2161)
Individual fixed effects	yes	yes	yes	yes	yes	yes
Community-year fixed effects	yes	no	no	yes	no	no
Community fixed effects	no	yes	yes	no	yes	yes
Year fixed effects	no	yes	yes	no	yes	yes
Regional controls	no	yes	yes	no	yes	no
N		11,736			16,756	

Coefficients, standard errors adjusted for clustering on the individual and the community level in parentheses. */**/*** denote statistical significance on the 10%, 5% and 1% level respectively. Control variables in all specifications are age and age squared and dummies for having completed elementary school, Jr. High School, High School and College with no schooling being the base alternative. Regional controls are the local population, a dummy indicating whether income opportunities have improved compared to last year, and dummies indicating whether the prices of corn, other food and health care have risen respectively.

Table 4:

Victimization risk and indicators of mental health

	Men			Women		
	FE I	FE II	IV	FE I	FE II	IV
	Suffered from stress last 4 weeks					
Considers victimization in	0.0007	0.0006	-0.0060	0.0012	0.0012*	0.0008
next year likely	(0.0010)	(0.0011)	(0.0055)	(0.0007)	(0.0007)	(0.0050)
Ever victimized	0.0021	0.0021	0.0005	0.0024	0.0024	0.0153
	(0.0018)	(0.0017)	(0.0050)	(0.0018)	(0.0019)	(0.0159)
	Hours of sleep per night					
Considers victimization in	-0.0309	-0.0323	0.3744	0.0249	0.0206	0.2611
next year likely	(0.0397)	(0.0370)	(0.3289)	(0.0373)	(0.0367)	(0.3748)
Ever victimized	-0.0808	-0.1251**	-2.0157***	-0.0980	-0.1321**	-3.2442**
	(0.0612)	(0.0637)	(0.6343)	(0.0646)	(0.0623)	(1.3328)
	Had problems sleeping last 4 weeks					
Considers victimization in	0.0087	0.0080	-0.0352	0.0134*	0.0146*	-0.0262
next year likely	(0.0068)	(0.0065)	(0.0489)	(0.0073)	(0.0078)	(0.0701)
Ever victimized	0.0081	0.0159*	0.2894***	0.0272*	0.0309**	0.4409*
	(0.0089)	(0.0084)	(0.0842)	(0.0142)	(0.0144)	(0.2540)
	Felt frequently pessimistic last 4 weeks					
Considers victimization in	0.0072	0.0066	-0.0408	0.0085	0.0095*	0.0527
next year likely	(0.0066)	(0.0065)	(0.0283)	(0.0056)	(0.0055)	(0.0552)
Ever victimized	0.0068	0.0046	0.0209	0.0250**	0.0261**	0.2246
	(0.0085)	(0.0084)	(0.0517)	(0.0119)	(0.0122)	(0.2118)
	Experienced frequent feelings of fear last 4 weeks					
Considers victimization in	0.0050	0.0052	0.0066	0.0140**	0.0150***	0.0387
next year likely	(0.0055)	(0.0053)	(0.0330)	(0.0061)	(0.0058)	(0.0512)
Ever victimized	0.0055	0.0028	-0.0433	0.0034	0.0046	0.1705
	(0.0066)	(0.0064)	(0.0431)	(0.0119)	(0.0118)	(0.1681)
	Frequently wished to die during last 4 weeks					
Considers victimization in	-0.0011	-0.0013	0.0136	0.0001	-0.0014	-0.0432
next year likely	(0.0038)	(0.0039)	(0.0214)	(0.0039)	(0.0037)	(0.0344)
Ever victimized	0.0028	0.0021	-0.0028	0.0178**	0.0180**	0.0766
	(0.0047)	(0.0047)	(0.0472)	(0.0084)	(0.0076)	(0.1108)
Individual fixed effects	yes	yes	yes	yes	yes	yes
Community-year fixed effects	yes	no	no	yes	no	no
Community fixed effects	no	yes	yes	no	yes	yes
Year fixed effects	no	yes	yes	no	yes	yes
Regional controls	no	yes	yes	no	yes	no
N		11,736			16,756	

Coefficients, standard errors adjusted for clustering on the individual and the community level in parentheses. */**/*** denote statistical significance on the 10%, 5% and 1% level respectively. Control variables in all specifications are age and age squared and dummies for having completed elementary school, Jr. High School, High School and College with no schooling being the base alternative. Regional controls are the local population, a dummy indicating whether income opportunities have improved compared to last year, and dummies indicating whether the prices of corn, other food and health care have risen respectively.