Crowding Out under Medicaid's Presumptive Eligibility

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Abstract:

By employing a generalization of difference in difference estimation technique, we model the multi-dimensional effect of presumptive eligibility on children's access to Medicaid. We find children of families whose income would tend to qualify them for Medicaid or SCHIP significantly increase their access to Medicaid in states with a presumptive eligibility rule in place, but at the same time, significantly reduce the likelihood of having private insurance. This suggests that presumptive eligibility at least partially crowded out private insurance, a conclusion bolstered by our additional finding that children most likely to have benefited from presumptive eligibility laws report little improvement in their health status. On the other hand, we find that the institution of presumptive eligibility is associated with a 4% (linear specification) to 8% (log-linear specification) decline in infant mortality rates.

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I. Introduction

More than 94% of the 7.1 million uninsured children in the United States live in households with incomes that would make them eligible for either Medicaid or SCHIP (Broaddus and Ku, 2000). One of the attempts to increase enrollment is a provision of the Balanced Budget Act of 1997 giving states the option to adopt "presumptive eligibility" for the treatment of children and pregnant women. Presumptive eligibility eliminates delays in the provision of health care by reimbursing health care providers for caring for persons presumed eligible whether or not they are subsequently found to be eligible. Presumptive eligibility increases enrollment in Medicaid/SCHIP by making application for coverage part of the process of care. Bringing uninsured children into Medicaid/SCHIP increases their access to regular care and reduces reliance on the use of emergency departments for the provision of non-urgent care (Hakim and Ronsaville, 2002).

Empirical research suggests that presumptive eligibility has increased Medicaid and SCHIP enrollment among children. Kronebusch and Elbel (2004) conclude that the adoption of presumptive eligibility could increase children's enrollment in Medicaid or SCHIP by 6.4 percent Kronebusch and Elbel also found that presumptive eligibility increased enrollment more effectively than dropping asset tests, eliminating self-declaration of income, allowing mail-in or telephone applications, and in most cases dropping waiting periods and premiums.

Thirty-one states enacted presumptive eligibility rules for pregnant women but only the 10 states listed in Table 1 adopted presumptive eligibility for children. It has been suggested that more states have not adopted PE for children because of cost concerns and because PE was believed to be redundant when other measures were taken to expand enrollment (Klein, 2003).

Another concern is that PE, may crowd out private insurance coverage. That is, presumptive eligibility may induce insured poor families to switch from private coverage to Medicaid/SCHIP coverage for their children. The passage of PE legislation may also lead employers to reduce family health insurance coverage for their low income employees or adopt incentives that induce employees to substitute Medicaid coverage of their children for their work related health insurance (Shore-Sheppard et al. 2000) The phenomenon of "crowding out" has been documented for other types of expansions in Medicaid coverage but it has not been studied in relation to the adoption of presumptive eligibility.

We use a difference-in-difference analysis to examine the effect of presumptive eligibility on poor families' insurance choices and the health of their children. Consistent with prior research, we find presumptive eligibility increases Medicaid coverage. Self reported measures of health are not affected by the shifts in coverage but there is a 4% to 8% decline in infant mortality.

II. Background

The debate on the extent to which expansions of Medicaid reduce private insurance coverage focuses on two issues, namely: the size of the effect and the welfare implications of crowding out.

Research on the size of crowding-out include a wide range of estimates. The estimates include: 50% of the increase in number of insured under Medicaid/SCHIP

(Cutler and Gruber, 1996 Lo Sasso and Buchmueller, 2004;); 21% to 30% (Bnasak and Raphael, 2005);23% (Blumberg et al. (1999) ; 17% (Dubay and Kenney 1996) and from 5.3-23.9%(Yazici and Kaestner, 1998), and no effect (Card and Shore-Sheppard, 2004; Lo Sasso and Meyer, 2006). An analysis of subsidized coverage for low income uninsured in four states found that from 0 to 100% of increases in insurance coverage occurred at the expense of private insurance coverage (Kronick and Gilmer, 2002). Exploring a different public/private insurance margin, namely long-term care insurance instead of health insurance, Brown and Finkelstein (1994) also find that Medicaid crowds out long-term care insurance.

The most serious obstacle faced by studies of crowding-out is the need to disentangle the impact of Medicaid expansion on private health insurance from the pronounced secular decline in employment related health insurance in the United States. Analysts are also challenged by reliance on the Current Population Survey and its well established problems with under-reporting of Medicaid enrollment and of more general over-reporting of the numbers of uninsured persons (Call et al. 2007; Kronick and Gilmer, 2002).

The study of presumptive eligibility offers several advantages relative to changes such as the liberalization of income limits for Medicaid. The advantages of using the presumptive eligibility (PE) program as the instrument to estimate crowding out is that PE is limited in scope (only ten states) ; and the PE began at different times in different states, negating the effects of secular declines in employer-sponsored health insurance. PE’s additional spatial-temporal variation makes it ideal for the estimation of the crowding-out effect.

The second issue concerning crowding is its impact on social welfare. The debate can , in the simplest terms, be divided into two opinions. The first is that crowding has a negative effect on social welfare because it is inefficient to substitute public subsidies for private expenditures. That is, to the extent that public expenditures substitute for private coverage represent an incurred cost with no increase in insurance coverage (Cutler and Gruber, 1996). The shift from private to public insurance can occur through individual choice or, as suggested by a study of firm behavior, employers adopting premium increases or other measures to encourage Medicaid-eligible workers to transition from employer provided insurance to Medicaid (Shore-Sheppard et al. 2000). Whatever the mechanism, others argue that the social benefit in the form of increased coverage for the uninsured outweighs the inefficiencies associated with crowding out private insurance (Holahan, 1997).

One of the key criteria for judging these opposing positions is to remember that the benefit of health insurance coverage is expected to be increased health for the insured and more cost effective care for non-urgent health conditions. No study of crowding out of private health insurance by Medicaid has provided empirical evidence of the direct effects on costs or on the health of the persons who were previously uninsured or the persons substituting away from private insurance. Our analysis does not address costs but it does include two measures of the health of children, namely: infant mortality and the self reported health status of older children.

II. Difference-in-Difference-in-Difference Estimators

Difference-in-difference models focus on shifts between groups in order to make casual inferences. Regression difference-in-difference models make this same comparison, while including other explanatory factors in the model. The application here compares the before and after number of months of Medicaid coverage (the outcome measure) for children living in presumptive eligibility states (the treatment measure), with the before and after Medicaid usage of a counterfactual population. The counterfactual population consists of children with the same socio-demographic characteristics , living in states without presumptive eligibility programs. The effect of the explanatory variables is assumed to be constant across PE and no-PE states.

Hence, the generic formula for estimating the net impact of presumptive eligibility is

1) Impact of PE = $(months_{PE}^{after} - months_{PE}^{before}) - (months_{no\ PE}^{after} - months_{no\ PE}^{before})$

Ours is in fact a difference-in-difference-in-difference estimator, where the differences in equation 1 for those with incomes that quality for Medicaid) are compared to families with incomes too high to qualify for Medicaid. This triple difference in a linear regression model is equivalent to the coefficient that identifies an after PE effect (those in a PE state after the law becomes effective) for those with families with low, qualifying incomes.

In our estimation below, we estimate this effect for our nonlinear models by including state and year effects, and then allowing for slope and intercept shifts in PE states, with an additional shifts in those states after the PE law becomes effective, and comparing the outcomes for the PE eligible families with and without PE laws, conditional on socio-economic status, including family income level.

Not only will we be able to measure access to Medicaid health coverage where presumptive eligibility has been put into place, we will also be able to measure private insurance coverage for children as well as their perceived levels of health of the children. If there is substantial crowding out of insurance, we anticipate that private insurance coverage will decline as the coverage of public insurance increases after the institution of the PE laws in the respective states. If insurance coverage is beneficial for a child's health, and if PE laws draw substantially more uninsured children into regular health care, then PE laws should increase the reported health of previously uninsured children. To the extent that Medicaid/SCHIP substitutes for private insurance, however, the health of the previously insured children will not change. The net effect depends on the increase in insurance coverage for the uninsured relative to the number of children who substitute public for private health insurance. Although it cannot be measured in this study, the outcome also depends on the extent to which Medicaid health care coverage is superior to the private health insurance coverage that low income families can afford.

III. Data

The 1996 to 2006 March supplements to the current population survey (CPS) were used in the analysis. Medicaid months of coverage and self-reported health status were included in the March supplement data set for the first time in 1996. We limited our sample to children up to 18 years of age, corresponding to Medicaid coverage. The data on our sample of children in Table 2 indicate that the age distribution is fairly uniform, with about 5 percent in each age category. 51 percent of the children are male, and 62.5 percent are non-Hispanic white, followed by 18 percent Hispanic and 12 percent black.

While 18.4 percent of the sample comes from states in which a PE law was in effect (California and New York contributed substantially to this fraction), only 4.9 percent or 27 percent of those in PE active law states (4.9/18.4=.266) had family size and incomes that qualified them for Medicaid,[1] as indicated in Table 2. The average real family income in 1982-1984 dollars is $35,359.

The outcome variable used in the analysis is *months on Medicaid.* Children spent an average of 1.82 months on Medicaid, though the variation is great as indicated by a standard deviation of over 4 months. Slightly more than two-thirds (67.5%) were covered by private insurance. Eighty-one percent reported that their health was either excellent or very good (the categories were asked each individual to rate their health care: 1-excellent, 2-very good, 3-good, 4-poor, and 5-very poor).

IV. Presumptive Eligibility Estimates

Presumptive Eligibility (PE) and Months on Medicaid

We present three alternative specifications for our difference in difference in difference estimation in Tables 3, 4 and 5. The far left hand model in each table is the basic specification, including year and state effects (though those coefficients are not presented in the table). Presumptive eligibility (*PE*) and presumptive eligibility for Medicaid income-qualifying poor families (*PE*family qualif*) are given as shifts in the intercept.

[1] On the basis of reported family size and family income, we assumed all those children whose families had income less than 150 percent of the Federal poverty level, qualified for Medicaid. Though there are slight variations over time and across states in Medicaid eligibility, this seemed to the modal cutoff values for participation.

The results described in the middle columns in these three tables accommodate structural shifts between PE and non-PE states, allowing for all the slope coefficients to differ between presumptive eligible and non-eligible states. The slope shifts in regressors are indicated by the "PE1_" prefixes. The right hand specification, adds in addition to PE state shifts, shifts in PE states after the law becomes effective to as indicated by "PE2_" prefixes to the respective variables. This relaxes the assumption that the difference-in-difference effects operate solely through shifts in the intercept.

The count regression for number of months on Medicaid given in Table 3 generally exhibits the expected pattern of responses. For example, toddlers, age 1 to 4, use Medicaid relatively more than other age groups, while those age 18 (the omitted group), use Medicaid the least. As the coefficients measure the percentage change in months on Medicaid for a unit change in the respective regressor, the ".8842" coefficient indicates that one year olds have nearly a 90 percent higher number of months on Medicaid than 18 year olds.

The small and statistically insignificant male coefficient indicates there are no gender differences in Medicaid usage. Ethnicity is another matter: non-Hispanic whites use Medicaid fifty-one percent less than the omitted race categories (Asians and Native Americans), while blacks and Hispanics tend to use Medicaid more. As family income increases, use of Medicaid falls but at a decreasing rate. These patterns are evident across all specifications.

The variables of interest include presumptive eligibility, PE—a binary indicating when the law was in effect in a particular state; and "PE*family qualif" the interaction variable that measures the difference-in-difference-in-difference effect, that is, families in

PE states when PE rules are in effect that by virtue of their income and family size, are most likely to qualify for Medicaid under PE rules. This latter coefficient is highly significant, and of the expected positive sign, in all three specifications of Table 3 and in Tables 4 and 5.

The *PE* and *PE*family qualif* coefficients must be jointly evaluated when considering the effect of presumptive eligibility on Medicaid usage. The coefficient on the *PE* variable measures the influence of presumptive eligibility for higher income families. The results presented in the middle columns are an attempt to control for differences between states that enact PE laws (relative to other states) by allowing all the coefficients to vary between PE and non-PE states. When we do so, the estimated PE coefficient drops to -.07, only a third of its size in the simple and close to its expected zero effect (higher income, non-qualifying families are less likely to be affected by PE laws), and the implied effect of PE laws on qualifying families increases from 22 percent (.44 + -.22) to 43 percent (.50 + -.07).

The large change in the PE effect when the model is generalized suggests that another generalization may be warranted: not only allow the PE states coefficients to vary (given by the PE1_ indexed variables), but also within PE states allow the coefficients to vary once the law becomes effective (given by the PE2_ indexed variables). This is done in the far right hand side specification of Table 3. In particular, note that among the changes in coefficients recorded by these implementation shifts, the coefficients on the income variables shift most substantially (relative to the standard deviations of the respective variables). The shift in income effects indicates that families with higher incomes benefited from the shift to PE rules, suggesting that some families at the margin of qualifying were able to benefit from the implementation of presumptive eligibility laws.

However, in this last specification (in the far right hand side), as all the variables are allowed to shift as PE laws become effective, the evaluation of PE effects on qualifying families is no longer possible by evaluating just the *PE* and *PE*family qualify* coefficients. As the generalized difference in difference estimates in the far right hand column permit shifts in the slope regressors as well as the intercept while PE rules are in effect, and because the specification is nonlinear, in Table 3a we calculate the marginal effect of the PE rules on eligible families by computing the means for qualifying PE families with and without PE rules in effect. The counterfactual, without PE rules in effect, was estimated using the specification in the right hand column of Table 3. This indicates that months on Medicaid increased from 4.74 to 6.04, or about 35 percent among families most likely affected by the rules. This represents a substantial increase in the use of Medicaid health care resources by the poor. But is it really a net increase in access? Or does it more likely reflect a change from privately accessed health care to publicly funded Medicaid? That is, has Medicaid crowded out private insurance?

Presumptive Eligibility and Private Insurance

To address the crowding out issue, in Table 4 we replicate our models in Table 3 but replacing months on Medicaid (in the count regression of Table 3) with the likelihood of having private coverage (in a logistic regression in Table 4), using the same sets of right hand side regressors. The demographic patterns for private insurance are generally the reverse of those for Medicaid: babies are the least likely to have private insurance, whites the most likely, and the likelihood of private insurance coverage increases with real income. The reversals in signs extend to the other specifications in Table 4 as well—for example, once PE is implemented in a state, those with higher incomes are slightly less likely to have private insurance (parallel to the increase in the likelihood of having more Medicaid coverage in Table 3).

When, as with the results in Table 3, we allow PE states to have different slope regressors than non-PE states, the net effect of PE laws on the qualifying poor families decreases the likelihood of having private insurance, from a 8 percentage point reduction measured at the sample means ((-.725 +.364)*.675*(1-.675)) to about a 12 percentage point reduction ((-.879+.313)*.675*(1-.675)). The reduction of private insurance among the qualifying poor families is readily understood, but the results also suggest that private insurance coverage increases among families not qualified for Medicaid. Whether higher income families react to increased health care advertising under PE programs, or suppliers of private insurance act to increase the coverage for the dependents of higher income families, or whether some other mechanism is in play, we do not know.

However the difference-in-difference-in-difference estimate of the effect of PE laws is large and negative for qualifying families in the far right hand columns. Once again, in order to calculate the effect of PE on qualifying families given the generalized specification in the far right hand side column, we calculate (for this sample of PE, income qualifying families) the likelihood of insurance with and without PE rules (averaging over all such qualifying families). These results, given in Table 4a, indicate the private insurance coverage falls from 34.2 percent of these families to 26.1 percent, a 8.1 percentage point fall, or about a 24 percent reduction. This is slightly smaller than the percent increase in the use of Medicaid (Table IIIa), but the magnitudes are roughly the same and could be readily explained by differences in the margins of use (coverage or no coverage is the extensive margin, while months on Medicaid is the intensive margin of use). Nonetheless, the results indicate a substantial crowding out of private insurance as the same time that use of Medicaid increased with the implementation of PE laws. This suggests that the increase in Medicaid overstates the increase in health care access by poor children—indeed, there may be no net increase in access if poor families simply switched from private to public insurance.

Presumptive Eligibility and Children's Self-Reported Health

One criterion by which the benefits of Medicaid expansions can be weighed against the inefficiencies associated with crowding-out of private insurance is the effect of the expansions on children's health. We use two measures of health, namely self-reported health status among children whose families were most likely affected by the implementation of presumptive eligibility and aggregate measures of infant mortality in each state. The results on health are described in Tables 5 and 6. The estimates are based on our earlier specifications(Tables 3 and 4) but now include a binary variable that takes a value of 1 for self-reported excellent or very good health, and 0 for good, poor, or very poor health. That is, we model the likelihood of being in excellent health in Table 5.[2]

[2] We can also present the same analysis using ordered logistic analysis, but the results are more difficult to interpret (except for the first and last categories), while the magnitudes and statistical significance remind the same as reported here. The descriptive statistics for poor, income-qualifying families in PE states were before the PE law change: 36.8% in excellent health, 31.4% in very good health, 27.5% in good health, 3.8 in fair health, and .6 in poor health; to after the law change: 36.7% in excellent health, 33.8% in very good health, 25.3% in good health, 3.5 in fair health, and .6 in poor health. That is, the only shift is a modest 2% shift between the very good and good health categories when PE was implemented.

The estimates from an aggregate regression of the effect of PE laws on infant mortality are presented in Table 6. These results are not restricted to families eligible for Medicaid. We speculate that the marginal reduction in infant mortality among low income families (where infant mortality is above average) is the most probable source of the change. Infant mortality among higher income families is generally quite low and unlikely to improve by changes in the Medicaid laws.

There are some differences in demographic responses between Table 5 and our earlier results: males, for example, report a slightly lower likelihood of being in excellent health than females, a result that though quantitatively small is statistically significant. Whites are in better health than other race/ethnic groups, and younger children are generally in more excellent health than older children, with 18 year olds the least likely to be in excellent health. Excellent health increases with income, though at a decreasing rate.

Our earlier results indicated that the children of higher income (non-qualifying) families were slightly more likely to enroll in private insurance, and in Table 5, they are more likely to be in excellent health as well. However, as is apparent from the middle columns of Table 5 and the results in Table 5a, there is virtually no change in the self-reported health status of those children most likely affected by the presumptive eligibility laws. This suggests, coupled with the information in Tables 3 and 4, that for most children PE operated less to enable additional uninsured poor children to be enrolled in a health care plan than it did simply to induce the family of poor children to switch them from private insurance to public insurance. That is, PE substantially crowded out private insurance for income qualifying families.

Access to ongoing health care is probably more important for infants than any other age group. Hence, as a second barometer of health status, we use aggregate state year infant (birth to one year of age) mortality rates (deaths per 1,000) as a second health outcome variable in Table 6. As the dependent variable is available (from the March of Dimes webpage) at the state\year level, our analysis regresses the infant mortality rate for each state and year on dummy variables for each state and each year, as well as dummy variable indicating when PE was in effect for those states with a PE law for children[3].

The results in Table 6 indicate that PE laws significantly reduced infant mortality rates, though the effect is small. In the linear specification on the right hand side of Table 6, the estimated reduction of the infant mortality rate by -.3349 per 1000, or about a 4 percent reduction in mortality. The log-linear specification in the right hand columns suggest a larger impact of PE: a 8 percent reduction in child mortality.

Prior research has not examined *PE laws for children* and infant mortality, though one study examined the effect of PE on the use of prenatal care by newly enrolled pregnant women as part of the introduction of Medicaid managed care in Tennessee. The study found an increased use of prenatal care which is generally associated with improved birth outcomes. (Piper, Mitchel, and Ray, 1994) Two studies of the same Medicaid program did not, however, find any significant differences in birth outcomes after the introduction of Medicaid managed care. , (Ray, Gigante, Mitchel, and Hickson, 1998; and Conover, Rankin, and Sloan, 2001). Our results on PE laws aimed at children , include some slight improvements in infant mortality.

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[3] 31 states have adopted PE laws for pregnant women as of 2007, but only 10 states have PE laws for children. The overlap between adoption is not great: the simple correlation between states with PR laws for pregnant women and PE laws for children is .40. Tennessee, for example, has a law for pregnant women but not a PE law for children.

V. Conclusions

Our results are much more limited in scope than those of Cutler and Grubber (1996) or Dubay and Kennedy (1996), as considered only the effects of the program on those children who could have been affected by the implementation of presumptive eligibility: not only did they have be in a state where a PE law was in effect, they also had to belong to a family whose income was 150 percent or less than the poverty level, given their respective family sizes. This limitation is also a strength of the analysis, as it provides more power to distinguish the effect of presumptive eligibility, which becomes effective in different years in different states, from confounder such as business cycle effects and secular declines in employer sponsored health insurance.

Although we have not attempted to give a precise estimate of the extent of crowding-out, the reductions in private insurance appears to be at least as large as those given by Cutler and Grubber (1996) and Dubay and Kenney (1996). The most likely explanation is that presumptive eligibility rules are designed to help, the near poor more than the desperately poor, and that group has been shown to have a more elastic response in terms of crowding out than the poor in general (Dubay and Kenney, 1996).

We have considered two issues regarding the potential effect of expansions in Medicaid coverage, namely: the size of the impact on private insurance and its associated inefficiencies and, second, the extent to which expansions in insurance coverage improve the health of children.

The implementation of presumptive eligibility laws clearly achieves increases in insurance coverage for previously uninsured children. The expansions in coverage are just a clearly are associated with some reductions in private insurance coverage of children. Both of these results are consistent with previous research. Our estimates that approximately 50% of the increases in Medicaid coverage represent crowding-out effects are at the high end of the wide range of previously published results. Our examination of the effects of the expansion on children's health is an addition to research on the topic. The self-reports of health for children do not significantly change after the introduction of presumptive eligibility laws. There is, however, a small but significant reduction in infant mortality.

The comparison of the inefficiencies inherent in crowding-out to the benefits of reducing infant mortality involves ethical questions well beyond the scope of this article. The remaining, relevant question is whether the improvements in children's health that are associated with the use of pre-emptive eligibility can be achieved without the same level of inefficiencies or, as some argue, are an inherent cost in producing those benefits. The answers to the question require a more extensive study of the health effects of Medicaid expansions. It is, after all, the presumption that increased health coverage improves the health of those who are otherwise uninsured that motivates public interventions to increase coverage.

Table 1: Effective Presumptive Eligibility Adoption Dates

State	Medicaid and/or SCHIP
California	July 1, 2002
Connecticut	November 1, 2005
Illinois	May 3, 2004
Massachusetts	August, 1998
Michigan	September, 2000
Missouri	March 27, 2003
New Hampshire	June, 1999
New Jersey	January, 2000
New Mexico	July, 1998
New York	1991

Note: the dates for the implementation of presumptive eligibility (for NY, it is for the SCHIP program) were retrieved by contacting by email/phone all the respective state agencies.

Table 2. Presumptive Eligibility Descriptive Statistics—Months on Medicaid

Variables	Means	Standard deviation
Dependent Variables:		
Months on Medicaid last year	1.8244256	4.2001443
Excellent Health	0.8109419	0.3915551
Private Insurance Coverage	0.6751326	0.4683257
Independent Variables:		
PE=Presumptive Eligibility (1=yes,0=no)	0.1840065	0.3874898
PE*Income Qualified Family	0.0489320	0.2157261
Age=0 year old	0.0438962	0.2048644
Age=1 year old	0.0490930	0.2160624
Age=2 year old	0.0501341	0.2182218
Age=3 year old	0.0512137	0.2204336
Age=4 year old	0.0513625	0.2207361
Age=5 year old	0.0525138	0.2230609
Age=6 year old	0.0533642	0.2247588
Age=7 year old	0.0534079	0.2248457
Age=8 year old	0.0540641	0.2261443
Age=9 year old	0.0543370	0.2266818
Age=10 year old	0.0544893	0.2269808
Age=11 year old	0.0547237	0.2274404
Age=12 year old	0.0557089	0.2293588
Age=13 year old	0.0558803	0.2296907
Age=14 year old	0.0549915	0.2279638
Age=15 year old	0.0545785	0.2271559
Age=16 year old	0.0543545	0.2267162
Age=17 year old	0.0532329	0.2244979
male	0.5113971	0.4998705
white	0.6253519	0.4840323
black	0.1192409	0.3240720
Hispanic	0.1821727	0.3859872
real household income ($1,000)	35.3592535	34.8267153
real household income squared	2075.93	6541.11

N= 571,507

Table 3. Negative Binomial Count Regressions of Months of Medicaid coverage: 1996-2006

	Difference-in-Difference		Shift for PE States		Shift for Post-PE months	
	coeff.	prob. sign.	coeff.	Prob. sign.	coeff.	prob. sign.
Intercept	1.4014	<.0001	1.5282	<.0001	1.5282	<.0001
Age=0 year old	0.4436	<.0001	0.4977	<.0001	0.4977	<.0001
Age=1 year old	0.8842	<.0001	0.9572	<.0001	0.9572	<.0001
Age=2 year old	0.7497	<.0001	0.7827	<.0001	0.7827	<.0001
Age=3 year old	0.6926	<.0001	0.7368	<.0001	0.7368	<.0001
Age=4 year old	0.6820	<.0001	0.7277	<.0001	0.7277	<.0001
Age=5 year old	0.6096	<.0001	0.6505	<.0001	0.6505	<.0001
Age=6 year old	0.5692	<.0001	0.6015	<.0001	0.6015	<.0001
Age=7 year old	0.5266	<.0001	0.5602	<.0001	0.5602	<.0001
Age=8 year old	0.5455	<.0001	0.5614	<.0001	0.5614	<.0001
Age=9 year old	0.4990	<.0001	0.5164	<.0001	0.5164	<.0001
Age=10 year old	0.4774	<.0001	0.5058	<.0001	0.5058	<.0001
Age=11 year old	0.4286	<.0001	0.4730	<.0001	0.4730	<.0001
Age=12 year old	0.4301	<.0001	0.4453	<.0001	0.4452	<.0001
Age=13 year old	0.3969	<.0001	0.4073	<.0001	0.4073	<.0001
Age=14 year old	0.3839	<.0001	0.4151	<.0001	0.4151	<.0001
Age=15 year old	0.2304	<.0001	0.2332	<.0001	0.2332	<.0001
Age=16 year old	0.2255	<.0001	0.2132	<.0001	0.2132	<.0001
Age=17 year old	0.1427	<.0001	0.1545	0.0001	0.1544	0.0001
male	-0.0029	0.7760	0.0068	0.5837	0.0068	0.5831
white	-0.5098	<.0001	-0.5908	<.0001	-0.5908	<.0001
black	0.2896	<.0001	0.1600	<.0001	0.1600	<.0001
Hispanic	0.0820	0.0006	-0.0855	0.0060	-0.0855	0.0060
real HH income	-0.0556	<.0001	-0.0570	<.0001	-0.0570	<.0001
real income sq.	0.0002	<.0001	0.0002	<.0001	0.0002	<.0001
PE	-0.2200	<.0001	-0.0716	0.0377	-0.6692	<.0001
PE*family qualif	0.4446	<.0001	0.5000	<.0001	0.7387	<.0001
PE1_age0			-0.1830	0.0128	-0.1236	0.2325
PE1_age1			-0.2460	0.0005	-0.2625	0.0086
PE1_age2			-0.1078	0.1263	-0.0689	0.4905
PE1_age3			-0.1484	0.0342	-0.1731	0.0806
PE1_age4			-0.1498	0.0327	-0.1488	0.1349
PE1_age5			-0.1385	0.0473	-0.1660	0.0926
PE1_age6			-0.1080	0.1198	-0.0954	0.3327
PE1_age7			-0.1074	0.1225	-0.1227	0.2150
PE1_age8			-0.0593	0.3932	-0.0311	0.7536
PE1_age9			-0.0570	0.4112	-0.0971	0.3232
PE1_age10			-0.0908	0.1904	-0.1548	0.1187
PE1_age11			-0.1406	0.0431	-0.2519	0.0115
PE1_age12			-0.0495	0.4753	-0.0891	0.3692
PE1_age13			-0.0342	0.6227	-0.0856	0.3927
PE1_age14			-0.1020	0.1441	-0.1300	0.1988
PE1_age15			-0.0069	0.9221	-0.0488	0.6300
PE1_age16			0.0378	0.5916	0.0264	0.7952
PE1_age17			-0.0357	0.6152	-0.0444	0.6644
PE1_male			-0.0246	0.2620	-0.0884	0.0041

PE1_white			0.2253	<.0001	0.2700	<.0001
PE1_black			0.3693	<.0001	0.4731	<.0001
PE1_Hispanic			0.4146	<.0001	0.4138	<.0001
PE1_HH income			0.0049	<.0001	-0.0057	<.0001
PE1_HH inc. sq.			-0.0000	<.0001	0.0000	<.0001
PE2_age0					-0.1233	0.3190
PE2_age1					0.0012	0.9923
PE2_age2					-0.0828	0.4859
PE2_age3					0.0167	0.8871
PE2_age4					-0.0262	0.8247
PE2_age5					0.0196	0.8673
PE2_age6					-0.0354	0.7615
PE2_age7					0.0037	0.9747
PE2_age8					-0.0668	0.5680
PE2_age9					0.0463	0.6909
PE2_age10					0.0841	0.4716
PE2_age11					0.1531	0.1922
PE2_age12					0.0490	0.6755
PE2_age13					0.0673	0.5676
PE2_age14					0.0281	0.8130
PE2_age15					0.0571	0.6319
PE2_age16					-0.0038	0.9748
PE2_age17					-0.0010	0.9931
PE2_male					0.1087	0.0031
PE2_white					-0.0443	0.5516
PE2_black					-0.1706	0.0535
PE2_Hispanic					0.0086	0.9111
PE2_HH income					0.0214	<.0001
PE2_HH inc. sq.					-0.0001	<.0001
Dispersion parm	13.1818		13.1596		13.1350	
Log-likelihood	1070271.5805		1070390.7630		1070513.6068	
Also included	---		PE1_ year dummy shifts		PE1__ year dummy shifts	
Joint-test "PE2_"	---		---		<.0001	

Notes: N=571507. Dummy variables for states and years in all specifications. Real family incomes have been adjusted with urban CPI 1982-1984=100. PE =1 if the state had a presumptive eligibility law in effect that year, PE=0 otherwise. "PE1_"-prefixes are interactions with a dummy variable indicating whether a state adopted a PE program (also included in the specification, but not reported here, are PE1_ interactions with the year dummy variables). "PE2-_"-prefixes are interactions with a dummy variable indicating post adoption period in those states adopting PE.

Table 3a. Presumptive Eligibility and Months on Medicaid, for Qualifying Families in Presumptive Eligibility States

	Means	Standard deviations
Predicted health without PE	4.7392426	2.5895202
Predicted health with PE	6.0374571	2.6742823

N=27965; predictions based on the far right hand side model above in Table III.

Table 4. Logistic Regression of Private Insurance Coverage: 1996-2006

	Difference-in-Difference		Shift for PE States		Shift for Post-PE months	
	coeff.	prob. sign.	coeff.	Prob. sign.	coeff.	prob. sign.
Intercept	-1.7257	<.0001	-1.8268	<.0001	-1.8268	<.0001
Age=0 year old	-0.2915	<.0001	-0.3280	<.0001	-0.3280	<.0001
Age=1 year old	-0.0242	0.2519	-0.0627	0.0130	-0.0627	0.0130
Age=2 year old	0.0386	0.0666	0.0012	0.9636	0.0012	0.9636
Age=3 year old	0.0498	0.0177	0.0269	0.2851	0.0269	0.2851
Age=4 year old	0.1274	<.0001	0.0889	0.0004	0.0889	0.0004
Age=5 year old	0.1321	<.0001	0.1074	<.0001	0.1074	<.0001
Age=6 year old	0.1308	<.0001	0.0900	0.0003	0.0900	0.0003
Age=7 year old	0.1725	<.0001	0.1363	<.0001	0.1363	<.0001
Age=8 year old	0.1730	<.0001	0.1551	<.0001	0.1551	<.0001
Age=9 year old	0.2165	<.0001	0.1843	<.0001	0.1843	<.0001
Age=10 year old	0.1973	<.0001	0.1706	<.0001	0.1706	<.0001
Age=11 year old	0.2094	<.0001	0.1728	<.0001	0.1728	<.0001
Age=12 year old	0.2222	<.0001	0.1789	<.0001	0.1789	<.0001
Age=13 year old	0.2200	<.0001	0.1781	<.0001	0.1781	<.0001
Age=14 year old	0.2146	<.0001	0.1780	<.0001	0.1780	<.0001
Age=15 year old	0.2129	<.0001	0.1774	<.0001	0.1774	<.0001
Age=16 year old	0.2092	<.0001	0.1934	<.0001	0.1934	<.0001
Age=17 year old	0.1489	<.0001	0.1275	<.0001	0.1275	<.0001
male	-0.0122	0.0658	-0.0134	0.0938	-0.0134	0.0938
white	0.7155	<.0001	0.7507	<.0001	0.7507	<.0001
black	-0.0306	0.0507	0.0783	<.0001	0.0783	<.0001
Hispanic	-0.2584	<.0001	-0.1873	<.0001	-0.1873	<.0001
real HH income	0.0722	<.0001	0.0760	<.0001	0.0760	<.0001
real income sq	-0.0002	<.0001	-0.0002	<.0001	-0.0002	<.0001
PE	0.3636	<.0001	0.3134	<.0001	0.9477	<.0001
PE*family qualif	-0.7250	<.0001	-0.8792	<.0001	-1.1476	<.0001
PE1_age0			0.1315	0.0052	0.0552	0.3937
PE1_age1			0.1394	0.0024	0.1356	0.0326
PE1_age2			0.1340	0.0035	0.1350	0.0338
PE1_age3			0.0880	0.0537	0.0900	0.1541
PE1_age4			0.1357	0.0030	0.1100	0.0817
PE1_age5			0.0926	0.0415	0.0785	0.2106
PE1_age6			0.1448	0.0014	0.1718	0.0062
PE1_age7			0.1254	0.0057	0.1440	0.0224
PE1_age8			0.0681	0.1327	0.0517	0.4113
PE1_age9			0.1129	0.0130	0.1140	0.0699
PE1_age10			0.0910	0.0447	0.1342	0.0338
PE1_age11			0.1258	0.0058	0.1035	0.1041
PE1_age12			0.1461	0.0013	0.1294	0.0421
PE1_age13			0.1428	0.0017	0.1556	0.0153
PE1_age14			0.1223	0.0076	0.1223	0.0586
PE1_age15			0.1196	0.0095	0.1498	0.0213
PE1_age16			0.0537	0.2446	0.1084	0.0945
PE1_age17			0.0694	0.1344	0.0256	0.6943
PE1_male			0.0036	0.8045	0.0319	0.1096
PE1_white			-0.1086	0.0002	-0.1467	0.0006
PE1_black			-0.3445	<.0001	-0.4114	<.0001
PE1_Hispanic			-0.2081	<.0001	-0.2199	<.0001
PE1_HH income			-0.0135	<.0001	-0.0005	0.6038
PE1_income sq			0.0000	<.0001	-0.0000	0.0037
PE2_age0					0.1630	0.0403
PE2_age1					0.0346	0.6559

PE2_age2					0.0182	0.8147
PE2_age3					0.0234	0.7611
PE2_age4					0.0788	0.3067
PE2_age5					0.0568	0.4571
PE2_age6					-0.0238	0.7555
PE2_age7					-0.0047	0.9506
PE2_age8					0.0612	0.4229
PE2_age9					0.0267	0.7265
PE2_age10					-0.0521	0.4951
PE2_age11					0.0643	0.4038
PE2_age12					0.0544	0.4792
PE2_age13					-0.0001	0.9987
PE2_age14					0.0183	0.8139
PE2_age15					-0.0305	0.6968
PE2_age16					-0.0769	0.3246
PE2_age17					0.0866	0.2697
PE2_male					-0.0515	0.0348
PE2_white					0.0482	0.3201
PE2_black					0.1153	0.0393
PE2_Hispanic					0.0176	0.7195
PE2_HH income					-0.0277	<.0001
PE2_income sq					0.0001	<.0001
Log-likelihood	-273798.0821		-273492.7798		-273157.0315	
Also included	---		PE1_ year dummy shifts		PE1_ year dummy shifts	
Joint-test "PE2_"	---		---		<.0001	

Notes: N=571507. Dummy variables for states and years in all specifications. Real family incomes have been adjusted with urban CPI 1982-1984=100. PE =1 if the state had a presumptive eligibility law in effect that year, PE=0 otherwise. "PE1_"-prefixes are interactions with a dummy variable indicating whether a state adopted a PE program (also included in the specification, but not reported here, are PE1_ interactions with the year dummy variables). "PE2_"-prefixes are interactions with a dummy variable indicating post adoption period in those states adopting PE.

Table 4a. Presumptive Eligibility and Private Insurance, for Qualifying Families in Presumptive Eligibility States

	Means	Standard deviations
Predicted Insurance without PE	0.3420826	0.1436814
Predicted Insurance with PE	0.2608260	0.1142937

N=27965

Table 5. Logistic Regression of Excellent Health: 1996-2006

	Difference-in-Difference		Shift for PE States		Shift for Post-PE months	
	coeff.	prob. sign.	coeff.	Prob. sign.	coeff.	prob. sign.
Intercept	0.2855	<.0001	0.2330	<.0001	0.2330	<.0001
Age=0 year old	0.5384	<.0001	0.5326	<.0001	0.5326	<.0001
Age=1 year old	0.4487	<.0001	0.4479	<.0001	0.4479	<.0001
Age=2 year old	0.4451	<.0001	0.4581	<.0001	0.4581	<.0001
Age=3 year old	0.4427	<.0001	0.4537	<.0001	0.4537	<.0001
Age=4 year old	0.4320	<.0001	0.4568	<.0001	0.4568	<.0001
Age=5 year old	0.4179	<.0001	0.4311	<.0001	0.4311	<.0001
Age=6 year old	0.4098	<.0001	0.4062	<.0001	0.4062	<.0001
Age=7 year old	0.3870	<.0001	0.3864	<.0001	0.3864	<.0001
Age=8 year old	0.3468	<.0001	0.3401	<.0001	0.3401	<.0001
Age=9 year old	0.3676	<.0001	0.3703	<.0001	0.3703	<.0001
Age=10 year old	0.3271	<.0001	0.3225	<.0001	0.3225	<.0001
Age=11 year old	0.3346	<.0001	0.3301	<.0001	0.3301	<.0001
Age=12 year old	0.2722	<.0001	0.2826	<.0001	0.2826	<.0001
Age=13 year old	0.2696	<.0001	0.2657	<.0001	0.2657	<.0001
Age=14 year old	0.2484	<.0001	0.2337	<.0001	0.2337	<.0001
Age=15 year old	0.1559	<.0001	0.1569	<.0001	0.1569	<.0001
Age=16 year old	0.1209	<.0001	0.1275	<.0001	0.1275	<.0001
Age=17 year old	0.0780	0.0002	0.0679	0.0066	0.0679	0.0066
male	-0.0257	0.0002	-0.0244	0.0038	-0.0244	0.0038
white	0.5522	<.0001	0.5480	<.0001	0.5480	<.0001
black	-0.0318	0.0454	-0.0046	0.8155	-0.0046	0.8155
Hispanic	0.0166	0.2625	0.0754	0.0001	0.0754	0.0001
real HH income	0.0221	<.0001	0.0237	<.0001	0.0237	<.0001
real income sq	-0.0001	<.0001	-0.0001	<.0001	-0.0001	<.0001
PE	0.0982	<.0001	0.1022	<.0001	0.2482	0.0011
PE*family qualif	-0.0525	0.0030	-0.1106	<.0001	-0.1760	<.0001
PE1_age0			0.0234	0.6316	0.0221	0.7374
PE1_age1			0.0083	0.8586	-0.0107	0.8655
PE1_age2			-0.0345	0.4542	0.0158	0.8025
PE1_age3			-0.0279	0.5426	0.0157	0.8021
PE1_age4			-0.0703	0.1235	-0.0297	0.6345
PE1_age5			-0.0348	0.4425	0.0154	0.8035
PE1_age6			0.0150	0.7407	0.0698	0.2614
PE1_age7			0.0039	0.9305	0.0052	0.9323
PE1_age8			0.0236	0.5987	0.0387	0.5291
PE1_age9			-0.0057	0.8981	0.0511	0.4078
PE1_age10			0.0157	0.7241	0.0486	0.4301
PE1_age11			0.0155	0.7293	0.0526	0.3974
PE1_age12			-0.0292	0.5089	0.0577	0.3502
PE1_age13			0.0136	0.7588	0.0434	0.4837
PE1_age14			0.0472	0.2893	0.1360	0.0305
PE1_age15			-0.0016	0.9716	0.0050	0.9348
PE1_age16			-0.0192	0.6631	0.0017	0.9784
PE1_age17			0.0322	0.4654	0.0246	0.6896
PE1_male			-0.0036	0.8084	-0.0053	0.7918
PE1_white			0.0237	0.4108	0.0029	0.9447
PE1_black			-0.0756	0.0231	-0.0966	0.0453
PE1_Hispanic			-0.1347	<.0001	-0.1783	<.0001
PE1_HH income			-0.0050	<.0001	-0.0023	0.0012
PE1_income sq			0.0000	<.0001	0.0000	0.8018

PE2_age0					0.0156	0.8477
PE2_age1					0.0474	0.5384
PE2_age2					-0.0828	0.2788
PE2_age3					-0.0691	0.3631
PE2_age4					-0.0617	0.4128
PE2_age5					-0.0801	0.2844
PE2_age6					-0.0897	0.2311
PE2_age7					0.0070	0.9256
PE2_age8					-0.0189	0.7989
PE2_age9					-0.0939	0.2059
PE2_age10					-0.0510	0.4904
PE2_age11					-0.0597	0.4221
PE2_age12					-0.1441	0.0502
PE2_age13					-0.0473	0.5226
PE2_age14					-0.1462	0.0505
PE2_age15					-0.0073	0.9216
PE2_age16					-0.0323	0.6594
PE2_age17					0.0153	0.8354
PE2_male					0.0028	0.9102
PE2_white					0.0259	0.5879
PE2_black					0.0352	0.5214
PE2_Hispanic					0.0773	0.1094
PE2_HH income					-0.0055	<.0001
PE2_income sq					0.0000	<.0001
Log-likelihood	-263364.5300		-263278.3231		-263242.9674	
Also included	---		PE1_ year dummy shifts		PE1_ year dummy shifts	
Joint-test "PE2_"	---		---		<.0001	

Notes: N=571507. Dummy variables for states and years in all specifications. Real family incomes have been adjusted with urban CPI 1982-1984=100. PE =1 if the state had a presumptive eligibility law in effect that year, PE=0 otherwise. "PE1_"-prefixes are interactions with a dummy variable indicating whether a state adopted a PE program (also included in the specification, but not reported here, are PE1_ interactions with the year dummy variables). "PE2_"-prefixes are interactions with a dummy variable indicating post adoption period in those states adopting PE.

Table 5a. Presumptive Eligibility and Health, for Qualifying Families in Presumptive Eligibility States

	Means	Standard deviations
Predicted health without PE	0.6998939	0.0767217
Predicted health with PE	0.7049169	0.0716391

N=27965

Table 6. Presumptive Eligibility and Infant Mortality

	Dep. var.=infant mortality rate		Dep. var.=log(infant mortality rate)	
Variables	Coefficients	Probability Significance	Coefficients	Probability Significance
Constant	6.4106	<.0001	1.8724	<.0001
Presumptive Eligibility	-0.3349	0.0020	-0.0813	<.0001
Log-likelihood	-1509.5091		1313.5835	
Joint Signif: year effects	<.0001		<.0001	
Joint Signif: state effects	<.0001		<.0001	

Note: State/year level information from 1980 to 2004. All tests adjusted for hetereoskedasticity using White's robust corrections.

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