HEALTH ECONOMICS:

POLICY OUTCOMES, INDIVIDUAL CHOICE,

AND ADOLESCENT BEHAVIOR

by

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To complement a varied and growing literature in health economics, this dissertation is conducted in three substantive parts. First, I investigate the effect of public policy on health use and health outcomes, exploiting variation in the generosity of Medicaid eligibility to low income pregnant women across states and over time to identify an effect on common, yet costly, pregnancy complications. I provide new evidence on this important question from a nationally representative sample of hospital discharges for 12 states between 1989 and 2001. Second, I explore heterogeneity in individual demand for health risk reductions. Utilizing individual stated-preference data from matching surveys conducted in both Canada and the United States, I employ the Value of a Statistical Illness Profile framework to investigate differences in average willingness-to-pay (WTP) for health risk

reductions across the two different cultures. Although existing literature has allowed for systematic variation in age to explain differences in health care demand, the differences in WTP have not been explained through systematic variation across other socio-demographic characteristics, subjective risks of the diseases in question, or differences between the Canadian and U.S. health care systems. I extend the literature by controlling for an expanded set of observable individual heterogeneity and comment on the degree to which estimates can be applied across cultures to inform varying policy decisions. The third paper studies factors affecting adolescent health risk behavior. Previous study finds that community size and the degree to which social networks are interconnected affect three economically significant outcomes: the frequency of adolescent misbehavior in school, degree of perceived safety in school, and grade performance. Other research has suggested peer effects on smoking behavior and drinking behavior. I investigate the degree to which social connectedness impacts adolescent health, specifically looking at outcomes for drinking and smoking, and the degree to which these effects can be disentangled from more commonly studied "peer effects" in health behavior.

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TABLE OF CONTENTS

Chapter Page

LIST OF FIGURES

Figure Page

LIST OF TABLES

CHAPTER I

INTRODUCTION

Health outcomes are important to economic policy for a variety of reasons. Costs associated with illness are significant to local, state, and national budgets. Adverse health conditions impair individuals' productivity in the labor market, and access to health care is a growing concern for the poor. Impacts to human health associated with air and water pollution account for the majority of benefits advocating environmental policy. Moreover, use of health care accounts for a major portion of individual households' consumption set, and there is a great deal of variation in the interplay between ex ante health risk behavior and ex post utilization of health care to treat illness.

These reasons, among others, motivate study of health outcomes, health choice, and health behavior. To complement a varied and growing literature in health economics, this dissertation is conducted in three substantive parts. The first chapter addresses the effects of health policy on health outcomes among the poor, while the second looks at individual heterogeneity in demand for preventative health care, while controlling for individual differences in lifestyle and attitudinal behavior. The final chapter looks more closely at factors affecting health risk behavior.

In Chapter II, I investigate the efficacy of public policy on health use and health outcomes, exploiting variation in the generosity of Medicaid eligibility to low income pregnant women across states and over time to identify an effect on common, yet costly, pregnancy complications. Specifically, the question of whether state expansions to Medicaid have been successful in increasing access to care and improving health remains in dispute. While some studies find an effect, others argue these findings are largely spurious. Moreover, the extant literature has primarily focused on outcomes related to child health; much less has been written on Medicaid expansions and maternal health. I provide new evidence on this important question from a nationally representative sample of hospital discharges for 12 states between 1989 and 2001 provided by the Health Care Utilization Project (HCUP). The analysis suggests a lower presentation of infectious disease-related complications among hospitalizations due to increased Medicaid generosity, and some evidence of a longer length of hospital stay among pregnancy-related admissions.

In Chapter III, I explore heterogeneity in individual demand for health risk reductions. Utilizing individual stated-preference data from matching surveys conducted in both Canada and the United States, I employ the Value of a Statistical Illness Profile framework to investigate differences in average willingness-to-pay (WTP) for health risk reductions across the two different cultures. Although existing literature has examined differences across Canada and the United States allowing for systematic variation with age, the differences in WTP are not explained through systematic variation across other sociodemographic characteristics, subjective risks of the diseases in question, or differences between the Canadian and U.S. health care systems.

I extend the cross-national literature to explain observed differences in individual WTP for health risk reduction programs by individual heterogeneity in each of these factors. Controls for these individual characteristics are necessary to prevent cross-national heterogeneity from showing up as spurious cross-national differences (or lack thereof) in health preferences. Moreover, from a policy perspective, any WTP number used for benefit-cost analysis should reflect the actual distribution of characteristics in the at-risk population for a particular policy or regulation. I find evidence of preference heterogeneity, with the differences largely explained by non-jurisdictional individual characteristics. I find substantial evidence of age profile effects which are generally consistent with other studies. However, age profiles with respect to WTP to avoid adverse health states are markedly different between Canadians and U.S. residents. In general, Canadians have a much flatter age profile for WTP, and this profile appear to peak at a substantially older age.

Finally, Chapter IV studies factors affecting adolescent health risk behavior. Previous study finds that community size and the degree to which social networks are interconnected (degree of network "closure") affects three economically significant outcomes: the frequency of adolescent misbehavior in school ("Since school started this year, how often have you had trouble getting along with other students?"), degree of perceived safety in school ("How strongly do you agree or disagree with the following statement: "I feel safe in my school""), and grade performance, measured by an equally-weighted GPA of the student in English, Math, Science, and History. Other research has suggested peer effects in smoking behavior and drinking behavior. I investigate the degree to which network closure impacts adolescent health, specifically looking at outcomes for drinking and smoking, and the degree to which these effects can be disentangled from more commonly studied peer effects in health behavior.

CHAPTER II
MEDICAID POLICY AND MATERNAL HEALTH

Access to medical care in the United States has been a topic of significant public interest since universal care was initially proposed by the Truman administration in the 1950s. To date, universal coverage has yet to be imparted to all Americans, with private-payer and employer-provided insurance accounting for the majority of coverage for the non-elderly. Nonetheless, public programs have aimed to fill the gap in access to care for the uninsured.

Medicaid has historically been the most significant public entitlement program for insuring the poor. Expansions to the program have been substantial. While in 1984 Medicaid expenditures totaled a mere $38 billion and covered roughly 22 million people, by 2006, Medicaid outlays totaled nearly $288 billion, covering over 60 million people, including health insurance for 30 million low income children, as well as long-term and acute care for roughly 5.6 million of the elderly (Kaiser Commission, 2004, and Georgetown University Health Policy Institute, 2008). In real terms, this represents an annual 7% percent increase in expenditures over the twenty year period. One of the single largest components of the Medicaid expansion has been provision of health insurance to low income pregnant women and children. During the 1980s and 1990s, federal standards of Medicaid income eligibility were sequentially lowered for low income pregnant women in an effort to increase access to prenatal and hospital care, and thereby improve health outcomes.

A number of studies have examined changes in the utilization and health outcomes induced by state-by-state expansions in Medicaid eligibility. In particular, Currie and Gruber (1996a,b) found that expansions increased utilization of care, and improved health outcomes through a reduction in the rate of infant and child mortality. Kaestner (1999), on the other hand, found little evidence of increased prenatal care use, nor improved outcomes for children, and cites omitted controls for state-specific trends in the prevalence of poverty as potentially generating spurious results in Currie and Gruber (1996a,b).

Therefore, the question of whether state expansions to Medicaid have been successful in increasing access to care and improving health remains in dispute. Moreover, these studies have primarily focused on outcome effects related to child health; much less has been written on Medicaid expansions and maternal health. This paper provides new evidence on this important question from a nationally representative sample of hospital discharges for 12 states between 1989 and 2001 provided by the Health Care Utilization Project (HCUP). I employ a similar methodology to Currie and Gruber (1996a,b), but include controls for the percent of the population in each state in a given year at or below the federal poverty standard, as suggested by Kaestner (1999).

As a measure of the degree of care provided, I investigate the effects of Medicaid eligibility expansions on hospital length of stay. Although length of stay may be associated with either improved or diminished health outcomes through either a lower likelihood of premature discharge or excess exposure to hospital-induced illness, this measure is an agnostic indication of the degree of medical care received during hospitalization, as well as an efficiency measure for hospital care received. Dafny and Gruber (2005) find negative effects on average length of stay for increased eligibility expansions to children, which was mediated by an increase in the number of procedures performed. However, the literature has not addressed whether similar effects are observed for pregnancy-related admissions of adult women. If increases in Medicaid eligibility resulted in primarily inducing coverage and care for the uninsured, it is plausible that expected reimbursement rates for hospitals increased as a result of the eligibility expansions. This may lead to an incentive for hospitals to retain patients longer. On the other hand, if expansions resulted in primarily inducing low-income privately insured individuals, these expansions may have led to a reduction in the reimbursement rate for hospital. This may lead to an incentive to shorten the length of stay for the marginal patient induced onto Medicaid as a result of the state eligibility expansions.

Second, I examine the effects of Medicaid expansions on maternal health outcomes, which has received relatively little attention in the past. A large literature suggests that Medicaid expansions have generated limited tangible health benefits due to low take-up rates and delayed utilization of prenatal care beyond the first and sometimes second trimester (Gruber, 2000). Although certain precautionary treatment in the first and second trimester is particularly important to reducing complications at birth, even late treatment can offer some benefit. If left untreated, bacterial diseases such as syphilis, chlamydia and gonorrhea can result in Pelvic Inflammatory Disease and ectopic pregnancies for the mother, and premature birth and low birth weight for the child. Since these diseases can be treated well into the third trimester, and prior to delivery, improved health outcomes can be expected, even in the case of delayed use of prenatal care services (American Pregnancy Association, 2003).

First stage regression results show that patient take-up of Medicaid coverage varies across hospital ownership types. An average 15 percent increase in the proportion of the population eligible for Medicaid between 1989 and 2001 resulted in a 2.3 percent increase in Medicaid-insured patients at private hospitals and a 1.2 percent increase at non-profit hospitals, with no statistically significant increase at government hospitals. Length of stay for Medicaid-financed patients increased on average by 0.13 days as a result of eligibility expansions over the period. The probability of infectious disease-related hospital admissions among pregnant women were roughly 0.6 percentage points lower than would have been the case in the absence of Medicaid expansions.

I begin with a discussion of the background of Medicaid expansions and review the prior literature in Section 1. Data and methodology for the empirical analysis are then presented in Section 2, followed by descriptive statistics in Section 3, and results in Section 4. Section 5 concludes.

1. Background

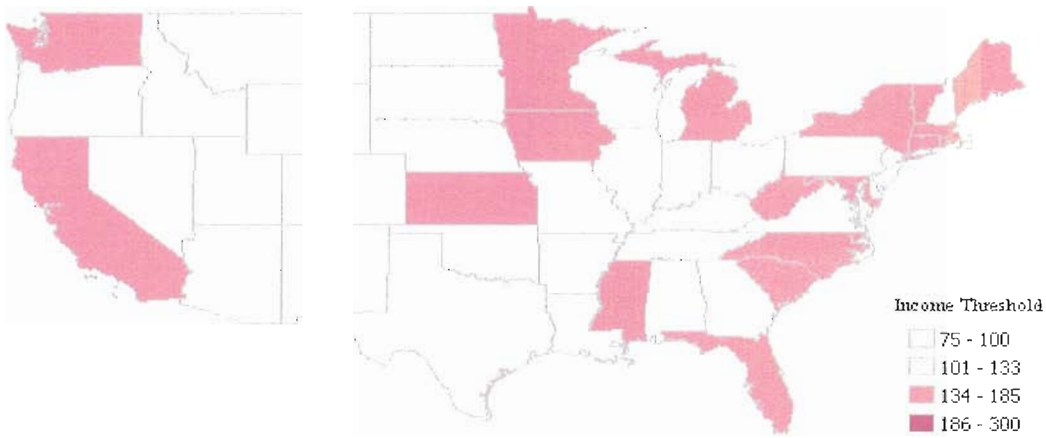
Medicaid eligibility was initially linked to the Aid to Families with Dependent Children (AFDC) program. Generally, AFDC was only available to single-parent families, and in some states the income threshold cutoff for eligibility was quite low. For example, in North Carolina the AFDC income threshold was set at 29 percent of the federal poverty standard in 1984, covering only a small fraction of the poor and medically needy (Currie and Gruber, 1996b). Moffit (1992) reported that the stigma attached with applying for welfare prevented otherwise eligible people from seeking AFDC, and thereby qualifying for Medicaid coverage. These limitations were partially offset by state-specific Medically Needy and Ribicoff programs which, respectively, allowed for 1) netting out medical expenses for income eligibility calculations, and 2) extending eligibility to two-parent households. Nonetheless, attachment of Medicaid eligibility to AFDC program limited access to Medicaid for many poor families.

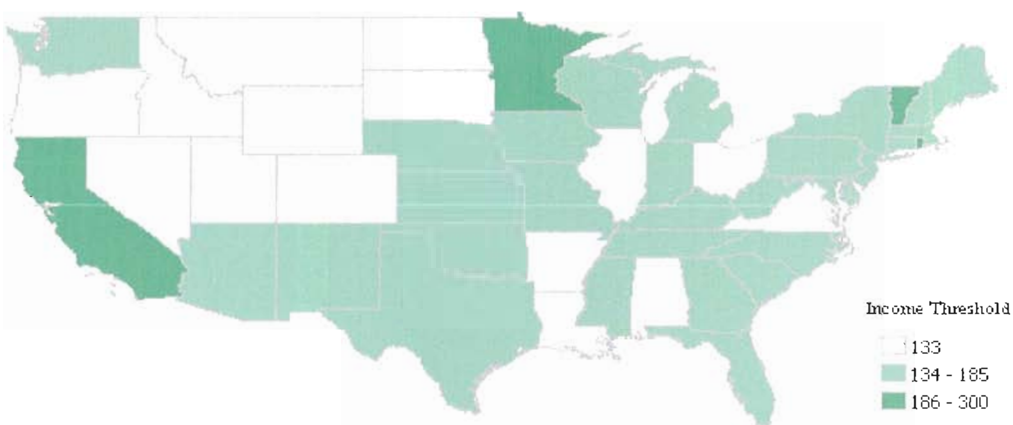
Following the Deficit Reduction Act of 1984, the tie between AFDC and Medicaid eligibility was successively weakened. Accompanying this de-linkage was a series of federal standards for Medicaid eligibility, lifting the income threshold for coverage of low income pregnant women. Initially, these standards were provisional and non-binding. The 1986 OBRA legislation granted states the *option* to provide care up to 100 percent of the poverty line, and a year later, this optional threshold was expanded to up to 185 percent. The 1988 Medicare Catastrophic Coverage Act (MCCA) *required* states to adopt a threshold of at least 100 percent of the poverty line, with a two-year phase in period. The required threshold was increased to 133 percent in 1990, while optional thresholds were expanded to 300 percent in 1997.[1] (Gruber, 2000)

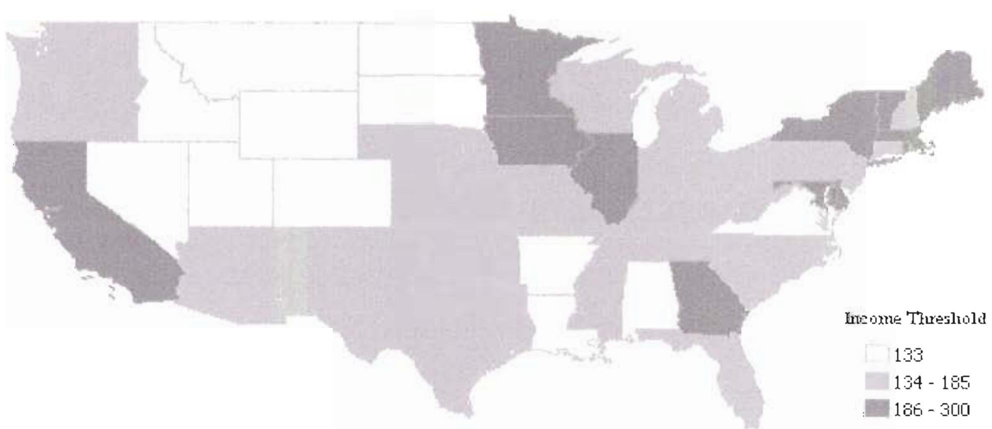
States responded differentially to these federal mandates – both in terms of the timing of implementation and the generosity of the income threshold. Figure 1 shows adopted threshold levels by state in 1989, 1995 and 2001, after most of the state-wide expansions had been implemented. Higher eligibility thresholds were adopted in the Northeast and Southwest, while many states in the Northwest, Central and Southern United States were less quick to respond, and imposed generally lower income thresholds.

[1] Although the MCCA was repealed in November of 1989, the 133 percent threshold mandate was carried on by the 1989 Omnibus Budget Reconciliation Act.

Figure 1: Medicaid Eligibility as a Percent of Federal Poverty Line 1989, 1995, and 2001







1.1. Coverage and Health Outcomes

A number of studies have indicated that Medicaid coverage, at least relative to a lack of insurance altogether, does increase utilization and improve health outcomes, particularly infant health outcomes. The uninsured are less likely to seek hospital care, and exhibit worse health outcomes (Kasper, 1986; Short and Lefkowitz, 1992). However, ascribing a causal effect to these findings is complicated by the fact that the uninsured differ from the insured in a number of ways that are correlated with both utilization and health outcomes. For example, the uninsured are more likely to be less educated and of lower incomes, both of which are presumably negatively correlated with health care utilization and good health. Moreover, health status and insurance coverage are correlated as well; those knowingly in need of medical care have an incentive to seek out insurance, which is the basis of much of the adverse selection literature.

To address these issues, several studies have isolated the effect of Medicaid policy changes within a single state before and after policy implementation. Piper et al. (1990) studies Tennessee's 1985 extension of Medicaid to low income married women. Haas, Udarhelyi, and Epstein (1993) examine the changes in the use of prenatal care and infant birth outcomes surrounding the 1985 Healthy Start program in Massachusetts, which provided health coverage to uninsured pregnant women with incomes at or below 185 percent of the federal poverty line. Epstein and Newhouse (1998) focus on expansions in coverage to women in California and South Carolina in 1989 using a linked dataset of hospital discharges and birth registries.

These studies fail to identify significant improvements in infant health outcomes as a result of Medicaid eligibility expansions. Gruber (2000) posits that the failure to find a significant effect on infant health in state-specific studies is due to two primary reasons. First, the prenatal care literature has advocated first-trimester care as the most important for improving fetal development. Creasy, Gummer, and Liggins (1980) found that over 60 percent of preterm births (the leading cause of low birth-weight deliveries) could have been identified with an initial prenatal care screening. Furthermore, relying on results from a series of clinical trials, the Institute of Medicine's 1985 report found that providing appropriate prenatal care, including screenings of complications associated with preterm delivery, could reduce the incidence of low birth-weight by 20 percent.

Even though economists have cautioned that self-selection may bias estimated effects in health outcomes, economic-based studies have still found a (smaller) effect of prenatal care use in decreasing the incidence of low birth weight (Rosenzweig and Schultz 1982, 1983, and 1988; Corman, Joyce, and Grossman 1987; Grossman and Joyce 1990; and Frank et al. 1991). However, given low take up of Medicaid and associated prenatal care by recently eligible women, it should not be surprising that use of prenatal care is often delayed until after the first, and sometimes well into the third, trimester. Piper (1990) found that more than two-thirds of eligible women enrolled in Medicaid after the first trimester, and a sizable 30 percent waited until after 30 days prior to expected delivery.

The second reason for lack of apparent effects on infant health relates to the nature of the design of these early studies. Since they focus on patterns before and after a policy change within a given state, these studies impose the identifying restriction that there were no other trends within a given state over time that were correlated with prenatal care use and infant health. For example, real incomes were declining over the 1980s, which is most likely correlated with prenatal care use, as well as fetal health. Moreover, state specific responses in utilization and efficacy of care may or may not be representative of the effects of national Medicaid policy as a whole.

Currie and Gruber (1996a) examine the effects of Medicaid expansions on prenatal care use and infant outcomes, while Currie and Gruber (1996b) study similar outcomes for child heath care use and associated outcomes. Exploiting cross-sectional variation across multiple states over time, they are able to control for national trends over time, and non-varying state-specific characteristics, as well as claim nationally representative results. They report that the average 30 percentage point increase in eligibility for Medicaid between 1979 and 1992 led to a 1.9 percent reduction in the probability of low birth weight, and an 8.5 percent reduction in the likelihood of infant mortality, despite prevailing evidence of delay in the trimester in which prenatal care is started. While the low birth weight finding was weakly significant (at the 10 percent level), the lower infant mortality was strongly significant and robust to alternate specifications (including the inclusion of state-specific time trends). Child health was also found to improve due to increased eligibility for children from low income families, such that the average increase in eligibility of 15.1 percentage points from 1984 to 1992 was associated with a 5.1 percent decrease in child mortality.

These findings were critiqued in Kaestner (1999) for failing to control for the size of the poor population in each state. Using individual self-reported data from the National Maternal and Infant Health Survey, Kaestner finds little effect of Medicaid insurance on utilization of prenatal care, or on low birth weight. His criticism of Currie and Gruber (1996a) is perhaps unfounded; to the extent that within-state patterns in the size of the poor population are time-invariant, the fixed effects framework adopted by Currie and Gruber (1996a) would control for the size of the poor population. Even if the size of the poor population within a given state was time-varying, state-specific trends in poor population size would have to be correlated with Medicaid eligibility thresholds within and across states to bias estimated effects of the eligibility measure. Finally, if a correlation existed, it would be unlikely to be a *positive* correlation: in a time of increasing fiscal constraints, it would be unlikely that a state would expand eligibility thresholds when demand for services was increasing. Therefore, to the extent that poor population size and state-imposed Medicaid eligibility are correlated, any induced bias in Currie and Gruber (1996a) estimates may be downward.

On balance, there is some evidence that expansions to Medicaid policy improved infant health, although several studies have suggested that the magnitude and significance of this effect is circumspect (Kaestner, Joyce and Racine 1999, Dubay et al. 2001, and Card and Shoe-Sheppard, 2004). In particular, Card and Shore-Sheppard (2004) use a very similar identification strategy as Currie and Gruber (1996b), applied to data from Survey of Income and Program Participation, the March Current Population Survey, and the Health Interview Survey. These data allow for a richer specification, including state-specific age trends in coverage for children, which are found to lower estimates of Medicaid coverage take-up rates reported in Currie and Gruber by roughly one-half. The most recent supportive evidence is given in Conway and Deb (2005), who show that controlling for the normality versus complexity of pregnancy is important in identifying both significance and the magnitude of the effects of prenatal care on infant outcomes.

1.2. Length of Stay

The literature on length of stay initially used duration of hospital stay as a measure for hospital efficiency. Changes in the late 1990s associated with the Balanced Budget Act

(BBA) of 1997 and Balanced Budget Refinement Act (BBRA) of 1999 lead to a switch in Medicare reimbursement policy: instead of determining reimbursement based upon the average hospital cost of care received, reimbursement rates were prospectively determined based upon specific care received (geographic differences in cost of care were allowed for by region-specific adjustment factors). Younis and Forgione (2008) show that this resulted in shorter length of stay for Medicare patients. Other studies have documented a similar reduction in hospital length of stay for newborn deliveries: between 1980 and 1992, postpartum length of stay for vaginal deliveries declined from 3.9 to 2.1 days on average, while postpartum length of stay for cesarean deliveries decreased from 7.8 days to about 4 days on average (Thilo et al., 1998, and Hyman, 1999).

These reductions in length of stay resulted in questions about the trade-off between efficiency and quality of care. The popular press noted a number of cases where early discharges resulted in preventable subsequent complications and hospitalization (Declercq, 1999, and Eaton, 2001). Between 1995 and 1998, 42 states responded by enacting minimum postpartum length of stay laws. The Newborns' and Mothers' Health Protection Act was adopted in 1996, mandating federal minimum stay requirements, with enforcement commencing in 1998 (Evans et al., 2008).

These requirements were shown to decrease early discharge rates for newborns, and although no impact was found on re-admission rates for privately insured and vaginally-delivered newborns, a significant reduction in re-admission rates for newborns with cesarean delivery was documented (Evans et al., 2008). However, this time period overlaps substantial increases in Medicaid eligibility income thresholds. Whether these eligibility expansions served as a separate channel for an increase in length of stay is an important question. Indeed, Dafny and Gruber (2005) find negative effects on average length of stay associated with increased eligibility expansions to children, which was mediated by an increase in the number of procedures performed. But, existing studies have not addressed whether similar effects are observed for pregnancy-related admissions of adult women. If increases in Medicaid eligibility resulted in primarily inducing coverage and care for the uninsured, it is plausible that expected reimbursement rates for hospitals increased as a result of the eligibility expansions, which may lead to an incentive for hospitals to retain patients longer. On the other hand, if expansions resulted in primarily inducing low-income privately insured individuals, these expansions may have led to a reduction in the reimbursement rate for hospital, which may lead to an incentive to shorten the length of stay for the marginal patient induced onto Medicaid as a result of the state eligibility expansions.

1.3. Maternal Health

While longer length of stay may ameliorate risks associated with early discharge, Medicaid expansions may also affect other maternal outcomes. Gruber, Kim, and Mayzlin (1999) look at how differential incentives provided by reimbursement rates under Medicaid over time affect likelihood of elective cesarean deliveries. Haas et al. (1993) develop a fairly narrow set of measures of maternal health to assess the impact of Medicaid expansions in Massachusetts on maternal health. Their measures include severe pregnancy-related hypertension, placental abruption, and whether or not the mother's hospital stay exceeds the infant's by at least one day, as well as whether cesarean methods were used in delivery. Using a standard difference-in-difference approach, they find no statistically significant change in the inter-payer difference in adverse outcomes relative to women with private insurance for either uninsured or Medicaid patients. They do find a reduction in the gap of cesarean deliveries for both uninsured and Medicaid mothers relative to private-payer patients.

A more recent study by Conway and Kutinova (2006) expands the set of maternal measures to include weight of the mother before and after contraception and delivery, as well as a measure for excessive hospitalization similar to Haas et al. (1993). They employ both two-stage least squares and bivariate probit techniques to model endogeneity and selection of prenatal care and health status, as well as stratify results by parity (whether or not the pregnancy is the woman's first pregnancy), race, and high school completion. They find that receiving timely and adequate prenatal care may be effective in maintaining a healthy weight after birth, and for African Americans, a slight reduction in the probability of excessive length of hospitalization associated with delivery.

Given the observed low take up rates, crowd out, and generally delayed use of prenatal care by new Medicaid enrollees[2], maternal health may or may not be affected by Medicaid policy expansions. While there is some evidence that the type of hospital care

[2] See Gruber (2000) for an exposition on crowd-out, take-up and prenatal care use literatures.

received can be affected both by insured status and relative reimbursement rates, differential health outcomes may be expected, but primarily for complications which can be avoided through prenatal care sought at any stage of pregnancy, up to even the last month of gestation. It is well documented that infectious diseases lead to a number of complications during child-birth – both for the mother and child (American Pregnancy Association, 2003). Given that diagnosis procedures and treatment for these diseases are readily available and relatively inexpensive to administer, increased access to prenatal care at any stage of gestation will likely result in improved health outcomes for both mother and child.[3]

2. Data and Methodology

For the analysis, I use data from the Health Care Utilization Project (HCUP) which includes the universe of all discharges from a representative sample of hospitals in 12 states between 1989 and 2001.[4] These data provide limited demographic information of each patient, admission-specific information (e.g. diagnosis codes, length of stay, and number of procedures), total hospital charges, and hospital-specific information (e.g. hospital size, teaching status, rural or urban location, and ownership control status).[5] State Medicaid income thresholds for eligibility of pregnant women are drawn from the National Governor's Association's "State Coverage of Pregnant Women and Children," Updates for 1990 through 2001.[6]

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[3] I was not able to establish an effect of the policy expansions on low birth-weight.

[4] Data are available for 16 states. I include data from Arizona, California, Colorado, Florida, Iowa, Massachusetts, Maryland, New Jersey, New York, Pennsylvania, Tennessee, and Wisconsin. Race/ethnicity information is not reported by Georgia, Illinois, Oregon and Washington. Given that race/ethnicity is an important covariate, I opted to retain only states which provide this information. See Appendix A for table showing the number of admissions by state by year.

[5] Preliminary descriptive statistics showed measurement error in HCUP hospital ownership data. For example, after 1998, many of the hospitals in several of the states omitted ownership from these data altogether, while in other years and other states, reported ownership was verified to be incorrect. Therefore, ownership was collected from a variety of sources. For 1988 through 1999, the National Bureau of Economic Research's (NBER) Prospective Payer System ownership control data were mapped to hospitals within the HCUP database, and supplemented with the American Hospital Association's Guide to the Health Care Field for 1992, 1998, and 2001. NBER data were derived from the Center for Medicaid and Medicare (CMS) database for hospital reimbursement through Medicare. For 2000 and 2001, hospital ownership control data were directly retrieved from the CMS system.

[6] January 1990 values are used for the 1989 year.

The central question of this study is whether increased provision of publicly-provided health coverage results in improved outcomes. However, identification of an effect of Medicaid coverage on either 1) longer/shorter length of stay, or 2) improved health outcomes for the mother requires isolating an effect of increased Medicaid coverage that is exogenous to other (potentially unobservable) characteristics or conditions which may affect length of stay and/or infection incidence. Medicaid recipients are generally poorer. As low economic status is strongly correlated with education and other observable and unobservable characteristics, persons qualifying for Medicaid are presumably, on average, less likely to be knowledgeable about disease risks and more likely to lack access to services or lifestyle amenities which are correlated with health (nutrition, housing conditions, hygiene, etc.). Alternatively, given low reimbursement rates, Medicaid may be perceived as lower quality coverage leading to sorting by illness status, or hospitals may be inclined to shorten the length of hospital stay.

Expansions to the income eligibility thresholds across states and over time are presumably exogenous to unobservable characteristics and conditions which jointly affect coverage and health outcomes. Since Medicaid expansions differed in timing and magnitude across states, state fixed effects can be used to control for all non-time-varying state-specific effects, while time fixed effects can control for time-varying trends common to all regions. Under the assumption that any state-specific time-varying trends which may be jointly correlated Medicaid coverage and health outcomes are uncorrelated with the exogenous state-year specific policy trends in Medicaid eligibility, income thresholds by state and year are an obvious instrument.[7]

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[7] If Medicaid recipients are poorer and therefore more or less likely to present illness, these unobservable characteristics may be considered by states in setting in Medicaid policy. While it may be that states differ in their tolerance for public provision of benefits to the poor, the level effect of this variation can be controlled for with state fixed effects. If legislative generosity becomes more or less generous over time depending upon perceived need, inclusion of additional covariates is needed; this is the justification for inclusion of the percent poor and unemployment rate discussed below. Alternative identification strategies offered in the literature have imposed a difference-in-differences estimator across otherwise similar states, but for changes to the Medicaid policy. As discussed above, these studies are limited by the potential for an unidentified trend shared across the states which is jointly correlated with coverage and health. The identification strategy used in this study benefits from identification off of variation across a large number of states over time, reducing the likelihood of spurious effects. It could be argued that relatively generous states for eligibility are relatively more or less generous with reimbursement, and reimbursement is correlated with outcomes. Again, the trend in reimbursement would have to correlate across states and over time. Gruber (2000) indicates a substantial degree of variation in Medicaid reimbursement rates across states, but no suggestion that reimbursement would be correlated with eligibility. A simple correlation of physician reimbursement at delivery with Medicaid generosity shows a very weak correlation for 1998 (-0.17). Additional work needs to be done to determine to

Following Currie and Gruber (1996a,b), a simulated instrument is used to capture "legislative generosity," rather than the income eligibility threshold itself. The instrument is designed to capture variation in the legislative generosity of Medicaid eligibility thresholds in each state and year. Specifically, I compute for each state the proportion of all child-bearing women in the Current Population Survey for a given year that would be eligible for Medicaid under the state's income threshold for that year. This instrument allows for variation in the *generosity* of state Medicaid eligibility standards, while avoiding the variation in actual Medicaid eligibility that is driven by state demographics and local business cycles.[8]

If legislative generosity is correlated with incomes and economic cycles, this may not alone be a valid instrument. Thus, I follow the suggestion in Kaestner (1999) by including controls for the proportion of the population at or below the federal poverty standard and the unemployment rate for each state in a given year, to control for underlying trends in each state's economy which may be correlated with both coverage and health, as well as legislative generosity. Hospital fixed effects control for any time-invariant neighborhood characteristics, such as race/ethnicity, socio-economic status, education, housing value, and household size.[9] Year fixed effects control for national trends, such as macroeconomic business cycles.

I employ a two-stage approach to evaluate first the impact of Medicaid eligibility expansions on Medicaid coverage, and second, given increased Medicaid coverage as a result of eligibility expansions, improved health outcomes. The first stage specification is as follows for individual hospitalizations *i*, in hospital *j*, state *k*, and year *t*:

$$C_i = \alpha + \beta Z_{kt} + \delta X_{ij} + \eta Y_{kt} + \theta H_{jk} + \varphi T_t + \mu_i, \qquad (1)$$

where C_{ikt} is one of three binary variables for whether an individual hospitalization, in a given state and year, was paid for under Medicaid, private insurance, or was classified as

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what extent reimbursement may affect length of stay, particularly controlling for changes in state minimum length of stay requirements over the time period.

[8] This measure has some important limitations. In particular, the CPS does not provide asset information, and therefore state asset requirements cannot be included in eligibility calculations. NGA reports indicate that while a number of states had removed asset requirements by 1989, California, Colorado, Illinois, and Iowa had not.

[9] See Rosenzweig, et al. (1982) for a comprehensive list of factors effecting fetal health.

uninsured. The key independent variable measuring Medicaid legislative generosity is Z_{kt}, which varies across states and over time. To control for hospital and individual characteristics, X_{ij} includes a series of individual and hospital-specific covariates, including race and age of the patient, size of hospital, whether the hospital is designated as urban or rural, ownership control status of the hospital, and teaching status of the hospital. Y_{kt} captures state-year specific controls, including the percent of state *k*'s population at or below the federal poverty standard in year *t*, and the percent of the state *k*'s population unemployed in year *t*. Hospital and year fixed effects are included as H_{jk} and T_t.[10] I control for state fixed effects via inclusion of hospital fixed effects, since hospitals reside in a fixed location.

The second stage specification for maternal health outcomes and length of stay are modeled as follows:

$$O_i = \gamma + \varsigma C_i + \vartheta X_{ij} + \tau Y_{kt} + \upsilon H_{jk} + \omega T_t + \varepsilon_i, \qquad (2)$$

where O_i is 1) an indicator variable for whether hospitalization was accompanied by an infectious disease related diagnosis code for the mother, or 2) a zero-truncated variable indicating the length of stay of hospitalization *i* in days. C_i is the first-stage predicted likelihood of being on Medicaid as a result of exogenous changes in Medicaid generosity. Covariates, as well as hospital and year fixed effects are specified similarly to first stage coverage regressions. Finally, for length of stay, a series of disease ICD9 classification code fixed effects are included, to control for differential length of stay by illness type.

To remove the effect of outliers, I limited the data to observations from hospitals with 1000 or more pregnancy-related admissions over the entire 13 year sample period. This reduced the sample from approximately 5.5 to 5.3 million pregnancy-related hospitalizations in the data set. Due to computational limitations, I drew a random sample of 3 million observations. Further, to ensure inclusion of only pregnancy-related admissions, all observations with pregnancy diagnoses, but classified as male hospitalizations were omitted, as well as outlier length of stay records reporting greater than a month hospital stay.

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[10] To examine differential effects of Medicaid generosity on Medicaid coverage, I interact hospital-specific variables for size, ownership, teaching status and location with the generosity variable *Zkt* to differentiate generosity effects by type of hospital.

3. Descriptive Statistics

3.1. Coverage

Table 1 presents descriptive statistics for the primary variables considered. On average, 36 percent of pregnancy-related hospitalizations over the sample period were covered under Medicaid. The average length of hospital stay was 2.4 days, with a considerable degree of variation. Infections were present in 2.6 percent of cases. Of hospitals which report race and ethnicity information, 15 percent of hospitalizations were African American, and 20 percent were of Hispanic origin.[11] Roughly 18 percent of the total population was below the poverty line, while the average unemployment rate was 6 percent.

Table 1: Descriptive Statistics

	Obs	Mean	Std. Dev.	Min	Max
Medicaid-payer	2,993,458	0.359	0.480	0	1
Private-Insured	2,993,458	0.562	0.496	0	1
Uninsured	2,993,458	0.059	0.236	0	1
Length of Stay	2,993,458	2.391	1.810	0	31
Infection	2,993,458	0.026	0.158	0	1
Coverage	2,993,458	0.349	0.068	0.158	0.542
For-profit Hospital	2,993,458	0.111	0.314	0	1
Non-profit Hospital	2,993,458	0.752	0.432	0	1
Government Hospital	2,993,458	0.137	0.344	0	1
Black	2,993,458	0.147	0.354	0	1
Hispanic	2,993,458	0.198	0.398	0	1
Percent in Poverty	2,993,458	0.182	0.049	0.082	0.269
Unemployed	2,993,458	0.060	0.018	0.021	0.106

Source: HCUP and CPS. Universe of all pregnancy related admissions in 16 states over 13 years. Dummy variables are presented without standard deviation.

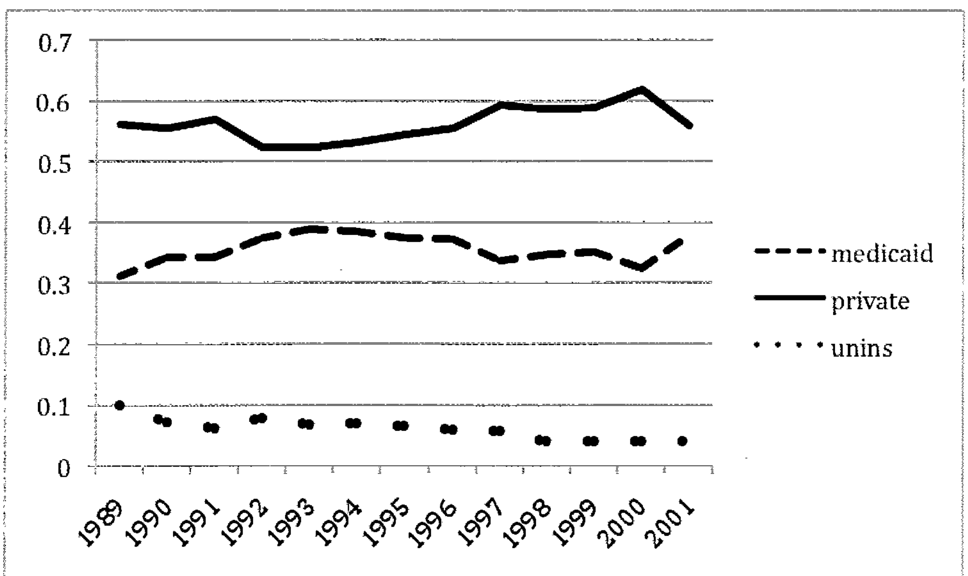
Unlike other studies, where it is possible to observe those who choose not to utilize health care services in addition to those that do, this study observes the choice of medical coverage conditional on use; that is, I observe hospital admissions and payer types. The first stage model for coverage therefore, is confined to a compositional analysis of payer type across hospital type, given changes to Medicaid policy.

[11] These figures indicate some sample selection of the hospitals which report race; higher than average proportions of Hispanics and African Americans indicate hospitals residing in diverse neighborhoods are more likely to report race/ethnicity, when these reporting requirements are not mandatory.

I present descriptive statistics for coverage and length of stay variables over time in Figures 2 and 3. Overall, Medicaid payer pregnancy-related hospitalizations increase from 29 percent in 1989 to nearly 40 percent of pregnancy-related hospitalizations in 1993. There is a slight decline, accompanied by an increase in private insurance hospitalizations through the mid-1990s, which is most likely associated with macroeconomic cycles. Throughout the period, there is a steady decline in the number of uninsured hospitalizations.

Length of stay by payer type exhibits a more erratic pattern. In general there is a decline in the length of stay for all three groups through 1995, followed by an increase for both private-payer and Medicaid patients. However, Medicaid and private-payer length of stay remain between 2 and 2.5 days by the end of the period, while length of stay for the uninsured was slightly lower on average for the uninsured. Note that some of these changes in length of stay may reflect compositional changes in addition to behavioral changes.[12]

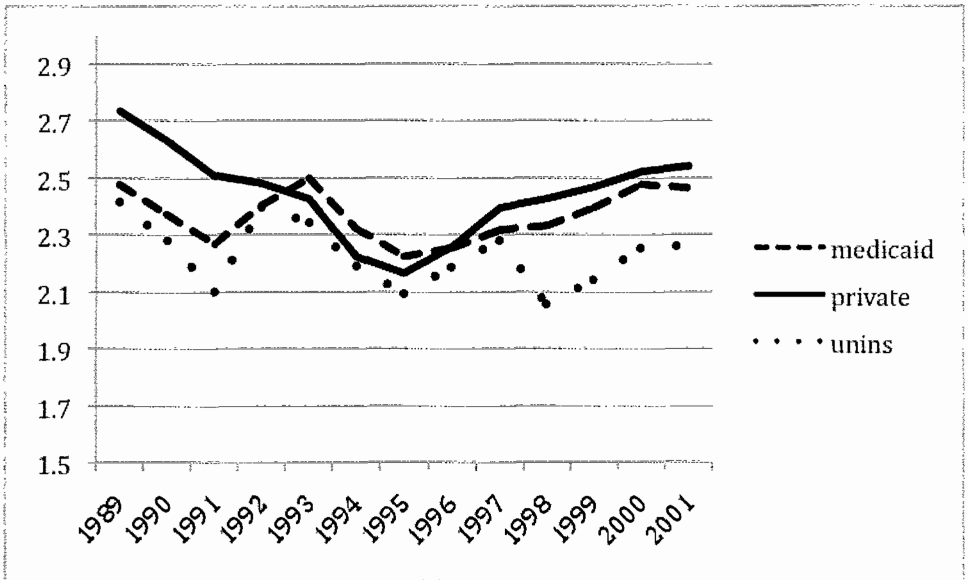
Figure 2: Trends in Medicaid, Private Insurance, and Uninsured Hospitalizations



Source: HCUP database.

[12] It should be noted that these patterns in length of stay are roughly in line with the timing of state and national minimum stay requirements. The more erratic pattern for the uninsured probably reflects the attachment of many of these mandates to *insurance providers* rather than the hospitals themselves.

Figure 3: Trends in Length of Stay for Pregnancy-Related Hospitalizations



Source: HCUP database.

Expansions in Medicaid eligibility income thresholds and coverage rates for pregnant women are presented in Table 2. Income thresholds adopted at the state level on average increase substantially in the first four years from 133 percent in 1989 to 164 percent of the federal poverty standard in 1992. The increase in the eligibility cut-off increases less rapidly there after, and by 2001 was nearly 200 percent of the poverty standard. Generosity of state Medicaid coverage increased from 24 percent in 1989 to just under a third of population of the child-bearing female population (15-44 years of age) in 1992, and gradually increased to a peak of 40 percent in 1998.

While Medicaid eligibility became increasingly generous over time, average utilization of Medicaid for pregnancy related hospitalization leveled off to 37 percent of hospitalizations after 1992. Therefore, although many more women were eligible to receive Medicaid hospital care following 1992, expansions to coverage beyond a certain proportion of the population ceased to induce greater Medicaid coverage. This suggests, *ceteris paribus*, that take up rates for women affected by later expansions may be close to zero, which is consistent with the broad literature suggesting publicly provided Medicaid coverage is relatively less attractive than privately administered coverage for those sufficiently able to pay for private insurance.

Table 2: National Trends in Medicaid Income Thresholds and Eligibility

	Income Threshold (% FPL)	Simulated Income Eligibility
1989	133%	24%
1990	155%	29%
1991	160%	31%
1992	164%	33%
1993	170%	35%
1994	170%	36%
1995	172%	35%
1996	172%	36%
1997	173%	38%
1998	189%	40%
1999	189%	39%
2000	193%	38%
2001	197%	37%

Source: NGA State Coverage of Pregnant Women and Children," MCH Update, selected years.
Income threshold represents average of yearly income thresholds used for eligibility across all states included in the HCUP database.
Income eligibility rates are yearly averages for the proportion of a nationally representative sample (CPS) of child-bearing-aged women who were income-eligible for Medicaid under state specific rules.

3.2. Hospital Composition

Table 3 shows that the Medicaid expansions had differential impacts on the patient mix at different hospital types. In general, Medicaid recipients are an increasing proportion of all admissions. However, while the proportion of pregnancy-related hospitalizations covered under Medicaid increased at non-profit, for-profit, urban and rural, non-teaching, and large-to-medium hospitals, a decrease was observed for government, teaching, and small hospitals. Notably, the most significant increases are seen between 1989 and 1992, which is consistent with earlier expansions to Medicaid eligibility affecting relatively poorer individuals than later expansions. The largest increases were seen at for-profit, rural, and non-teaching hospitals. For profit hospitals proportion of Medicaid payers increased from 28 percent to 38 percent between 1989 and 1992, rising to 44 percent by 2001. Similarly at rural hospitals, Medicaid-payer admissions rose from 30 percent of all admissions in 1989 to 42 percent in 1992 and 45 percent by 2001. At government hospitals, the share of Medicaid admissions decreased from 58 percent in 1989 to 52 percent in 2001. Similar trends were observed at non-teaching and small hospitals.

Table 3: Composition of Medicaid Payers by Hospital Type

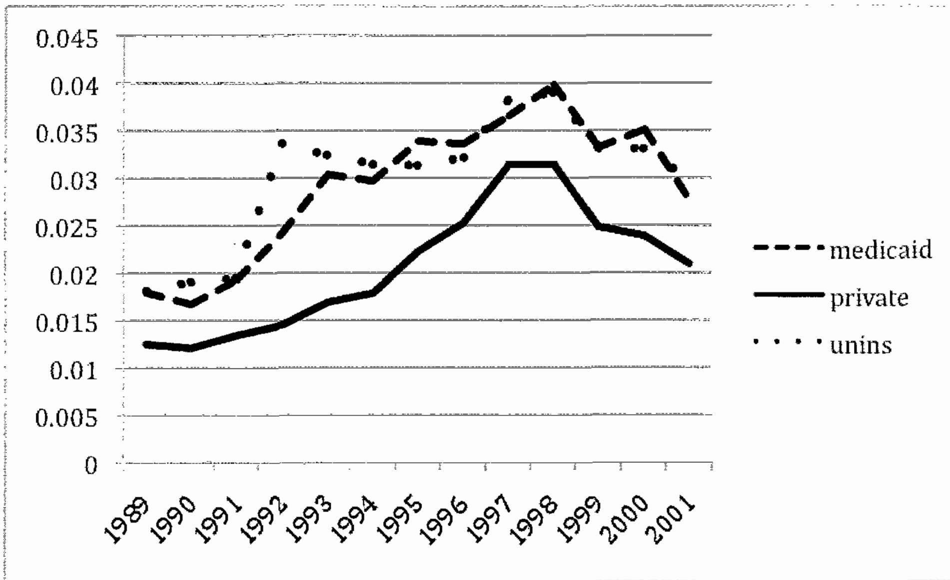
Ownership type	1989	1992	1996	2001
non-profit	24%	33%	34%	35%
for-profit	28%	38%	41%	44%
government	58%	55%	53%	52%
Urban or Rural				
rural	30%	42%	43%	45%
urban	31%	37%	37%	37%
Teaching Status				
teaching	44%	41%	40%	36%
non-teaching	23%	36%	36%	40%
Hospital Size				
large	28%	35%	35%	38%
medium	31%	36%	36%	38%
small	51%	56%	47%	39%

Source: HCUP database, selected years.

3.3. Health Outcomes

The measure for maternal health is given by the incidence of infectious-disease diagnoses associated with pregnancy-related admissions. To my knowledge, it is the first time that this measure of preventable complications has been used in studies of Medicaid. As mentioned earlier, infectious diseases are easily diagnosed and treated with adequate access to prenatal care. However, if left untreated, bacterial infections commonly escalate to complications during child birth, such as pelvic inflammatory disease and ectopic pregnancies, which can cause death for the mother and child. These complications not only increase risks to mother and child; they are quite costly as well. Average hospitalization costs of normal deliveries across all states was $3,590, while hospitalizations with infectious complications was $7,823 (2001$). The time trend over the sample period of infectious disease-related diagnoses and related complications is presented in Figure 4.

Figure 4: Incidence of Infection in Pregnancy-Related Hospitalizations



Source: HCUP database, selected years.

There is a markedly higher incidence in infectious disease rates between 1989 and the peak incidence in 1998 (1.8 percent of admissions versus 3.6 percent). Incidence was even more remarkable for Medicaid and uninsured hospitalizations, reaching a peak in 1998 of just over 4 percent.[13] This increase in the incidence of infectious disease-related hospitalizations among pregnancy-related admissions was primarily attributed to an increase in non-specific (not otherwise specified, or not elsewhere classified) infectious diseases, and the incidence of Pelvic Inflammatory Disease. Incidence of both gonorrhea and syphilis are generally constant or declining over the sample period.

However, the trend in infection rates varied across states. In states with increasingly generous eligibility requirements, the growth rate of infection and low birth was lower than in states with relatively less generous eligibility expansions. Table 4 presents a comparison of incidence of infectious disease-related admissions to Medicaid generosity between relatively generous states (California, Iowa, and Massachusetts) and relatively less generous states

[13] The reason for the decline and generally lower rates for infection among the uninsured is not clear. Expansions to Medicaid made it virtually impossible for low income mothers not to receive Medicaid coverage, such that if an uninsured and low income mother showed up to the hospital she would be enrolled in Medicaid automatically. Therefore, the lower observed rates for the uninsured could accurately reflect selection by relatively health persons into uninsured status, or the inclusion of "self-insured" in the uninsured classification in HCUP.

(Arizona and Colorado). These descriptive statistics suggest that in relatively generous states, the impact of an outbreak in infection was mitigated through policy intervention, a hypothesis I will investigate in the next section.

Table 4: Infection Incidence and Medicaid Generosity Over Time

	Infection Incidence		Coverage Rates	
	Low Generosity AZ, CO	High Generosity CA, IA, MA	Low Generosity AZ, CO	High Generosity CA, IA, MA
1989	NA	1.44%	12%	31%
1990	NA	1.40%	22%	31%
1991	NA	1.53%	23%	33%
1992	NA	1.52%	24%	33%
1993	1.52%	2.02%	24%	34%
1994	2.91%	2.11%	25%	35%
1995	2.57%	2.53%	25%	37%
1996	2.54%	2.32%	26%	37%
1997	4.58%	2.40%	25%	37%
1998	2.19%	2.09%	24%	50%
1999	2.68%	1.63%	23%	48%
2000	3.21%	1.89%	23%	48%
2001	2.43%	1.80%	22%	48%

Source: HCUP Database and MCH Updates
AZ did start reporting to HCUP until 1995; CO did not start reporting race/ethnicity until 1993. Since I include controls for race and ethnicity in all regressions, infection incidence for these states is omitted here from 1989 through 1992.

4. Results

4.1. Insurance Coverage and Length of Stay

As mentioned in the literature review, toward the end of the sample period (1998 onward) federal minimum length of stay requirements were imposed on all states, and during much of the middle of the sample period, there were state-by-state expansions in minimum length of stay laws. To the extent that these expansions across states and over time were correlated with the state-by-year legislative generosity, estimation of an effect of generosity on length of stay could be biased. I present results here for length of stay with the heavy-handed caveat that causal inference is not warranted.[14]

With that said, results from first-stage coverage regressions are presented in Table 5. Column 1 shows OLS results for the estimated effect of Medicaid expansions on the probability of Medicaid coverage for hospital services is positive and significant at the 5 percent level. The point estimate suggests that a 100 percentage point increase in the proportion of the population eligible for Medicaid results in a 7.8 percentage point increase in the share of hospital services paid for by Medicaid. This effect is relatively small; given an average increase in eligibility of 15 percentage points, coverage rates on average increased by 1.2 percentage points as a result of more generous policy. These expansions are accompanied by decreases in private-payer and uninsured coverage (Column 2 and 3).[15] While the estimates are statistically insignificant for private-payer and uninsured coverage, the negative point estimates suggest that, on average, some of the increased coverage stems from a decrease in the uninsured, while some is due to crowd-out.

Columns 4-7 show differential impacts of eligibility expansions across hospital type. In general, the largest effects were at small, non-profit and for-profit, and urban hospitals. A 100 percentage point increase in the proportion of the population eligible for Medicaid results in a 29 percentage point increase in Medicaid share at small hospitals, an 8 to 16 percentage point increase at non-profit and for profit hospitals, and a 9 percentage point

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[14] Future work is necessary to isolate an effect of generosity, holding constant these changes in length of stay standards, before length of stay results are purported to be causal. Here I merely highlight the statistical relationship present in the data available. Results should be taken as suggestive of a relationship, but not indicating that policy aims to extend Medicaid eligibility induced longer length of stay for pregnancy admissions. There is certainly a financial incentive which may lead to longer length of stays; however, the Medical field, in principle, should not retain patients for longer or shorter periods purely based upon financial incentives.

[15] Although the standard errors are quite large, the point estimate for crowd-out (a reduction in private-payer coverage resulting from Medicaid expansions) is negative and similar in magnitude to the results reported by Gruber and Simon (2007). The crowd out literature has stressed the importance of acknowledging the family effects of crowd out; however, the nature of the HCUP abstract data does not allow for more precise modeling of individual choice. Also, these estimates show the effect of coverage contingent upon use of hospital services. Therefore, differential impacts of crowd out might be predicted if privately insured individuals have different utilization patterns for hospital vs. preventative/clinic care. The precise magnitudes, however, are uncertain.

increase at urban hospitals. Given an average increase in eligibility of 15 percentage points over the sample period, expansions to Medicaid eligibility resulted in an increase in Medicaid coverage share of 4.4, 1.2, 2.4, and 1.4 percentage points at small, non-profit, for-profit, and urban hospitals, respectively. African Americans and Hispanics are more likely to be covered by Medicaid for hospital services, and Medicaid coverage is greater on average in states, and in times, when a greater proportion of the population has incomes below the federal poverty line. Finally, age has a negative effect on the likelihood of Medicaid coverage.

Table 6 presents results from the length of stay specifications. Both reduced form and instrumental variables models show that expansions to Medicaid eligibility resulted in a longer length of stay for new enrollees on Medicaid. Column 2, for the reduced form model, shows that a 100 percentage point increase in the proportion of the population eligible for coverage results in an increase of about 0.9 days. Instrumental variable models (column 4-5) suggest that the that the length of stay for the marginal person induced onto Medicaid coverage as a result of the increase in legislative generosity was 12 days longer. Certainly, the magnitude of the coefficient is unlikely.[16] However, the sign does suggest that length of stay for the marginal person induced onto Medicaid coverage was longer than it would have been without increased legislative generosity. In terms of the reduced form effect, the observed increase in generosity resulted in nearly a 5.5 percent increase in the length of admission stay relative to the average stay of 2.4 days. The reduced form effect across ownership type suggests larger effects at government hospitals than at either non-profit or for-profit hospitals.

[16] Large changes in magnitude between reduced form and instrumental variables specifications are commonly associated with nonlinear relationships in the data. Although accommodating nonlinear effects within a linear two stage least squares model is possible, doing so imposes strong assumptions on the functional form of the estimator. Alternative approaches could be explored into non-linear least squares estimation, but were deemed beyond the scope of this study. Future work should investigate the extent to which non-linear effects in legislative generosity on length of stay contributes to the magnitude of the coefficient.

Table 5: Medicaid Expansions and Insurance Coverage by Hospital Type

	(1) Medicaid	(2) Unins	(3) Private	(4) Medicaid	(5) Medicaid	(6) Medicaid	(7) Medicaid
Medicaid Generosity	.07803	-.03903	-.0438				
	(2.51)**	(1.46)	(0.89)				
* Large Hospital				.01641			
				(0.46)			
* Medium Hospital				.1211			
				(2.58)**			
* Small Hospital				.2887			
				(3.85)***			
* Non-profit					.07757		
					(2.15)*		
* For-profit					.1547		
					(3.75)***		
* Government					.01068		
					(0.30)		
* Teaching						.08081	
						(1.69)	
* Non-teaching						.07625	
						(1.71)	
* Urban Hospital							.08808
							(3.19)***
* Rural Hospital							-.1106
							(0.96)
Black (1=African American)	.1766	-.006016	-.1735	.1767	.1766	.1766	.1766
	(16.11)***	(0.79)	(13.47)***	(16.11)***	(16.11)***	(16.09)***	(16.12)***
Hispanic (1=Hispanic origin)	.1617	.02639	-.1799	.1618	.1617	.1617	.1616
	(9.94)***	(1.53)	(10.58)***	(9.93)***	(9.94)***	(9.94)***	(9.94)***
Age in years at admission	-.06667	-.001977	.0675	-.06668	-.06667	-.06667	-.06667
	(11.48)***	(2.07)*	(10.97)***	(11.49)***	(11.48)***	(11.48)***	(11.47)***
Age squared	.0008765	.0000311	-.0008853	.0008766	.0008764	.0008765	.0008764
	(10.78)***	(2.46)**	(10.18)***	(10.79)***	(10.78)***	(10.78)***	(10.77)***
Percent below poverty line	.3898	-.3006	-.06833	.3795	.3866	.3899	.3807
	(2.45)**	(1.87)*	(0.65)	(2.35)**	(2.46)**	(2.46)**	(2.49)**
Unemployment Rate	-.1493	-.0992	.2351	-.1567	-.1478	-.149	-.1538
	(0.63)	(0.52)	(0.78)	(0.65)	(0.62)	(0.63)	(0.65)
Constant	1.295	.189	-.5141	1.28	1.295	1.295	1.293
	(10.66)***	(6.34)***	(4.25)***	(10.20)***	(10.75)***	(10.42)***	(10.61)***
Observations	2993458	2993458	2993458	2993458	2993458	2993458	2993458
R-squared	0.33	0.11	0.37	0.33	0.33	0.33	0.33

Robust t statistics in parentheses (clustered at the hospital state level)

All models regress indicator variables for whether the hospitalization was paid for by Medicaid, private insurance, or was uninsured, on Medicaid Generosity and covariates. Medicaid Generosity reflects the proportion of the U.S. population in a given year which would be eligible for Medicaid in a specific state in a given year under each state's income eligibility threshold.

All models include fixed effects for each hospital, controls for hospital type (to control for changing mix of hospitals and hospital reclassification), and year fixed effects.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Length of Stay by Hospital Type

	(1) OLS	(2) RF	(3) RF Interactions	(4) IV	(5) IV Interactions
Medicaid (1=Medicaid-payer hospitalization)	.03226			11.84	
	(2.37)**			(8.74)***	
* Non-Profit					9.364
					(8.85)***
* For-Profit					10.48
					(8.94)***
* Government					17.4
					(6.25)***
Medicaid Generosity		.8821			
		(2.67)**			
* Non-Profit Hospital			.6947		
			(1.91)*		
* For-Profit Hospital			1.103		
			(5.05)***		
* Government Hospital			1.367		
			(5.06)***		
Black (1=African American)	.1566	.1621	.162	-1.876	-1.61
	(11.11)***	(10.36)***	(10.36)***	(8.04)***	(8.18)***
Hispanic (1=Hispanic origin)	-.011	-.006504	-.006297	-1.889	-1.61
	(0.74)	(0.42)	(0.41)	(8.76)***	(9.07)***
Age in years at admission	-.02821	-.03047	-.03048	.7665	.6749
	(10.09)***	(9.46)***	(9.47)***	(8.40)***	(8.60)***
Age squared	.0006541	.0006843	.0006844	-.009792	-.008616
	(14.73)***	(13.44)***	(13.47)***	(8.16)***	(8.31)***
Percent below poverty line	.6567	.8506	.7649	-3.908	-3.599
	(0.91)	(1.02)	(0.93)	(6.76)***	(6.78)***
State Unemployment Rate	-.7172	-.7818	-.707	1.678	1.511
	(0.53)	(0.47)	(0.43)	(3.12)***	(2.99)***
Weekend (1=Weekend admission)	-.07587	-.07448	-.07447	-.1219	-.1147
	(27.47)***	(22.28)***	(22.30)***	(13.57)***	(14.16)***
Constant	2.227	2.041	1.963	-12.08	-12.23
	(15.93)***	(9.79)***	(11.08)***	(7.21)***	(7.96)***
Observations	2993458	2993458	2993458	2993458	2993458
R-squared	0.21	0.21	0.21		

Robust t statistics in parentheses (clustered at the hospital state level for RF specification, and using White's method for IV models).

All models regress length of stay on Medicaid Generosity and covariates. Medicaid Generosity reflects the proportion of the U.S. population in a given year which would be eligible for Medicaid in a specific state in a given year under each state's income eligibility threshold.

All models include fixed effects for each hospital, controls for hospital type (to control for changing mix of hospitals and hospital reclassification), and both diagnosis code and year fixed effects.

* significant at 10%; ** significant at 5%; *** significant at 1%

4.2. Health Outcomes

Table 7 presents results for models for the incidence of bacterial infection in pregnancy-related hospitalizations. In both reduced form and instrumental variables models, Medicaid expansions are found to reduce the probability of infectious disease-related admissions. The weakly significant reduced form estimate suggests that a 100 percentage point increase in the proportion of the population eligible for Medicaid results in a 4.1 percentage point reduction in the probability of infection. The strongly significant instrumental variable point estimate shows that the marginal person covered by Medicaid as a result of the policy expansion exhibited a 68 percentage point lower rate of infection. Certainly, the magnitude of the IV coefficient is implausibly large given an average infection rate of 2.6 percent across all hospitalizations in the sample.[17] However, the sign is suggestive of a significant reduction in the likelihood of disease at admission. Focusing on reduced form effects, Medicaid eligibility expansions on average contributed to lowering disease incidence by 0.6 percentage points, which, given the average infection rate of 2.6 percent, represents a 24 percent reduction in the rate of infection.[18]

As expected, the OLS model (column 1) indicates that being on Medicaid results in a significantly higher likelihood of disease incidence. As discussed above, these basic results are biased, since a host of other variables pertinent to the likelihood of disease incidence are correlated with the independent variable of interest – being on Medicaid. Instrumental variables identifies only off of the exogenous variation associated with state-by-state expansions to Medicaid eligibility. Thus, although in a descriptive sense, persons receiving care through Medicaid are more likely to present infectious disease complications during delivery, policy expansions enabling more people better access to care through Medicaid results in a lower likelihood of disease than otherwise would be the case.

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[17] Future work will investigate the extent to which non-linear effects in legislative generosity on infection contributes to the magnitude of the coefficient.

[18] These figures are plausible when I consider relative infection rates by insurance source. Those covered on Medicaid are more likely to present infectious disease at hospitalization: 5 percent of Medicaid pregnancy-related admissions were present with infection, compared to 3.4 percent for those covered by private insurance and 4 percent for the uninsured. It is important to note that these estimates assume linear effects of policy expansions, which are presumably non-linear. Early expansions would predictably result in larger effects, while later expansions would predictably result in smaller effects.

Table 7: Eligibility Expansions on the Incidence of Infection

	(1)	(2)	(3)	(4)	(5)
	OLS	RF	RF Interactions	IV	IV Interactions
Medicaid (1=Medicaid-payer hospitalization)	.004104			-.6847	
	(3.10)**			(6.39)***	
* Non-Profit					-.4802
					(8.88)***
* For-Profit					-.4026
					(7.18)***
* Government					-.5628
					(4.40)***
Medicaid Generosity		-.04103			
		(1.91)*			
* Non-Profit Hospital			-.04737		
			(1.83)*		
* For-Profit Hospital			-.04205		
			(1.96)*		
* Government Hospital			-.01689		
			(0.46)		
Black (1=African American)	.02007	.0208	.0208	.1417	.1059
	(9.11)***	(8.95)***	(8.93)***	(7.48)***	(10.45)***
Hispanic (1=Hispanic origin)	.001646	.002341	.00235	.1131	.07953
	(1.27)	(1.63)	(1.64)	(6.52)***	(8.73)***
Age in years at admission	-.001095	-.001365	-.001366	-.04701	-.03346
	(3.12)***	(4.12)***	(4.12)***	(6.57)***	(8.58)***
Age squared	.00002171	.00002525	.00002526	.0006253	.0004472
	(3.97)***	(5.03)***	(5.03)***	(6.65)***	(8.69)***
Percent below poverty line	-.003559	-.01042	-.01299	.2524	.1726
	(0.07)	(0.20)	(0.25)	(5.95)***	(6.71)***
State Unemployment Rate	-.03866	-.03663	-.03424	-.1368	-.1165
	(0.33)	(0.32)	(0.30)	(4.19)***	(4.71)***
Constant	.02658	.04355	.04585	.3658	.4977
	(2.68)**	(6.60)***	(5.77)***	(7.28)***	(8.56)***
Observations	2993458	2993458	2993458	2993458	2993458
R-squared	0.02	0.02	0.02		

Robust t statistics in parentheses (clustered at the hospital state level for RF, and using White's method for IV models).

All models regress an indicator variables for whether the hospitalization accompanied by an infectious disease diagnosis code on Medicaid Generosity and covariates. Medicaid Generosity reflects the proportion of the U.S. population in a given year which would be eligible for Medicaid in a specific state in a given year under each state's eligibility threshold.

All models include fixed effects for each hospital, controls for hospital type (to control for changing mix of hospitals and hospital reclassification), and year fixed effects.

* significant at 10%; ** significant at 5%; *** significant at 1%

5. Robustness

I tested the sensitivity of the results to use of a linear probability model by estimating reduced form probit models for both first state coverage and reduced form infection models. These results are presented in Table 8. Although significance for coverage is reduced, the magnitude and significance for direct effects of legislative generosity on length of stay and disease incidence are similar, suggesting any bias from use of a linear model is relatively small.

As with all time-series cross-sectional analyses, correlated time trends can produce spurious results. I therefore include state-specific time-trend controls. The results from Table 8 show that the magnitude of reduced form estimates for the effect of Medicaid generosity are lower for both infectious disease incidence and hospital length of stay when state-specific time trends are included. Significance for the length of stay regression is noticeably reduced.

Table 8: Robustness of Coverage, Length of Stay, and Disease Incidence Results

	Medicaid	Medicaid Generosity
Linear probability model bias		
Coverage (Probit)	.09693	
	(1.53)	
Infection (Probit)		-.02566
		(1.79)*
State time-trend bias		
Infection (Time trend)		-.02634
		(1.87)*
Length of Stay (Time trend)		.3302
		(1.81)*
Construct validity		
Coverage (Men 45-65)	-.01107	
	(0.79)	
Infection (Men 45-65)		-.00064
		(0.55)
Length of Stay (Men 45-65)		1.028
		(1.70)

All models include same covariates as in initial regressions.
Construct validity tests limit the sample to hospitalizations among men aged 45-65.
* significant at 10%; ** significant at 5%; *** significant at 1%

Finally, as a construct validity test, I limit the sample of hospitalizations to men aged 45 to 65, and look for effects on coverage, disease incidence, and length of stay of Medicaid policy targeting pregnant women. Insignificance of these results suggest that Medicaid coverage specifically designed to increase coverage and medical treatment for pregnant women do not affect coverage and disease incidence for men. However, length of stay results show similar levels of significance (and higher magnitude) than for pregnancy-related admissions, suggesting that trends in length of stay of hospitalization may be less a result of increased coverage, and more a result in underlying medical trends across states correlated to the policy variable of Medicaid generosity.

6. Conclusions

Medicaid, as the largest public entitlement program for health care in the United States, has been heavily studied. Existing literature has focused on the effects of policy expansion on both the use of health insurance coverage (primarily through prenatal care use), and health outcomes for children born to mothers under Medicaid. Far less studied are impacts of Medicaid coverage expansions on maternal health and hospitalization length of stay resulting from increased legislative generosity. In a descriptive sense, during the 1990s, infectious disease incidence for pregnancy related admissions was significant. Moreover, there has been concern over generally declining length of hospital stays associated in part with adoption of the Prospective Payer System reimbursement policy, and other cost-based policy designs to increase care efficiency. As noted by Declercq (1999) and Eaton (2001), shorter hospitalization stays have been associated with incidence of unnecessary complications and hospital re-admission associated with early discharge, particularly for newborns (Evans et al., 2008).

This study has utilized a relatively untapped data resource provided by HCUP, which offers expansive data on hospitalizations in the United States. These data have enabled this study to identify a new and relevant effect of Medicaid expansion on increases in Medicaid coverage. There is some evidence that policy expansions resulted in longer length of hospital stays, and stronger evidence that increases in legislative generosity resulted in a decrease in the incidence of infectious disease. However, mandatory minimum length of stay laws implemented during the sample period at both the state and federal levels warrant a fair degree of caution in claiming a causal effect of Medicaid generosity on hospital length of stay.

Regardless, the maternal health findings alone are important. The probability of infectious disease-related hospital admissions were roughly 0.6 percentage point lower than would have been the case in the absence of Medicaid expansions. Given an average cost savings of $3,873 per avoided infectious disease-related admission, this 0.6 percentage point reduction resulted in approximately $4.7 million over the sample period. Compared to the multibillion dollar budget for Medicaid, this number is relatively small. However, the decrease in disease incidence is only one potential benefit to coverage expansions. Beyond monetary benefits associated with the policy, increased coverage resulted in important and potentially life-saving benefits to low income mothers.

As motivation for future work, anecdotal evidence suggests that extension of Medicaid coverage to low income women may have increased the usage of epidurals during delivery, since in many states Medicaid covers epidural injections. Since the procedure is costly, and may be considered elective, expansions to Medicaid may have also offered pain reduction often desired by mothers during delivery. This, as an additional dependent variable, may be considered in future renditions of this work.

CHAPTER III

HETERGENEITY IN WILLINGNESS TO PAY FOR HEALTH RISK REDUCTIONS

A large share of the social benefits stemming from environmental regulations in both the United States and Canada is derived from their effect on human health outcomes. Alberini (2005) reports that more than eighty percent of monetary benefits supporting clean water legislation in the U.S. are derived from associated reductions in human mortality. The standard measure of mortality risk reduction benefits in the literature has been the Value of a Statistical Life (VSL). This statistic measures the marginal rate of substitution between mortality risk and income or wealth. It is common to estimate wage-risk or wealth-risk tradeoffs Viscusi (1993) by assuming that the individual considers just a single health threat, for which the risk is reduced by a small amount in the current period (Dreze, 1962; and Jones-Lee, 1974). For example, based in part upon a series of these wage-related revealed preference (RP) studies, the U.S. Environmental Protection Agency uses a one-size-fits-all VSL estimate of roughly $6-7 million. Two recent meta-analyses for wage-risk tradeoff studies have found mean VSL estimates from $3.7 million Mrozek (2002) to $10.8 million Viscusi (2003), while in Canada the figure has ranged from $6.2 to $9.9 million Chestnut (2007).

Several shortcomings of wage-risk VSL studies have been highlighted in the literature. First, they are limited to workplace risks, while environmental and public safety and health policies often pertain to risks outside the workplace. Second, these studies implicitly assume full information concerning the relevant risks both within and across occupations underlying the work choice decision. Finally, it is often difficult to isolate the risk premium of a particular occupation from other non-pecuniary attributes of a job (time-flexibility, workplace setting, etc.). As an alternative, stated preference (SP) studies allow for risks to differ across populations by the use of hypothetical choice scenarios. Survey respondents are typically asked to choose among alternative types of risk reductions at differing costs. Their choices reveal their implied willingness to pay (WTP) for specific risk reductions which lie within the range of the scenarios posed in the choice questions. The survey instruments used in SP choice studies are designed to educate respondents about all of the information pertinent to their decisions, and the survey's choice contexts can be designed to isolate the effect of a specific risk reduction associated with a given policy choice. These studies tend to find smaller VSL figures Kochi (2006). Yet, while SP studies ameliorate a number of problems with the revealed-preference wage-risk studies, they rely on what an individual says he or she would do, rather than actual economic choices. Thus there are several important protocols which must be observed so that the researcher can argue for the so-called construct validity of the resulting willingness-to-pay estimates. These measures are described in detail in Cameron (2006) and will not be reiterated here.

It has been common in both the RP and SP literatures on the valuation of mortality risk reductions to point out the limitations of a one-size-fits-all measure.[19] Suppose a policy or regulation targets an environmental threat that bears most heavily upon the health of a particular sub-population (say, the elderly). VSL metrics derived primarily from the contemporaneous employment decisions of prime-aged white males in blue-collar occupations are not necessarily appropriate for estimating the willingness to pay of the elderly to reduce their risk of death in the current period or in future periods. In a recent Associated Press article entitled "In the numbers game of life, we're cheaper than we used to be," Seth Borenstein raised questions (and the ire of many readers) about the fact that the U.S. EPA has used different VSL numbers over time. This flurry of outrage in the U.S. press again underscores the difficulty of interpretation and potential for misunderstanding with respect to the benefits of mortality risk reductions within the policy arena.

As an alternative to the standard VSL measure, Cameron (2006) build a utility-theoretic model for the Value of a Statistical Illness Profile (VSIP). This measure allows for different valuations of health risk reductions across a variety of health states that make up a future "illness profile" (including a pre-illness current health state, illness-years, post-illness recovered/remission years, and lost life-years). By allowing marginal utilities to vary across

[19] Baker (2008) consider the conditions on the underlying social welfare function that would be necessary to justify the application of a single VSL estimate. They also address whether discounts or premia might be applied to take account of age or vulnerability of the population exposed to the risk. Sunstein (2004) raises the issue in the legal literature that VSL estimates should vary across individuals.

the different phases of an entire illness profile, the model integrates health states that have previously been valued in separate models or separate studies. It recognizes that "sudden death in the current period" is not the typical illness profile for most environmentally induced illnesses. Generally, such deaths are preceded by a period of pre-mortality morbidity that may have a substantial effect on individuals' willingness to pay to reduce their risk of suffering from such a health threat. Starting from this more-general concept of the VSIP, it is possible to extract a special case that is close to the more-conventional VSL measure (i.e. reducing the risk of sudden death in the current period). However, the new construct allows for illness profiles which involve latency periods and protracted periods of pre-mortality morbidity (illness-years). It depends fundamentally upon the individual's current age and income. It is also a natively per-year measure, obviating the need for ad hoc calculation of the "value of a statistical life-year" (VSLY) based on dividing a conventional VSL by the average remaining life expectancy in the population.[20]

Utilizing individual stated-preference data from matching surveys conducted in both Canada and the United States, I utilize the VSIP framework to investigate differences in average WTP to pay for health risk reductions across the two different cultures. Only one recent study has directly compared WTP for health risk reductions between the U.S. and Canada. (Alberini, 2004) studied a sample of respondents from Hamilton, Ontario, and compared them to another sample from the U.S. They find that Canadians have lower WTP, at least for those aged forty years and older. Although the study allows for systematic variation with age, the differences in WTP are not explained through systematic variation across other sociodemographic characteristics, subjective risks of the diseases in question, or differences between the Canadian and U.S. health care systems. I extend the cross-national literature to explain observed differences in individual WTP for health risk reduction programs by individual heterogeneity in each of these factors.[21]

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[20] Sunstein (2003) addresses the question of whether benefit-cost analysis should employ the value of statistical lives, or statistical life-years.

[21] See Hammitt (2007) for an exposition on the opportunity for inclusion of systematic variation in WTP studies. Krupnick (2002) identify variation in WTP across age of the individuals, showing weak support for the notion that WTP for health risk reductions declines with age - evidence of a "life-cycle effect," where individuals expect to derive increasing marginal utility from reducing health risks that come to bear later in their lives. DeShazo and Cameron (2005) find statistical evidence that as people age, there is a systematic downward shift in their anticipated schedule of marginal utility for risk reduction a future ages. These two effects offer evidence of time inconsistency: at younger ages, individuals seem to value future health more, however, as they get older, they value future health less.

This individual heterogeneity is important. First, controls for these individual characteristics are necessary to prevent cross-national heterogeneity from showing up as generic cross-national differences (or lack thereof) in health preferences. Second, from a policy perspective, any WTP number used for benefit-cost analysis should probably reflect the actual distribution of characteristics in the at-risk population for a particular policy or regulation. Based on the detailed attitudinal and subjective health perception variables collected in the survey, I have identified a number of variables for which the distribution (especially by age) differs between the U.S. and Canada. For example, members of the Canadian sample appear to express higher subjective probabilities associated with the risk of heart disease, cancers, respiratory disease, diabetes, and Alzheimer's disease. They are also more inclined to say they could improve their health by cutting back on smoking and improving their diet, but are less inclined to believe they can reduce their risk of traffic accidents through increased use of seat belts. Depending on age, they feel they have more or less opportunity to improve their health through additional exercise.

Given the universal payer system in Canada and the private-payer system in the United States, individual perceptions can presumably differ about the efficacy of health care and its overall accessibility. The survey elicits information about each individual's confidence in diagnosis and treatment under their respective health care systems. Moreover, the health risk reduction programs used in the stated choice scenarios for Canadians were stipulated as being outside the normal course of care under the universal health system, so information was also collected about their personal experience with instances where they may have gone outside their provincial health plan for prior medical diagnostic and testing services.

Finally, fitting a WTP model that acknowledges individual heterogeneity and differing illness profiles allows for computation of WTP that is tailored for specific populations and health risks. For example, it is of interest to Canadian policy makers to know if the vast array of WTP studies based upon U.S. preferences (primarily of prime-aged blue collar males), can be used to inform environmental and public-health related programs geared toward Canadians. Can point estimates for mean WTP based upon U.S. preferences be transfer to the Canadian population? Perhaps it is better to estimate a function for the underlying preferences, depending on age, gender, education, marital status, etc., and use the Canadian populations' composition across these demographic characteristics to estimate a

Canadian specific WTP? But even this application may fail to properly inform Canadian policy-makers, if the underlying ways in which age, gender, and other demographics affect preferences over health risks differ between Canadians and individuals from the U.S. Therefore, to the extent possible, it is beneficial to know how heterogeneity in individual preferences differs both across individual characteristics and between countries with different cultures and health systems.

In general, I find evidence that U.S. and Canadian preferences differ, with the differences largely explained by non-jurisdictional individual heterogeneity. I find substantial evidence of age profile effects which are generally consistent with other studies. However, age profiles with respect to WTP to avoid adverse health states are markedly different between Canadians and U.S. residents. In general, Canadians have a much flatter age profile for WTP, and this profile appears to peak at a substantially older age.

Section 1 describes the stated preference survey used in this analysis. Section 2 details a number of differences across countries in the attitudinal and behavioral characteristics of survey respondents. Section 3 sketches the basic utility theoretic model used in the empirical estimation, while Section 4 presents empirical results. Section 5 discusses the results, focusing on the sudden death scenario in the WTP simulations to facilitate the most-direct comparisons with the extant literature on VSLs. Section 6 concludes.

1. Survey Design and Data

The data collected for Cameron and DeShazo (2006) provide a unique opportunity to identify cross-national differences in preferences to avoid adverse health states. The stated preference dataset was conducted twice—first for Canadian residents using the internet consumer panel maintained by Ipsos Reid (selected so that the proportions of the sample in different sociodemographic groups mimic the general population), and a few months later for the United States using the representative consumer panel maintained by Knowledge Networks, Inc. Careful administration of the Canadian survey allowed for collection of key demographic information for Canadians mirroring demographic characteristics included in Knowledge Network's standing consumer panel for the United States. Information on the age, income, educational attainment, marital status, gender, and race/ethnicity are available for both samples.

In addition to collection of demographic characteristics, the survey collected four other categories of information from each respondent. First, information was collected concerning the individual's personal health history and their perceptions of their susceptibility to specific categories of major health risks. These questions asked about the respondent's own prior experience with the specific classes of disease that they would subsequently be asked to consider in the conjoint choice experiments. They were also asked about the prior experiences of friends and family members with these illnesses, about the extent to which they believe these disease risks can be controlled through health habits and life-style choices, and about their personal room to improve their health habits along seven dimensions, including opportunities to see the doctor more regularly, lose weight, exercise more, cut down on alcohol consumption, use a seat belt more regularly, improve their diet, and cut back on smoking.[22]

The second part of the survey provided a risk tutorial and trained respondents carefully about how to interpret each of the attributes of the different risk reduction programs that form the core of the survey. Respondents were required to answer a simple skill-testing question to evaluate their comprehension of the notion of risk, since risk comprehension is crucial to the choice tasks.

After about 25 pages of preparation, in the third and main section of the survey, each respondent is faced with the first of five independent choice scenarios. The first choice scenario presents all of the quantitative information used in the tutorial section in a simplified one-page "choice table." See Figure 5 for an example. The individual is asked to evaluate two health programs offering a reduction in health risk at a monthly cost against the status quo alternative (i.e. no health risk reduction program, but no expense either). The respondent was then asked to choose their most preferred option among the three options available. Each of the two health programs offered randomly assigned reductions in the probability of getting sick or injured, and described the expected time-to-onset, duration, and potential for recovery from the illness or injury, as well as the extent to which this health

[22] Although the nominal life expectancies used in the illness profiles for the survey's choice experiments were based upon actuarial life expectancies, respondents were asked at the end of the survey to report their individual subjective life expectancy based on their health and family history.

threat would shorten their expected lifespan. Each illness profile was randomly assigned a disease name, subject to a few exclusions for plausibility (e.g. no recovery from diabetes or Alzheimer's disease).[23]

For all disease risks (other than the traffic accidents) each program reduces the risk of disease incidence via a diagnostic pin-prick blood test administered once per year by the individual's doctor. The test would indicate whether the individual is at risk of developing the illness in question. If so, the individual would be prescribed medication and/or lifestyle changes to reduce the chance of suffering the illness profile in question.[24] Each illness profile consists of a brief description of the approximate age at which the individual would get sick, the duration of sickness, symptoms, treatments and prognosis, and anticipated effects on overall life expectancy. The health programs offered where characterized by both a reduction in the probability of illness, and associated cost of the program in both annual terms and as monthly payments.

The final section of the survey consisted of debriefing questions. Some of these were posed directly after each choice scenario. Another was a general question about the respondent's confidence in the ability of health care providers to diagnose and treat illnesses under their respective health care systems. Debriefing questions also included assessments of scenario "buy-in," such as whether or not the individual personally believed they would benefit from the risk reduction program, and the age at which the individual subjectively believed they would benefit from the program. For the Canadian respondents, information on the number of times each respondent sought care outside of their universal health plan was solicited, since the health programs used in the choice scenarios were described as extra-ordinary care which would not be covered under their provincial health plan.[25]

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[23] Other work has found that the disease labels (regardless of the underlying illness profile) do affect individual preferences to avoid adverse health states. These differences are addressed in Cameron, DeShazo, and Johnson (2008). However, the randomization of disease labels across illness profiles the respondent is asked to consider assures that point estimates remain unbiased. Any variation induced by subjective beliefs about specific disease names would be orthogonal to the illness profiles considered in each scenario.

[24] For traffic accident scenarios, the program was described as car equipment such as new airbags, braking systems, and impact reduction technologies which could be retrofitted to existing vehicles, on included as an option on new vehicle purchases, with capital costs amortized into monthly payments.

[25] Through debriefing questions following each stated choice, respondents who said they would not choose either offered program had the option to indicate that this was because their provincial health plan should cover those tests.

Figure 5: One Randomization of a Conjoint Choice Set

Choose the program that reduces the illness that you most want to avoid. But think carefully about whether the costs are too high for you. If both programs are too expensive, then choose Neither Program.

If you choose "neither program", remember that you could die early from a number of causes, including the ones described below.

	Program A for Heart Disease	**Program B For Colon Cancer**
Symptoms/ Treatment	Get Sick when 71 years old Two weeks of hospitalization No surgery Moderate pain for remaining life	Get sick when 68 years old 1 month of hospitalization Major surgery Severe pain for 18 months Moderate pain for 2 years
Recovery/ Life expectancy	Chronic condition Die at 79	Recover at 71 Die of something else at 73
Risk Reduction	5% From 40 in 1,000 to 38 in 1,000	50% From 4 in 1,000 to 2 in 1,000
Costs to you	$15 per month (=$180 per year)	$4 per month (=$48 per year)
Your choice	**o Reduce my chance of heart disease**	**o Reduce my chance of colon cancer**

o Neither program

The survey was administered to 2,439 respondents from the United States and 1,109 Canadians.[26] Certain Canadian and U.S. respondents were excluded for three main reasons. First, if the respondent did not correctly answer the risk comprehension question, he or she was excluded from the analysis. Second, if the respondent rejected both programs in a particular choice scenario solely because they did not believe the program would work, the respondent's choice under that scenario was dropped from the analysis. Finally, randomization of illness profiles inadvertently resulted in a small number of implausible

[26] The response rate for the US survey was 79% (out of 3000 initially solicited). The Canadian survey was administered over the internet by Ipsos Reid.

health profiles (about 1%), and these were dropped to preclude any biases stemming from how they might have been interpreted.[27]

Although the Canadian survey was administered to the exclusively computer-literate Ipsos Reid consumer panel, the sample is reasonably similar to the Canadian population on several observable dimensions. Table 9 presents a comparison of the Canadian sample to the U.S. sample and the Canadian population. Particularly with respect to the age distribution, the Canadian sample closely mirrors the Canadian population as a whole. Although the sample has fewer elderly (2% compared to 8% in the population), this is expected from a survey administered over the internet. The income distribution for the sample is skewed towards lower incomes compared to the population as a whole. The sample has a greater proportion of females to males, and a slightly greater proportion of the sample is married. Finally, although there are fewer nonwhites in the sample, the educational attainment (those earning a college degree of more) is similar between the sample and the Canadian population as a whole. These differences highlight the importance of allowing for the possibility of systematic variation in WTP across observable characteristics, so that differences in the types of people in the sample are not interpreted as differences in preferences for similar types of individuals.

[27] For the US sample, this resulted in dropping 2,191choices from the US sample and 1,040 choices from the Canadian sample due to risk comprehension failure, 1,791 choices from the US sample and 393 choices from the Canadian sample due to scenario rejection, and 215 choices from the US sample and 108 choices from the Canadian sample due to randomization error.

Table 9: Demographic Statistics by Population and Sample - Canada and US

	Canada		US	
	Population	Sample	Population	Sample
Age (years)				
25-44	45%	44%	47%	39%
45-64	36%	41%	34%	39%
65-74	11%	13%	10%	14%
75+	8%	2%	9%	7%
Gender				
Male	50%	42%	49%	48%
Female	50%	58%	51%	52%
Race				
White	87%	96%	77%	80%
Nonwhite	13%	4%	23%	20%
Marrital Status				
Married	48%	67%	54%	69%
Nonmarried	52%	33%	46%	31%
Education				
High school or less	56%	58%	69%	70%
College Degree +	44%	42%	31%	30%
Income (US$1000)				
10-	3%	14%	10%	6%
10-25	20%	31%	19%	18%
25-45	35%	36%	24%	36%
45-65	21%	12%	21%	11%
65-100	14%	5%	14%	21%
100+	7%	2%	12%	8%

Source: Statistics Canada, US Census Bureau, and survey data (after exclusions). Interpolation required for income brackets (equal weight given to $5000 increments). Domestic partners in Canada counted as married.

2. Differential Patterns in Health Beliefs and Health Care Systems

Descriptive statistics presented in Table 10 show differences between Canadian and U.S. respondents in attitudes and beliefs about illness risks, subjective beliefs about behavior, as well as different levels of confidence in the timeliness and quality of health care in the event that the individual contracts a major illness or suffers a major injury. Canadian respondents perceive themselves to be less at risk of acquiring a disease or being in a traffic accident. Moreover, they believe there is less opportunity for lifestyle improvements through seeing a doctor more regularly, wearing a seatbelt more often, or reducing their consumption of alcohol. However, they show more opportunity to moderate their weight, exercise more, and smoke less. They are generally less confident that their health care system will allow them to obtain timely and effective diagnosis and treatment, and only about sixteen percent of the Canadian sub-sample has gone outside their provincial health plan for diagnostic tests.

Table 10: Health Risk and Behavior Beliefs, and Health Care System Controls

	US Sample			Canadian Sample				
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Min	Max
Health (self-reported illness risk)								
average subjective risk	1,801	-0.26	0.88	700	-0.07	0.85	-2	2
Behavior (room to improve on:)								
see doctor	1,783	-0.49	1.35	697	-0.64	1.27	-2	2
control weight	1,794	0.11	1.41	699	0.24	1.38	-2	2
exercise	1,793	0.62	1.16	697	0.69	1.11	-2	2
healthy diet	1,792	0.31	1.16	695	0.32	1.10	-2	2
seatbelt use	1,788	-1.25	1.29	692	-1.64	0.94	-2	2
smoking	1,754	-1.02	1.63	680	-0.70	1.80	-2	2
alcohol consumption	1,771	-1.25	1.18	686	-1.35	1.05	-2	2
System Controls								
confidence	1,801	0.16	0.67	700	0.03	0.67	-1	1
out-of-plan (absolute)				700	0.88	1.07	0	5
out-of-plan (binary)				700	0.16	0.37	0	1

Average subjective risk taken as the average of subjectively reported risks for diseases randomly selected in all five choice scenarios. Statistics after exclusion criteria. Out-of-plan variables reflect either the absolute number of times the respondent sought care outside the Canadian universal health plan, or a binary variable for whether or not the patient sought care outside the universal plan.

However, the differences in health-related attitudes and beliefs between Canadian and U.S. respondents differ with the age of the individual. Appendix II provides and assortment of figures illustrating differences across countries, by age, in a variety of different measures. These graphs depict age-wise means and intervals defined by plus and minus two standard deviations, where the standard deviations are adjusted to reflect sample size in the age group in question. To enhance the main trends, these three age-wise statistics are presented as twenty-year moving averages. In each figure, the triple of solid lines applies to the U.S. sample and the triple of dashed lines applies to the Canadian sample.

The figures in Appendix I reveal differences in subjectively reported risks of suffering from heart disease, respiratory disease, and traffic accidents, as well as differences in subjectively reported room for improvement in personal health behaviors. Perceived risk for Alzheimer's disease and diabetes is generally higher for younger and lower for older Canadian respondents compared to individuals from the U.S. Perceived risk of acquiring one of five cancers (prostate, breast, colon, lung or skin) was lower for Canadian respondents. For the risk of heart disease, younger and middle-aged Canadians reported higher subjective risks, while older Canadians (75 years and up) reported lower subjective risks (although this may reflect self-selection into the possibly healthier older internet-using sample in Canada). Canadian respondents reported substantially higher risks of acquiring respiratory disease for nearly all age groups, with the differential inverting only for those 75 and older. A similar pattern is seen for risk of strokes, while little difference is seen in perceived risk of traffic accidents up until the age of retirement, whereupon Canadians generally begin to report lower risks. Again, this could reflect selection biases in the older internet sample in Canada.

While Canadian respondents report similar ability to improve lifestyle habits with respect to losing weight and improving their diet, they report generally less opportunity at all ages to wear a seat belt more regularly, or see a doctor more frequently. In general, respondents from both samples reported similar opportunities to cut back on smoking. But Canadian respondents between the ages 35 and 45 reported substantially greater opportunities to cut back, compared to respondents from the U.S. Younger Canadian respondents reported less opportunity to reduce alcohol consumption, with the relationship reversing at about age sixty, at which point older Canadians report significantly more opportunity to cut back on alcohol consumption.

It is worth noting that these age-specific and disease-specific profiles reveal some degree of correlation between subjective beliefs about health risks and associated lifestyle behaviors. The higher perception of risk for diabetes and heart disease among Canadian respondents is correlated with a greater propensity to see more opportunity for exercise. Similarly, the higher reported risk among Canadians for respiratory disease is correlated with reports of more opportunity to cut back on smoking. I might expect that Canadians with higher risk perceptions of respiratory disease are more willing to pay for health risk reductions. Likewise, the higher perceived risk of heart disease and diabetes suggests that Canadian respondents may be more willing to pay for health risk reductions for these diseases. On the other hand, Canadians who report more opportunity to cut back on smoking or exercise may prefer either cutting back or exercising more to paying for health risk reductions. The risk reduction programs to be offered in the stated choice scenarios may be perceived as substitutes for these other health enhancement activities, or a complementary measures.

Finally, there are stark differences in the confidence of diagnosis and treatment of health problems across the two systems. Canadian respondents are generally less confident in the timeliness and quality of diagnosis and treatment until about age seventy, beyond which there is little difference in the perceived efficacy of care. Regarding experience with going outside of their provincial health plan for medical services, Canadian respondents have, on average, gone outside of their plan for about one in five of the procedures mentioned in the survey.[28] However, only about 16 percent of the Canadian sample had gone outside of their health plan for diagnostic testing (analogous to the risk reduction program used to elicit willingness to pay information in the survey's choice scenarios).

[28] In addition to diagnostic tests, these medical services included physical exams, flu shots, major surgery, cosmetic surgery, immunizations for children or for travel, and "other."

3. Structural Utility-Theoretic Model

This utility-theoretic choice model is described in detail in Cameron (2006), but I offer a brief explanation of the model in this paper. Denote the two risk reduction programs in each choice set as A and B, and the status quo alternative for "neither program" as N. Each program reduces the risk of facing a specified illness profile, but involves a specified annual cost. The program cost is assumed to apply only during pre-illness years and recovered years, so the individual would not pay for the program while sick (or dead) if he or she were to fall victim to the illness or injury. An illness profile is a sequence of future health states that includes a mutually exclusive and exhaustive combination of pre-illness years, sick years, post-illness recovered/remission years and lost-life years, and only single spells of any given illness. Respondents are assumed to maximize utility subject to their budget constraint, and thus to choose the alternative that gives them the highest level of utility.

For simplicity, consider just the pair-wise choice between program A and N.[29] I assume that the utility of an individual, *i*, at time, *t*, depends upon net income in that period, Y_{it} minus the cost of any program, c_{it}, and the health state they experience in that period. In any given period, the individual will be in one of the four possible health states, which are captured using four indicator variables: $1(pre_{it})$ for pre-illness years, $1(ill_{it})$ for illness-years, $1(rcv_{it})$ for post-illness recovered/remission years, and $1(lyl_{it})$ for lost-life years. Write the individual's indirect utility function in each time period, t, as:

$$V_{it} = f\left(Y_{it} - c_{it}\right) + \alpha_0 1(pre_{it}) + \alpha_1 1(ill_{it}) + \alpha_2 1(rcv_{it}) + \alpha_3 1(lyl_{it}) + \eta_{it} \quad (3)$$

There is uncertainty about whether the individual will actually fall sick from the disease, so I model each choice as depending upon expected indirect utility, with the expectation taken across the sick (S) and healthy (H) outcomes. Participation in program A vs. N is described as altering the probability of getting sick from Π_i^{NS} to Π_i^{AS}. Furthermore, each illness profile extends through the remainder of the individual's life expectancy, so I discount future time periods using a constant discount rate *r* and discount factor $\delta^t = \left(1+r\right)^{-t}$ to get the present discounted value (PDV) of expected indirect utility

[29] The three-way choice between two programs and neither program is analogous.

for individual *i*. The individual is assumed to choose program A over N if his or her discounted expected utility is greater under A:

$$PDV\left(\Pi_i^{AS}V_i^{AS}+\left(1-\Pi_i^{AS}\right)V_i^{AH}\right)-PDV\left(\Pi_i^{NS}V_i^{NS}+\left(1-\Pi_i^{NS}\right)V_i^{NH}\right)>0 \tag{4}$$

The present discounted number of years making up the remainder of the individual's nominal life expectancy, T_i, is given by $pdvc_i^A=\sum_{t=1}^{T_i}\delta^t$. Discounted time periods spent in the pre-illness state, the recovered/remission state, and as lost life-years from $t=1$ to $t=T_i$ are given by:

$$pdve_i^A=\sum\delta^t 1\left(pre_{it}^A\right),\ pdvi_i^A=\sum\delta^t 1\left(ill_{it}^A\right),$$

$$pdvr_i^A=\sum\delta^t 1\left(rcv_{it}^A\right),\text{ and }\ pdvl_i^A=\sum\delta^t 1\left(lyl_{it}^A\right).$$

Since the different health states exhaust the individual's nominal life expectancy, $pdve_i^A + pdvi_i^A + pdvr_i^A + pdvl_i^A = pdvc_i^A$. Finally, to accommodate the assumption that each individual expects to pay program costs only during the pre-illness or recovered post-illness periods, $pdvp_i^A = pdve_i^A + pdvr_i^A$ is defined as the present discounted time over which payments must be made.

To further simplify notation, let:

$$cterm_i^A=\left(1-\Pi_i^{AS}\right)pdvc_i^A+\Pi_i^{AS}pdvp_i^A$$

$$yterm_i^A=\left[pdvc_i^A-\left(\Pi_i^{AS}pdvi_i^A+\Pi_i^{NS}pdvl_i^A\right)\right],\text{ and}$$

$$pterm_i^A=\Pi_i^{AS}\left[\alpha_1 pdvi_i^A+\alpha_2 pdvr_i^A+\alpha_3 pdvl_i^A\right].$$

The complexity of $cterm_i^A$ and $yterm_i^A$ merely reflect the fact that net income over the future will depend on whether the individual will be sick or dead, with probabilities depending upon the chance of getting sick, with and without the testing program.

Then the expected utility-difference that drives the individual's choice between program A and N can then be defined as follows:

$$\Delta PDV\left(E_{S,H}\left[V_i\right]\right)=\left\{f\left(Y_i-c_i^A\right)cterm_i^A-f(Y_i)\,yterm_i^A\right\}+pterm_i^A+\varepsilon_i^A \tag{5}$$

The option price, in the sense of (Graham, 1981), is the common maximum certain payment that makes an individual indifferent between paying for the program and having the risk reduction, or not paying for the program and not having the risk reduction. Here, I solve for the common payment which makes the difference in discounted expected utility between program A and N equal to zero:

$$\hat{c}_i^A = Y_i - \left(\frac{\beta \sqrt{Y_i}\, yterm_i^A - pterm_i^A - \varepsilon_i^A}{\beta cterm_i^A} \right)^2 \tag{6}$$

where $f(Y) = \beta\sqrt{Y_i}$ has been selected as the best-fitting simple functional form.[30] The square root form introduces some curvature with respect to net income, yet preserves the monotonic form. The expected present value of this common certain payment can then be calculated for the individual's remaining lifetime and can be written as:

$$E_{S,H}\left[PV\left(\hat{c}_i^A\right)\right] = cterm_i^A \left[\hat{c}_i^A\right] \tag{7}$$

Divide $E_{S,H}\left[PV\left(\hat{c}_i^A\right)\right]$ by the size of the risk reduction, $\left|\Delta\Pi_i^A\right|$ to get a construct we can call the Value of a Statistical Illness Profile (*VSIP*):

$$VSIP = E_{S,H}\left[PV\left(\hat{c}_i^A\right)\right] \Big/ \left|\Delta\Pi_i^A\right| \tag{8}$$

This *VSIP* is a roughly a generalization of the more-familiar *VSL*. The *VSIP* is a marginal rate of substitution (the ratio of the marginal utility of the sequence of health states to the marginal utility of income) scaled arbitrarily to correspond, like a VSL, to a risk change of 1.0. Due to the reaction to this metric (as seen with Seth Bornstein's AP article), I might alternatively normalize upon an equally arbitrary 1/1,000,000 risk reduction, which is expressed as the value an individual's WTP for a risk reduction that is more in the range of many policies. This normalization might spare uninitiated readers from the idea that economists are unilaterally deciding upon the worth of a human being.

The marginal utility of an adverse illness profile is in the numerator of the *VSIP*, so an increase in the marginal disutility of any component of an illness/injury profile of health

[30] Suggested by a line-search across Box-Cox transformation parameters.

states—illness years, recovered/remission years, and lost life-years—will increase the *VSIP*. Since the marginal utility of income is in the denominator, an increase in the marginal utility of income will decrease the *VSIP*.

To illustrate the implications of the fitted model for willingness to pay for health risk reductions, it is necessary to choose a particular individual and a particular illness profile. In this paper, I will focus on the illness profile that is assumed in most wage-risk *VSL* studies—sudden death in the current period. However, the VSIP framework allows one to simulate willingness to pay to reduce the risk of a vast array of different illness profiles: with or without latency, with different lengths of illness, with or without recovery, and with or without any decrease in life expectancy.

To build a distribution of WTP values for a particular type of environmental risk for a particular population, broader simulations would be used. It would be necessary to specify the distribution of illness profiles that is likely to result from the health threat, the magnitudes of the risk reductions, and the types of individuals (ages, genders, incomes) who would be affected by these risk reductions. WTP estimates could then be simulated for each of a large number of random draws from the distributions of risks (possible illness profiles) and affected individuals to produce a distribution of WTP estimates for the policy in question. In this paper, however, I will simply illustrate the disparities in predicted willingness to pay for a standardized illness profile, emphasizing the interpersonal and international differences in WTP for this standard profile.

4. Empirical Analysis

In Table 11, I begin with a simple four-parameter model (Model 1) which allows for differences between U.S. and Canadian preferences by interacting each baseline variable with an indicator for the Canadian sub-sample.[31] Rather than simply maintaining the hypothesis that marginal utilities from each health state are independent of the duration of that state and the accompanying durations of other health states that characterize each profile in question, a shifted log functional form allows for diminishing marginal (dis)utilities for increased lengths of time in each adverse health state (Cameron and DeShazo, 2007). This basic model, therefore, includes a net income term (net of program cost, if risk reduction program

[31] Complete results, with t-statistics are provided in Table 22, Appendix G.

is selected) along with terms for illness years, $\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$, recovered/remission years, $\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$, and lost life-years $\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$.

The results for Model 1 suggest a higher marginal utility of income and considerably less disutility from lost life years for Canadians. As expected, for individuals from both countries, the marginal utility of net income (i.e. other consumption) is positive (but diminishing, given the square root functional form). The marginal utilities associated with each of the three health states are negative (and diminishing, given the log functional form).[32]

Model 2 in Table 11 presents the results of a utility specification with ten parameters which allows for systematic variation by age in the marginal (dis)utility from lost-life years. I adopt the model specified in Cameron and DeShazo (2006), which is the parsimonious version including just the statistically significant terms in a fully translog model (including all squares and pairwise interaction terms for the three log terms). The construct called $pterm_i^A$ becomes:

$$\Delta\Pi_i^{AS}\begin{bmatrix}\alpha_1\log\left(pdvi_i^A+1\right)+\alpha_2\log\left(pdvr_i^A+1\right)+\alpha_3\log\left(pdvl_i^A+1\right)\\+\alpha_4\left\{\log\left(pdvl_i^A+1\right)\right\}^2+\alpha_5\left\{\log\left(pdvi_i^A+1\right)\log\left(pdvl_i^A+1\right)\right\}\end{bmatrix}\qquad(9)$$

To accommodate age, the α coefficients are allowed to differ systematically with the respondent's current age wherever this generalization is warranted by the data. This leads to a model where $\alpha_3=\alpha_{30}+\alpha_{31}age_i+\alpha_{31}age_i^2$, and analogously for α_4 and α_5.[33]

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32 I initially considered use of a quadratic-in-income model specification in conjunction with the shifted-log functional form for health states. Parameter estimates from the quadratic-in-income model are consistent with all expectations: positive and decreasing marginal utilities of income, which are positive over the range of incomes included in the sample. However, moving to a square root functional form for preferences over income had two advantages: 1) it improves tractability of the model results (especially when all covariates are included), and 2) produced superior log-likelihood statistics. I therefore retain this restriction throughout.

33 Inclusion of the squared lost life-years term allows for the marginal utility of the lost life-years term to depend on the point of reference for lost life-years. This model also allows the marginal disutility from a discounted lost life-year to depend upon the number of preceding sick-years. Heterogeneity in preferences over health risk reductions has documented both an increasing and a decreasing willingness to pay for lost life years with age. (Alberini, Cropper et al., 2004; and Cameron and DeShazo, 2006). Initially, willingness to pay seems to increase with age (perhaps as the prospects for illness or death become more salient). Beyond a certain age, however, it declines (as experience with the aging process lends recognition that life years at older ages are somehow diminished in value through reduced mobility, aches and pains, loss of self-sufficiency, loss of loved ones and family, etc.). And inclusion of an interaction term with the number of years spent sick and the number of life years lost allows for the plausible effect that, the greater the number of years spent ill, the less value attached to lost life years. There may be fates worse than death.

Table 11: Empirical Results (point estimates and statistical significance only)

	Model 1		Model 2		Model 3	
	U.S.	CDN Δ	U.S.	CDN Δ	U.S.	CDN Δ
Net income term (complex formula)	.0129***	.0126***	.0129***	.0103***	.0143***	-
× 1(female)	-	-	-	-	.0105***	-
× 1(mod low risk of this illness)	-	-	-	-	-	.01572**
× 1(high risk of this illness)	-	-	-	-	-.0076**	
× 1(not confident in health care)	-	-	-	-	-	.0185**
× 1(confident in health care)	-	-	-	-	.00483**	-
Illness Years: $\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-27.13**	-2.493	-47.4***	-23.68	-57.53***	-57.8***
× 1(female)	-	-	-	-	32.87***	-
× 1(low risk of this illness)	-	-	-	-	35.98**	-
× 1(mod low risk of this illness)	-	-	-	-	24.63*	-
× 1(mod high risk of this illness)	-	-	-	-	-14.48	-
× 1(high risk of this illness)	-	-	-	-	-33.71**	-
× 1(mod. High opp. exercise)	-	-	-	-	-30.87***	-
× 1(high opp. exercise)	-	-	-	-	-41.16***	-
× 1(very low opp. impr smoking)	-	-	-	-	-	43.83***
× 1(mod low opp. impr smoking)	-	-	-	-	-	187.3**
Recovered Years: $\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-22.81**	-7.764	-17.54*	-7.952	-	-
× 1(female)	-	-	-	-	-67.88***	44.76*
Lost Life Years: $\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-29.23**	20.01**	-428***	-27.75	-443.5***	-
× age	-	-	12.04*	-5.734	27.48***	-24.77***
× age^2	-	-	-.08826	.1363	-.277***	.3654***
× 1(female)	-	-	-	-	22.82**	36.44*
× 1(college degree or more)	-	-	-	-	-32.5***	37.11**
× 1(non-married)	-	-	-	-	35.94***	-34.01*
× 1(low risk of this illness)	-	-	-	-	66.8***	-
× 1(mod low risk of this illness)	-	-	-	-	31.08**	-
× 1(mod high risk of this illness)	-	-	-	-	-44.3***	-
× 1(high risk of this illness)	-	-	-	-	-70.09***	-
× 1(not confident in health care)	-	-	-	-	26.03**	-
× 1(confident in health care)	-	-	-	-	-17.74	46.32**
× 1(have gone outside CDN plan)	-	-	-	-	-	-34.57*
× 1(very low opp. impr. doct.)	-	-	-	-	-17.22*	-
Squared: $\left[\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\right]^2$	-	-	145.1*	60.41	149.1*	-
× age	-	-	-4.919	.7678	-10.89***	9.454***
× age^2	-	-	.041	-.0443	.1123***	-.1426***
Interaction: $\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\log\left(pdvl_i^A+1\right)$	-	-	31.14***	28.06*	-30.29***	93.07***
Scenario Adjustment Controls	No		No		Yes	
U.S. Sample Selection Controls	Yes		Yes		Yes	
Observations	32079		32079		31836	
Log-Likelihood	-16707		-16644		-15617	

Absolute value of z statistics in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%

Inclusion of age heterogeneity and more flexible functional form assumptions certainly improves the explanatory power of the model. However, many of the apparent differences between the Canadian and U.S. parameters disappear in Model 2. A number of important attitudinal differences remain unexplained in this model. Canadian and U.S. individuals have very different age profiles for exercise and smoking behaviors, as well as in perceived opportunities to see a doctor (among the other attitudinal variables discussed previously). These differences in the two samples could obscure genuine differences in preferences for people who might otherwise seem similar. Since the Canadian and U.S. samples differ along a number of demographics (such as marital status, education, and gender), it is reasonable to assume that controlling for these differences matters.

Finally, as addressed in Cameron (2007), the survey was designed carefully to illicit preferences over the stated health scenarios through tutorials and explicit statements. However, the potential for respondents to subjectively adjust the choice scenarios to more closely reflect their own situation was assessed through follow-up questions. A share of the sample either over- or under-estimates the illness latency, and/or reports a different estimate of their own life expectancy than was specified in their (age- and gender-indexed) copy of the survey. If these extra-scenario beliefs factor into the respondent's selection of a most-preferred alternative, then the effect of these scenario adjustments could yield bias. The final model therefore includes a number of nuisance variables to control for possible "scenario adjustment" by respondents.

First, following each choice task, respondents were asked about their personal expected latency for each of the health threats in question. If the respondent expected never to benefit from a program, or expected the latency of the illness to be longer or shorter than what was described in the illness profile, this information can be used to construct shift variables to accommodate over- or under-estimation of the latency. Second, at the end of the survey, respondents were questioned directly about their individual subjective life expectancy. To control for deviations between expected and nominal life expectancy in the choice scenarios, the deviation was similarly allowed to shift the utility parameters in the model.

Full-fledged selectivity correction models in multiple-choice conditional logit models are challenging, so I do not attempt them here. Moreover, non-response modeling data are not available for the Canadian sample. Here, I do have the data needed to estimate a response/non-response model that produces fitted response probabilities for each individual in the U.S. sample. For each U.S. respondent, I use the deviation of this fitted response propensity from the median response propensity among all 500,000-plus members of Knowledge Network, Inc.'s initial random-digit-dialed recruiting sample. For Canadian respondents, the variable takes on a value of zero, such that no "correction" is made for deviation between predicted response propensity and average response propensity. While Canadian response/non-response propensities are left uncorrected, I note that the models control for all the observables upon which the Canada and US samples differ in terms of the marginal distributions, and this strategy will minimize the impact of selection bias on the basic coefficients.[34]

Model 3 in Table 11 presents a parsimonious specification of the expanded ten parameter model when additional covariates are interacted with income and illness-state variables, and scenario adjustment and sample selection controls are included, in addition to selected significant interaction terms involving the Canadian-sample indicator variable. The results clearly show that differences between Canadians and U.S. individuals are apparent across illness state profiles.

Perceived risk of disease affects the marginal utility of income differently for Canadians and U.S. individuals. While high perceived risk results in a lower marginal utility of income for all respondents (and hence higher marginal rate of substitution between income and illness states), low perceived risk results in a higher marginal utility of income for only Canadians.[35]

Individuals from both countries who are highly confident in the quality of diagnosis and treatment under their respective health care system have a higher marginal utility of income and lower marginal rate of substitution, while Canadians who are less confident in

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[34] Under ideal circumstances, every respondent would reveal subjective latencies that match the ones used in the choice scenarios. They would each have a subjective life expectancy that matched the nominal life expectancy for someone their age and gender that was used in their copy of the survey instrument. Finally, all members of the recruitment pool would have equal propensities to show up in the estimating sample. Under these conditions, all of the nuisance variables (expressed as deviations from their intended values) would be zero, so I use zero values for these variables in the simulations.

[35] Van Houtven (2008) offer a recent national survey that distinguishes between accident-related deaths and cancer deaths, noting the presence of a cancer premium. Different types of health threats may be more or less salient to different respondents.

care efficacy have a higher marginal utility of income, and lower marginal rate of substitution. This effect was only for those reporting a low (-1) but not the lowest (-2) level of confidence. Therefore, Canadians who rank their health care system below average, but have at least some confidence in the health care system seem to have different health-income preferences than individuals from the U.S.

Canadians in general value avoided sickness years more than individuals from the U.S. For both countries, however, subjectively reported risk of disease had a positive effect on the marginal disutility of illness years (low risk has a positive effect on the marginal utility of illness years, and high risk has a negative effect). Females for both countries have lower aversion to sick years, while those who have significant opportunity to exercise more also fear illness more. However, for Canadians, smoking has a strong effect on the marginal disutility of becoming sick. Non-smokers, or those who have very little opportunity to reduce smoking, have substantially smaller disutilities associated with sick years.

For both countries, males tend to place little marginal value to reducing the number of recovered/remission years, while women from both countries (and the U.S. in particular) are willing to pay to avoid recovered/remission years. This provides an interesting contrast: for women, the morbidity still present in the recovered/remission state seems to matter, whereas men appear to perceive recovered/remission years as fully recovered and providing a level of utility equivalent to their pre-illness state. Men appear to attach value only to avoided illness-years and avoided lost life-years.

Results for preferences over lost life years are particularly interesting (and comprise the most significant part of overall willingness to pay for health risk reductions). In general, age effects are substantially smaller for Canadians, and relatively pronounced for U.S. individuals. Age affects both the baseline marginal utility of lost life years (the log-term) as well as the degree of diminishing marginal utility over the number of lost life years *across* the number of life years lost. Put simply, older individuals seem to value lost life years less, with the value of any individual lost life year decreasing more with the number of years lost overall. However, this age effect is almost (though not completely) offset by the opposite sign for Canadians, suggesting that at least for the sample, Canadians exhibit smaller age effects.

Having a college degree increases the marginal value attached to lost life years, while being non-married reduces it; however, this effect is present only for U.S. respondents, with the point estimates on the Canadian interaction terms almost exactly offsetting the effect. For Canadians, having had experience with going outside of the provincial health plan for diagnostic testing has a weakly significant and positive effect on the disutility of dying early. Having confidence in the timeliness and quality of diagnosis has a positive effect on the value attached to avoiding early death for U.S. individuals, but it appears to reduce the value from avoided premature death for Canadians. For residents of both countries, a lack of confidence in the health care system seems to reduce the marginal value attached to reductions in lost life years. For the U.S., greater confidence in the timeliness and quality of care may translate into higher willingness to pay for avoided lost life-years, but the effect is not statistically significant. For Canadians, however, greater confidence in timely and high-quality care seems to reduce the marginal value attached to avoiding early death. And finally, for both countries, subjective perception of being at low risk for the disease considered in the choice set tends to lower the value attached to lost life year risk reduction, while perception of being at high risk increases it.

Canadians and U.S. individuals exhibit strikingly different coefficients on the interaction term between illness-years and lost life-years. While U.S. individuals derive greater disutility from lost life-years after a longer period of illness, the opposite effect seems to be present in Canada. For Canadians, the disutility from lost life-years is reduced as the number of preceding illness-years increases. Thus, in Canada, it may be the case that a long period of illness may evolve into a "fate worse than death."

Model 3 illustrates the importance of including a rich set of attitudinal, demographic and survey design controls in modeling differences in preferences. Failure to control for individual heterogeneity, in the presence of different types of respondents in the two countries, can easily bias the coefficients on the interaction terms involving the indicator for the Canadian sub-sample and imply that residence in the Canadian jurisdiction, per se, somehow accounts for different preferences.

5. Simulation Results

Based upon the preferred specification (Model 3), I simulate WTP for 1/1,000,000 risk reduction of sudden death for Canadian and U.S. individuals, males and females, individuals with and without a college education, and those who are married or not married. Additionally, for Canadian males, I simulate WTP for those with and without experience with out-of-plan diagnostic testing procedures.

The simulations are benchmarked for average sample income in the U.S. (roughly $42,000 U.S.). I assume a discount rate of 5%, and focus on the illness profile consisting of sudden death in the current period (i.e. death with no latency and no prior illness) so that the model's predictions can be compared to standard VSL estimates. Fitted WTP based on Model 3 is calculated with subjective and attitudinal variables simulated at their median values. These subjective and attitudinal variables include perceived risk of the illness or injury in question for the corresponding program, opportunity to increase exercise, reduce smoking, and see a doctor more regularly, and confidence in diagnosis and treatment under Canadian or U.S. health systems.

For each type of simulation, I vary age in five year increments from 25 to 80 years to permit graphing the implied age profile. In each case, 1000 random draws are taken from the asymptotically joint normal distribution of the maximum likelihood parameter estimates. This variability in parameter values, in combination with specified values for each of the explanatory variables which appear in the model, allow generation of a distribution for WTP that reflects the degree of precision in the estimated parameters.

Appendices III through VI present graphical depictions of the simulation results across age groups—broken out by gender, educational attainment, marital status, and out-of-plan experience. Individual figures show either 1) the median (solid line) and 5th and 95th percentiles (dashed lines) for 1000 draws from the estimated joint distribution of parameters calculated at each five-year age level between 25 and 80 years, or 2) just the median simulated value, for each of several different types of individuals, to compare age profiles for WTP across groups.

The age profile of WTP for sudden death is remarkably different. Canadians, regardless of gender, education, or marital status, have a substantially flatter age profile of WTP to reduce risk of early death, with peak WTP realized at a substantially older age (60 for Canadians compared to 35-40 for individuals from the U.S.). In general Canadians are WTP slightly more at older ages, but individuals from the U.S. are WTP substantially more at younger ages. Across the 1000 sets of parameter draws, peak median WTP for Canadians males is $9.17 annually (age 60), compared to $10.68 for males from the U.S. (age 35).[36] Females have substantially lower WTP for risk reduction of sudden death regardless of country of residence: a peak median WTP of $5.79 for U.S. females (age 35), and $3.17 for Canadian females.

While males, and individuals from the U.S., are willing to pay more for health risk reduction programs, college education and marital status goes a long way to explain the U.S./Canadian gap. Those who are married and have a college degree reveal substantially higher WTP in the U.S., but not in Canada. Peak median WTP for college-educated males in the U.S. is $13.59 (age 35), and for unmarried males in the U.S. it is only $7.62 (age 35). By contrast, peak median WTP for males in Canada is $8.55 (age 55) for those with a college degree, and to $8.99 (age 55) for those who are unmarried.

Perhaps most striking result, however, is that the difference between Canadian and U.S. male WTP values is almost entirely explained by Canadian experience with out-of-plan medical diagnostic tests. Peak median WTP for Canadians with out-of-plan experience jumps to $11.89 (age 60), with a fairly wide confidence band, and is well within the 90% interval for U.S. males.

[36] Aldy (2008) determine from age-specific hedonic wage equations that workers' VSLs rise from about $3.7 million between ages 18-24 to about $9.7 million in the 35-44 age bracket, then decline to about $3.4 million in the 55-62 year old bracket. The question of age profiles of WTP to reduce mortality risks is also addressed in Krupnick (2007) and Aldy (2007).

6. Conclusions

I have augmented an existing analysis of roughly 1800 U.S. survey respondents with an independent sample of roughly 1000 Canadian respondents to an analogous survey. The goal has been to assess the extent to which preferences for measures to reduce risks to life and health might differ across the two countries. The sampling properties of the internet consumer panel used for the Canadian survey (Ipsos Reid) is of somewhat lesser quality than the consumer panel for the U.S. survey (Knowledge Networks), but both samples exhibit distributions of age, gender, race, marital status, education and income that roughly match the population distributions in each country. Differences may exist in terms of how computer-savvy the respondents may be, especially among the older age groups. This stems from the fact that Knowledge Networks recruits panelists using random digit dialed telephone calls and equips non-internet-ready households with webTV equipment to permit them to answer surveys, whereas the Ipsos Reid sample is recruited primarily via the internet.

I find significant differences between Canadian and U.S. individuals in the marginal value of risk reduction programs, and these vary systematically with age, gender, education, and marital status. Moreover, differences in attitudinal and subjective health perception variables for the U.S. and Canadian samples account for small to large differences in marginal utilities associated with health risk reduction programs. In particular, the extent to which respondents felt they could get more regular exercise, or visit the doctor more frequently, affects both U.S. and Canadian appetites for additional programs to reduce the risks of different health threats, while being a non-smoker in Canada appears to substantially reduce the marginal value attached to avoiding illness.

The age profile of WTP to reduce the risk of sudden death in the current period (the risk reduction that maps most closely to a conventional VSL measure) is remarkably different across the two countries. Canadians, regardless of gender, education, or marital status, have a substantially flatter age profile of WTP to reduce risk of early death, with peak WTP realized at a substantially older age (60 for Canadians compared to 35-40 for individuals from the U.S.). Important gender differences are also seen for willingness to pay to avoid recovered/remission years: women are willing to pay to avoid additional time in this state, while men are not. This suggests that men, on average, view the recovered/remission state as equivalent to their current (pre-illness) state. While males and individuals from the

U.S. are willing to pay more for health risk reductions, educational attainment and marital status go a long way to explain the U.S./Canadian gap. Those who are married and have a college degree reveal substantially higher WTP in the U.S., but this is much less the case in Canada.

Perhaps most strikingly, differences between Canadian and U.S. male WTP is almost entirely explained by Canadian experience with out-of-plan diagnostic testing. Canadians who have more experience with U.S.-style health care provision, by going outside their provincial health plan to pay for services, convey preferences with respect to health risk reductions which are more similar to those of U.S. respondents.

This study has shown that failure to control for individual heterogeneity, in the presence of different types of respondents in the two countries, can easily bias the coefficients on the interaction terms involving the indicator for the Canadian sub-sample and imply that simply residence in Canada somehow accounts for different willingness to pay for health risk reductions. Different patterns in sociodemographic and attitudinal heterogeneity across the two countries account for a good deal of heterogeneity in choice behavior in the experiments, but there remain many dimensions where there are further differences that I can so far attribute only to the difference in jurisdictions, suggesting that there are limits to "benefit transfers," in WTP estimates across jurisdictions. Of course, there may still be other unobservable factors which differ across jurisdictions (e.g. other cultural differences) that could explain remaining differences in WTP across the two countries.

CHAPTER IV

CONNECTEDNESS AND BEHAVIOR

The degree of anonymity within a given social network should presumably impact an individual's cognitive or emotive attachment to other individuals in their community. It has been shown that large communities, where the degree of anonymity is high, generally have lower participation in volunteer activities, work in public projects, and informal assistance to friends and neighbors (Putnam, 2000). Allcott, Karlan, Mobius, Rosenblat, and Szeidl (2007) present evidence that the effect of community size on social engagement can be partially explained through network structure. The extent to which one's network of friends is interconnected determines the degree of "network closure" – or overlap between friends within a social network. Smaller communities generally have a high degree of overlap between friendship networks, and therefore an individual in a smaller community might be more likely to feel deeper ties to his or her local community than those living in large communities where the degree of overlap is generally small.

While Coleman (1990) was the first to suggest the theoretical connection between network closure and outcomes, Allcott et al. (2007) present the first statistical evidence that degree of network closure is negatively correlated with community size and significantly related to three outcomes: the degree to which an individual feels safe in their community ("How strongly do you agree or disagree with the following statement: "I feel safe in my school""), the propensity to get in trouble in school ("Since school started this year, how often have you had trouble getting along with other students?"), and grade performance (as measured by G.P.A. in math, science, English, and history).

I investigate the extent to which network closure is related to adolescent health behaviors. A number of sociological and psychological studies have shown a negative correlation between social isolation and measures self-efficacy. Presumably, the degree of anonymity within a given community affects an individual's attitudes toward engaging in behaviors contributing to health risk. Both excessive alcohol and tobacco use are associated with poor health outcomes, and have been shown to adversely impact labor market outcomes (Renna, 2008). And, the extent to which anonymity affects these outcomes is pertinent to informing policy geared toward reducing teen alcohol and tobacco use.

However, a large peer effects literature would suggest that the primary motivation for alcohol and tobacco use is found in socially demarcated norms of acceptable behavior. That is, being connected to a network of smokers or drinkers probably increases one's propensity toward smoking or drinking. Independent of peer influence, however, there may be a separate effect of feeling "well connected" that decreases one's propensity to engage in self-destructive behavior. The degree of connectedness presumably impacts the strength of social sanctions both for and against unhealthy behaviors. Whether or not there is an independent effect of connectedness on health risk behavior, separate from peer influence, remains a pertinent and valuable question.

It is nonetheless a difficult question to tease out empirically. The connectedness of a social network is by definition the aggregate result of individual choice. To the extent that friend selection is correlated with the choices underlying alcohol and tobacco use, or contemporaneous behavior may have no direction of transmission, identifying a causal effect of network structure on individual health behaviors is difficult.

Allcott et al. (2007) have presented suggestive evidence that the degree of connectedness matters to feelings of self-efficacy and pro-social behavior. However, their paper fails to adopt an identification strategy that is empirically defensible, and they make no effort to disentangle peer effects from a more general measure of social connectedness. This paper attempts to remedy these shortcomings. Through inclusion of peer-behavior and an identification strategy which removes the lion's share of endogenous choice from the empirical specification, I find that there is little evidence to suggest a separate and causal impact of connectedness on health risk behavior, and evidence of a robust impact of peer behavior on individual choice. In Section 1, I present a brief synopsis of the existing literature, and highlight identification approaches advocated in recent research. Section 2 proceeds to describe the closure concept presented by Allcott et al. (2007) and used here, while Section 3 describes the data used for empirical analysis. The methodology is outlined in section 4. I proceed to present replicated results of Allcott et al. (2007) and alternative specifications relevant to both outcomes presented in their paper, as well as smoking and drinking behavior in Section 5. Section 6 presents results from empirical estimation utilizing the panel structure of the data to better identify a causal impact, and Section 7 concludes.

1. Literature

As Bramoulle et al. (2009) term it, in recent years there has been a "virtual explosion" of literature on peer effects. Peer effects have been found in the propensity to engage in criminal activity (Glaeser, 1996), teenage violence (Case and Katz, 1991), welfare participation (Bertrand et al., 2000), teen pregnancy (Evans et al., 1992), retirement plan participation (Saez and Duflo, 2003), saving (Duflo and Saez, 2002), labor force participation (Woittiez and Kapteyn, 1998; and Aronsson et al., 1999), extracurricular choice (Bramoulle et al., 2009), and school achievement (Hoxby, 2000; Sacerdote, 2001; Zimmerman, 2003; Fertig, 2003; Hanushek et al., 2003; and Arcidiacono and Nicholson, 2005).[37] In the health field, much of the literature has relied on the AddHealth data to show peer effects on obesity (Christakis and Fowler, 2007; and Trogdon et al., 2008), alcohol use (Kremer and Levy, 2008), and alcohol, tobacco, and drug use (Lundborg, 2006; and Clark and Loheac, 2006). These papers have all found significant effects, and highlight some of the challenges in identification.

The problem of identification is probably best presented in Manski's (1993) seminal paper where he distinguishes between *endogenous*, *exogenous*, and *correlated* effects in social interactions. While endogenous effects (the influence of peer behaviors) and exogenous effects (the influence of peer characteristics) are descriptive of social interactions, correlated effects (where an individual's behavior is influenced by commonly shared characteristics of the group) are confounding factors separate (but correlated with) social interactions which should be empirically specified. Even if data exist which are rich enough to control for correlated effects, distinguishing between endogenous and exogenous effects has been the major hurdle. In what Manski termed the *reflection problem*, simultaneity in behavior makes it difficult to isolate exogenous (causal) effects from endogenous behavior. To the extent that

[37]For recent surveys of the literature in this field, see Blume and Durlauf (2005), and Soetevent (2006).

my own behavior influences the behavior of my peer reference group, inferring a causal relationship of my peers' behaviors on my own is troubling.

Insufficient care in identification has received harsh criticism in the economics literature. For example, in response to Christakis and Fowler (2007), Cohen-Cole et al. (2008a) find that omission of confounding environmental factors which overlap with the decisions of network members, and failure to use lagged peer characteristics, result in the spurious suggestion that network similarity in weight transmits obesity. Further, Cohen-Cole et al. (2008b) test omission of these contextual factors on specifications showing peer effects in such implausible things as acne, headache prevalence, and height on network similarity, and find significant results when contextual effects are omitted, and insignificant results when contextual effects are included.

More successful studies have used a variety of identification approaches. As Kremer and Levy (2008) point out, it may be the case that individuals within social groups converge due to unobserved similarities, or have come together to achieve similar outcomes, rather than influence each others' behavior. In their paper, Kremer and Levy utilize a random lottery assignment of roommates at a large state university, and the lag of roommate (peer) behavior to isolate the exogenous formation of peer groups (random assignment) and contemporaneous choice (propensity to drink prior to enrollment), from exogenous peer effects in alcohol use and school performance. For males, they find significant peer effects in both use of alcohol and school performance, but interestingly enough find no effects of family background characteristics of roommates.

Other recent work has appealed to the "arrow of time," exploitable in increasingly rich and available panel datasets. Clark and Loheac (2007) investigate the social influence of peers in tobacco, alcohol, and marijuana use with AddHealth data. They build upon the standard linear model for peer interaction offered in Pollak (1976), using the lag of peer group behavior, and school fixed-effects to isolate plausibly exogenous variation between grade cohorts of "peer" behavior. By lagging peer group behavior they "avoid one aspect of the reflection problem: while my behavior may depend on what my peers did in the past, their past behavior cannot depend on what I currently do," (767). Moreover, in addition to controlling for school fixed effects (and thereby, any non-time-varying trends in unobserved characteristics that are shared among adolescents in a given school, or characteristics of the school itself which may affect behavior), they include individual-level contextual controls for parent smoking, drinking and drug behavior, as well as individual school choice by whether the student was a recent mover and whether the parent's choice of neighborhood was influenced by the local school's characteristics. Finally, they vary the reference group's composition (both same grade, and grade above the individual student), arguing that it is less likely that the older cohorts will respond to the behavior of their younger peers, as well as allowing for non-linear effects by using split sample techniques. They find strong peer effects in smoking and drinking behavior, which vary in magnitude by sex, but little evidence of peer effects in marijuana use.

Finally, Bramoulle et al. (2009) provide a theoretical econometric insight inspired by spatial econometric theory, and apply their theory to AddHealth data. They show that it is possible to find groups of networks with sufficient structure to identify a causal peer effect. Provided that there exists a networked grouping of friends who are linked in such a way that within that grouping there exist "friends of an individual's friend with whom that individual is not also a friend," the characteristics of the friends-once-removed can be used to instrument for the behavior of the individual's own friend. The authors give the example of an "intransitive triad," with a set of students i, j, k, where i is affected by j, and j is affected by k, but i is not affected by k, as the type of network structure exploitable for identification. They apply their econometric model to the AddHealth data, and find significant exogenous peer effects in extracurricular choice among students for the sub-sample for which this identification methodology was relevant.

While much has been written on peer effects, the literature on the effect of connectedness on individual behavior is thin. Other than Allcott et al. (2007), to my knowledge, only one paper has been published which identifies a separate and causal effect of the degree of connectedness on labor (or health) behavior. Babcock (2008) uses a simple measure of connectedness (the sum of nominated friends and nominations as a friend of others) as a key variable to explain educational attainment, employing much the same identification strategy as in Clark and Loheac (2007). They remove the influence of an individual's own linkages in a mean grade-cohort measure of interconnectedness to present a plausibly exogenous measure of connectedness, exploit the panel structure of AddHealth to lag reference group connectedness as explanatory of current individual behavior, and rely on school fixed effects to control for unobserved contextual effects. They find connectedness of the grade-cohort (minus an individual's own influence) matters significantly to educational attainment decisions later in life. These findings are motivated by the notion that there are consumption effects in schooling: those well connected in high school gain higher utility from schooling when they are well connected (perhaps due to social benefits or working together, and/or participation in non-academic exercises within or outside of schooling).

2. Network Closure

While Babcock (2008) presents some intriguing results, their measure for connectedness is rather coarse, and fundamentally grade-school specific. Although he employs an instrumental variables specification with grade-school nominations as an instrument for individual connectedness, he does not attempt to confine the analysis to a pertinent sphere of friends. It is important to stress the notion of a *pertinent* sphere of friends. It very well may be that if we take no account for the *number* of "degrees of separation" between people, an individual in a particular grade, in a particular school, very well may be connected to everyone else in that particular grade and particular school. Under such an approach, no attempt is made to capture the degree of interconnection that individual has within his *socially relevant* group of friends – i.e. those people surrounding the person who may (or may not) impact his behavior choices, and/or give him a sense of being "well-connected" or "popular." Allcott et al. (2007) offer a direct and individual-specific measure for social connectedness, which is bounded to a pertinent sphere of social connection. I adopt their measure for network closure, which was first outlined in Mobius and Szeidl (2006).

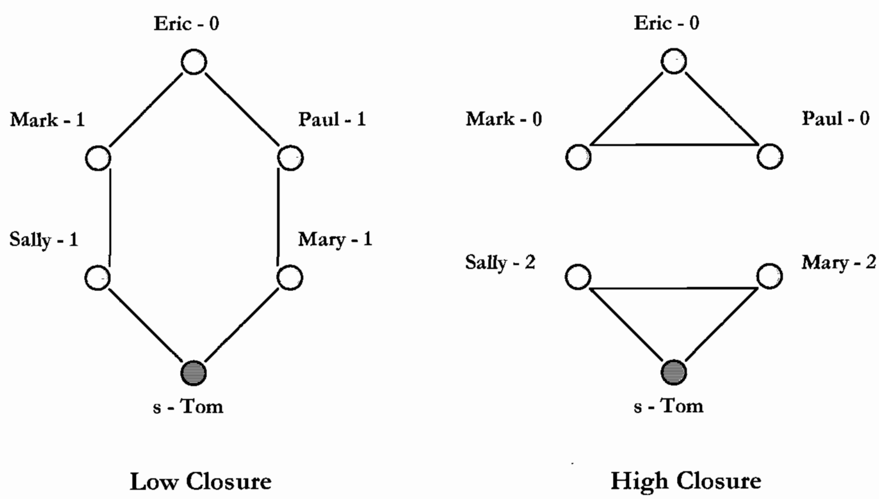
I begin with several definitions necessary to understand the metric. Define a node as a location-specific point associated with an individual (or "agent') within a social network of friends. Further define an edge as a pairwise connection between two immediately adjacent nodes. Define a path as a sequence of contiguous edges linking any two nodes s and t (otherwise called "agents') within the social network of friends. A trust flow indicates the degree of connection between any two agents. Specifically, let a trust flow between two agents s and t be defined as the number of disjoint paths connecting s and t. A path is disjoint if each edge is assigned a flow capacity equal to 1, such that between any two agents, the set of all potential paths connecting the two agents cannot use a particular edge in the network more than once. To limit the sphere of friends to a relevant network size, all edges in a path must be within distance *K* from agent *s*. Any edge can be assigned a value denoting distance from agent *s*. First, the minimum distance to each node defining a particular edge can be taken as the length (number of edges) of the shortest path originating from *s* to reach the node. Total distance to the edge is, therefore, the simple average of the minimum distance to each node.

Given the above preliminaries, the formal definition of network closure can be presented. For any positive integer *m*, network closure (*m,K*) for agent *s* can be computed as the share of total trust between *s* and all other agents, comprised of paths within distance *K*, that have pairwise trust flows exceeding *m*. High closure values have a greater proportion of total trust originating from pairwise connections with a high degree of trust, while low closure has a greater proportion of total trust originating from pairwise connections with a low degree of trust.

Take, for example, six connected individuals – Tom, Sally, Mary, Paul, Mark, and Eric. Suppose these individuals form friendship connections in two distinct ways, as shown in Figure 6. We are interested in computing the closure of Tom. In the first diagram, Tom has nominated (or been nominated by) Sally and Mary as direct friends. Sally nominated (or was nominated by) both Tom and Paul, while Mary nominated (or was nominated by) both Tom and Mark. Eric, at the top of the diagram, is directly linked to both Mark and Paul. If we can use paths connecting these individuals once, and there was no restriction on the circle of trust size, we could get to each of these individuals one of two ways (go either direction in the connected circle, and we can get to each of these people in two ways using the linkage path edges each only once). But, if we are interested in computing trust between Tom and each of the other individuals, but only with a circle of trust size K=1.5, the paths connecting Eric and Mark and Eric and Paul must be excluded. To see why, note that Paul is two people removed from Tom, as is Mark. Eric is three people removed from Tom. Therefore, the path connecting Eric to Mark is on average 2.5 people away from Tom because one node defining this edge is a distance 3 from Tom and the other is a distance 2 from Tom. The same is the case for the edge connecting Eric and Paul. Therefore, there is no path within a distance of 1.5 connecting Tom to Eric, and so "trust" between Tom and Eric is zero.

Moreover, because we cannot use these two edges between Mark and Eric and Paul and Eric to compute different paths connecting the other people to Tom (within a distance of 1.5 from Tom), there are no longer two complete paths connecting the other individuals in the network. There is only one direct path connecting Tom to Sally, Tom to Mary, Tom to Mark, and Tom to Paul. Therefore, from Tom's perspective, there is a flow of trust to each of these people equal to 1. If closure is the proportion of total trust (here equal to 4) greater than m=1, closure is zero in the first example.

Figure 6: Network Closure for Simplified Networks, with K=1.5, and m=1



Source: Allcott et al. (2007). Numbers assigned to each node represent the trust flow from agent *s* to each individual/node in the network. In the left panel, network closure (1.5,1) for agent *s* (Tom) would be zero, while in the right panel, network closure would be 1. Note that, in the left panel, trust flow between agent *s* and the top-most individual is 0, because either edge needed to connect s to this node is of distance 2.5 from *s*.

In the second example, friendship nominations are quite different. While Tom is friends with both Sally and Mary, and Sally and Mary are both friends, none of these people are friends with the other sub-group (who are similarly linked). Clearly from Tom's perspective, Mark, Paul and Eric have zero trust (there is no direct path connecting them), while there are two ways of getting to both Sally and Mary (all paths connecting Tom to Mary, Tom to Sally, and Sally to Mary are within a distance of 1.5, since the paths connecting Tom to Sally and Tom to Mary are each 0.5 away from Tom, while the path connecting Mary and Sally is on average 1.5 away since the first node is 1 away, and the second node is 2 away). If closure is the proportion of total trust greater than m=1, all of the nodes with positive trust (i.e., people sufficiently close to Tom to be included in the computation) have trust greater than 1, and so closure equals 1.

3. Data

The National Longitudinal Study of Adolescent Health (AddHealth) was administered to all students in 142 schools in the United States, with responses totaling roughly 86,000 individuals in grades 7-12 during the 1994-95 school year. The initial survey used systematic sampling methods with implicit stratification, and was followed by three subsequent waves, the most recent of which was conducted in 2008. The survey includes data on respondents' social, economic, psychological and physical well-being, with contextual data on the family, neighborhood, community, school, friendships, peer groups, and romantic relationships – all of which were derived from a combination of respondent-specific in-home and in-school questionnaires, a school administrator questionnaire, and a parent questionnaire. While the in-school survey (pencil and paper) was administered to all respondents (and administrator questionnaire to all schools), the in-home and parent surveys (audio computer-assisted self-interviewing) were administered to a 1:5 sample of all respondents. The survey data includes respondent-identified names of up to 10 friends, 5 male and 5 female.[38] These friends are linked in a friendship network file, allowing for computation of measures of network structure.

[38] Inherent in survey design is top-coding of friend respondents – that is, if a respondent named 5 friends, either male or female, it is plausible and even likely that that person is friends with more than five people of the same sex. I include a crude control for this inherent limitation in survey design by including dummy variables for overall top coding (10 friends nominated), and male/female friend top coding.

4. Methodology

While Babcock (2008) finds an effect of connectedness on length of schooling using a relatively simple measure for connectedness. To date, however, there has not been an attempt to isolate a casual effect on health behavior. The model specifications presented in Allcott et al. (2007) offer a useful starting point from which to study the effects of connectedness on behavior. Using data from the in-school AddHealth sample[39], I first adopt their specification relating community size (as measured by a student's grade size) to network closure. For individual respondent i and student grade size j, $i \in j$:

$$\textbf{closure}_i = \alpha + \beta \textbf{ community size}_j + \gamma \textbf{ controls}_i + \varepsilon, \tag{10}$$

where $controls_i$ include background and demographic controls for father's education and race, and so-called fractionalization controls as motivated in the literature.[40] Additionally, equation (10) is expanded to include friend fixed-effects and grade fixed-effects in later specifications. Outcome specifications follow the third model specification in Alcott et al. (2007), including closure as the key variable, and controlling for community size:

$$\textbf{outcome}_i = \delta + \theta \textbf{ community size}_j + \varrho \textbf{ closure}_i + \gamma \textbf{ controls}_i + \dot{\upsilon}. \tag{11}$$

As Allcott et al. (2007) admit, it is difficult to identify a causal relationship between network structure, community size, and outcomes, since it would be impossible to control of all factors which are jointly correlated with explanatory variable(s) and the outcome of interest. Although they "attempt to alleviate these [omitted variable] concerns by including a rich set of controls, such as social and economic factors, in all regressions," they admit "we cannot fully rule out alternative explanations, however, and hence we interpret the findings from the analysis about closure and outcomes above as suggestive evidence." The availability of individual response data across a representative sample of schools, allow for

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39 The in-school sample has the advantage of having been administered to a larger collection of high school students. However, it was only administered once, in Wave 1, and for a limited set of questions. Further identification strategies are explored below.

40 Fractionalization is the probability that two people chosen randomly from the grade/school will be of different categories. Inclusion of these controls is advocated in Alesina and Laferrera (2003).

the inclusion of fixed effects (Babcock, 2008; and Clark and Loheac, 2007). Moreover, the unbalanced sample of respondents from schools of varying size, which has been shown to yield bias interval estimates, can be further accommodated by common econometric techniques. To build on equation (11), I include a host of additional controls, revised variable specification, fixed effects, and clustering at the school level to mediate some of these deficiencies.[41] These models can then be extended to outcomes of smoking and drinking behavior.

Incorporating these adjustments, a linear model of individual behavior on behavior outcomes can be expressed as follows, for individual *i*:

$$\mathbf{O}_i = \alpha + \beta + \gamma + \delta\mathbf{X}_i + \psi\mathbf{Z}_i + \mu\mathbf{N}_i + \varepsilon_i, \tag{12}$$

where: O_i = outcome variable (Allcott et al., 2007 outcomes, smoking and drinking)

α = constant

β = friendship network size deviation from mean α

γ = school-specific deviation from mean α

X_i = vector of individual-specific contextual controls

Z_i = vector of individual-specific controls, and

N_i = degree of network closure.

Since the sizable peer effects literature has focused on the role of social networks in establishing behavioral norms, it is important to control for peer behavior for a separate "connectedness" effect to be identified. Omission of peer effects known to influence behavior could potentially bias estimated results from any specification of outcomes on

[41] Variable definition was found to be a key concern in the Allcott et al. (2007) study. A number of categorical variables (including outcome variables for "feel safe" in their community and getting in "trouble" in school) were unadjusted from an arbitrary scale weighting. Generally, these variables are best converted to discrete variables, both to ameliorate concerns about specification bias, as well as interpretation of results. Additionally, treatment of missing data was not handled rigorously in their study, and as such, I was not able to replicate their sample size. For the purposes of expanded specifications based upon the in school sample, I treat missing and "refused to comment" data as missing, such that these observations are dropped from the analysis when appropriate. Finally, closure was computed for a larger number of individuals in Allcott et al. (2007) than could be supported by friendship nomination data received from University of North Carolina as of August 2007 (that is, if an individual neither nominates friends *contained in the sample*, nor is nominated by others in the sample, closure cannot be computed). Sample sizes differ in the analyses; but parameter estimates are similar, both in terms of magnitude and significance.

network closure if closure is correlated with peer level behavior. However, aforementioned identification outlined in the peer effects literature needs to be adequately addressed. I use the calculation of trust flows from the closure computation and level of peer behavior for peers contained within the sample to compute a trust-weighted average of peer behavior for each individual *i*. To the extent that an individual chooses their friends based upon their friend's behavior, endogeneity bias will remain. Therefore, I remove the level effect of directly nominated friends from the trust-weighted average of peer level behavior, and include this value as an independent control in equation (12) regressions for all Allcott et al. (2007) outcomes, as well as drinking and smoking:[42]

$$\mathbf{O}_i = \alpha + \beta + \gamma + \delta\mathbf{X}_i + \eta\mathbf{O}_{-i} + \psi\mathbf{Z}_i + \mu\mathbf{N}_i + \varepsilon_i, \tag{13}$$

where O_{-i} is a vector of each individual's trust-weighted aggregation of peer level behavior.

However, by definition, individual choice matters to network formation. One chooses who their friends are, and they may even choose their friends for the friends with whom their friends are friends. If friend selection is correlated with a behavior of interest (such as a friend choosing a group of individuals who are socially active, and perhaps more prone to drink), what appears as the influence of being connected on the behavior (drinking) may well simply be a reflection of the individual's choice. Moreover, network formation happens in a contemporaneous fashion: friends choose each other simultaneously. I take two approaches to remedy (at least in part) this potential for endogeneity.

To begin with, the exogenous choice of *K*, the network of immediate and pertinent friends, allows for an obvious and powerful instrument. The average degree of network closure of all friends immediately adjacent to those within distance *K*, but excluded from the closure computation by the researcher-defined selection of *K*, will be strongly correlated with

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[42] Including this measure of peer behavior as an explanatory control variable is somewhat of an adhoc version of the identification suggested by Bramoulle et al. (2009). However, 1) since trust flow is computed based upon the inclusion of direct friends, 2) to the extent that direct friends influence the level of trust of friends of friends, and 3) if the structure of a given individual's friendship network does not fulfill the intransitivity assumption upon which Bramoulle et al. (2009)'s identification is based, these measures will likely be at least partially endogenous. Therefore, interpretation of estimated coefficients for peer-level variables should be made with care, and results interpreted as suggestive of a relationship, and not necessarily causal. I argue however, that these measures are reasonably removed from direct bias associated with friend selection, and provides for sufficient control of peer effects for the primary aim of identifying an effect of closure on behavior.

any particular individual's degree of closure, but is presumably removed (to a large degree) from the potentially behavior-related and contemporaneous selection of friends. I estimate equation (13) using a two stage model (least squares for continuous dependent variables, and maximum likelihood for discrete cases), with the average closure beyond *K* as an instrument for N_i, the individual's degree of network closure (m,K).

Second, I follow much of the peer effects literature, utilizing the panel nature of the data as follows:

$$\mathbf{O}_i = \alpha + \beta + \gamma + \delta\mathbf{X}_{i-1} + \eta\mathbf{O}_{-i} + \psi\mathbf{Z}_{i-1} + \mu\mathbf{N}_{i-1} + \varepsilon_i, \qquad (14)$$

To the extent that an individual's friend's behavior in Wave I of the survey cannot be dependent upon that individual's Wave III behavior, I avoid much of the *reflection problem.* However, in the case of behaviors which have an addiction aspect (in particular, smoking), endogenous effects could persist into Wave III, if behavior is fully established by Wave I. Therefore, I specify models pertaining to health behavior in Wave III as conditional on having not smoked (or consumed alcohol) in Wave I as follows:

$$(\mathbf{O}_i \mid \mathbf{O}_{i-1} = \mathbf{1}) = \alpha + \beta + \gamma + \delta\mathbf{X}_{i-1} + \eta\mathbf{O}_{-i-1} + \psi\mathbf{Z}_{i-1} + \mu\mathbf{N}_{i-1} + \varepsilon_i, \qquad (15)$$

The interpretation of the effect of Wave I closure on Wave III behavior is modified to one of whether connectedness while in school contributed to the propensity to engage this behavior later in life, having not smoked in Wave I. However, these results cannot be applied to all individuals, since the decision to abstain from smoking (or drinking) early in life is most likely associated with behavior later in life.[43]

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[43] I would posit, however, that these characteristics are negatively correlated with the decision to smoke later in life, yielding attenuation bias in the estimated results.

5. Empirical Results

5.1. Descriptive Statistics

Table 12 presents descriptive statistics for the variables used in the replication of results in Allcott et al. (2007). Dependent variables used in the replication include Feelsafe, Trouble, and Grade Point Average (GPA) as defined in their study. On average, respondents feel relatively safe in their community (although this measure is subjective to each respondent's interpretation of the five-point scale).[44] In general, students infrequently get in trouble with other students in their school. Responses to this question were 0 for

Table 12: In School Summary Statistics

Alcott et al. (2007)	Obs	Mean	Std. Dev.	Min	Max
Dependent Variables					
Feelsafe in community	68,341	3.690	1.065	1	5
Trouble in school	73,039	1.531	1.460	0	4
Grade point average	67,571	2.672	1.006	0	4.0
Interest Variables					
Closure (m=2, k=2.0)	77,020	0.603	0.355	0	1
Size of Student's Class (per 100 students)	77,020	2.727	1.476	0.10	6.97
Demographic Controls					
Race: Caucasian	77,020	0.529		0	1
Father education (less than high school)	77,020	0.081		0	1
Father education (high school diploma)	77,020	0.209		0	1
Father education (some college)	77,020	0.349		0	1
Location and Fractionalization Controls					
Location: Suburban	77,020	0.569		0	1
Location: Rural	77,020	0.103		0	1
Fractionalization: Grade	77,020	0.700	0.109	0.037	0.834
Fractionalization: Race	77,020	0.328	0.143	0	0.5
Fractionalization: Education	77,020	0.798	0.124	0.159	0.997

Source: AddHeath Wave 1, In School Sample 1994-1995

"never," 1 for "just a few times," 2 for "about once a week," 3 for "almost everyday," and 4 for "everyday." However, the average is between "just a few times" and "once a week," where a weekly occurrence of "getting in trouble with other students" could be burdensome (depending on what "trouble" means to each student). The bounded continuous range for GPA follows the usual 4.0 scale, with students on average doing better than a typical 2.0 average.

[44] Following Allcott et al. (2007), I recode responses to 5 for "strongly agree" and 1 for "strongly disagree."

Table 13 presents reclassified descriptive statistics for all variables from the Wave I in-school sample used in the empirical analysis. In particular, ordinal variables for Feelsafe and Trouble are recast as discrete dependent variables, where Feelsafe=1 if the respondent "agreed" or "strongly agreed" to "feeling safe" their school, and Trouble=1 if the respondent "got in trouble" once a week or more. While most (63 percent) students felt safe in their school, a sizable 36 percent of students get in trouble with other students as regularly as once a week or more.

Variables Smoke and Drink are converted to discrete variables as well to account for unequal intervals of answers available to the respondents. A sizable 17 percent of students in the sample smoked at least three times per week or more in the previous month, while a larger-than-expected 45 percent of these students had not consumed alcohol in the last year. On average, students have a relatively high degree of network closure (defined here and in all specifications using m=2 and k=2.0). A full 60 percent of trust within a distance of k=2.0 (roughly speaking, this includes 3 to 4 friends once removed) comes from nodes with three or more paths of interconnection. This average appears to be slightly higher than presented in Allcott et al. (2007),[45] although their analysis appears to include observations for which closure is zero due to having nominated no friends. Here, and in all subsequent analysis, I exclude these observations since they could mean many things: it could be that the student really isn't interconnected, or alternatively, the survey was not filled out completely, and we have very limited information on their actual friendship network, or their nominated friends were not included in either the in-home interviews or in-school survey (for example, if the student's friends were both absent on the day of the school survey was administered, and was also not selected for the in-home interview, they would be excluded from the sample upon which closure is computed).

[45] Although Allcott et al. (2007) do not include descriptive statistics in their paper, they include a graph of closure against friendship size. Using a back-of-the-envelop calculation for the average number of friends nominated, I guess their average to be roughly between 0.40 and 0.50 for closure (2,2).

Table 13: Reclassified In School Summary Statistics

Respecification	Obs	Mean	Std. Dev.	Min	Max
Dependent Variables					
Feelsafe (1 = agree or strongly agree)	62,276	0.634		0	1
Trouble (1= at least once a week)	66,430	0.363		0	1
Grade point average	61,553	2.680	1.001	0.0	4.0
Smoke (1 = 3 or more times a week)	69,737	0.173		0	1
Drink (1= never drank in last 12 months)	65,736	0.446		0	1
Interest Variables					
Closure (m=2, k=2.0)	62,276	0.615	0.349	0	1
Size of Student's Class (per 100 students)	62,276	2.737	1.491	0.10	6.97
Feelsafe (peer-weighted)	62,276	0.489	0.168	0	1
Trouble (peer-weighted)	62,276	0.275	0.132	0	1
GPA (peer-weighted)	62,276	2.010	0.563	0	4.0
Smoke (peer-weighted)	62,276	0.125	0.115	0	1
Drink (peer-weighted)	62,276	0.336	0.168	0	1
Demographic Controls					
Age of respondent (Wave 1)	62,276	15.107	1.670	10	19
Age-sqrd of respondent (Wave 1)	62,276	231.001	50.145	100	361
Sex: Male = 1, Female =0	62,276	0.478		0	1
Race: 1 = African American	62,276	0.161		0	1
Race: 1 = Hispanic Ethnicity	62,276	0.098		0	1
Race: 1 = Asian or Pacific Islander	62,276	0.047		0	1
Race: 1 = American Indian	62,276	0.007		0	1
Race: 1 = Other non-white	62,276	0.012		0	1
Race: 1 = Multicultural	62,276	0.128		0	1
Father education (less than high school)	62,276	0.079		0	1
Father education (some college)	62,276	0.113		0	1
Father education (bachelor degree)	62,276	0.165		0	1
Father education (grad school)	62,276	0.095		0	1
Father education (uncertain)	62,276	0.065		0	1
Mother education (less than high school)	62,276	0.097		0	1
Mother education (some college)	62,276	0.152		0	1
Mother education (bachelor degree)	62,276	0.195		0	1
Mother education (grad school)	62,276	0.077		0	1
Mother education (uncertain)	62,276	0.065		0	1
Location and Fractionalization Controls					
Region: West	62,276	0.186		0	1
Region: Midwest	62,276	0.207		0	1
Region: South	62,276	0.447		0	1
Region: Northeast	62,276	0.159		0	1
Location: Suburban	62,276	0.566		0	1
Location: Rural	62,276	0.104		0	1
Fractionalization: Grade	62,276	0.703	0.105	0	0.833
Fractionalization: Race	62,276	0.484	0.187	0	0.815
Fractionalization: Father's education	62,276	0.874	0.059	0.447	0.996
Friend Truncation Controls					
Total friend truncation	62,276	0.415		0	1
Male friend truncation	62,276	0.554		0	1
Female friend truncation	62,276	0.555		0	1

Source: AddHeath Wave 1, In School Sample 1994-1995. Reclassification includes coding missing and refused observations for variables correctly, categorical treatment of ordinal variables where appropriate, and excluding observations for which insufficient friend linkage information was available to compute closure.

In Table 14, I present descriptive statistics for all variables used in the in-home sample. By Wave 3, where respondents were between the ages of 18 and 28, about a third of the sample had smoked cigarettes, and on average respondents drank five or more drinks in one sitting 20 times in the previous year. I include a similar measure for peer weighted drinking behavior in Wave 1, and a continuous variable for peer-weighted Wave 1 smoking behavior.

Attrition from Wave I in the Wave 3 sample is relatively high (about three out of four from the Wave 1 sample responded to the Wave 3 survey). The extent to which this selection into Wave 3 could bias estimated results is not examined; suggestive evidence is that the propensity to drop out of the sample is not strongly correlated with health risk behavior or key observables. For example, 58 percent of those in Wave 1 who did not participate in Wave 3 had reported smoking at some point in their life, compared to 57 percent for the entire sample. About 48 percent of those not responding to the Wave 3 survey had consumed alcohol in the prior twelve months as of Wave 1, compared to 47.5 percent for the entire sample. Additionally, parental income (as reported in Wave 1) was about $44,000/year for drop outs, compared to roughly 46,000/year for the entire sample. There were minimal differences in terms of age and sex as well.[46] However, it is worth noting that there is a stronger representation of minorities in the Wave 3 sample than observed in the in-school sample due to intentional over-sampling by AddHealth.

Additional controls are included for parent smoking behavior, and neighborhood selection as advocated in Babcock (2008). Roughly a third of parents smoked and/or drank alcohol and parental use of illegal drugs was relatively uncommon. About 43 percent of the sample had moved to their current residence within the 5 years prior to the Wave 1 sample period, and only 12 percent of parents chose the neighborhood location primarily for the local school's perceived quality.

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[46] Sample selection controls are not included, and selection models are not employed to date. However, these may be explored in the future.

Table 14: In Home Summary Statistics

	Obs	Mean	Std. Dev.	Min	Max
Dependent Variables					
Bachelor Degree	12,778	0.119		0	1
Smoke (Wave 3)	12,768	0.326		0	1
Drink 5 or More (Wave 3)	12,727	19.924	50.745	0	365
Interest Variables					
closure_20	12,768	0.632	0.362	0	1
GPA (peer-weighted)	12,768	0.846	0.676	0	4
Smoke (Wave 1 peer-weighted)	12,768	0.502	1.093	0	40
Drink 5 or more (Wave 1 peer-weighted)	12,768	0.072	0.151	0	6
Demographic Controls					
Age of respondent (Wave 1)	12,768	15.593	1.714	11	21
Age-squared of respondent (Wave 1)	12,768	246.069	53.116	121	441
Sex: Male = 1, Female =0	12,768	0.467		0	1
Race: 1 = African American	12,768	0.219		0	1
Race: 1 = American Indian	12,768	0.035		0	1
Race: 1 = Asian or Pacific Islander	12,768	0.086		0	1
Race: 1 = Hispanic Ethnicity	12,768	0.158		0	1
Race: 1 = Multicultural	12,768	0.128		0	1
Race: 1 = Other non-white	12,768	0.012		0	1
Parent education (less than high school)	12,768	0.133		0	1
Parent education (high school)	12,768	0.259		0	1
Parent education (some college)	12,768	0.253		0	1
Parent education (bachelor degree)	12,768	0.129		0	1
Parent education (grad school)	12,768	0.085		0	1
Religion: Weekly Attendance	12,768	0.401		0	1
Religion: Monthly Attendance	12,768	0.199		0	1
Religion: Some Attendance	12,768	0.178		0	1
Location Controls					
Unemployment Rate (County)	12,768	0.068	0.023	0.026	0.145
Proportion Urbanized (County)	12,768	0.637	0.395	0	1
Proportion Rural (County)	12,768	0.250	0.275	0	1
Parent Behavior					
Parents Drink	12,768	0.304		0	1
Parent Drug Use	12,768	0.031		0	1
Parents Smoke	12,768	0.307		0	1
Neighborhood Selection					
Moved since 1990	12,768	0.431		0	1
Parents Chose Neighborhood for Schools	12,768	0.121		0	1
Friend Controls					
Friend Nomination (total)	12,768	2.994	2.555	0	10
Friend Nomination (in sample)	12,768	1.827	2.021	0	10

Source: AddHealth Wave 1 and 3, in-home sample, 1994-1995 and 2003.

5.2. Empirical Results

In Table 15, I present replicated results for the effects of network closure on community size as presented in Allcott et al. (2007). Although I was not able to replicate sample size, primarily due to insufficient friendship nomination data, the sample sizes, as well as the degree and magnitudes of estimated coefficients, are similar.[47] The first four models follow their published results, while the fifth specification includes clustering at the school level to account for the unbalanced panel. As with Allcott et al. (2007), I find a strong negative relationship between closure and student class size.

Table 15: Allcott et al. (2007) Closure and Community Size Results

Closure m=2, k=2.0, dependent	(1)	(2)	(3)	(4)	(5)
	model1	model2	model3	model4	model 4 + clust
Size of Student's Class					
	-.05007	-.04719	-.03582	-.03457	-.03457
	(59.64)**	(62.87)**	(45.27)**	(42.33)**	(4.16)**
Constant	.7384	.7306	.4862	.3852	.1088
	(284.48)**	(314.82)**	(37.10)**	(18.90)**	(0.68)
Friend fixed-effects	No	Yes	Yes	Yes	Yes
Grade fixed-effects	No	No	Yes	Yes	Yes
Background/demographic controls	No	No	Yes	Yes	Yes
Fractionalization controls	No	No	No	Yes	Yes
Observations	77463	77463	77463	77463	77463
R-squared	0.04	0.05	0.10	0.11	0.28

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%

Replicated results for outcomes Feelsafe, Trouble, and GPA are presented in Table 16. The first specifications for each dependent variable follow the last specification in Allcott et al. (2007), including both friend nomination fixed effects, as well as fractionalization controls. As with the closure-on-community size results, the initial specification produce results which are similar in magnitude and strongly significant as reported in their study. High closure is associated with a higher propensity to feel safe at school, a lower propensity to get in trouble with other students at school, and improved academic performance.

[47] See footnote 42.

Table 16: Allcott et al. (2007) "Prosocial" Outcome Results

	Feelsafe				Trouble				Grade point average			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Allcott	Clust	FE and Clust	IV	Allcott	Clust	FE and Clust	IV	Allcott	Clust	FE and Clust	IV
Closure (m=2, k=2.0)	.2238	.2238	.2369	.42	-.1643	-.1643	-.1629	-.125	.2923	.2923	.3235	.3994
	(16.86)**	(7.20)**	(9.92)**	(3.29)**	(9.36)**	(7.81)**	(8.09)**	(1.34)	(23.68)**	(8.11)**	(14.70)**	(2.31)*
Size of Student's Class	-.01987	-.01987	.02113	-.01786	-.02957	-.02957	.095	-.03466	-.02441	-.02441	-.08202	-.0308
	(6.59)**	(1.26)	(1.74)	(1.00)	(7.38)**	(3.06)**	(5.36)**	(3.63)**	(8.70)**	(1.30)	(5.52)**	(1.74)
Friend fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background/demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fractionalization controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School fixed-effects	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Observations	68676	68676	68676	68199	73398	73398	73398	72875	67881	67881	67881	67374
R-squared	0.05	0.05	0.01	0.04	0.05	0.05	0.02	0.05	0.09	0.10	0.06	0.09

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%

Subsequent specifications include clustering at the school level, inclusion of fixed effects and instrumenting for closure with the closure of connected schoolmates beyond the researcher-defined circle of trust *K*. Except for the Trouble variable, magnitude and significance of Feelsafe and GPA on closure are relatively unaffected by additional controls for contextual factors (school fixed effects), error correction (clustering), or endogeneity (instrumentation for closure). The loss of significance on the propensity to get in trouble in school for the instrumental variables specification is telling: it suggests that the mechanism by which connectedness affects prosocial behavior is directed in certain ways. While connectedness seems to matter to the cognitive perception of one's own safety in school and school performance, it has less of an effect on one's own propensity to get in trouble with other students. It is possible that the effect of network structure on individual choice may be more of a "social norm" story for Trouble, and more of a personal attachment (or "connectedness") story for Feelsafe and GPA. That is, the "level effect" of peer behavior could establish social norms for trouble-laden behavior, while success and comfort in school is affected both by the level effect of peer safety perceptions and school performance, but independently by the degree of connectedness to school peers.

Table 17 presents results for an expanded set of controls, and inclusion of peer-weighted levels for the dependent variable of interest. Additional controls are included for education (the educational attainment for both the mother and father, re-specified for missing refused responses), race controls (for race and ethnicity beyond just Caucasian and re-specified for missing refused responses), religion (monthly, weekly, or some attendance), and fractionalization (education and race include the full set of categories included in these data).

When the level effect of peer behavior is included in the specification, connectedness seems to once again matter to *all three* prosocial outcomes. It is possible that omission of peer behavior, added demographic controls, and improved variable measurement induced omitted variables bias. Clearly, an effect of closure on the likelihood to get into trouble with other students in one's own school is found only once controls for the "social norm" effect are included. This is perhaps unsurprising: being connected to peers who are prone to get in trouble with other students establishes a social norm of acceptable "trouble-laden" behavior.

Table 17: Revised "Prosocial" Outcome Results using Peer-weighted Controls

	Feelsafe (1= agree or strongly agree)				Trouble (1= 3 times/week or more)				Grade point average			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Base	Peer-weighted	Peer-weighted FE	Peer-weighted IV	Base	Peer-weighted	Peer-weighted FE	Peer-weighted IV	Base	Peer-weighted	Peer-weighted FE	Peer-weighted IV
Closure (m=2, k=2.0)	0.073	0.047	0.079	0.171	-0.046	-0.041	-0.045	-0.220	0.248	0.147	0.195	0.275
	(4.67)**	(3.74)**	(8.12)**	(3.05)**	(5.99)**	(5.91)**	(6.86)**	(6.75)**	(6.09)**	(4.13)**	(10.91)**	(2.77)**
Feelsafe (peer-weighted)		0.417	0.183	0.173								
		(12.14)**	(9.07)**	(7.66)**								
Trouble (peer-weighted)						0.390	0.285	0.259				
						(12.73)**	(16.81)**	(9.86)**				
GPA (peer-weighted)										0.370	0.385	0.415
										(10.97)**	(16.19)**	(16.51)**
Size of Student's Class	0.001	0.001	0.004	0.007	-0.007	-0.004	0.029	0.025	-0.024	-0.025	-0.096	-0.092
	(0.14)	(0.18)	(0.67)	(1.07)	(2.46)*	(1.93)	(6.58)**	(2.98)**	(1.67)	(1.81)	(4.55)**	(4.36)**
Friend truncation controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Friend fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background/demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fractionalization controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School fixed-effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Observations	62276	62276	62276	61833	66430	66430	66430	65945	61553	61553	61553	61083
R-squared/Log Likelihood	-39292	-38819	0.01		-41614	-41330	0.03		0.14	0.17	0.13	

Absolute value of t statistics in parentheses. All errors clustered at the school level. Probit (marginal effects) used for model (1) and (2); linear probability model used for (3) and (4).

* significant at 5%; ** significant at 1%

Without controlling for this influence, the apparent negative effect of being connected on the likelihood of getting in trouble with other students will be attenuated, since the effect of peer behavior works in the *opposite direction*. To the extent that my measure of peer behavior, with direct-friend influence removed, can be considered plausibly exogenous, these results show both an independent effect of connectedness, as well as confirm evidence from the peer effects literature that peer behavior influences outcomes (in particular student performance as in Kremer and Levy (2008), but applied to a nationally representative sample of high school students).[48]

The effect of closure on health behavior is first explored in Table 18. Both for smoking and drinking behavior, closure appears to have a significant and robust effect on individual health behavior. Higher closure (higher connectedness) appears to have a negative effect on the likelihood of being a habitual smoker (three or more times per week), and a lower likelihood of abstaining from alcohol while in high school (or at least for the previous twelve months for high-school-aged students). If smoking is an anti-social behavior, and drinking a prosocial behavior, these findings would be consistent with Allcott et al. (2007)'s contention that closure has a positive effect on prosocial behavior.

These results, however, are contingent on the potentially strong assumption that the instrument (average closure of connected individuals immediately adjacent to the researcher-defined circle of trust size K) is truly exogenous: to the extent that an individual's own friend choice influences the choice of friends beyond this distance, endogeneity bias will remain. Following Babcock (2008) and Clark and Loheac (2007), I utilize the panel structure of the data and investigate the effect of lagged (Wave 1) closure and peer behavior on current (Wave 3) outcomes. Table 19 presents results for smoking and drinking behavior, while Table 20 presents results for educational attainment (likelihood of getting an undergraduate 4-year degree).[49]

[48] More so for school performance, the contemporaneous choice of friends with the decision to complete homework assignments and engage in academic activities, could matter to the significance of closure on school performance, if academic engagement is correlated with friend choice – a matter I investigate in Wave 3 specifications.

[49] Educational attainment results are also included in this work; although the primary focus is on health behavior, this work aims to also relate to the extant literature.

Table 18: Health Behavior Results using Peer-weighted Controls

	Smoking (high)				Drinking (low)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Base	Peer-weighted	Peer-weighted FE	Peer-weighted IV	Base	Peer-weighted	Peer-weighted FE	Peer-weighted IV
Closure (m=2, k=2.0)	-0.076	-0.037	-0.042	-0.094	-0.027	-0.029	-0.027	-0.127
	(7.75)**	(5.28)**	(4.84)**	(3.34)**	(2.31)*	(3.42)**	(4.18)**	(2.76)**
Smoking (peer-weighted)		0.752	0.907	0.935				
		(23.04)**	(18.81)**	(19.98)**				
Drinking (peer-weighted)						0.755	0.642	0.669
						(21.46)**	(47.08)**	(20.10)**
Size of Student's Class	-0.004	-0.004	0.011	0.010	0.008	0.006	0.004	0.001
	(1.21)	(2.75)**	(2.41)*	(2.11)*	(1.60)	(2.09)*	(0.91)	(0.29)
Friend truncation controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Friend fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background/demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fractionalization controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School fixed-effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	69737	69737	69737	69192	65736	65736	65736	65257
R-squared/Log Likelihood	-30789	-28349	0.10		-41998	-40517	0.08	

Absolute value of t statistics in parentheses. All errors clustered at the school level. Probit (marginal effects) used for model (1) and (2); linear probability model used for (3) and (4).

* significant at 5%; ** significant at 1%

Table 19: Wave 3 Smoking and Drinking Results

	Smoking - Unconditional			Smoking - Conditional on non-smoking Wave 1			Drinking - Unconditional			Drinking - Conditional on non-drinking Wave 1		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	Base	Peer-weighted	Peer-weighted FE	Base	Peer-weighted	Peer-weighted FE	Base	Peer-weighted	Peer-weighted FE	Base	Peer-weighted	Peer-weighted FE
Closure (m=2, k=2.0)	0.015	0.006	-0.010	-0.010	-0.012	-0.043	6.508	6.290	7.567	5.238	5.173	5.529
	(0.99)	(0.47)	(0.78)	(0.39)	(0.47)	(1.51)	(4.43)**	(4.28)**	(5.80)**	(3.06)**	(3.02)**	(3.44)**
Peer-weighted Health Behavior		0.047	0.047		0.018	0.014		7.804	10.220		4.433	3.035
		(5.63)**	(3.92)**		(2.79)**	(2.10)*		(2.41)*	(2.38)*		(1.18)	(0.80)
Observations	12768	12768	12768	3241	3241	3241	12727	12727	12727	6620	6620	6620
R-squared/Log-Likelihood	-7561.8	-7500.7	0.05	-2040.8	-2036.3	0.03	0.06	0.06	0.04	0.05	0.05	0.03

Absolute value of t statistics in parentheses. All errors are clustered at the school level. Probit (marginal effects) used for model (1) and (2); linear probability model used for (3).

* significant at 5%; ** significant at 1%

Table 20: Wave 3 Educational Attainment Results

	Bachelor Degree			
	(1)	(2)	(3)	(4)
	Base	Peer-weighted	Peer-weighted FE	IV and FE
Closure (m=2, k=2.0)	0.037	0.038	0.085	0.227
	(5.57)**	(5.58)**	(7.70)**	(3.74)**
Peer-weighted GPA		-0.004	0.029	0.032
		(0.81)	(1.80)	(1.66)
Observations	12778	12778	12778	12519
R-squared/Log-Likelihood	-3407.0	-3405.1	0.11	0.20

Absolute value of t statistics in parentheses. All errors are clustered at the school level. Probit (marginal effects) used for model (1) and (2); linear probability model used for (3) and (4).

* significant at 5%; ** significant at 1%

It is important to recognize that smoking behavior has a strong addictive component to it. If Wave 1 influence is transmitted into Wave 3 behavior through the chain of cigarette addiction over time, this identification strategy will fail. To a lesser extent, the same concerns are warranted for drinking (here measured by the intensity of drinking: the number of days in the prior year upon which five or more servings were consumed in one sitting). To address this complication, I present both unconditional results for the full sample, as well as conditional results. In particular, I condition on *not* smoking or drinking in Wave 1: this is based upon the notion that peer influence and connectedness induce behavior which would not have occurred otherwise. For those who did not engage in risky health behaviors in high school, does having been "connected" and being around smoking or drinking peers in high school influence the likelihood to smoke or excessively drink later in life?[50]

I find a strong and robustly significant effect of connectedness on drinking behavior and educational attainment, but not for smoking behavior. Although peer effects seem to matter for smoking behavior and educational attainment, this is not the case for the conditional analysis and drinking behavior. There are several implications. First, the decision to smoke – a plausibly anti-social behavior – does not seem to be influenced by the level of connectedness early in life. For he who abstained from smoking early in life, the level of interconnection between him and his peers seems to be irrelevant to his decision to smoke later in life. This could be because the effect of social isolation on the individual vulnerability to smoke is met early in life, or it could simply be that there is no effect at all.

Second, peer behavior seems to matter to drinking behavior when measured contemporaneously, but not so (at least for non-drinkers in high school) later in life. Drinking behavior is a rather established norm in American society for those older than high school age. It could be that peer behavior establishes this norm (where it would otherwise be absent) early in life, but is irrelevant later in life when a broader societal norm kicks in.

[50] I acknowledge that closure and peer behavior matters to the reverse – that they may affect the decision to not smoke or drink in Wave 1, and that this decision could be correlated with unobserved individual heterogeneity which could influence Wave 3 choices. This plausible impact should be taken as a caveat to any implications claimed; in particular, Wave 3 estimations for smoking and drinking were run for IV specifications with and without school fixed effects; when both IV and fixed effects are included, closure no longer matters to Wave 3 drinking behavior. This could be due to either correlated individual heterogeneity, or measurement error in the degree of connectedness. The former would work against my findings, while the latter would explain to some extent the drop in significance. As shown in Table 9, instrumentation and school fixed effects do not remove significance for educational attainment, which is further supportive of findings reported in Babcock (2008).

Regardless, it would appear that connectedness matters to the decision to drink heavily later in life, although the transmission of that effect remains unclear. An instrumental variables specification (not reported) showed that the significance and magnitude of the effect vanishes when endogenous effects are plausibly removed. I would therefore interpret the effect of the degree of connectedness on Wave 3 drinking behavior as potentially a result of the correlation between closure and the ability to make friends with otherwise well interconnected people, which could increase access to opportunities for excessive drinking.

The results for educational attainment are more concrete. It appears that the level of connectedness matters to the choice to enter and complete college, this is a result already documented in Babcock (2008), but I use closure as a different and more refined measure for connectedness. The Babcock study has better options for identification (using variation within schools across grades), but relies on a rather coarse measure for connectedness. What remains to be shown (in future work) is whether this effect is transmitted through peer effects in the quality of early schooling, or connectedness. This work makes an initial attempt to differentiate the two effects. However, since GPA performance in high school could be correlated with the likelihood to complete a college degree later in life, I still take these results as suggestive of an effect, but not establishing a causal relationship.

6. Conclusions

The idea of closure, first proposed in Mobius and Szeidl (2006), is an interesting idea. However, the complicated process by which networks are developed creates real hurdles for causal inference. This study has attempted to present more than suggestive evidence that connectedness matters to individual behavior, but additional work is warranted on the subject. There is some evidence presented here, where reasonable attempts at identification have been made using an individual and non-aggregated approach, that both connectedness and peer influence matter to an individual's propensity to engage in health risk behavior.

It appears that closure is related to excessive drinking, but the transmission of this effect remains unclear. It is clear that smoking behavior is less influenced by connectedness than is alcohol consumption. The decision to smoke seems to be more related to the development of socially accepted norms of behavior. Though young adults are bombarded with the very real health implications of tobacco use, many still continue to engage in this risky behavior. It seems plausible that if they have friends early in life who smoke, this could have very real impacts on their own decisions to engage in this behavior.

Finally, I provide some evidence that connectedness matters to educational attainment, in support of the prior literature. Though I use a measure of connectedness which presumably captures, at least to some extent, the effect of individual and joint choice, I have made reasonable attempts to remove these effects, and have included peer-level behavior as an additional explanatory variable. This has the added benefit of distinguishing effects and linking two important literatures.

APPENDIX A

NUMBER OF HOSPITAL ADMISSIONS BY STATE BY YEAR

Table 21: Number of Hospital Admissions by State by Year

	AZ	CA	CO	FL	IA	MA	MD	NJ	NY	PA	TN	WI
1989		59,566			12,710	16,396		19,237				
1990		70,736			11,037	22,202		19,005				14,060
1991		73,436			9,194	16,938		13,109				24,242
1992		82,129		100,682	11,999	19,208		16,843				33,768
1993		54,459	4,410	81,635	9,951	16,491	28,136	7,683	33,646			25,830
1994		71,305	3,055	77,000	9,174	12,450	28,216	11,172	26,559			27,989
1995	11,108	72,305	4,190	65,184	7,131	11,480	29,703	12,411	23,359	13,484		23,730
1996	12,857	70,510	3,889	64,505	7,021	7,572	29,993	14,126	23,401	12,303		23,039
1997	9,576	73,614	7,151	56,785	7,061	9,940	24,351	18,826	25,476	15,869	15,501	21,895
1998	8,912	71,462	6,270	55,302	5,749	12,025	22,027	16,747	27,226	12,846	11,477	20,919
1999	9,999	71,773	5,737	53,797	7,646	11,215	19,361	9,885	25,424	10,151	9,302	19,350
2000	18,813	60,643	8,145	34,059	7,410	14,155	13,185	10,936	35,553	17,762	4,590	20,725
2001	12,108	83,295	7,770	29,661	4,955	12,775	10,823	8,575	27,198	11,448	5,617	10,193

Source: HCUP database, after sampling.

APPENDIX B

ATTITUDINAL AND SUBJECTIVE BELIEFS BY AGE

Attitudinal and Subjective Beliefs by Age (moving average of age-wise medians,5th and 95th percentiles in raw data)

KEY: solid lines = U.S. sample; dashed lines = Canadian sample

Figure 7: Subjective risk of Alzheimer's Disease

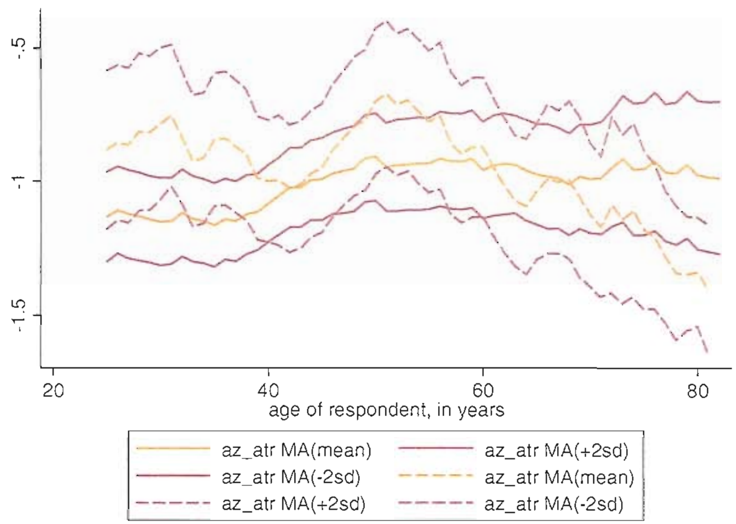


Figure 8: Subjective risk of Cancer (all cancers grouped)

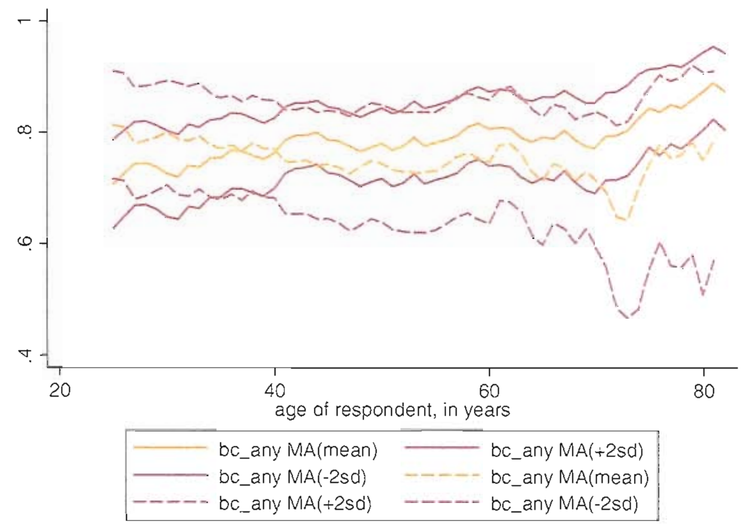


Figure 9: Subjective risk of Diabetes

Figure 10: Subjective risk of Heart Attack/Disease

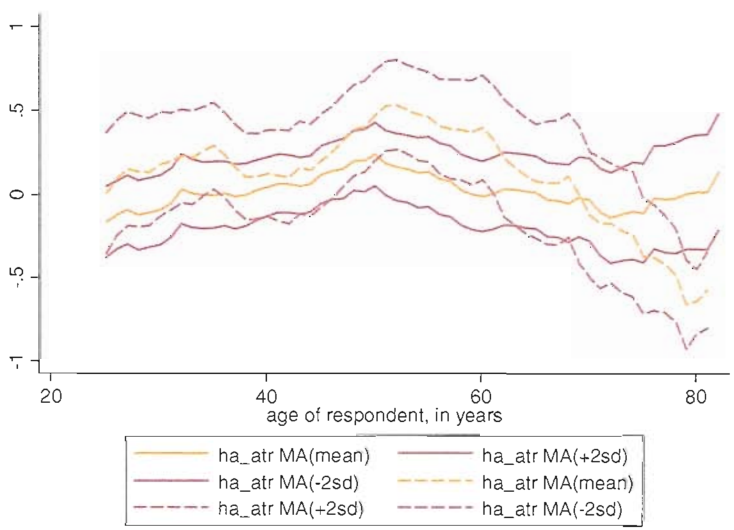


Figure 11: Subjective risk of Respiratory Disease

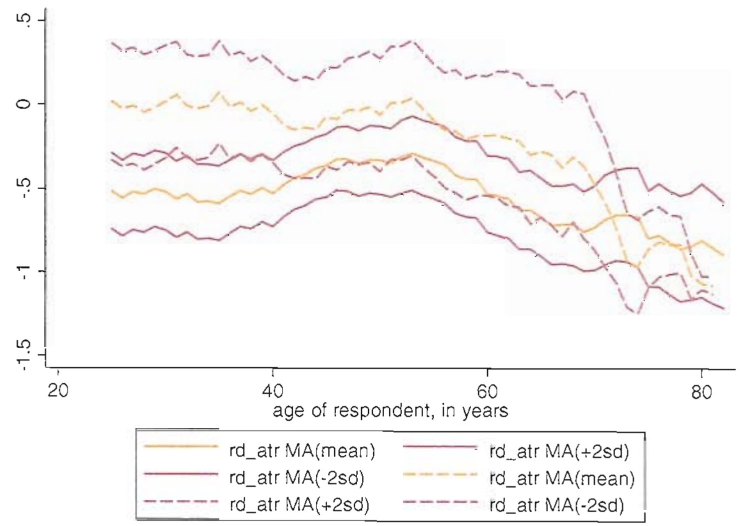


Figure 12: Subjective risk of Stroke

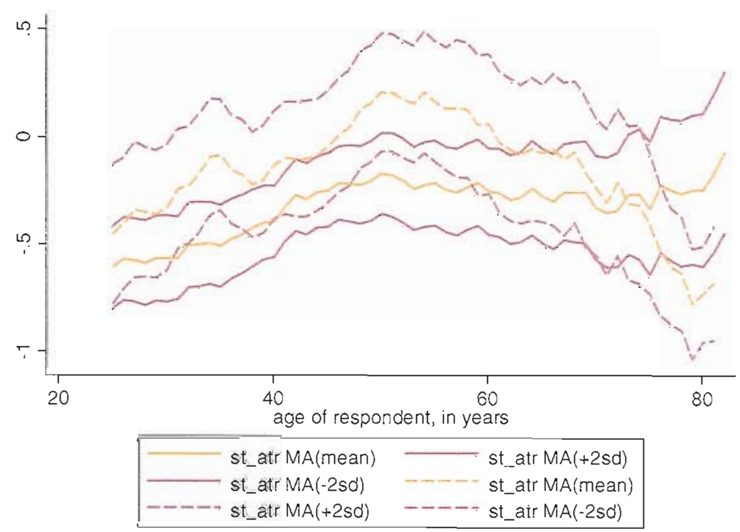


Figure 13: Subjective risk of Traffic Accident

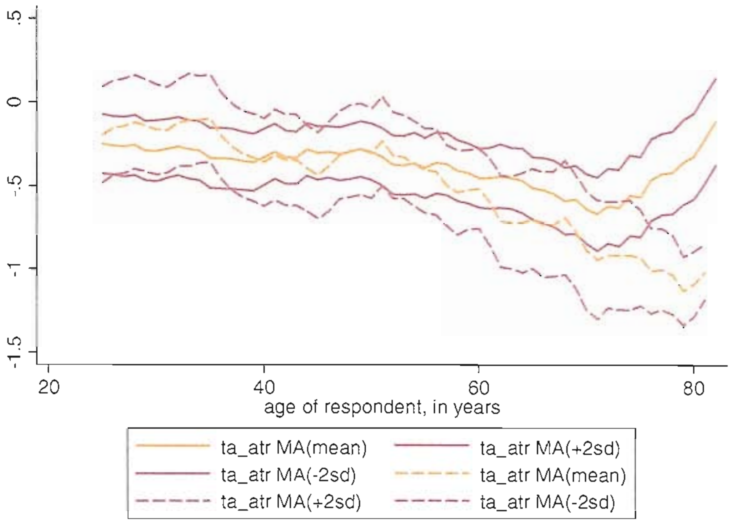


Figure 14: Room to Improve on Doctor Visits

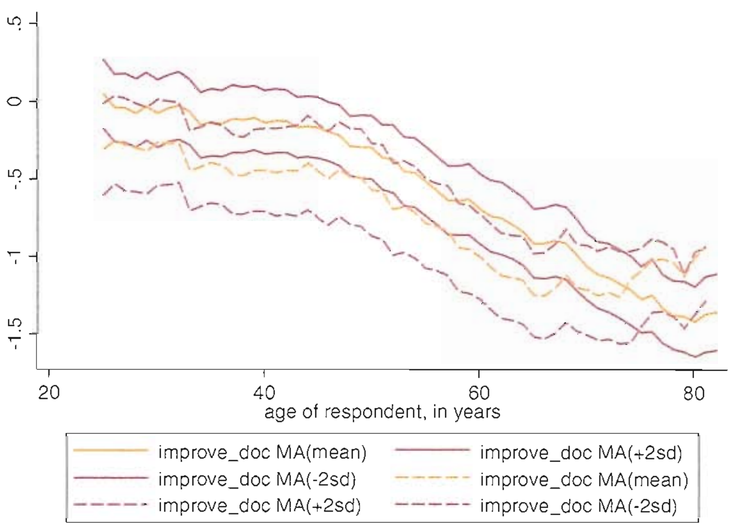


Figure 15: Room to Improve on Seat Belt Use

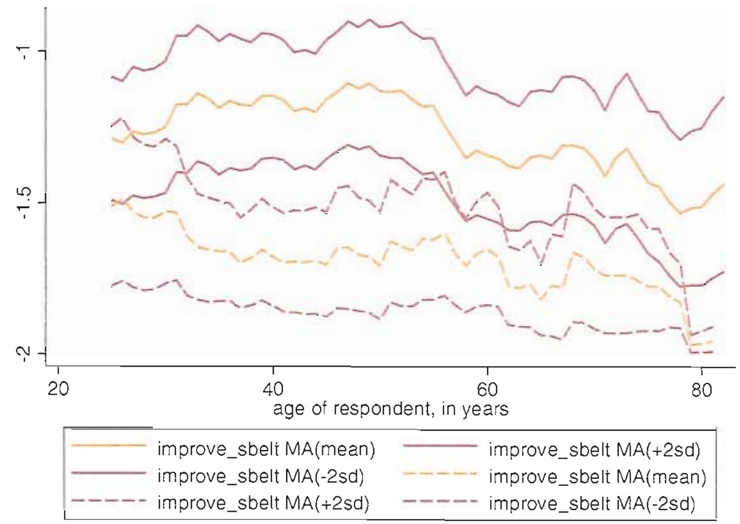


Figure 16: Room to Improve on Smoking (cut back)

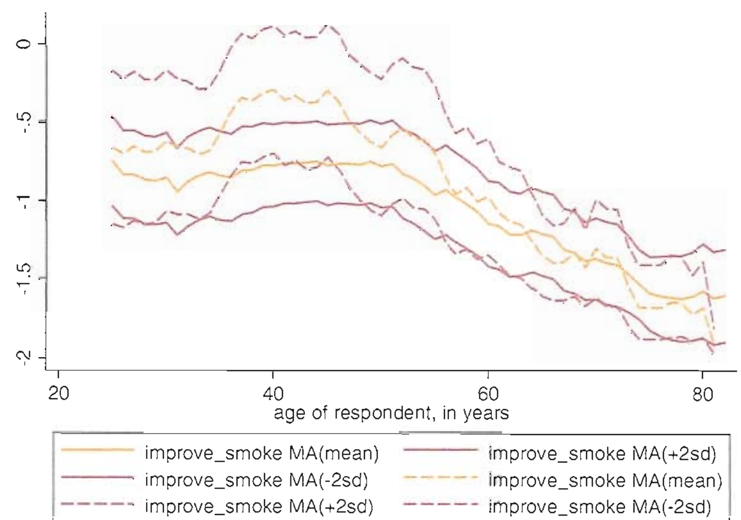


Figure 17: Room to Improve on weight (lose weight)

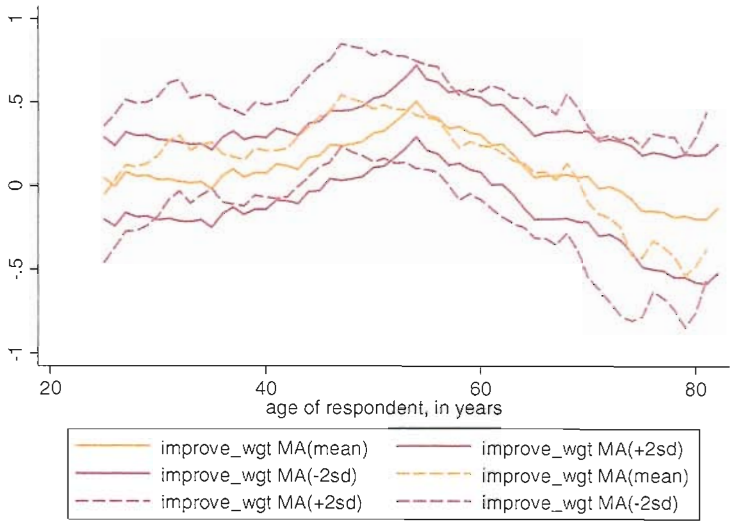


Figure 18: Room to Improve on Diet (eat healthier)

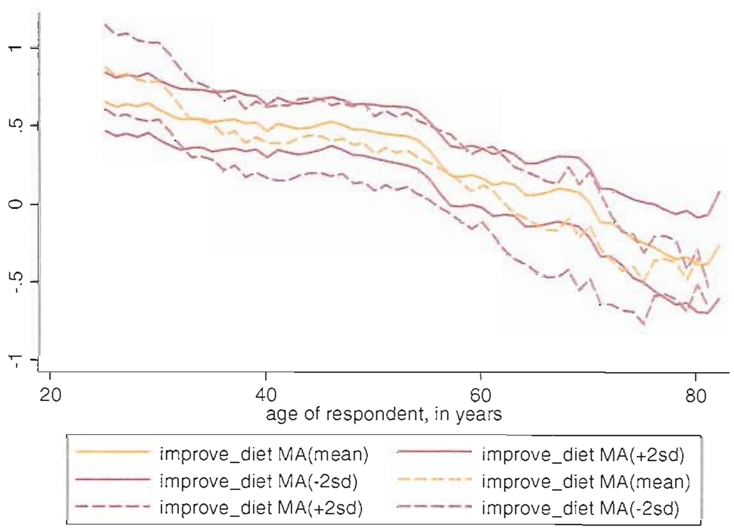


Figure 19: Room to Improve on Exercise (more)

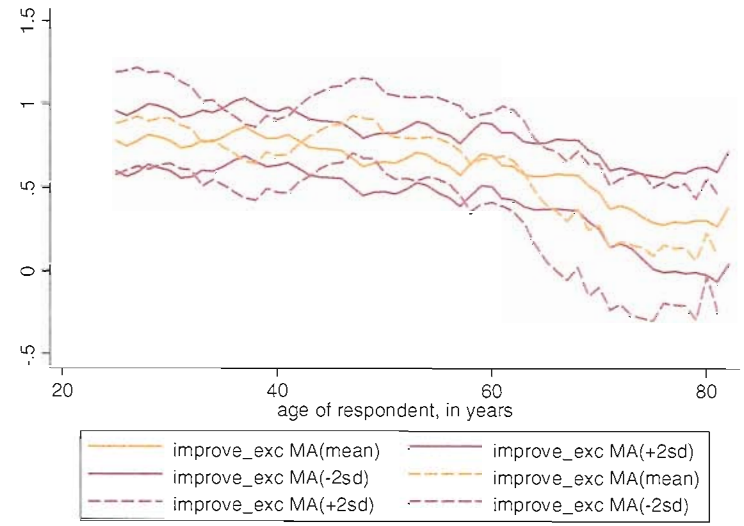


Figure 20: Room to Improve on Alcohol (drink less)

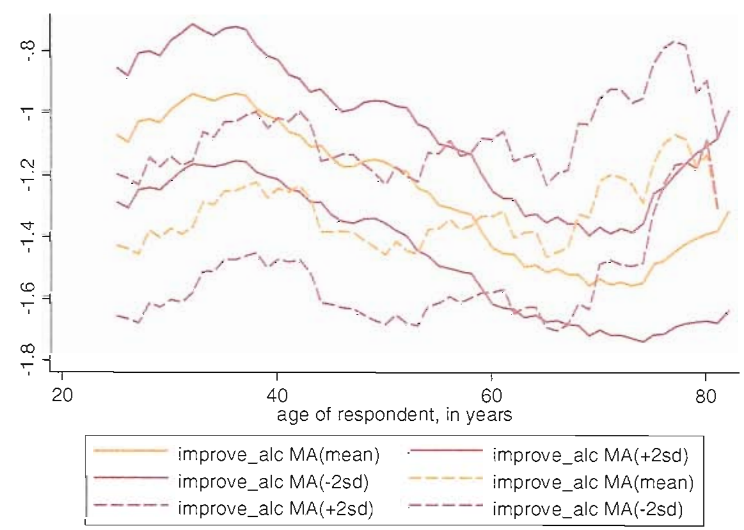
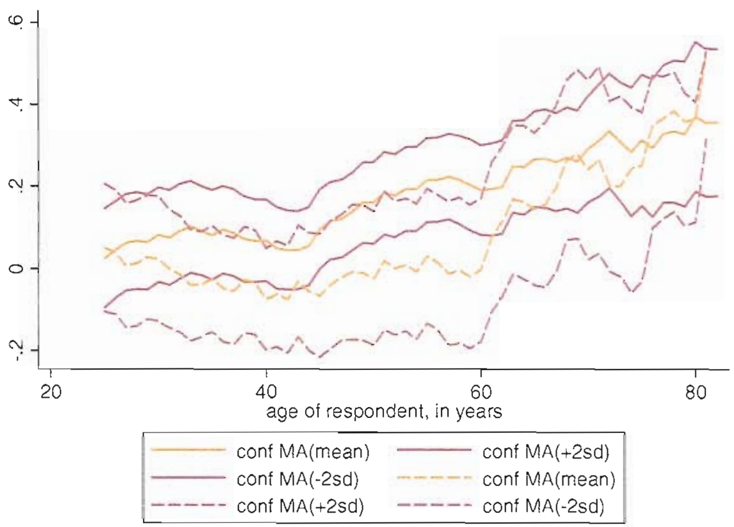


Figure 21: Confidence in Diagnosis and Treatment Efficacy



APPENDIX C

FITTED DISTRIBUTION OF WTP BY GENDER

Fitted distribution of WTP estimates by <u>gender</u> (median, 5[th] and 95[th] percentiles; 1000 random draws of parameters)

Figure 22: WTP Canadian Males

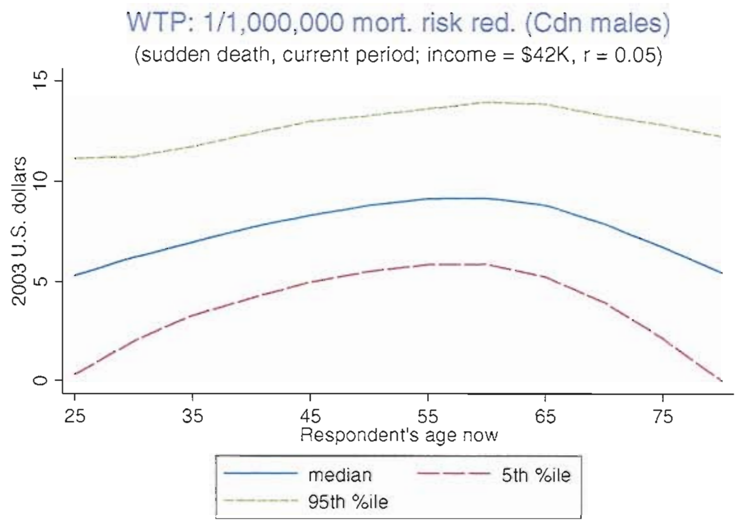


Figure 23: WTP U.S. Males

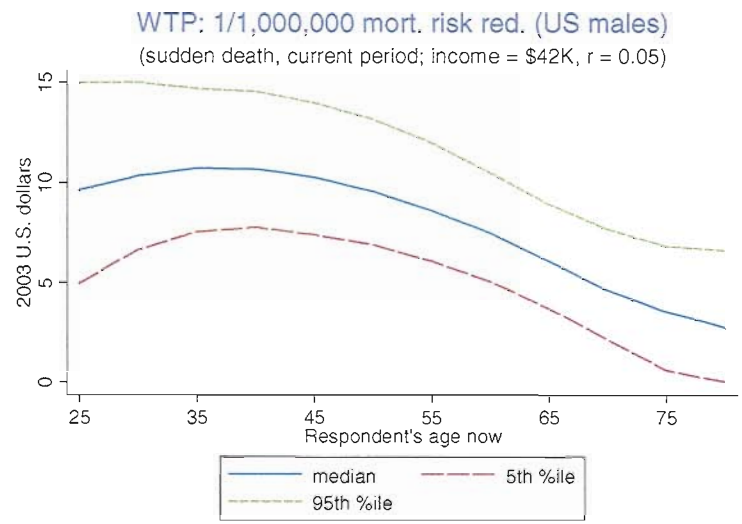


Figure 24: WTP Canadian Females

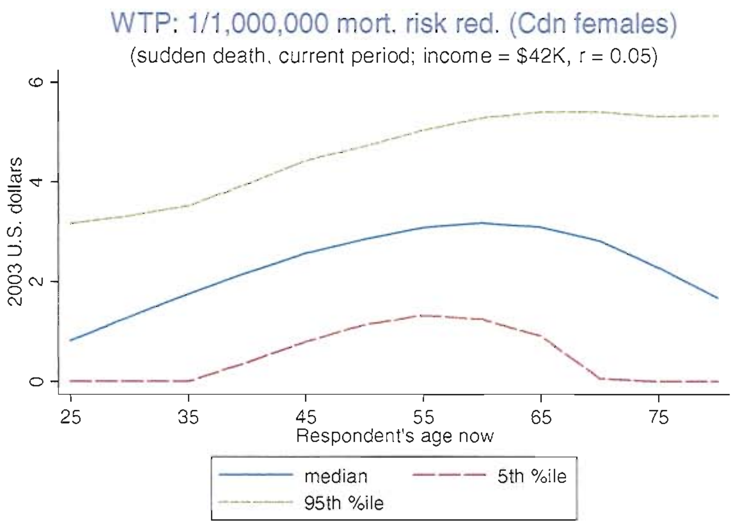


Figure 25: WTP U.S. Females

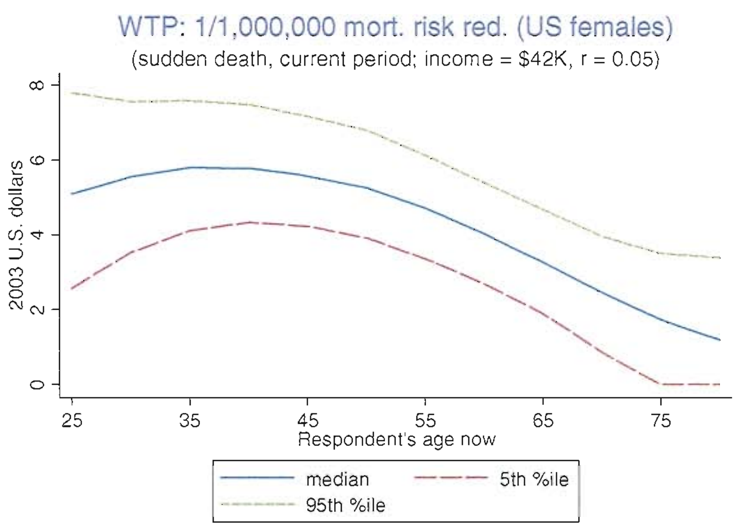


Figure 26: WTP U.S./Canadian Males

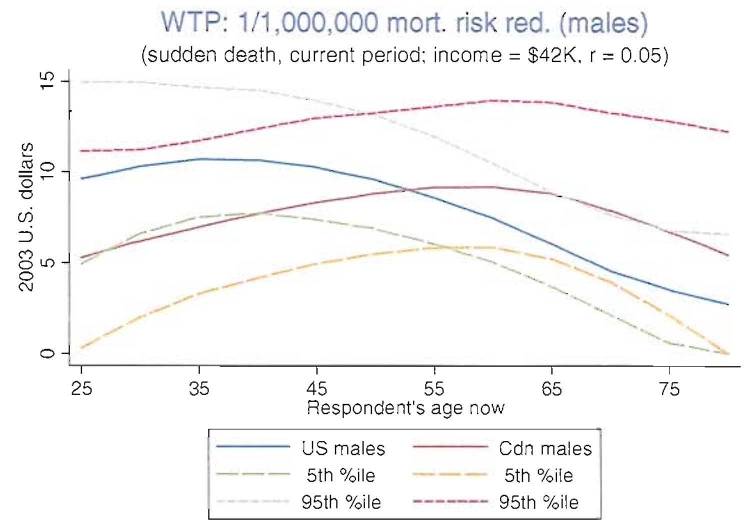


Figure 27: WTP U.S./Canadian Females

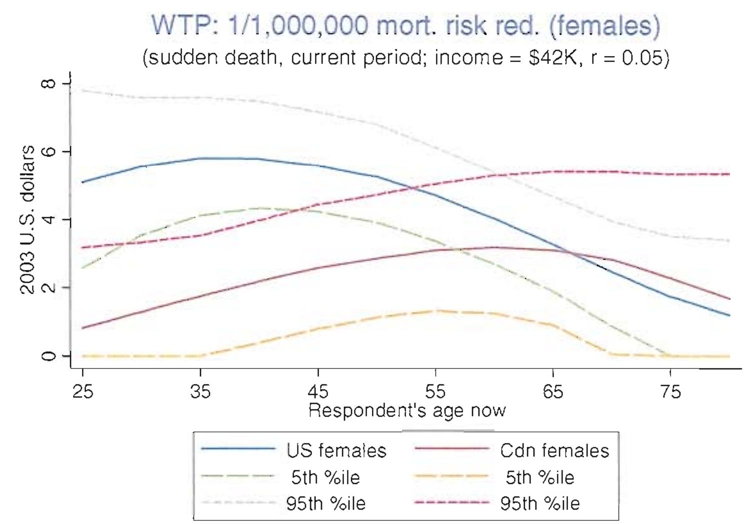
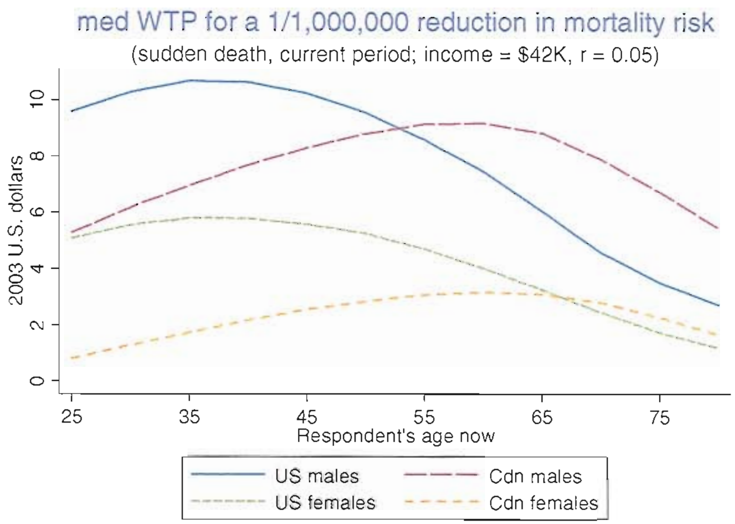


Figure 28: WTP U.S./Canadian Males (median case)



APPENDIX D

FITTED DISTRIBUTION OF WTP BY EDUCATION

Fitted distribution of WTP by education (median, 5th and 95th percentiles across 1000 random draws of parameters)

Figure 29: WTP Canadian Males with College

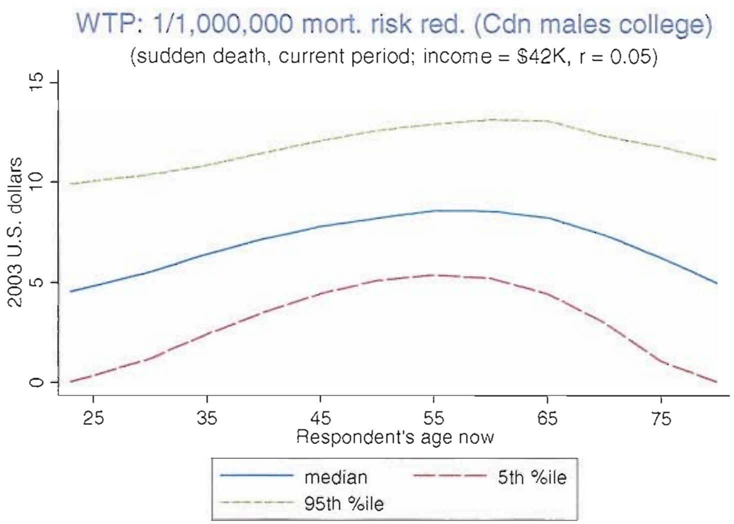


Figure 30: WTP U.S. Males with College

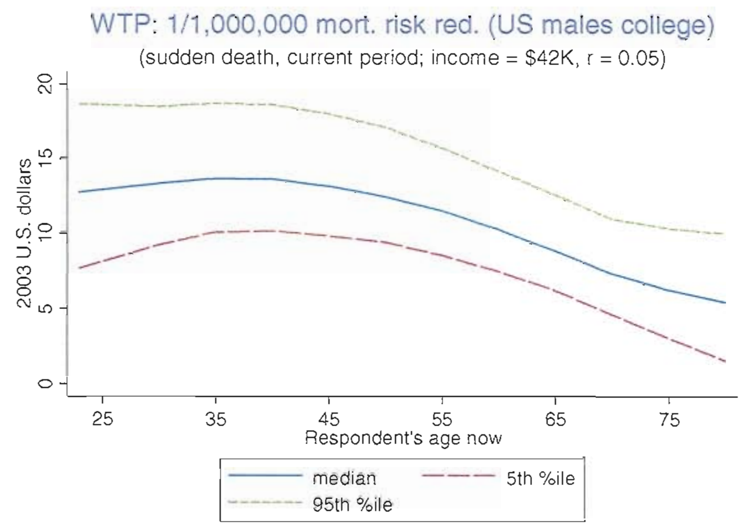


Figure 31: WTP U.S./Canadian Males with College

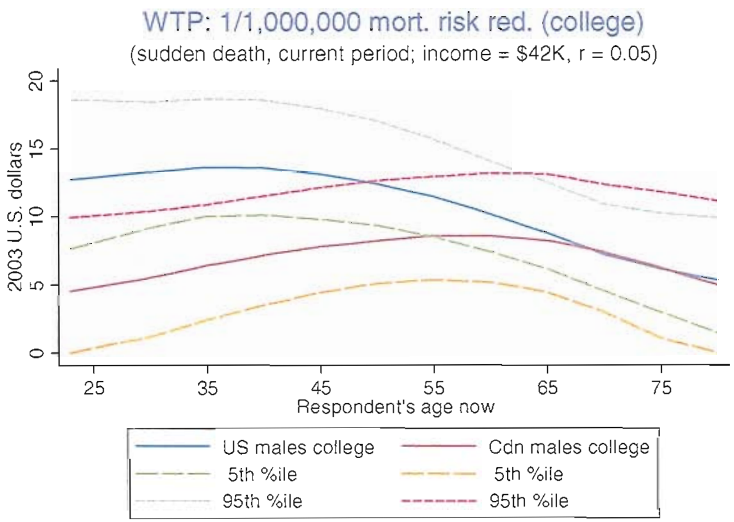


Figure 33: WTP U.S. Males without College

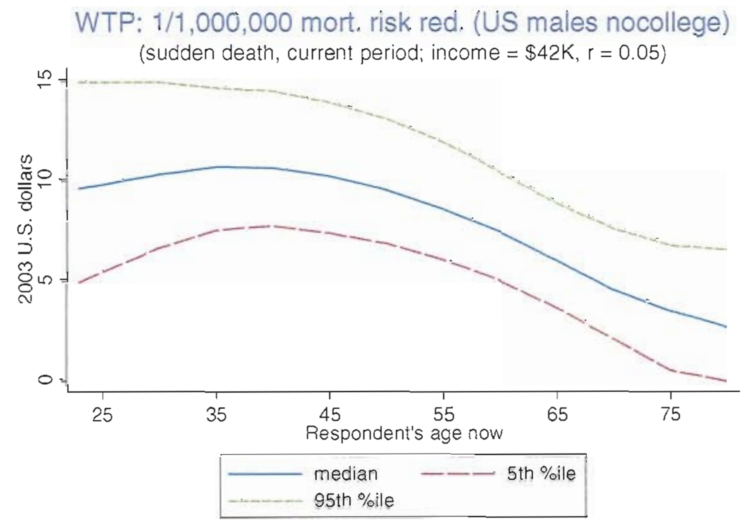


Figure 32: WTP Canadian Males without College

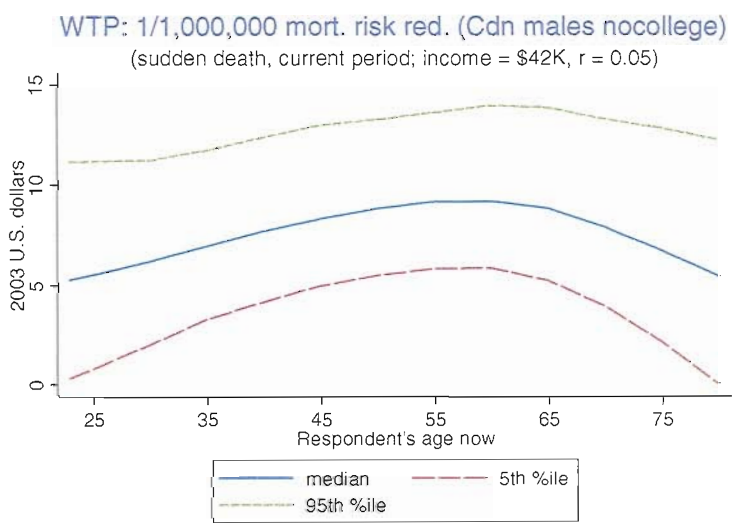


Figure 34: WTP U.S./Canadian Males without College

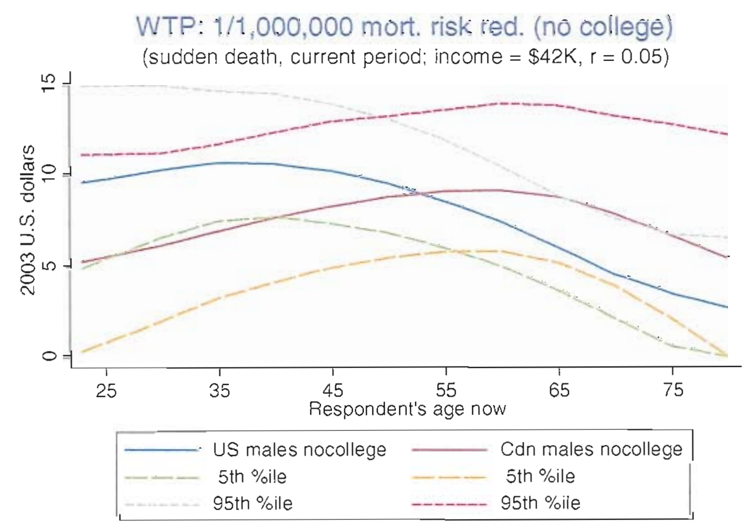


Figure 35: WTP Canadian Males College/No College

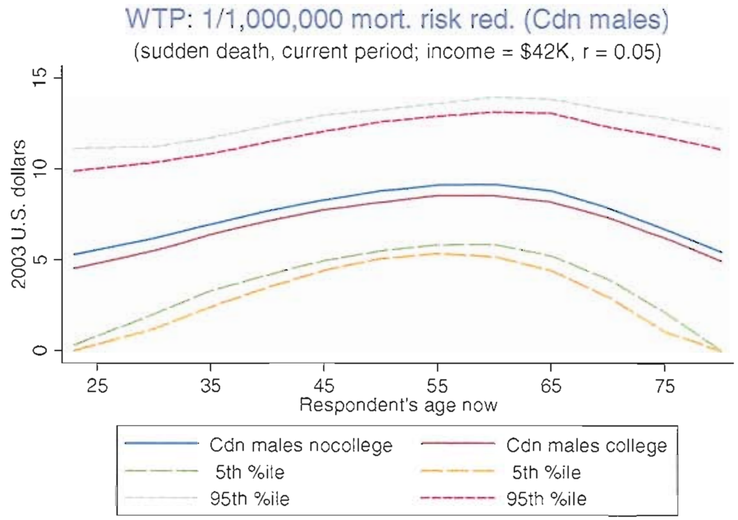


Figure 36: WTP U.S. Males College/No College

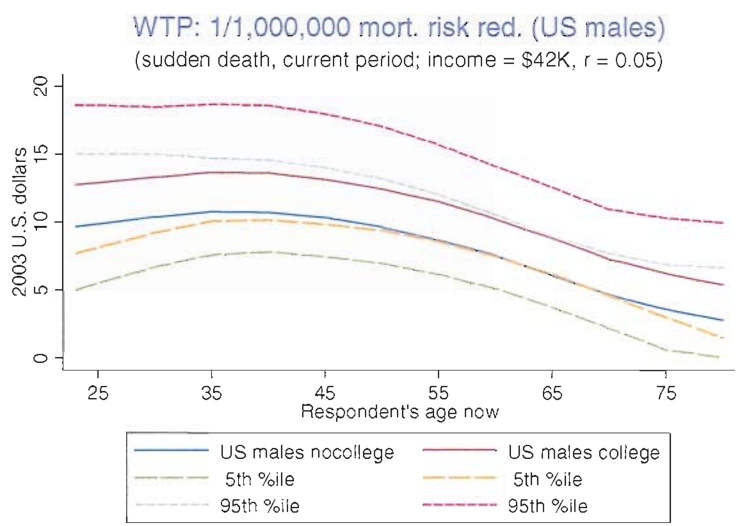
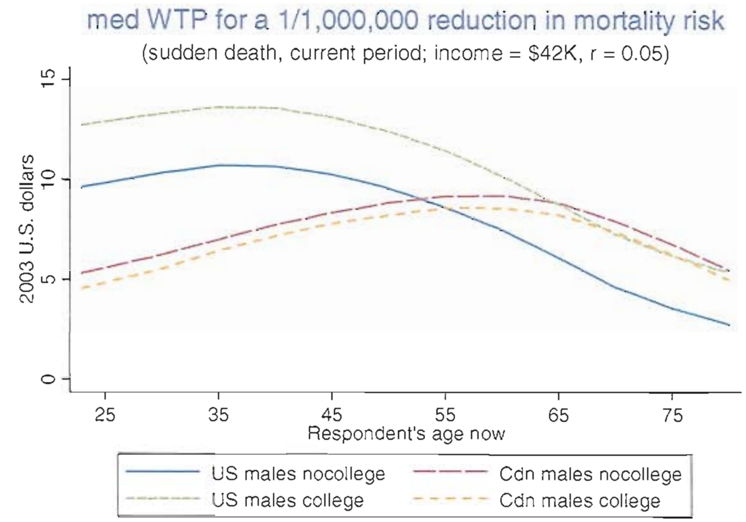


Figure 37: WTP U.S./Canadian Males without College (median case)



APPENDIX E

FITTED DISTRIBUTION OF WTP BY MARITAL STATUS

Fitted distribution of WTP by <u>marital status</u> (median, 5th and 95th percentiles across 1000 random draws of parameters)

Figure 38: WTP Canadian Married Males

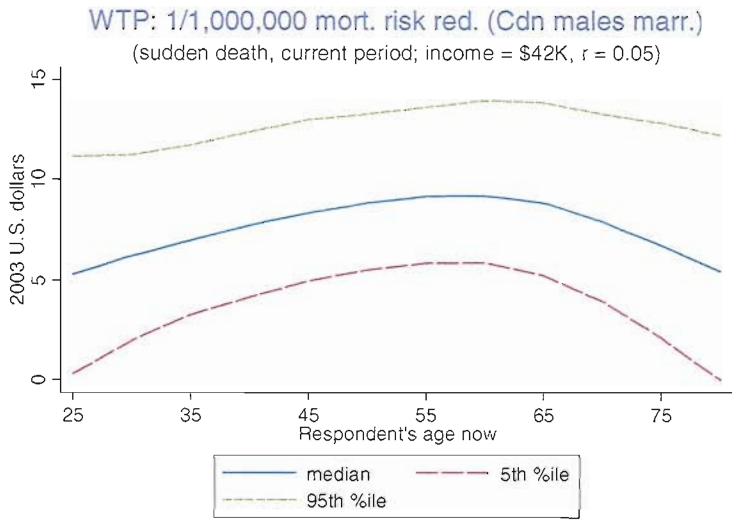


Figure 39: WTP U.S. Married Males

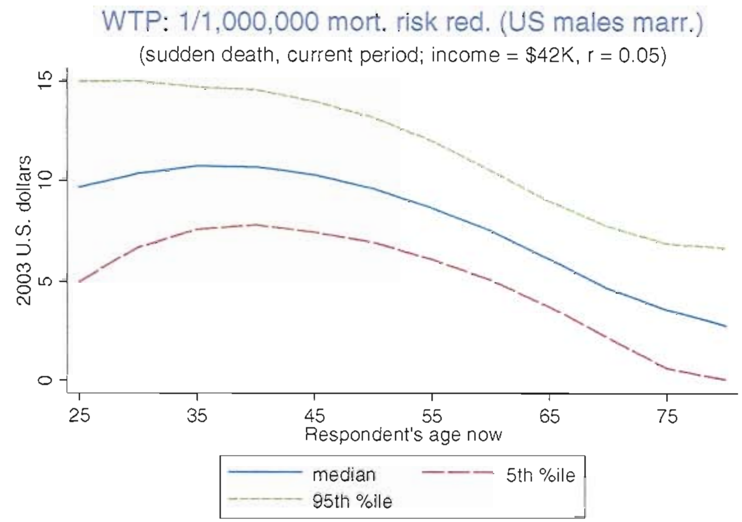


Figure 40: WTP U.S./Canadian Married Males

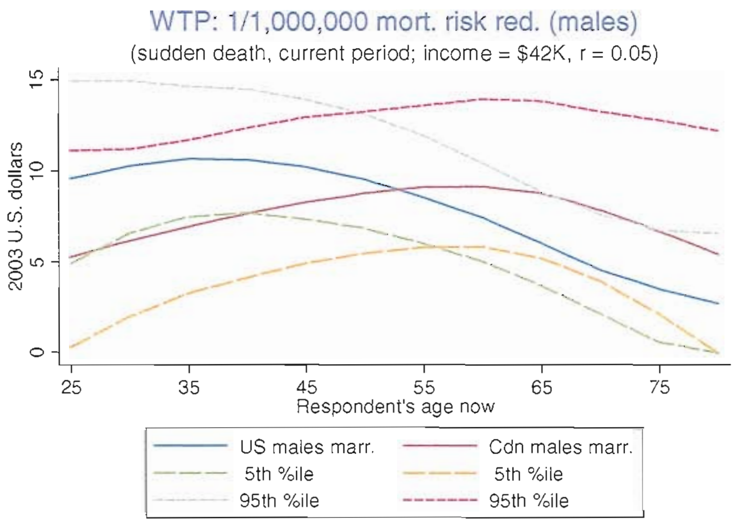


Figure 41: WTP Canadian non-Married Males

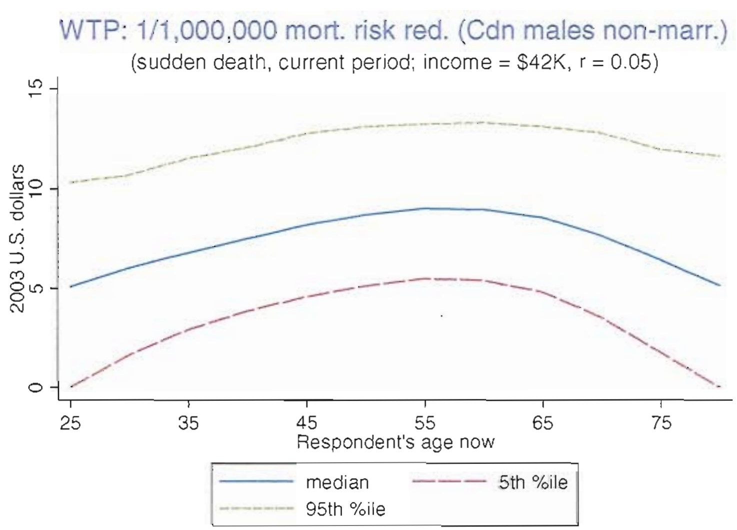


Figure 42: WTP U.S. non-Married Males

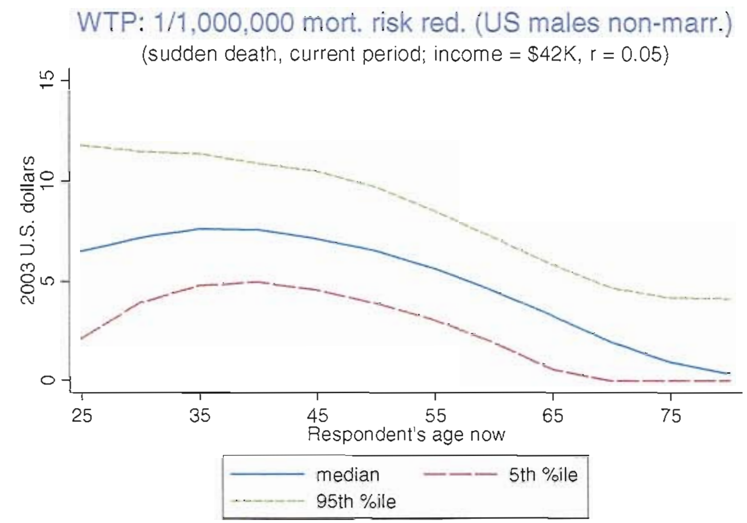


Figure 43: WTP U.S./Canadian non-Married Males

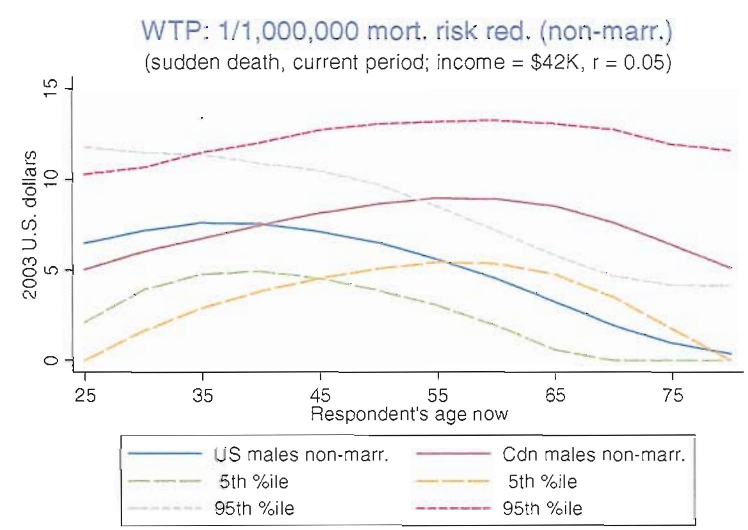


Figure 44: WTP Canadian Males – Married vs. non-Married

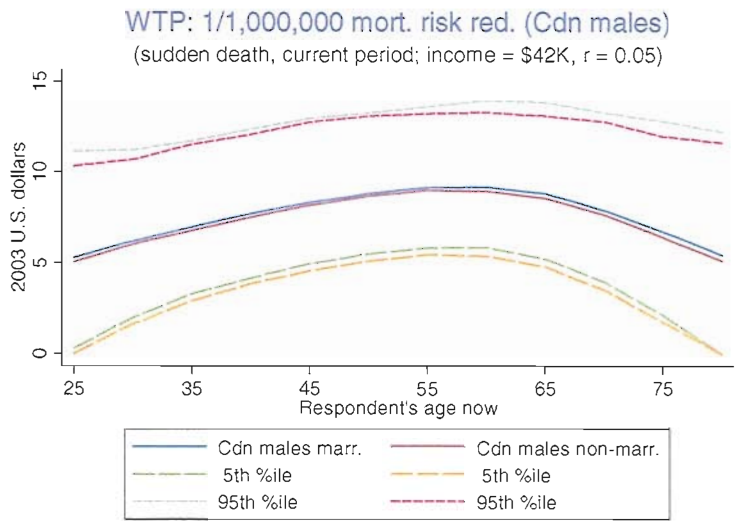


Figure 45: WTP U.S. Males – Married vs. non-Married

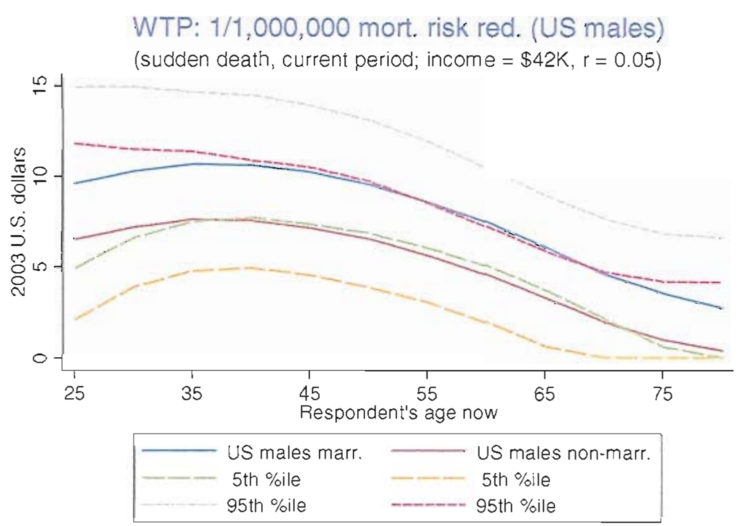
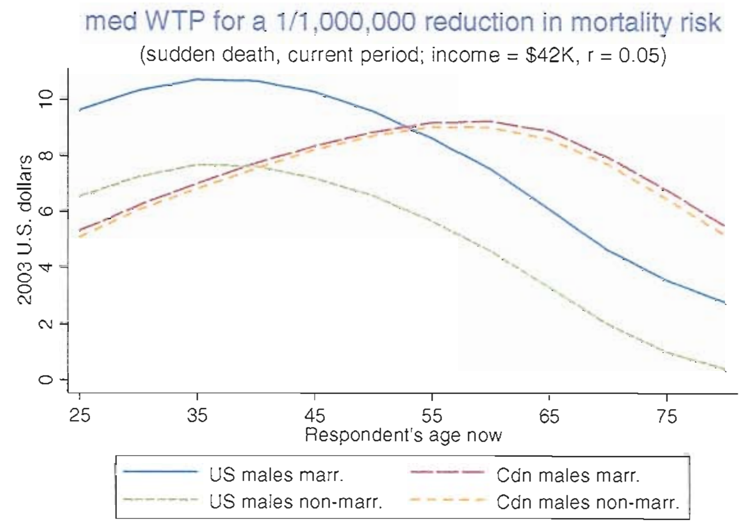


Figure 46: WTP U.S./Canadian Married/non-Married Males (median case)



APPENDIX F

FITTED DISTRIBUTION OF WTP BY OUT-OF-PLAN EXPERIENCE

Fitted distribution of WTP by <u>experience with out-of-plan medical tests</u> (median, 5th and 95th percentiles; 1000 random draws)

Figure 47: WTP Canadian Males with Out-of-plan Experience

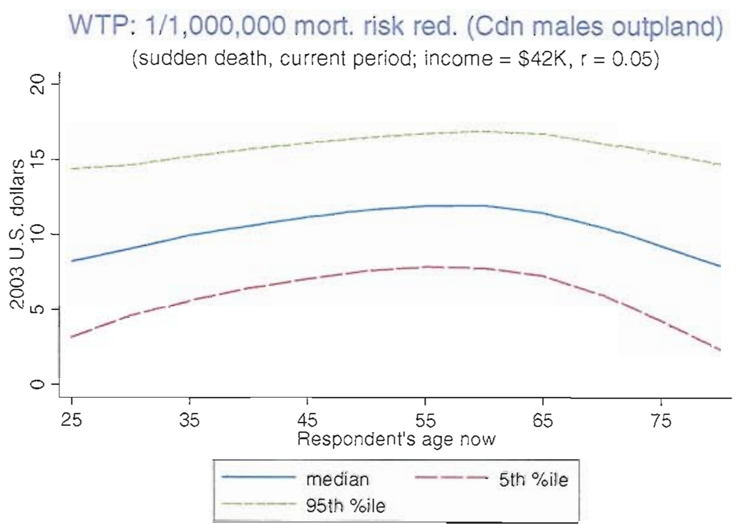


Figure 48: WTP Canadian Males without Out-of-plan Experience

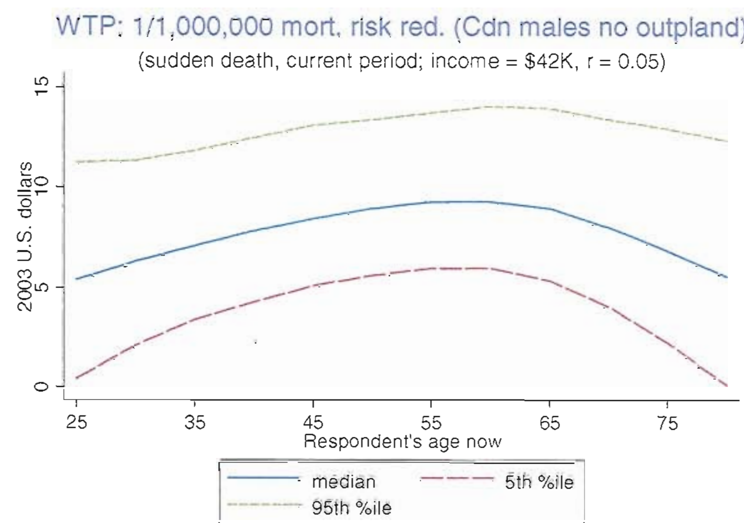


Figure 49: WTP Canadian Males with and without Out-of-plan Experience

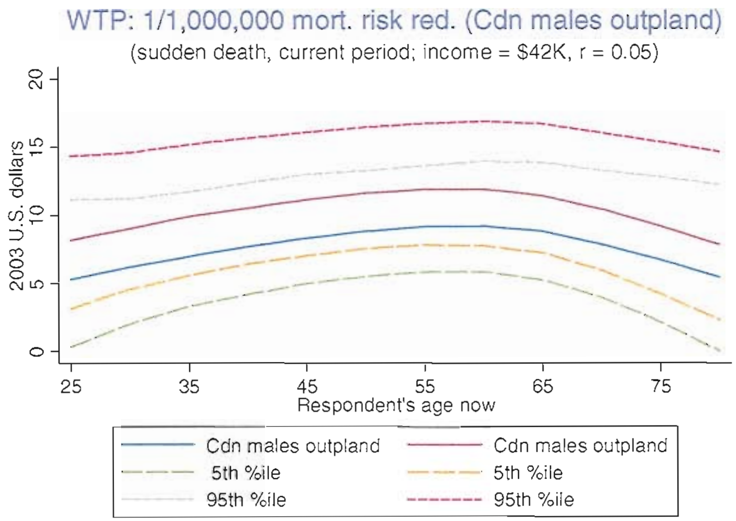
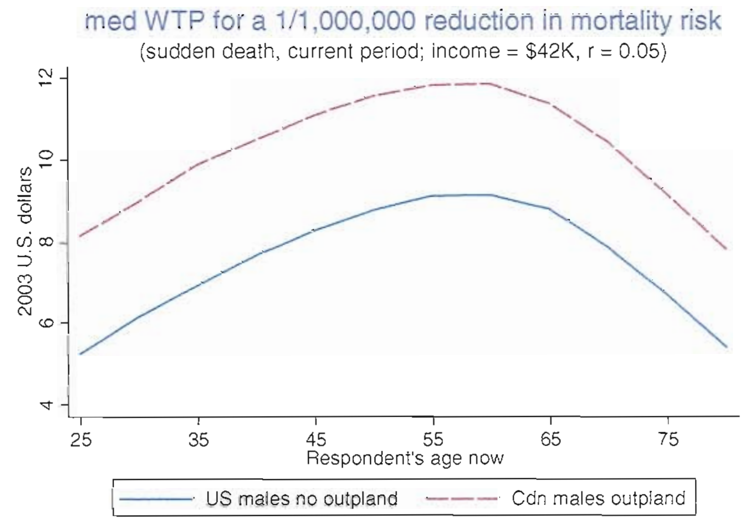


Figure 50: WTP Canadian Males with and without Out-of-plan Experience (median case)



APPENDIX G

WTP EMPIRICAL RESULTS (WITH T STATISTICS)

Table 22: Empirical Results (with t-test statistics)

	Model 1		Model 2		Model 3	
	U.S.	CDN Δ	U.S.	CDN Δ	U.S.	CDN Δ
Net income term (complex formula)	.01285	.01258	.01287	.01031	.01429	
	(10.48)***	(3.85)***	(9.46)***	(2.81)***	(6.16)***	
$\ldots\times 1$(female)	-	-	-	-	.01047	
					(4.23)***	
$\ldots\times 1$(mod low risk of this illness)	-	-	-	-		.01572
						(2.18)**
$\ldots\times 1$(high risk of this illness)	-	-	-	-	-.00761	
					(2.56)**	
$\ldots\times 1$(not confident in health care)	-	-	-	-		.0185
						(2.54)**
$\ldots\times 1$(confident in health care)	-	-	-	-	.004833	
					(1.99)**	
Illness Years: $\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-27.13	-2.493	-47.37	-23.68	-57.53	-57.8
	(4.71)***	(0.24)	(5.44)***	(1.51)	(3.83)***	(2.89)***
$\ldots\times 1$(female)	-	-	-	-	32.87	-
					(3.11)***	
$\ldots\times 1$(low risk of this illness)	-	-	-	-	35.98	-
					(2.50)**	
$\ldots\times 1$(mod low risk of this illness)	-	-	-	-	24.63	-
					(1.84)*	
$\ldots\times 1$(mod high risk of this illness)	-	-	-	-	-14.48	-
					(1.12)	
$\ldots\times 1$(high risk of this illness)	-	-	-	-	-33.71	-
					(2.08)**	
$\ldots\times 1$(mod. high opp. impr exercise)	-	-	-	-	-30.87	-
					(2.87)***	
$\ldots\times 1$(high opp. impr exercise)	-	-	-	-	-41.16	-
					(3.84)***	
$\ldots\times 1$(very low opp. impr smoking)	-	-	-	-	-	43.83
						(2.68)***
$\ldots\times 1$(mod low opp. impr smoking)	-	-	-	-	-	187.3
						(2.40)**
Recovered Years: $\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-22.81	-7.764	-17.54	-7.952	-	-
	(2.45)**	(0.45)	(1.87)*	(0.45)		
$\ldots\times 1$(female)	-	-	-	-	-67.88	44.76
					(4.82)***	(1.87)*

Table 22 (cont.)

	Model 1		Model 2		Model 3	
	U.S.	CDN Δ	U.S.	CDN Δ	U.S.	CDN Δ
Lost Life Years: $\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-29.23	20.01	-428.1	-27.75	-443.5	-
	(5.88)***	(2.20)**	(2.65)***	(0.08)	(2.87)***	
…$\times$age	-	-	12.04	-5.734	27.48	-24.77
			(1.86)*	(0.40)	(4.45)***	(9.10)***
…$\times$age²	-	-	-.08826	.1363	-.2769	.3654
			(1.44)	(0.96)	(4.71)***	(8.05)***
…$\times$1(female)	-	-	-	-	22.82	36.44
					(2.06)**	(1.90)*
…$\times$1(college degree or more)	-	-	-		-32.5	37.11
					(2.93)***	(2.02)**
…$\times$1(non-married)	-	-	-	-	35.94	-34.01
					(3.25)***	(1.78)*
…$\times$1(low risk of this illness)	-	-	-	-	66.8	-
					(4.97)***	
…$\times$1(mod low risk of this illness)	-	-	-	-	31.08	-
					(2.57)**	
…$\times$1(mod high risk of this illness)	-	-	-	-	-44.3	-
					(3.67)***	
…$\times$1(high risk of this illness)	-	-	-	-	-70.09	-
					(4.77)***	
…$\times$1(not confident in health care)	-	-	-	-	26.03	-
					(2.19)**	
…$\times$1(confident in health care)	-	-	-	-	-17.74	46.32
					(1.49)	(2.20)**
…$\times$1(have gone outside CDN plan)	-	-	-	-	-	-34.57
						(1.77)*
…$\times$1(very low opp. impr. doct. visits)	-	-	-	-	-17.22	-
					(1.81)*	
Squared: $\left[\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\right]^2$	-	-	145.1	60.41	149.1	-
			(1.80)*	(0.36)	(1.93)*	
…$\times$age	-	-	-4.919	.7678	-10.89	9.454
			(1.51)	(0.11)	(3.50)***	(7.54)***
…$\times$ age²	-	-	.04097	-.04427	.1123	-.1426
			(1.31)	(0.63)	(3.73)***	(6.46)***
Interaction: $\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\log\left(pdvl_i^A+1\right)$	-	-	31.14	28.06	-30.29	93.07
			(3.81)***	(1.87)*	(2.96)***	(5.73)***
Scenario Adjustment Controls:						
(Net income term)$\times$overest. of latency	-	-	-	-	.0008043	-
					(6.54)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times$1(benefit never)	-	-	-	-	206.8	-
					(4.66)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times$overest. of latency	-	-	-	-	8.399	-
					(8.95)***	

Table 22 (cont.)

	Model 1		Model 2		Model 3	
	U.S.	CDN Δ	U.S.	CDN Δ	U.S.	CDN Δ
$\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)\times 1$(benefit never)	-	-	-	-	639.3	-
					(4.17)***	
$\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)\times$ overest. of latency	-	-	-	-	11.86	-
					(14.31)***	
$\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)\times$ age $\times 1$(benefit never)	-	-	-	-	-7.035	-
					(2.77)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\log\left(pdvl_i^A+1\right)$ $\times$ overest. of latency	-	-	-	-	-4.933	-
					(4.43)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\log\left(pdvl_i^A+1\right)\times$ age $\times 1$(benefit never)	-	-	-	-	-14.72	-
					(4.18)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\log\left(pdvl_i^A+1\right)\times$ age^2 $\times 1$(benefit never)	-	-	-	-	.2216	-
					(3.97)***	
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$ $\times$ overest. of life expectancy	-	-	-	-	-1.918	-
					(3.75)***	
$\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$ $\times$ overest. of life expectancy	-	-	-	-	-.7151	-
					(1.52)	
U.S. Sample Selection Controls:						
$\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)\times\left[P(sel_i)-\overline{P}\right]$	-	-	-	-	3.936	-
					(2.43)**	
Observations	32,079		32,079		31,836	
Log-Likelihood	-16706.6		-16644.2		-15617.2	

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