Cash Welfare and Health Spending

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Abstract

This article explores the interplay between cash welfare and the health spending of potential recipients by linking welfare and medical spending data. I document that health spending doubles in the year prior to a welfare application and then partially returns to normal levels within three years. Then, using quasi-experimental variation in application adjudicators, I estimate that being approved for welfare has, at most, small positive effects on subsequent universally insured healthcare utilization. Together, these two findings imply that welfare is insuring health risk without significantly affecting health or medical treatment. Access to welfare does, however, markedly increase pharmaceutical spending, which is not universally insured but for which welfare recipients are subsidized. This implies that a lack of drug insurance considerably limits access to medication among low-income households that cannot access welfare.
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Health and social policy are closely linked, as poor health can impair a person's ability to work and maintain their income (e.g., Dobkin et al. (2018); Stepner (2019)). This is the rationale for programs such as disability insurance and workplace injury compensation. Even transfer programs that do not require ill health for eligibility, such as cash welfare, may insure against health-related income losses which are otherwise uninsured. Furthermore, economists, public health experts, and policy makers often speculate that these programs directly impact health by changing financial resources and precarity, stress levels, and time use (e.g., Herd and Moynihan (2020); The Lancet (2020)). I examine these interactions in Canada's cash welfare system: to what degree does welfare insure against health shocks, and does access to welfare causally affect subsequent health?

My context is the Income Assistance (IA) program in British Columbia, Canada, which offers subsistence-level cash transfers to very low-income households, regardless of their health or disability status, often conditional on searching for work. There are separate programs explicitly designed to cover health-related nonemployment.[1]

Previous attempts to understand the relationship between health and welfare have been hindered by inadequate measurement of health outcomes and a lack of high-quality linkages between health and welfare records. I overcome this challenge by accessing all administrative IA records and almost all medical spending data, such as inpatient and day surgery visits, physician visits, and pharmaceuticals. Canada is especially advantageous for this research since most health care is conducted through the public system, meaning that it is captured in the data.

Consistent with the notion that welfare insures health risk, hospital and outpatient spending among welfare applicants doubles in the months before application. These spending spikes appear in diagnostic categories that could represent substantial impediments to work, such as mental health and physical injury, but not among minor ailments such as influenza. The health shocks are relatively temporary, as spending partially reverts to baseline levels within three years. These patterns imply that welfare insures against negative work-limiting health events. This mirrors the

[1]These include disability insurance (DI), workplace injury compensation, and short-term sickness benefits embedded in the unemployment insurance system. In British Columbia, DI benefits are offered under the same heading as "Income Assistance", but the DI arm of the program is largely excluded in this paper.

finding that disability insurance, which is intended to cover health risk, also insures a substantial degree of nonhealth risk (Deshpande, Gross and Su, 2021; Deshpande and Lockwood, 2022). Both cash welfare and disability insurance, are in fact, offering some degree of overlapping insurance.[2]

Describing the system in these terms serves to emphasize the importance of studying the interactions between welfare and health together, rather than as two distinct issues. An integral part of this relationship is whether welfare directly affects health. Such effects would be important on their own and could affect recipients' long-term reliance on transfer programs. The challenge lies in finding exogenous variation in welfare access to estimate causal effects.

To obtain causal estimates, I use quasi-experimental variation in the approval propensity of application adjudicators. During the sample period (1997-2005), applications were submitted at local field offices and adjudicated by officers in the respective office. I show that the assignment of applicants to adjudicators is effectively random within an office and that there is considerable variance in the leniency of adjudicators. These conditions allow the use of adjudicator assignment as an instrumental variable for application approval. Unlike the many papers that leverage "judge" assignment as an instrument (e.g., Maestas, Mullen and Strand (2013), Bhuller et al. (2020), French and Song (2014), Kling (2006)), I also estimate causal effects using an experiment in which some offices were allocated additional adjudicators. In this experimental setup, both monotonicity and independence are satisfied almost by definition, making it a useful validation check for estimates derived from the adjudicator assignment strategy. The downside is that the experiment has 1/8th the sample size and, as a result, much lower precision.

I first estimate whether application approval implies just short-term differences in IA, or persistent long-term differences, relative to denied applicants. Approval causes 6.42 months of benefit receipt in the three years after application, relative to denied applicants, which translates to $4,551 CAD in benefits ($7000 in 2022 dollars). However, by the third year after the application, there is no difference between initially approved and denied applicants. This is consistent Green and Warburton (2004) who show that most approved applicants eventually exit IA, but small and equal-

[2] A related example is found in Arteaga and Barone (2022) who show that the opioid crisis in the US increased the use of the Supplemental Nutrition Assistance Program.

sized subsets of both approved and denied applicants use IA 3 to 5 years later. Because benefits are time-unlimited, exits among approved applicants represent voluntary, rather than forced, exits.

Next, I examine the effects of application approval on health spending. I find no significant effects on total hospital and outpatient spending in the three years following application, ruling out effects greater than 0.23 and smaller than -0.11 standard deviations. Every dollar paid in welfare causes non-pharmaceutical spending to rise by only 3.0 to 7.5 cents. The lack of an aggregate effect is not a result of offsetting effects in different areas of health spending. I also fail to find effects on mortality, or fertility among women.

There are, however, detectable increases in pharmaceutical spending and physician visits associated with pharmaceutical prescribing. Drug spending in the three years after application increases by $408, or 0.20 standard deviations. Unlike hospital and outpatient spending, which is universally insured regardless of welfare receipt, government subsidies for drugs are much higher for welfare recipients. If welfare affects drug spending solely through lower prices (via the subsidy), the implied price elasticity is between -0.92 and -1.6. This elasticity is higher than found in other contexts, but as Goldman, Joyce and Zheng (2007) emphasize, there is a paucity of estimates specifically for low-income non-seniors (to which my results contribute).

Finally, motivated by the fact that childless adults are ineligible for welfare (TANF) in the US, but are eligible in Canada and some European countries, I estimate effects separately for each demographic.[3] Both show strong increases in pharmaceutical spending, but mixed results on non-pharmaceutical spending. Parents (mostly mothers) are more likely to see increases in treatment for physical injuries, while childless adults (mostly men) are more likely to see increased mental health treatment. Potential explanations for these differences are discussed in section 4.5.

My first contribution comes from the linkage of medical records to welfare records. The limited number of studies that estimate causal effects of welfare on health are based on self-reports of healthiness (Kaestner and Tarlov, 2006; Bitler, Gelbach and Hoynes, 2005; Basu et al., 2016;

[3]Coverage of childless adults under the Earned Income Tax Credit and SNAP are active policy debates in the US (Carlson, Rosenbaum and Keith-Jennings, 2016; Crandall-Hollick, 2021; Meer and Witter, 2022).

Narain et al., 2017).[4] Self-reports may both contain substantial measurement error (Baker, Stabile and Deri, 2004) and cause biased estimates if receiving welfare changes one's perception of their health (Currie and Madrian, 1999). My administrative data are free of measurement error and contain detailed breakdowns of most health spending. Using health care spending data does mean that I study effects on health care utilization, which may not fully capture shifts in underlying health — for instance, if welfare access changes one's time available to seek health care.

My data are also useful because, in contrast to the prior literature, I observe welfare applications, which allow me to examine how health spending evolves before and after an application. Most similar to this is the concurrent work by Wu and Zhang (2022) linking survey data on transfer programs to medical records from the US Department of Veterans Affairs. They show that among military veterans, health worsens around the time of enrollment in safety net programs, consistent with my results for approved applicants. My data have the advantage of being able to examine denied applicants as well, and without the specific focus on veterans.

A second contribution comes from the causal identification itself. In a review article, Shahidi et al. (2019) surveyed 17 papers from developed countries documenting the association between health and welfare programs. Thirteen of those papers are descriptive and plagued by selection bias, such as comparing recipients to non-recipients or comparing changes in health outcomes as people transition on and off welfare. Of the remaining four that use quasi-experimental variation, two study whether short-term, small-scale welfare-to-work experiments in Florida and Connecticut affected long-term mortality rates (Muennig, Rosen and Wilde, 2013; Wilde et al., 2014).[5] The other two (discussed above) use the 1990s US welfare reforms (Basu et al., 2016; Narain et al.,

[4]The exceptions are Kaestner and Lee (2005) and Leonard and Mas (2008) who study infant mortality and birth weight. In the DI literature, Black et al. (2018) and Gelber and Strand (2018) estimate the effect of DI benefits on mortality — Black et al. (2018) find that access to DI worsens mortality, while Gelber and Strand (2018) find that, conditional on access, increases in the benefit amount lower mortality. And in a contemporary working paper, Silver and Zhang (2022) find that variations in the dollar amount of a DI benefit, specifically for military veterans in the US with a mental health disability, have limited effects on both health spending and self-reported health.

[5]Both compare regular AFDC use against welfare that was time-limited and conditioned on job search and training. Therefore, they do not estimate the effect of receiving benefits versus no support. The experiments have small sample sizes (about 2000 per group) and mortality rates of 4%-5%, which poses power issues. Neither of the two find statistically significant effects (Muennig, Rosen and Wilde (2013)'s original result did, but the errata that appears after the bibliography corrects this). Low power is a common issue in social policy experiments (Courtin et al., 2020).

2017). Finally, most similar to my paper is a companion paper (Hicks et al., 2022) in which we study the effect of a Canadian welfare reform on the prosperity of mothers and their children.

Those papers studying US reform illustrate a third contribution that comes from studying this question in Canada. US welfare reform directly reduced health insurance coverage as some low-income households were disenrolled from Medicaid without a corresponding enrollment in employer-sponsored insurance (Kaestner and Kaushal, 2003; Bitler, Gelbach and Hoynes, 2005; Cawley, Schroeder and Simon, 2006). As a result, any observed relation between welfare and health may reflect changes in insurance coverage. In fact, Finkelstein et al. (2012) show significant increases in both self-reported well-being and health spending due to the enrollment of uninsured households in Medicaid. In Canada, health insurance is nearly universal regardless of welfare status, providing an ideal setting to study the health effects of welfare without confounding insurance changes. Illustrating this point is that the clearest causal effects that I find are on pharmaceutical spending, the one domain in which welfare does affect health care prices.

Overall, I conclude that cash welfare provides a certain degree of insurance against health shocks without creating long-term reliance on the system and without inducing large spillovers into health care expenditure. However, the large effect on pharmaceuticals, driven by subsidies available to welfare recipients, suggests that a lack of insurance coverage is holding back medication access among low-income households that cannot access welfare. Low-income households also face barriers to accessing other non-insured medical expenses, such as dental care, while some of these are partially subsidized for welfare recipients. My results are, therefore, also relevant to recent efforts to extend coverage for dental and pharmaceutical care to low-income households (Green, Rhys and Tedds, 2021; Robson, Schirle and Lindsay, 2022; Health Canada, 2019).

The paper proceeds as follows. Section 1 outlines the institutional background. Section 2 describes the data and sample. Section 3 examines trends in health spending around the time of application. Section 4 contains the causal analysis of benefit approval on health and dependency. Section 5 concludes.

1 Institutional Details

1.1 Overview of the British Columbia (BC) IA System

BC's IA system is the primary income support program available to low-income and low-asset individuals. Throughout the paper, I use "welfare" and IA interchangeably. It is intended as a last resort for people who do not qualify for or have exhausted UI benefits. The program broadly classifies clients into those with work-limiting disabilities and those without them. Benefits are largest for persons with disabilities, but the application requirements are more arduous, including having to receive a medical designation of disability. Persons deemed employable constituted the large majority of IA recipients during my sample period and are the focus of this study. Benefits are time-unlimited, but "employable" recipients were expected to search for and accept work while receiving IA (except for mothers with young children).[6]

Compliance with obligations was overseen by case workers (Financial Assistance Workers (FAW) in official terminology). In the Ministry's words, these case workers were "*responsible for tracking recipients' progress after referral to the Ministry of Skills, Training and Labour, employment and training programs or the Family Maintenance Program [child support payments]. The FAW also helps recipients take advantage of the full range of available federal and provincial benefits and services for which they may be eligible*"(Ministry of Human Resources, 1997*b*). The responsibility of ensuring that IA recipients take advantage of available services could include health care treatment.

The system underwent substantial reform in 2002 (see Green et al. (2021); Kneebone and White (2009) for overviews) causing the caseload to decline from 9% of the population in 1999 to 5% in 2005. This reform tightened eligibility criteria, increased work search requirements, and added additional complexity to the application process, but left benefit levels relatively unchanged.

Average monthly benefits in 2002 were approximately $500 (CAD) for childless adults and

[6]Mothers were exempted from the work search requirement if their youngest child was less than a certain age: age 12 before 1996, age 7 from 1996 to 2002, and age 3 from 2002 onward.

$900 for families with children, or $6,000 and $11,000 in annual benefits, respectively. As a reference point, the 10th percentile of household income in 2002 was approximately $12,400.[7] This contrast highlights that welfare was a last resort rather than a desirable long-term option.

1.2 The Early Detection Program

Spurred by rising caseloads in the 1990s, the Early Detection Program (EDP) was established in 1996 as a means to detect "errors" in the determination of applicants' eligibility.[8] My causal identification strategy leverages a unique feature of the EDP, namely, Verification Officers.

At the time, eligibility was determined at field offices where applicants applied. The EDP instituted a new staffing position in these offices called Verification Officers (VOs). Under this system, an Intake Officer would conduct an initial assessment of an application, after which they could approve, deny, or refer the application to a VO for further examination. Upon referral, the VO would investigate applicants' eligibility, including specific concerns raised by the Intake Officer, then make a recommendation to the Intake Officer on whether to approve the application.[9] The Intake Officer then made the final decision. Intake Officers followed the VO's recommendation in 70% of cases. I use "VO" and "adjudicator" synonymously.

Formal criteria outlined which cases an Intake Officer should refer to a VO, which included (i) missing or potentially fraudulent documentation, (ii) missing or incomplete address information, (iii) prior investigations of an applicant for misreporting, (iv) potentially misleading or fraudulent statements about child dependents or family status, or (v) indicators suggesting that the applicant may not meet the income or asset thresholds for eligibility (Ministry of Human Resources, 1997*a*).

VOs did not specialize in particular types of applicants, nor could applicants control which VO reviewed their application. Furthermore, applications were handled first-come-first-serve by both Intake Officers and VOs (Ministry of Human Resources, 1997*a*). The VO who was working

[7] Source: https://www150.statcan.gc.ca/n1/en/daily-quotidien/151217/dq151217c-eng.pdf?st=xBocgdx

[8] The inaugural head of the EDP was a retired police officer which embodied the mindset of improving enforcement of eligibility criteria and detecting potential mistakes or fraudulent reporting by applicants. More generally, early EDP training manuals indicate that one measure of the program's success was the number of "diverted" applications (Ministry of Human Resources, 1997*a*).

[9] The VO's review was intended to be completed in 2 to 5 days following referral.

and available at the time of referral would receive the application. As a result, the assignment of applications to VOs was as good as random within a field office. Supporting evidence for random assignment is provided in section 4.

As the program was launched in 1996 and 1997, fewer than 15% of applicants were reviewed (see Figure B.1). By 2000, 38% were reviewed. When the 2002 reform established more stringent eligibility requirements, up to 43% were reviewed. The EDP ended in 2006 when the application system moved towards centralized telephone and internet platforms.

The EDP launched as a pilot program in late 1995 in which VOs were sent to a subset of offices. Then, these offices identified comparable offices that could be used as a control group. Green and Warburton (2004) used this experiment to estimate the causal effect of application approval on long-term IA receipt. I use it as a validation check on the main identification strategy.

1.3 Health Care

Most health care in Canada is universally insured, regardless of IA status. The key exceptions are pharmaceuticals, dental care, vision, and some medical devices. IA recipients receive larger subsidies than low-income non-recipients for some of these uninsured medical expenses. The most prominent of these is the 100% subsidization of pharmaceuticals.[10] The analysis below separates insured medical spending (hospital and outpatient) from pharmaceutical spending and directly estimates the causal effect of IA receipt on net-of-subsidy drug prices facing individuals.

[10]Details of dental care coverage are provided in Table 9 in the following document:https://web.archive.org/web/20220121012153/https://www.caphd.ca/sites/default/files/Environmental_Scan.pdf. At least as of 2002, adult IA recipients that were considered employable did not receive coverage. IA recipients considered to have persistent barriers to work do receive some dental coverage. Children from low-income households, in theory, received the same dental coverage under the *Healthy Kids* program regardless of whether their parents received IA. In practice, there is imperfect take-up of Healthy Kids coverage among eligible families. BC's pharmaceutical coverage during my sample period is discussed in Hanley et al. (2008). Before 2002, only IA recipients and seniors received pharmaceutical subsidies, except in catastrophic circumstances. After 2002, non-senior, non-IA recipients received public coverage, but with a copay of 30% and a deductible that scaled with household income, from $0 for families with income less than $15,000, 2% of gross family income if income between $15,000 and $30,000, and 4% of family income if income greater than $40,000. Hanley et al. (2008) report that the extension of coverage to this group had very little effect on the out-of-pocket expenses of non-IA households, presumably due to most pharmaceutical needs not exceeding the deductible. "Plan F" of the provincial Pharmacare provides 100% cost coverage for psychiatric medications for clients of provincial Mental Health Centers, for a limited time. Finally, drugs delivered in a hospital inpatient setting are universally-insured (as is all hospital spending).

2 Data and Sample Selection

IA Applications and Caseloads: I access all applications for approved and denied applicants between 1995 and 2005. Because families apply jointly for IA, all adults listed on the application are considered applicants. Details on the construction of the application data set are in Appendix A.1. I then link the full set of applications to the EDP's records for those that were reviewed by a VO. I define an applicant as approved if they received benefits in the month of or the month after the application. I observe which office processed the application and a unique identifier for the VO. For approved applicants, monthly-level payment amounts and program classification are observed (i.e., people with disabilities versus employable).

Health Data: The first data set includes all payments made to physicians under the provincial universal health insurance plan, the Medical Insurance Plan (MSP). The second data set is the Discharge Abstract Database, which contains all hospital inpatient visits and day surgeries performed in a hospital setting. For each physician billing and hospital record, I observe the associated diagnosis codes and the cost. See Appendix A.2 for details of hospital costing. The main exclusions from these combined data are non-medically necessary dental and vision services and allied health services (e.g. chiropractors).[11] The hospital data do not include emergency department visits unless they result in a hospital admission. However, emergency department visits will appear in the MSP data if a non-hospitalist performs the treatment. The third data set is Pharmanet, the provincial tracking system for prescriptions filled in community pharmacies.[12] It includes the cost of the prescription and the AHFS Pharmacologic-Therapeutic Classification code. These three data sets jointly cover the majority of health spending.[13]

Using diagnostic codes from hospital and physician spending, I construct measures of total spending for specific issues: physical injuries, mental health, and cold and respiratory illnesses.[14]

[11] I exclude hospital inpatient admissions due to pregnancies.

[12] Pharmanet excludes prescriptions filled at hospital pharmacies, designated mental health center, and the BC Cancer Agency.

[13] Hospital, physicians, and insured drugs make up approximately 70% of government health spending (excluding capital expenses). Source: https://www.cihi.ca/en/national-health-expenditure-trends

[14] Physical injuries are ICD9 diagonistic codes ranging from 800 to 900 (injuries and poisonings). Cold

I also measure visits to general practitioners (GPs) who are a primary gateway to health care in Canada. Using the AHFS code, I create an indicator for drugs that are typically prescribed for mental health treatment.[15]

Proxy for Employment: The public health insurance program (MSP) is funded by household premiums. In some cases, an employer will pay the premiums on behalf of its employees. I observe when this happens and use it as a proxy for employment. This proxy considerably under counts employment by excluding employed individuals who pay their own premiums.[16] Because premiums are typically paid at the household level, if one adult has premiums paid by an employer, then other adults in the household may appear to be employed, leading to an over-count for multi-adult families. I use this employment proxy with caution.

Inferring Parental Status: In section 4.5, I examine heterogeneity in causal effects separately for parents and childless adults. I infer that an applicant is a parent if any of the following are true: (a) they were linked to a child on the MSP health premiums registration; (b) they applied for IA while listing a child present in the home; (c) they appeared in the birth records as parents of a child born in the past 19 years. As a result, some adults will be classified as parents even if they do not live with the child.

2.1 Sample Descriptives

I focus on applications that were reviewed by a VO between 1997 and 2005. I restrict attention to adults aged 19 to 60.[17] I follow an applicant for one year before and three years after the application.

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and respiratory illness is ICD9 codes 461 to 466 and 480 to 488. Mental health treatment is ICD9 codes 290 to 319. A complete listing of ICD9 codes is available here: https://www2.gov.bc.ca/gov/content/health/practitioner-professional-resources/msp/physicians/diagnostic-code-descriptions-icd-9

[15] AHFS4 codes Anticonvulsants (28:12) (used for personality disorder treatment), Psycho-therapeutic Agents (28:16), Anti-manic Agents (28:28), Opiate Antagonists (28:10).

[16] Indigenous persons' MSP premiums were paid through Health Canada regardless of employment status. Indigenous people are approximately 5% to 10% of the BC population.

[17] Adults older than 60 are approaching retirement age, which would see them move off IA and onto retirement benefits. People under 19 years of age will usually be under the care of their parents, although not always. The sample of children that can be reliably linked to their parents' applications is too small to generate meaningful causal estimates, although I do show trends in children's health spending around application in section B.5.

Table I shows the demographic characteristics of applicants at the time of the application and their average health outcomes during the 12 months before the application. The characteristics of all applicants and the subset that were reviewed by a VO are shown to understand the selection into the VO review. Of the 507,731 adult applicants, 156,819 were reviewed.[18]

Two aspects of the composition of applicants stand out. First, most applicants are childless: 57.4% of all applicants and 51.6% of applicants reviewed by a VO. Second, 66%-78% of applicants had received IA at some point before the application. This reflects the widespread use of IA in the 1990s (Green et al., 2021). Finally, 3% of applicants die within four years of applying. In the analysis below, I drop person-year observations in which the applicant is deceased.

3 Trends Around the Time of Application

Examining trends in health and employment outcomes around the application for welfare serves multiple purposes. The nature of health degradation prior to application characterizes what shocks IA is effectively insuring. Post-application trends indicate how persistent the shocks are.

In Figures I to III, I plot the monthly average of each outcome from 12 months before the application to 36 months after, separately for approved and denied applicants. Panel (a) of Figure I starts with welfare receipt itself. Approximately 1/3rd of applicants received IA in the 12 months before their application, which reflects the large number of users who frequently transitioned on and off assistance during this time (Green et al., 2021). By definition, denied applicants do not receive IA in the month of application, but 23% of these applicants reapply and receive benefits over the following 12 months. Fifty percent of approved applicants still received benefits after 12 months. By 36 months after, only 31% still received benefits, implying that IA is a temporary support for most applicants. Among that 31%, about half had transitioned to disability insurance or benefits that exempt work search requirements for medical reasons (see Figure B.2).

Trends in the employment proxy are shown in panel (b). Approved applicants show a gradual 3.8 p.p. (38%) decrease in employment in the 12 months prior to application. Denied applicants

[18] Adults who applied more than once are counted as separate applications in these numbers.

Table I: **Application Descriptive Statistics by Approval Status**

	All Applications				Reviewed Applications			
	All	$D=1$	$D=0$	ρ	All	$D=1$	$D=0$	ρ
Approved	0.6	1	0		0.76	1	0	
Inferred Resident Before App	0.88	0.88	0.88	0.02	0.89	0.89	0.89	0.01
Parent	0.43	0.43	0.42	0	0.48	0.49	0.48	0.08
Female	0.45	0.45	0.45	0.03	0.44	0.44	0.43	0
Age	34.42	34.47	34.34	0	34.96	34.91	35.12	0
No IA History	0.33	0.25	0.46	0	0.23	0.2	0.31	0
Employer Paid Insurance Premiums	0.18	0.15	0.23	0	0.12	0.11	0.14	0
Any Medical Care	0.72	0.71	0.73	0	0.72	0.71	0.74	0
Treated in Hospital	0.11	0.11	0.11	0	0.12	0.12	0.12	0.02
Outpatient Spending	450.31	440.33	465.4	0	469.34	459.36	501	0
Hospital and Outpatient Spending	1005.44	949.66	1089.82	0	1048.3	992.6	1224.94	0
Visited GP	0.67	0.66	0.68	0	0.66	0.66	0.69	0
GP Visits Spending	115.97	115.22	117.1	0	124.98	123.86	128.52	0
Treated for Injury	0.23	0.24	0.23	0	0.26	0.26	0.27	0
Injury Spending	29.68	29.64	29.74	0.83	34.91	34.3	36.84	0.02
Treated for Colds	0.18	0.17	0.18	0	0.18	0.17	0.18	0
Colds Spending	9.1	9.01	9.24	0.05	9.15	9	9.64	0.07
Treated for Mental Health (MH)	0.27	0.27	0.26	0	0.3	0.3	0.3	0.19
Mental Health Spending	77.99	77.43	78.85	0.17	86.64	83.35	97.09	0
Received Pharmaceuticals	0.56	0.55	0.57	0	0.63	0.63	0.63	0.01
Pharmaceutical Spending	194.88	188.66	204.28	0	200.87	193.94	222.84	0
Share Drug Cost Uninsured	0.76	0.7	0.84	0	0.66	0.65	0.71	0
Day Supplied	162.09	160.49	164.51	0	166.12	163.51	174.41	0
Received MH Pharmaceuticals	0.2	0.2	0.2	0	0.25	0.25	0.24	0
MH Pharmaceutical Spending	66.49	65.55	67.92	0.01	66.92	65.12	72.62	0
4-Year Mortality Rate	0.02	0.02	0.02	0	0.03	0.03	0.03	0.55
N adult applicants	321167	208593	165233	1	118476	96102	32922	1
N	507731	305682	202049	1	156819	119224	37595	1

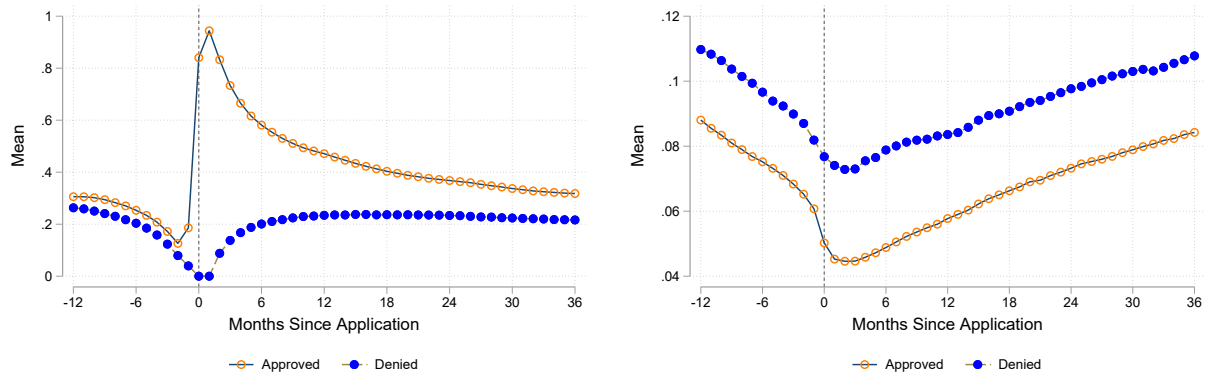
Note: This table contains mean pre-application characteristics for all applicants from 1997 to 2005 and the subset of those applications that were reviewed under the EDP. Averages are also shown separately for approved and denied applicants — ρ is the p-value testing the difference of means between the two. The health outcomes are measured over the 12 months preceding application. Spending is expressed in 2002 CAD and winsorized at the 99th percentile.

exhibit a similar downward trend, but approximately 35% higher baseline employment than approved applicants. Both groups return to their baseline employment rates within 3 years, suggesting that there is no long-term employment displacement. The most salient observation is the very low employment levels before application, suggesting that the employment proxy is a substantial under-count.

Trends in health outcomes are shown in Figure II. As shown in panel (a), the monthly probability of receiving outpatient or hospital medical care increases from 29% to 39% among approved

Figure I: **Welfare and Employment Around Application for IA**

(a) **IA Use** (b) **Employer Paid Premiums**



Note: Average outcomes are plotted for each month around the time of application ($t = 0$) among the set of EDP-reviewed applications, separately for approved and denied applicants. Panel (a) shows welfare receipt. Panel (b) shows the fraction of applicants who had their health insurance premiums paid by an employer.

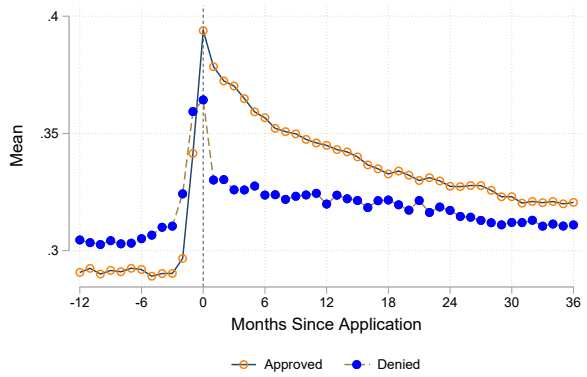
applicants and from 31% to 36% among denied applicants. The corresponding spending doubles from $45 to $85. Spending peaks in the month before application and then reverts downward to near-baseline levels within a few years. It seems that the culmination of health shocks prompts an application for assistance, while these shocks are somewhat temporary.

In panels (c)-(e), I plot the fraction of applicants who (i) visited a GP, (ii) received mental health treatment, and (iii) received treatment for a physical injury. All three exhibit a spike in the months before application, with elevated levels among approved applicants afterwards. Since GPs are a primary gateway to specialist care, the spike in GP visits is intuitive. The spikes in mental health and physical injury represent potentially work-limiting health shocks. On the contrary, there is no spike in treatment for respiratory illnesses such as influenza (panel (f)), consistent with these being minor health events that do not significantly inhibit work. However, among approved applicants, treatment for respiratory illness increases by 25% after the application, which is consistent with IA recipients having more time available to seek health care (due to not working full-time).

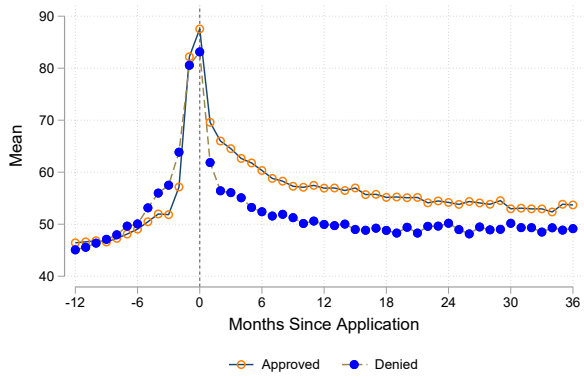
Trends in pharmaceutical outcomes are shown in Figure III. Pharmaceutical spending does not spike in the months before application, but does increase sharply after application among approved applicants. Panel (b) shows that, for approved applicants, the fraction of drug costs paid by the government increases from 23% in the month before application to 78% in the month after, due

Figure II: **Universally-Insured Health Spending Around Time of Application**

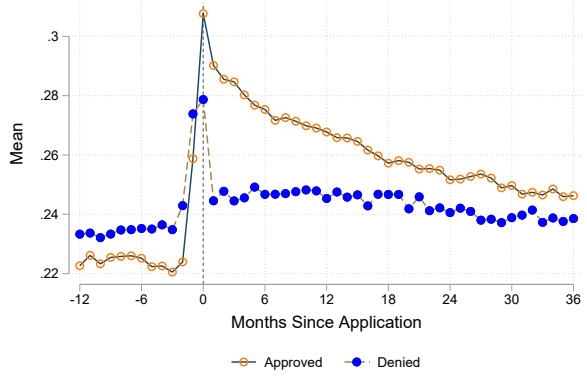
(a) **Probability of Receiving Medical Care**



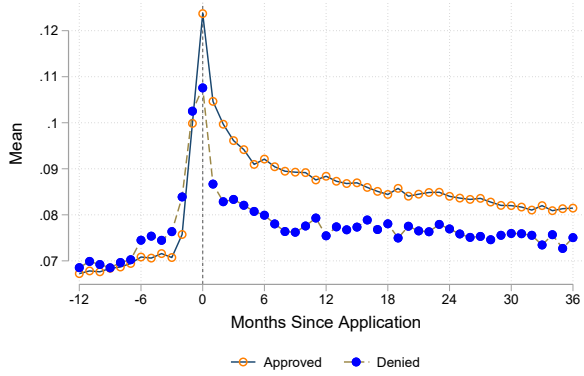
(b) **Hospital and Outpatient Spending**



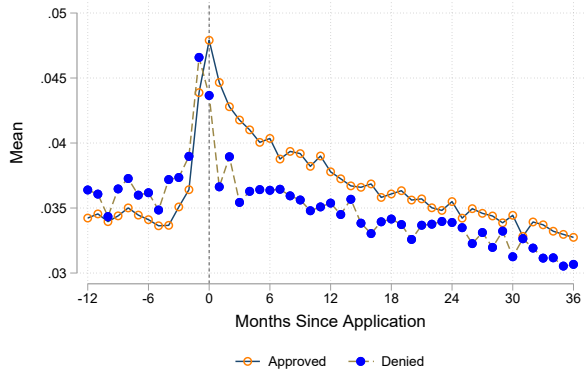
(c) **General Practitioner (GP) Visit**



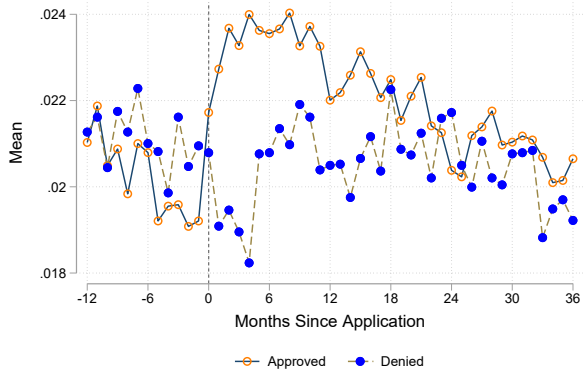
(d) **Mental Health Treatment**



(e) **Physical Injury**



(f) **Respiratory and Flu**



Note: Average outcomes in each month around the time of application ($t = 0$) are plotted for the set of EDP-reviewed applications, separately for approved and denied applicants. The outcomes, in order, are: (a) an indicator for receiving any hospital or outpatient treatment, (b) total hospital and outpatient spending, (c) an indicator for visiting a general practitioner; (d) an indicator for receiving mental health treatment; (e) an indicator for receiving treatment for physical injury; (f) an indicator for receiving treatment related to respiratory illness.

to the drug subsidy available to IA recipients.[19] As a result, the sharp spending increase among approved applicants is consistent with subsidy-driven spending. In fact, the spending gap between approved and denied applicants decreases over time, mimicking the pattern of IA receipt in Figure I and the average copay gap in Figure III.

In panels (c) and (d) of Figure III, it is apparent that the rise in GP visits among approved applicants is driven by persons who filled a prescription in the same month, suggesting that the drug subsidy increases outpatient spending associated with writing prescriptions.

Assuming the drug spending increases among approved applicants is caused entirely by the government subsidy, a one-month price elasticity is found by comparing the change in spending against the change in the share of costs paid by the government. Monthly pharmaceutical spending increases from $15.68 in the month before application to $25.57 in the month after, implying a price elasticity of -0.44.[20] This is in the range of elasticity estimates surveyed by Goldman, Joyce and Zheng (2007).

In section B.5, I show that health spending does not spike among children that can be linked to adult applicants. However, like their parents, pharmaceutical consumption increases among approved children after the application, consistent with the subsidy. In section B.4, I show that the trends are very similar for the full sample of applications.

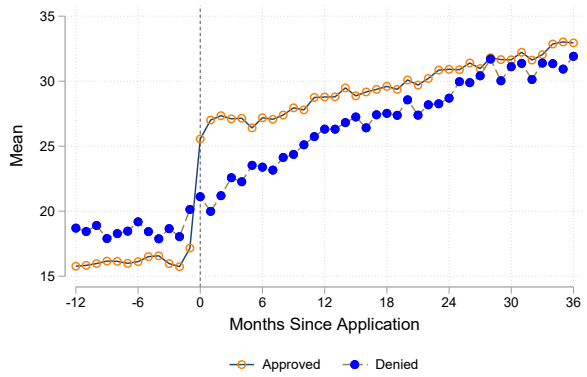
In summary, spikes in hospital and outpatient spending, among plausibly work-limiting diagnoses, precipitate an application for welfare, which illustrates that the cash welfare system insures health risk despite not being explicitly designed to do so. Furthermore, approved applicants have higher health spending long after the application. In the following section, I estimate whether these health spending differences are, at least partially, caused by being granted access to welfare itself.

[19] IA recipients receive a 100% subsidy for eligible drugs. The less-than-100% rate in Figure III comes from two sources: (1) someone that fills a prescription early in the month, then becomes an IA recipient later in the same month, would not have had coverage for that drug spending. (2) A small subset of IA recipients are in the category of "hardship assistance" which does not automatically imply drug coverage.

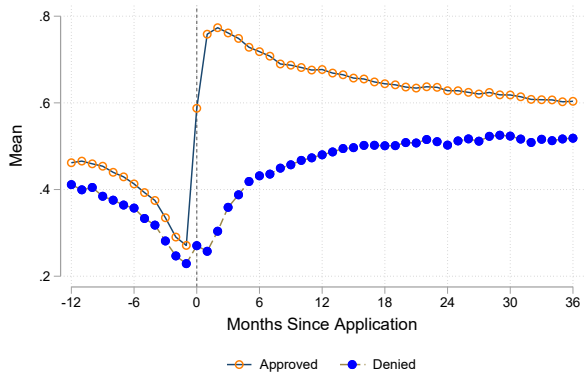
[20] Calculated as $\frac{(25.57-15.68)/15.68}{(0.22-0.77)/0.77}$, which is the % change in expenditure divided by the % change in the fraction of drug costs paid by the individual, from $t-1$ to $t+1$. This is a lower bound on the price elasticity, since the pharmaceuticals of non-IA recipients may be subsidized by a private health insurance plan, which I do not observe.

Figure III: **Pharmaceutical Spending Around Time of Application**

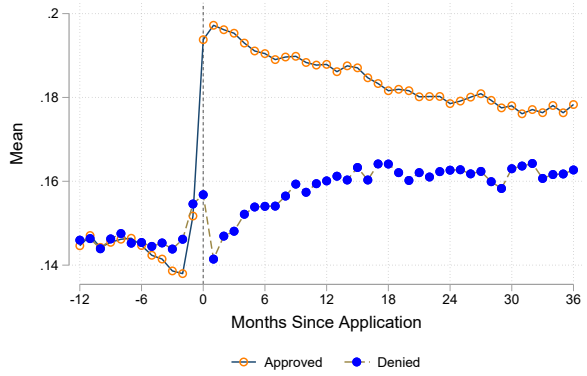
(a) **Drug Costs**



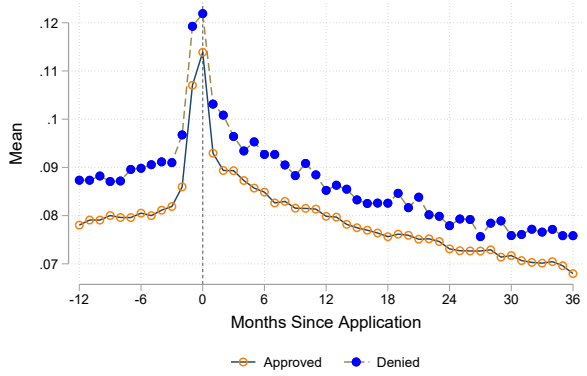
(b) **Fraction of Drug Costs Paid by Government**



(c) **Visited GP and Received Pharmaceutical**



(d) **Visited GP, Did Not Receive Pharmaceutical**



Note: This figure plots average outcomes around the time of application ($t = 0$) among the set of EDP-reviewed applications, separately for applicants that were approved and those that were denied. The outcomes, in order, are (a) total pharmaceutical spending, (b) the fraction of drug costs paid by the government, (c) an indicator for visiting a GP in the same month that pharmaceutical prescription was filled, and (d) an indicator for visiting a GP without a prescription filled in the month.

4 Causal Effects of Application Approval

4.1 Potential Mechanisms

Precarity: Welfare provides income support that prevents people from entering more precarious and stressful circumstances in the face of negative shocks. This could mean avoiding eviction. Or avoiding remaining with an abusive spouse. Or avoiding illicit activities to make ends meet. Or avoiding having to scrape by on low-wage employment as a single mother. By preventing precarity, and associated negative health events that may follow, welfare may improve health and

reduce health spending.

Time Use: Welfare also affects time use. Hicks et al. (2022) find that a reform which pushed single mothers off full-time welfare and into employment caused a reduction in family doctor visits. The likely mechanism is that single mothers have less time to seek medical care while working full-time, thus reducing health spending.

Promotion of Medical Care: Access to welfare may also directly promote medical care. Recipients get more generous pharmaceutical subsidies than non-recipients, which promotes pharmaceutical use and visits to physicians that prescribe them. Caseworkers may also encourage recipients to seek medical care for existing conditions, potentially as a means of promoting eventual return to work. In fact, case workers were trained to understand the health benefits available to IA recipients (both universally insured healthcare and supplemental subsidies for drugs, dental and medical devices) and to be able to direct clients to health services (Ministry of Human Resources, 1997*b*).[21]

Risky Behaviours and Idleness: Finally, it's at least possible that welfare negatively impacts health. Dobkin and Puller (2007), Riddell and Riddell (2006), and Evans and Moore (2011) document adverse health events around the day that welfare checks are issued. However, this does not mean that income worsens health, but rather that concentrated bursts promote risky behaviours. Indeed, adverse health events follow more general income receipt, such as the payday of public bureaucrats (Andersson, Lundborg and Vikström, 2015). Nevertheless, welfare may promote risky behaviours and disincentivize the pursuit of long-run employment growth, both of which could worsen health. The evidence on changes in risky behaviours is mixed: among mothers pushed off welfare due to reform in the US, Basu et al. (2016) find increased binge drinking, Kaestner and Tarlov (2006) find the opposite, and Corman et al. (2013) find reduced illicit drug use[22].

[21] In reference to health services, the manual states "This program is offered in co-operation with the Ministry of Health and provides partial to full coverage of medical costs (see Volume 3, Health and Other Services). It is important for staff to be familiar with the rules of eligibility for health benefits and services for recipients of all other Income Support Programs. These program rules and relationships are an important component of on-the-job and residential core training of financial assistance workers."

[22] The mechanisms for changes in illicit drug use are unclear – it could be that mothers returning to work face more drug tests which deters drug use. It could also be changes in income or time use.

4.2 Causal Setup

The goal is to estimate the effect of application approval in time t on subsequent outcomes in $t+h$. Denote application approval as $D_{i,t} = 1$, denial as $D_{i,t} = 0$, and subsequent outcomes as $Y_{i,t+h}$. The subscript h denotes the first, second, or third post-application year.[23] The outcome determination model is:

$$Y_{i,t+h} = \beta_{i,t+h} D_{i,t} + \alpha_{t,o} + \beta_X X_{i,t-1} + \eta_{i,t} \tag{1}$$

Where $\alpha_{t,o}$ represents office (o) by application year (t) fixed effects and $X_{i,t-1}$ are individual-specific controls. Throughout, two specifications are shown: one where $X_{i,t-1}$ is empty and one where $X_{i,t-1}$ contains the preapplication outcome variable ($Y_{i,t-1}$), which tends to improve the precision of the estimated effects. For individual i in application year t, the causal effect of being approved is $\beta_{i,t+h}$. The parameter of interest is some weighted average of treatment effects $\beta_{i,t+h}$. I identify this weighted average using the variation in adjudicators' propensity to recommend an application for approval ("leniency") as an instrument for $D_{i,t}$.

Random Assignment: This approach requires that the assignment of adjudicators is independent of potential outcomes. Since adjudicators are specific to field offices throughout the province, the composition of applications that each adjudicator receives varies with regional differences in the applicant pool. I condition on office fixed effects to account for these differences. Then the assumption is that, within an office, assignment is random. I go a step further by including office-by-year fixed effects to allow regional differences to change over time.[24]

As discussed in Section 1, adjudicators were assigned applications on a first-come-first-serve basis within an office — whichever adjudicator had availability would receive the application for review. Adjudicators did not specialize in particular types of applicants. These institutional fea-

[23] The first post-application year is months 1 to 12 following application in month 0. The second post-application year is months 13 to 24, and so on.

[24] Including the fully saturated set of controls (office by year fixed effects) also ensures that 2SLS estimation of equation (1) satisfies the LATE theorem, which relies explicitly on fully saturated, nonparametric controls (Imbens and Angrist, 1994; Blandhol et al., 2022).

tures imply that adjudicator assignment was likely random. To support this assumption, in the following subsection, I show that the estimated adjudicator leniency is not correlated with a rich set of applicant characteristics.

Exclusion Restriction: Identification also requires that adjudicators affect post-application outcomes only through their effect on application approval ($D_{i,t}$). This is likely true. Adjudicators only evaluated applicants' eligibility for welfare — they did not determine eligibility for other supports such as subsidized housing. Adjudicators may not even interact with applicants, unless an interview is conducted as part of the investigation process.

Estimation: The typical approach to using adjudicator leniency has been to estimate adjudicator fixed effects with the application decision as the outcome variable: $D_{i,t} = V_{j(i,t)} + \gamma_{t,o} + u_{i,t}$, where $V_{j(i,t)}$ are adjudicator fixed effects.[25] Then, their estimated counterparts $\hat{V}_{j(i,t)}$ are used as a just-identified continuous instrument for $D_{i,t}$ in equation (1). To avoid mechanically violating the independence assumption, $\hat{V}_{j(i,t)}$ are estimated on a leave-out-i basis.[26] In my case, I leave out the entire family unit, since families apply jointly.

This approach is equivalent to including adjudicator dummy variables directly as instruments for $D_{i,t}$ in the jacknife instrumental variable estimator (JIVE) proposed by Angrist, Imbens and Krueger (1999). However, the JIVE can be biased when there are many control variables (Kolesar, 2013) — in my case, the office-by-year fixed effects. Therefore, I employ the unbiased jacknife instrumental variable estimator (UJIVE) proposed by Kolesar (2013), and recently used by Norris, Pecenco and Weaver (2021), which performs well with many control variables. Just like the JIVE, the UJIVE can be implemented by estimating $\hat{V}_{j(i,t)}$, then using $\hat{V}_{j(i,t)}$ as a continuous instrument for $D_{i,t}$ in a two-stage least squares estimator of equation (1). See Appendix C for more details.

Recommendation versus Actual Approval: Rather than using application approval ($D_{i,t}$) to estimate adjudicators' leniency, I use their recommendation $R_{i,t}$. So, in the first step, adjudicator fixed effects are estimated by:

[25] I follow Dobbie, Goldin and Yang (2018) in allowing adjudicator fixed effects to differ in each year. I show below substantial year-over-year persistence in each adjudicator's measured leniency.

[26] Examples of this approach are Maestas, Mullen and Strand (2013) and Black et al. (2018) who estimate the effects of receiving Social Security Disability Insurance

$$R_{i,t} = V_{j(i,t)} + \gamma_{t,o} + u_{i,t} \tag{2}$$

and estimated $\hat{V}_{j(i,t)}$ are used as instruments for $D_{i,t}$ in equation 1. As noted in Section 1, VOs only made recommendations. Intake Officers did not follow the recommendation in 30% of cases. Using $R_{i,t}$ rather than $D_{i,t}$ is justified by the concern that some applicants are better at influencing the Intake Officers' final decisions. For illustration, assume that all VOs are equally lenient and randomly assigned applications. By pure chance, some VOs will be assigned more applicants that are strong self-advocates. Even if the VO recommends rejection, strong self-advocates can persuade Intake Officers to disregard that recommendation. This makes some VOs seem more lenient simply by chance when measured using approval rather than recommendation. This issue disappears when the number of applications reviewed by each VO becomes large, but the number of applications per VO is quite finite in practice.

Inference: Most, if not all, applications of jacknife estimators to "judge fixed effect" IV strategies use the 2SLS asymptotic distribution to calculate standard errors (e.g., Norris, Pecenco and Weaver (2021); Bhuller et al. (2020); Dobbie, Goldin and Yang (2018); Maestas, Mullen and Strand (2013)). I follow this convention. In doing so, I cluster standard errors at the adjudicator-year level, which is the effective level of random assignment.[27]

Monotonicity: To interpret the estimand as a weighted average of treatment effects among compliers, some form of monotonicity must hold. Strict (or pairwise) monotonicity assumes that if applicant i is more likely to be approved with adjudicator j than with adjudicator k, then the same must be true for *all applicants*. Strict monotonicty is a tall order, however. Frandsen, Lefgren and Leslie (2023) develop a test that jointly tests the exclusion restriction and strict monotonicity. Both Dobbie, Goldin and Yang (2018) and Norris, Pecenco and Weaver (2021) fail this test, as does my setup. Fortunately, a weaker form of monotonicity (*average monotonicity*) still allows the

[27] It should be noted, however, that the 2SLS limiting distribution can differ from the jacknife limiting distributions when the number of instruments grows proportionately with the number of observations. Chao et al. (2012) derive limiting distributions of the jacknife estimator under heteroskedasticity when instruments grow with the sample size, and Mikusheva and Sun (2021) develop a test for the jacknife estimator that works under even less restriction conditions.

estimand to be interpreted as a weighted average of treatment effects among compliers (Frandsen, Lefgren and Leslie, 2023).[28] Average monotonicity requires only that for any applicant i, the covariance between their approval outcome ($D_{i,t}$) and adjudicators' leniency is positive. I adopt this assumption and in the following section show evidence that it holds.

Analysis Sample Restriction: I restrict the sample to adjudicator-year pairs in which the adjudicator reviewed at least 25 applications to remove imprecisely estimated leniency measures from the first stage. I further restrict to office-year pairs in which there were at least two adjudicators, to ensure there is always a within-year-within-office comparison of adjudicators. Table B.1 shows that these restrictions reduce the sample from 156,819 to 98,953 applicants without changing their average characteristics. In this analysis sample, there are 350 adjudicators that were present in the data for 2 years on average. Each adjudicator reviewed an average of 148 applications per year.

I focus on adults aged 19 to 60 years. The sample of children that can be linked to their parents' application is small. And among these children, there is no change in health spending around the time of application and no difference between those whose parents were approved versus denied (see Section B.5).

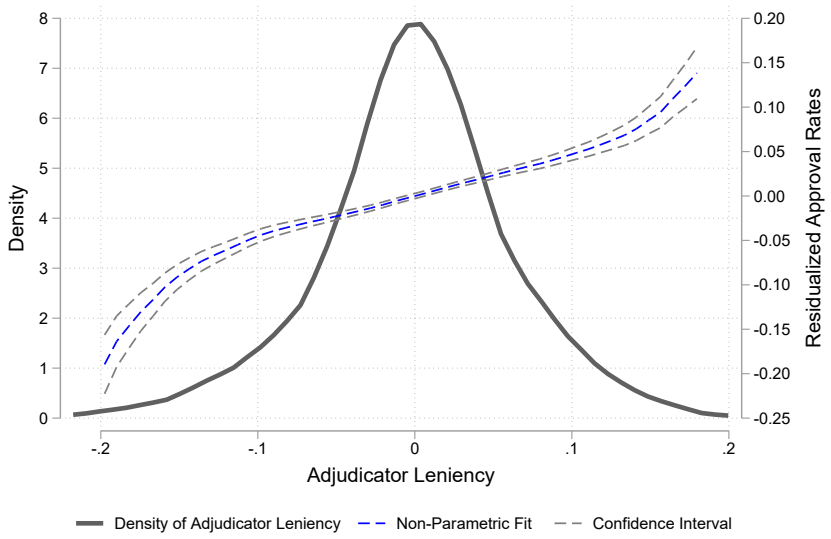
Comparison to OLS with Controls: The IV estimates are relevant only to a specific complier group and are generally much less precise than OLS. For this reason, I will also show OLS estimates of equation 1 that control for a rich set of applicant background characteristics in $X_{i,t-1}$ which I describe below. The OLS estimator is well-identified if $X_{i,t-1}$ fully captures selection into approval.

4.3 First Stage Tests

To assess the underlying variation in adjudicator leniency, in Figure IV, I plot the distribution of estimated leniency ($\hat{V_{j(i,t)}}$ estimated from equation (2)) and a local linear regression of application approval on $\hat{V_{j(i,t)}}$. The 1st and 99th percentiles of leniency are -0.197 and 0.179, respectively, and moving between these percentiles increases the approval probability by 32 percentage points.

[28] The weights are the covariance between the individual's adjudicator-specific treatment status and adjudicators' average treatment propensities across the full sample.

Figure IV: **Adjudicator Leniency and Approval Rates**



Note: The first series is the density of estimated adjudicator leniency (as described in the text). The second series is the kernel-weighted local linear regression (with a bandwidth of .02) between leniency and application approval, after controlling for office-by-year fixed effects. The 95% confidence intervals are also shown. Adjudicator leniency is residualized at the 1st and 99th percentiles for the purpose of graphing.

In Table II, I show the linear first stage coefficient — $D_{i,t}$ regressed on $\hat{V}_{j(i,t)}$ — for the full sample, childless adults, and parents. Both in the full sample and each subsample, $\hat{V}_{j(i,t)}$ is strongly predictive of application approval. The F statistic for this first-stage relationship is 479 in the full sample, 337 among childless adults, and 220 among parents. By linearity of the first stage, the share of the sample that are compliers is $\pi \times (V_{max} - V_{min})$. The sample estimate of this is 0.35, indicating that approximately one third of the sample are compliers.

Table II: **First Stage: Adjudicator Leniency on Application Approval Probability**

	(1) Full	(2) Childless	(3) Parent
Adjudicator Leniency	0.462***	0.483***	0.433***
	(0.0211)	(0.0263)	(0.0292)
Observations	98333	52051	46282
F-statistic	479.7	337.0	220.9
Year-over-Year Correlation in Adjudicators' Leniency	0.461		
Approximate Complier Share	0.354	0.360	0.332

Note: This table shows the estimated effect of leniency on application approval for the full sample, childless adults, and parents. Regressions control for office-year fixed effects and cluster standard errors at the adjudicator-year level. The Year-over-Year Correlation in Adjudicators' Leniency is $Cor(V_{J,t}, V_{J,t+1})$. * $\rho < .05$ ** $\rho < .05$ *** $\rho < .1$

Supporting Random Assignment: To investigate random assignment, I regress $\hat{V}_{j(i,t)}$ on applicant characteristics observed in the 12 months before the application and office-by-year fixed effects. The results are shown in Table III. Under random assignment, leniency should be orthogonal to applicant traits. And indeed it is. Of the 14 included characteristics, only one is significant at the 5% level (spending on treatment for respiratory illness) and the joint F statistic is 1.66. However, these 14 characteristics strongly predict application approval, with a joint F-stat of 47.

Supporting Average Monotonicity: A testable implication of average (and strict) monotonicity is that $\hat{V}_{j(i,t)}$ constructed using the full sample should non-negatively correlate with application approval all subsamples of the data (Frandsen, Lefgren and Leslie, 2023). Following this logic, I split the sample based on five characteristics: above vs. below median age, childless vs. parents, female vs. male, first time vs. prior user, and above vs. below total medical spending. I then take all combinations of those two-way splits, for 32 groups in total. For each, I estimate the first stage equation ($D_{i,t} = \pi \hat{V}_{j(i,t)} + \gamma_{t,o} + u_{i,t}$), and plot the estimates of π and the associated T statistics in Figure V for each group. Every $\hat{\pi}$ is positive, and the majority are statistically significant at the 95% level, implying that average monotonicity unambiguously holds.

Characterizing Compliers: I follow the approach used by Norris, Pecenco and Weaver (2021) to estimate the average characteristics of compliers.[29] Table B.2 shows the average characteristics of the full sample in column (1), of compliers in column (2), and their ratio in column (3). Most notably, 64% of compliers are childless compared to 53% in the full sample, and as a result, more likely to be male. They are also more likely to have received welfare previously — only 16% are first-time users, compared to 23% in the full sample. Like the full sample, compliers experience a large increase in total health spending in the months before an application, and have similar overall health spending averaged over the 12 months preceding an application.

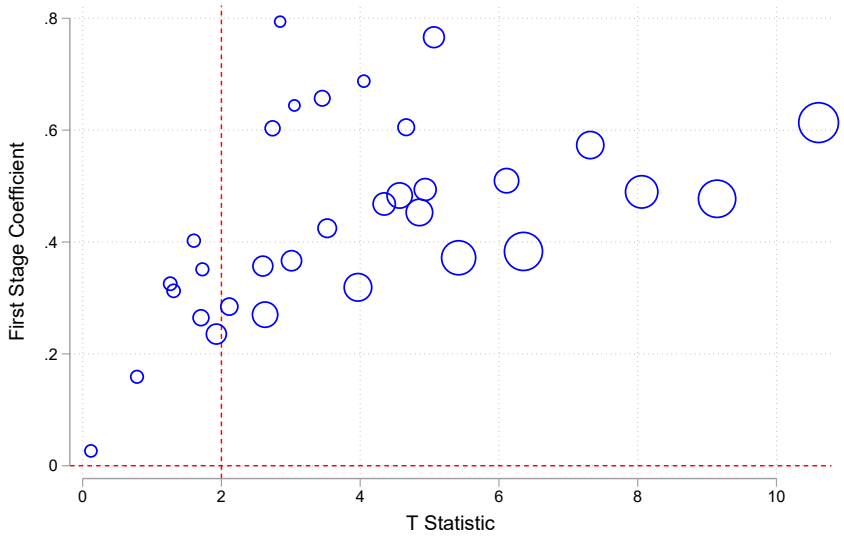
[29] Specifically, estimating a regression where the outcome variable is the characteristic of interest interacted with the binary treatment variable, and the endogenous variable is the binary treatment, while instrumenting (2SLS) using the first stage equation. That is, the second stage is $X_{i,t}D_{i,t} = \kappa D_{i,t} + \alpha_{t,o} + v_{i,t}$, and the first stage $D_{i,t} = \pi \hat{V}_{j(i,t)} + \gamma_{t,o} + u_{i,t}$. The estimate of κ is the estimate of $E[X_{i,t} \mid \text{Complier}]$.

Table III: **Correlation Between Adjudicator Leniency and Applicant Characteristics**

	Approved	Leniency
Age	0.00310**	0.00200
	(0.00114)	(0.00240)
Age Squared	-0.0000498***	-0.0000366
	(0.0000149)	(0.0000308)
Childless Man	-0.00813*	0.0179
	(0.00407)	(0.0104)
Childless Woman	0.00731	0.00802
	(0.00477)	(0.00998)
Male Parent	-0.0197***	-0.000775
	(0.00393)	(0.00766)
Employer Paid Premiums	-0.0250***	-0.0166
	(0.00474)	(0.00976)
Resident Prior to Application	-0.0327***	0.00157
	(0.00517)	(0.0141)
No IA History	-0.0981***	-0.0131
	(0.00437)	(0.00764)
Total Hospital and Outpatient Spending	-0.00686	0.0106
	(0.00468)	(0.0132)
Mental Health Spending	0.00464	0.00702
	(0.00284)	(0.00604)
Cold or Flu Spending	-0.00347	-0.0149*
	(0.00320)	(0.00736)
Injury Spending	-0.00250	-0.00164
	(0.00309)	(0.00717)
GP Visit Spending	-0.00752***	0.000509
	(0.00215)	(0.00435)
Drug Spending MH	-0.00653	-0.0102
	(0.00376)	(0.00754)
Drug Spending	-0.0000127	-0.000647
	(0.00335)	(0.00604)
Observations	98942	98322
Fstat	47.24	1.661

Note: This table shows the results of regressing (i) application approval, and (ii) estimated adjudicator leniency, on applicant characteristics and office-by-year fixed effects. Adjudicator leniency is normalized to standard deviation 1 to ease interpretation of coefficients. “Employer Paid Premiums" is a binary variable indicating whether an employer paid the applicant’s health insurance premiums at some point in the 12 months prior to application. The health spending variables are total spending (in a given category) over the 12 months prior to application, each normalized to standard deviation equal to one. “Resident Prior to Application" indicates whether the person appeared in at least one administrative dataset in the 12 months prior to application. Standard errors are clustered at the adjudicator-year level. The Fstat is the F-statistic for the test of joint significance. * $\rho < .05$ ** $\rho < .05$ *** $\rho < .1$

Figure V: **Average Monotonicity**



Note: I split the sample based on five characteristics: above vs. below median age, childless vs. parents, female vs. male, first time vs. prior user, and above vs. below total medical spending. I then take all combinations of those two-way splits, for 32 groups in total. For each, I estimate the first stage equation ($D_{i,t} = \pi \hat{V_{j(i,t)}} + \gamma_{t,o} + u_{i,t}$), and plot the estimates of π and associated T-statistics. The circle size is proportionate to the # of observations in the group.

4.4 Causal Effects on Post-Application Outcomes

In Figures VI and VII, treatment effects (from equation 1) are plotted for each post-application year and outcome. The estimated effects on the cumulative three-year outcomes are shown in Table IV.

Panel (a) of Figure VI plots the effect on extensive margin IA use. In the first post-application year, approval causes a 68 p.p. increase in extensive margin IA use. This is less than 100 p.p. because 32% of denied applicants reapply and receive IA. By the third post-application year, the effect of approval is less than 5 p.p. and statistically insignificant.[30] Using the 1996 experiment (which I examine in section 4.6), Green and Warburton (2004) found the same result, and demonstrated that the zero long-term treatment effect is driven foremost by approved applicants exiting welfare, and secondarily by a small sub-group of denied applicants eventually gaining access. The implication is that, for most applicants, being granted welfare does not represent a permanent change in transfers but rather medium-term support in the face of health and employment shocks.

Table IV shows that, over three years after application, the causal effect of approval is approx-

[30] Of that 5 p.p., 2.p.p. is attributable to persons that transitioned to disability insurance (see Figure B.9).

imately 6 months of benefits, corresponding to $4000 (2002 CAD) in total, or $705 per month. As a reference point, $705 is equivalent to 88 hours of work at the minimum wage in 2002. Note that these causal effects are not the average amount that an approved applicant receives, but rather the difference between what approved and denied applicants receive.

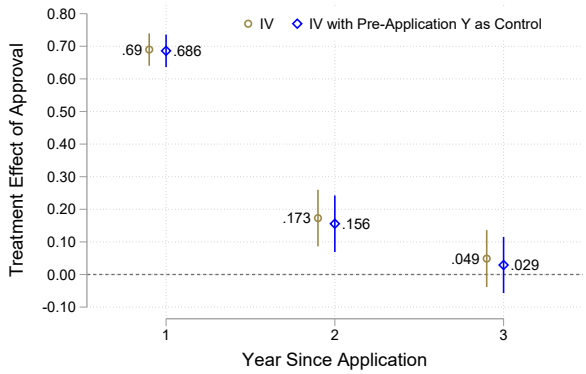
The remaining rows show effects on cumulative health outcomes, expressed both in dollars and in standard deviations. Benefit approval causes total hospital and outpatient spending to increase by a statistically insignificant $296, or 0.06 standard deviations. Or, put differently, every dollar issued in IA benefits is associated with 7.4 cents in nonpharmaceutical medical spending. Most of this effect derives from the first year after application, as shown in Figure VI. There are similar effects (in terms of standard deviations) on injury and mental health spending, both in the first year. Spending in both of these categories spiked before application, so the minor positive causal effects on post-application spending could mean that gaining access to welfare grants individuals the time to seek treatment (due to not working) and the potential encouragement by case workers to do so.

Unlike hospital and outpatient care, there are large increases in pharmaceutical spending: $408, or 0.2 standard deviations, over three years. This increase is associated with an increase in GP visits associated with the writing of a prescription (panel (c) of Figure VII). These effects are clearest in the first post-application year, where the treatment effect is $99 to $172 depending on the specification (panel (a) of Figure VII). In that first year, the average effect of approval on the percent of the total drug cost paid by the individual is 36 p.p. Using the average pharmaceutical spending and the average copay in the year before application as the base for calculating elasticities ($200 and 66%, respectively, as shown in Table I), the causal effect on pharmaceutical spending translates to a price elasticity of between -0.92 and -1.6.[31] These is on the upper range of existing estimates from other countries (see Goldman, Joyce and Zheng (2007) for a meta-review). The high elasticity here may reflect additional marginal increases in health care more generally due to reasons other than the price change (thus overstating the price elasticity). Or it could be that price elasticities for younger and lower income groups are higher than those estimated among

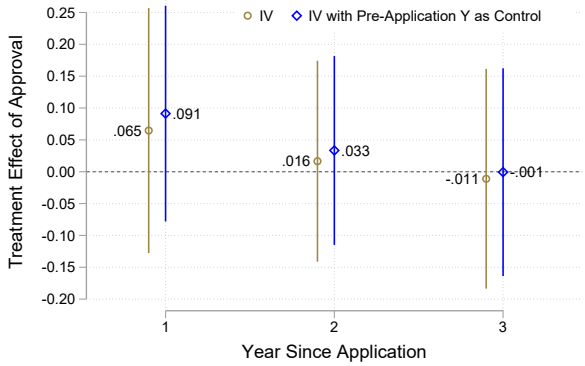
[31] Calculated as $\frac{99/200}{-35.6/66}$ and $\frac{172/200}{-35.4/66}$ respectively.

Figure VI: **Dynamic Treatment Effects on IA Receipt and Health Spending**

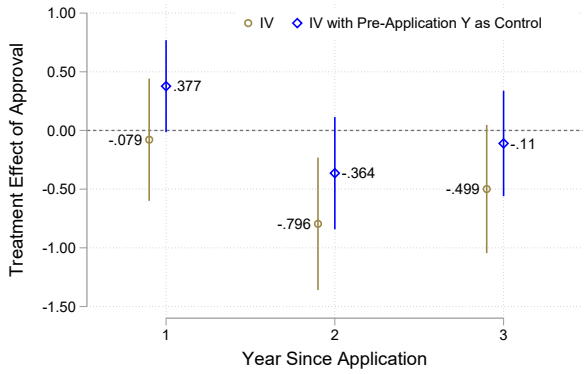
(a) **Received Income Assistance**



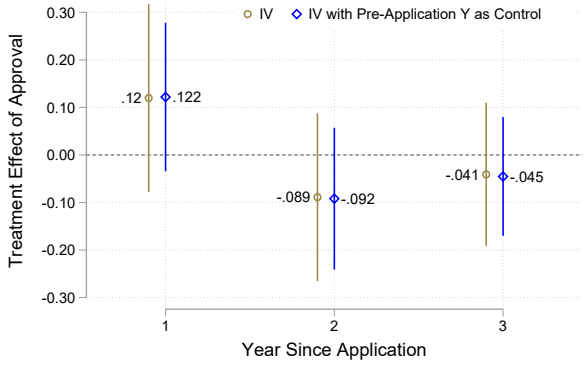
(b) **Total Medical Spending Standardized**



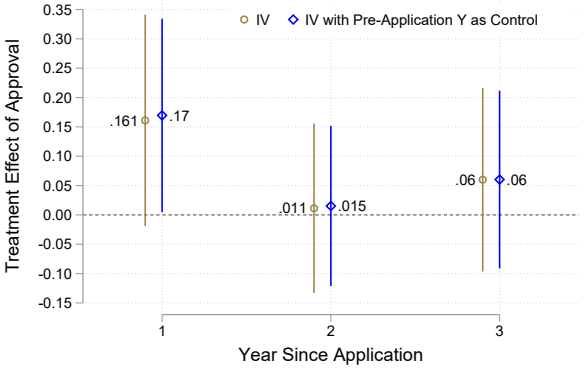
(c) **# General Practitioner (GP) Visits**



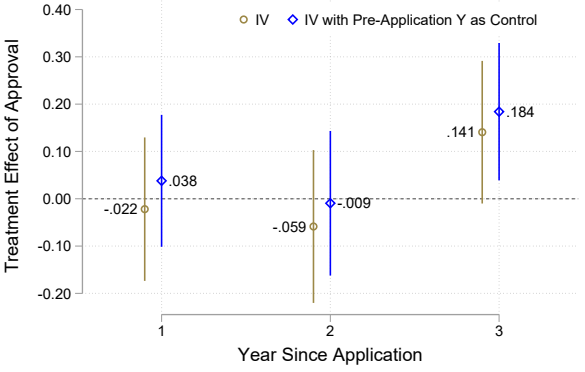
(d) **Mental Health Spending Standardized**



(e) **Physical Injury Spending Standardized**



(f) **Respiratory and Flu Spending Standardized**



Note: IV effects of application approval on outcomes in each of the three years following application are shown, along with 90% confidence intervals. The outcomes, in order, are: (a) whether the applicant received IA in the year, (b) total non-pharmaceutical medical spending, (c) the number of visits to a general practitioner, (d) spending on mental health treatment, (e) spending on physical injury, and (f) spending on respiratory illness. Spending variables are winsorized at the 99th percentile in each year and are standardized to have a standard deviation equal to one in each year. Standard errors are clustered at the adjudicator-year level.

seniors who are often the focus of drug subsidy studies.[32] Goldman, Joyce and Zheng (2007) specifically emphasize the lack of estimates among low-income populations as a major weakness of the literature.

In Figure B.10, the effects of treatment on mortality rates and fertility (among women) are plotted. I find no statistically significant effects on either, with the caveat that both outcomes are extremely rare to begin with — Table I shows that only 2.99% of applicants died within four years of the application. This is consistent with applicants being relatively young – 34 years old on average. In Appendix B.3, I show that benefit approval has no detectable long-term effect on the likelihood of remaining in the province. This means that the estimated treatment effects of benefit approval on health outcomes are not attenuated by selective out-migration.

Table IV: **Effects on Cumulative Three Year Outcomes**

	OLS		IV		IV w Controls	
	B	SE	B	SE	B	SE
Dollar Amount of IA	6076.43	87.65	4551.41	1189.40	3958.15	1144.59
Months of IA	8.58	0.10	6.42	1.48	5.67	1.45
Months of Employer-Paid Insurance Premiums	-0.63	0.06	-0.99	0.76	-0.27	0.67
Hospital and Outpatient Spending	235.22	37.12	219.49	547.63	296.80	482.98
Total Pharmaceutical Spending	179.29	11.79	193.24	224.28	408.35	164.57
Injury Spending	7.65	1.38	34.53	20.89	34.61	19.25
Mental Health Spending	37.20	4.49	-2.44	75.11	-6.35	58.89
Cold and Flu Spending	2.96	0.34	3.57	4.71	6.71	4.28
Mental Health Pharma Spending	45.32	5.18	119.58	100.04	158.17	72.52
Number of GP Visits	0.95	0.05	-1.24	0.92	0.01	0.71
Hospital and Outpatient Spending (Std. Deviations)	0.04	0.01	0.04	0.10	0.06	0.09
Total Pharmaceutical Spending (Std. Deviations)	0.09	0.01	0.09	0.11	0.20	0.08
Injury Spending (Std. Deviations)	0.04	0.01	0.18	0.11	0.18	0.10
Mental Health Spending (Std. Deviations)	0.06	0.01	-0.00	0.11	-0.01	0.09
Cold and Flu Spending (Std. Deviations)	0.06	0.01	0.07	0.10	0.14	0.09
Mental Health Pharma Spending (Std. Deviations)	0.05	0.01	0.14	0.11	0.18	0.08

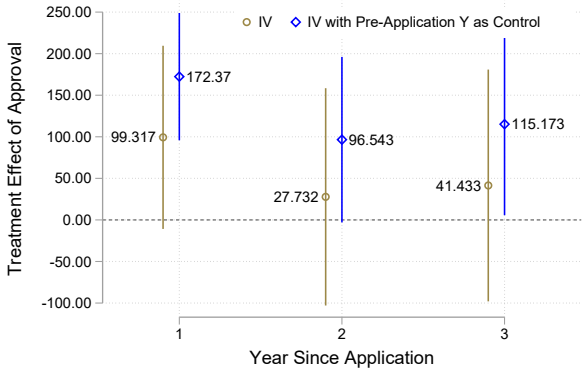
Note: This table shows estimates of the effect of application approval on cumulative outcomes over the subsequent three years. The models, in order, are OLS with controls, IV, and IV with controls, all of which are described in section 4. Spending variables are winsorized at the 99th percentile. Standard errors are clustered at the adjudicator-year level.

Comparison to OLS: Table IV also shows the OLS estimates with controls. The controls

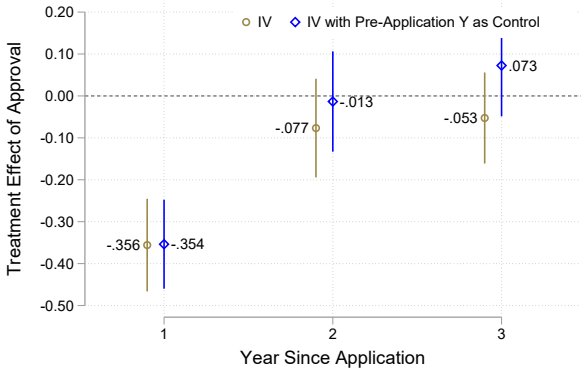
[32]Prominent studies of drug price elasticities focus on Medicare Part D in the US (Yin et al., 2008; Einav, Finkelstein and Polyakova, 2018), which is a subsidy for seniors. Studies using the RAND Health Insurance experiments to estimate price elasticities struggle with the fact that the experiment affected health care prices beyond pharmaceuticals, generating potential cross-price effects that need to be separated from own-price effects (Yeung et al., 2018).

Figure VII: **Dynamic Treatment Effects on Pharmaceutical Outcomes**

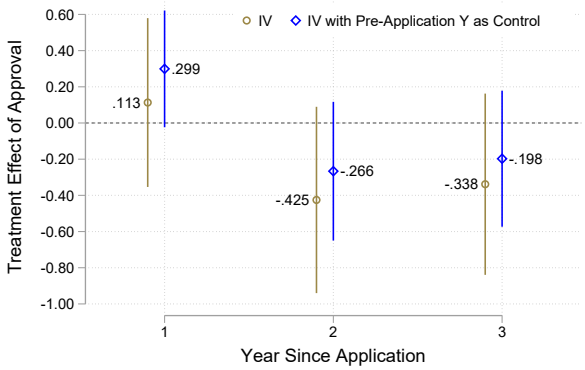
(a) **Drug Costs**



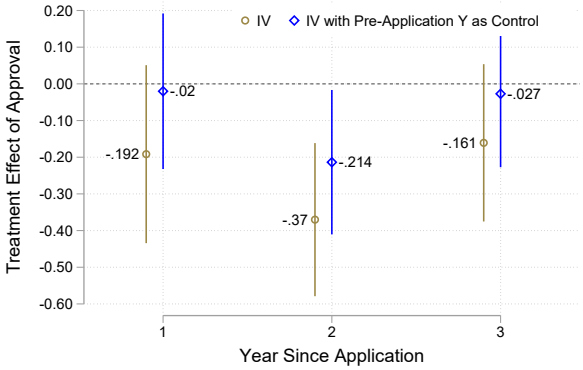
(b) **Fraction of Drug Cost Paid by Individual**



(c) **# GP Visits with Pharmaceutical**



(d) **# GP Visits without Pharmaceutical**



Note: This figure plots estimated IV effects of application approval on outcomes in each of the three years following application, and 90% confidence intervals. The outcomes, in order, are (a) total pharmaceutical spending, (b) the fraction of drug costs paid by the individual (as opposed to the government) among persons filling prescriptions, (c) the number of GP visits wherein a pharmaceutical prescription was filled in the same month, and (d) the number of GP visits without a prescription filled in the month.

include all the pre-application characteristics used for the balance tests in Table III. In comparing the OLS with the IV estimates, it is important to recall that the IV compliers are skewed towards childless adults (see Table B.2), who may experience different health effects. The OLS estimates for total benefits and benefit months received are larger: $6,000 over 8.58 months. The OLS estimates on health spending are generally smaller than the IV, but have the same qualitative pattern: higher effects on pharmaceuticals than non-pharmaceutical spending. Of course, given the greater efficiency of OLS, the small positive effects on spending are statistically significant. The effect on non-pharmaceutical spending ($235) equates to 3.8 cents per welfare dollar paid.

So, the general take-home message is consistent across estimators: average effects on health-care utilization are quite minimal. Using the IV estimates, we can rule out non-pharmaceutical spending effects larger than 0.23 and smaller than -0.10 standard deviations. Using the OLS, we can rule out effects greater than 0.06 standard deviations. The conclusion is that welfare is offering insurance against health shocks without generating substantial spillovers into health spending. Small positive effects are consistent with (a) recipients having more non-work time to seek out health care, (b) case workers encouraging recipients to seek treatment for existing issues, and (c) the pharmaceutical subsidy inducing more pharmaceutical spending and physician billings associated with the prescribing of those pharmaceuticals.

Comparison to Literature: These results are broadly consistent with Hicks et al. (2022) wherein we find mostly zero or small effects on mother and child health spending from a Canadian welfare reform. My paper differs in that the majority of compliers are childless adults, as opposed to mothers with young children. In the next section, I compare causal effects for childless adults with those of mothers. Hicks et al. (2022) also leverage a different empirical strategy: the imposition of work search requirements. This distinction matters because the compliers generated from work search requirements were very employable, whereas the compliers in my paper may not be.[33]

Evidence from US welfare reforms similarly finds either no effect (Kaestner and Tarlov, 2006) or small positive effects (Basu et al., 2016; Narain et al., 2017) of welfare access on self-reported health among mothers.[34][35] Importantly, studies of US reform are confounded by changes in health insurance (Kaestner and Kaushal, 2003; Bitler, Gelbach and Hoynes, 2005; Cawley, Schroeder and Simon, 2006), since former welfare recipients lost Medicaid coverage, which makes it difficult to distinguish the effects of losing health insurance from the effects of losing welfare benefits. Indeed, Finkelstein et al. (2012) find that Medicaid insurance coverage strongly affects self-reports of

[33] In Hicks et al. (2022), we were able to link income tax records to welfare caseloads in order to measure employment. In the present paper, I was unable to link welfare *applications* to tax returns.

[34] For instance, Basu et al. (2016) find that welfare reform, which reduced access for mothers, had small negative effects on self-reported "days of good mental health". In a related example, Batra, Jackson and Hamad (2023) find that an expansion the Child Tax Credit in the US during Covid reduced the prevalence of self-reported negative and anxious feelings. These findings are quite intuitive; financial uncertainty naturally raises stress.

[35] This lack of contemporary effect may mask long-term health consequences driven by changes in unhealthy behaviors, such as drinking and smoking. Evidence on these outcomes is mixed, as described above.

healthiness and subjective well-being, which fits with minor negative health effects of US welfare reform. It also fits with the findings of this paper: The most detectable changes in health patterns were on pharmaceuticals, the one domain of health spending in which welfare access influences insurance coverage.

4.5 Heterogeneity by Family Type

Unlike TANF in the US, Canadian IA programs provide welfare to both families with children and childless adults. Childless adults may be affected quite differently by approval/denial for welfare relative to parents. To assess this possibility, I estimate effects separately for each group. Understanding these potential differences facilitates comparison to empirical findings across contexts where the applicant composition varies. The definition of a parent using administrative data is detailed in section 2.

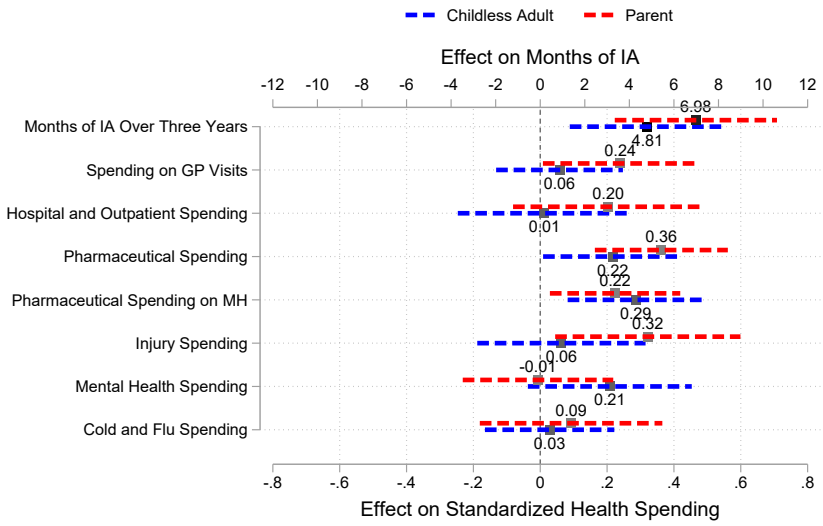
I estimate equation (1) separately for each group, while instrumenting for application approval with leniency estimated from equation (2) using the full sample. Using $\hat{V}_{j(i,t)}$ from the full sample is permissible under the assumption of average monotonicity, and improves precision by more precisely estimating adjudicator leniency. The first-stage coefficients for each group are shown in Table II. I focus on health spending in the first post-application year, as this is where the marginally significant effects appeared in the full sample.

Estimates are shown in Figure VIII. Point estimates are expressed in terms of *full sample* standard deviations, such that effect sizes are directly comparable across subsamples. Both parents and childless adults see increases in pharmaceutical spending, although more substantially among parents (0.36 versus 0.22 standard deviations). Childless adults are more likely to be treated for mental health in a hospital or outpatient setting and marginally more likely to receive drugs for mental health treatment. Parents are more likely to see increases in GP visit spending and treatment for physical injuries.

The greater increase in GP visits among parents coincides with the greater increase in pharmaceutical spending. It is also consistent with Hicks et al. (2022) who find that a welfare reform

reduced the frequency of GP visits for mothers and their children, which Hicks et al. (2022) partially attribute to the greater time constraints on single mothers who work full time. What about the increase in mental health treatment among childless adults (mainly men)? An ubiquitous fact is that men are less likely to seek treatment for mental health illness, relative to women, of their own accord (Galdas, Cheater and Marshall, 2005). In this context, gaining access to welfare and interaction with case workers may be the shove that men require to seek treatment for mental health issues.[36] This interpretation, is, of course, speculative and should be considered a way marker for future research.

Figure VIII: **Effects on Childless Adults and on Parents**



Note: This figure plots estimated effects of application approval on health outcomes in the first post-application year and 95% confidence intervals, separately for childless adults and parents (as defined at the time of application). The point estimates are listed beside the markers. Treatment effects on the health spending variables are in standard deviation units. Spending variables are winsorized at the 99th percentile in each year since application. Standard errors are clustered at the adjudicator-year level.

[36] In related work, Wickham et al. (2020) study the effects of the UK's 2012 welfare reform which consolidated existing welfare programs into a "'Universal Credit" that imposed additional conditionality, a longer waiting period for new applicants, and less frequent payments. They find suggestive evidence that these changes worsened self-reported mental health among unemployed individuals.

4.6 Comparison to Estimates Using the Pilot Experiment

In this section, I use the 1996 experimental pilot of the EDP to instrument for benefit approval. Causal estimates from this exercise act as a validation check for the IV estimates from the previous section because in the experiment, independence and monotonicity hold almost by definition. The trade-off is greater imprecision because the sample size in the experiment is 1/8th the size.

For the pilot, a set of high-volume offices was selected to receive the first round of VOs. Each treated office identified a comparable non-treated office to be used as a control group. Because this was a pilot program, most, if not all, applicants would have been unaware of the additional scrutiny their application could receive at a treated office. This alleviates concerns that applicants sought control offices to avoid additional scrutiny. It is possible that after being rejected at a treated office, an applicant would reapply at a control office. To guard against this contamination, I keep only the first application in the sample period among those who applied more than once.

Table B.3 shows that the composition of applicants in the treated offices is statistically and economically indistinguishable from those of the control offices, suggesting that the selected control offices were a suitable control group. Furthermore, in panel (a) of Figure IX, I show that approval rates were very similar in both levels and trends between the treated and control offices before the pilot. After the implementation of the pilot in late 1995, approval rates dropped in the treated offices by approximately 10 p.p. relative to control offices.

To estimate the effect of approval on subsequent outcomes, I use a first-stage equation of:

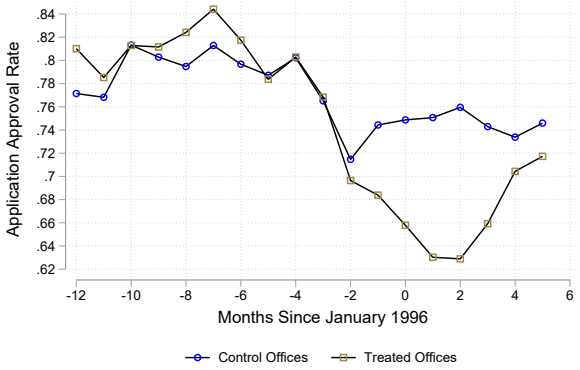
$$D_{i,t} = \pi T_{i,t} + \pi_t + u_{it} \tag{3}$$

where $T_{i,t}$ is an indicator for applying at the treated office in month t and π_t are fixed effects for the month of application. I estimate 2SLS effects of approval using equation (3) as the first stage. Standard errors are clustered at the office level. Since there are only 36 offices, I show both wild-cluster bootstrap CIs and regular cluster-robust standard errors.

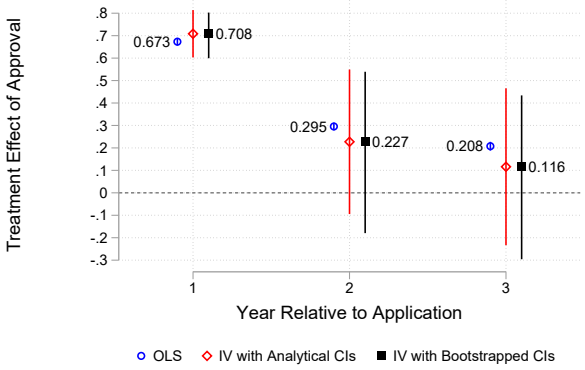
Effects on long-term welfare use are shown in panel (b) of Figure IX. The OLS implies long-

Figure IX: **1996 EDP Pilot Experiment**

(a) **Approval Rates by Treatment Status**



(b) **Effects on Long Run IA Use**



Note: Panel (a) shows the fraction of applications that were approved for benefits in the treated and control offices over time. Panel (b) shows the estimated effect of application approval on extensive margin IA receipt in each year after application, among applicants that applied during the pilot period. See text for estimation details. Standard errors are clustered at the office level. To account for the relatively small number of clusters, wild-cluster bootstrapped CIs are also reported (with 499 repetitions), again with clustering at the office level.

term persistence of 20.8 p.p., while the 2SLS estimate is 11.6 p.p., implying that persistence mostly disappears 3 years after application, consistent with the results in section 4 and Green and Warburton (2004).

Effects on health spending in the first year after application are shown in Table V and, in Figure X, compared with the analogous estimates from section 4. The experiment produces strikingly similar point estimates: the effect on total hospital and outpatient spending is -.04 standard deviations, comparable to 0.06 from the leniency approach. On pharmaceutical spending, 0.19 in the experiment versus 0.17 standard deviations. On GP visit spending, 0.06 versus 0.09 standard deviations. This similarity suggests that the identification strategy that exploits variation in adjudicator leniency is not biased due to some unobserved non-randomness in the assignment of adjudicators.

5 Discussion and Conclusion

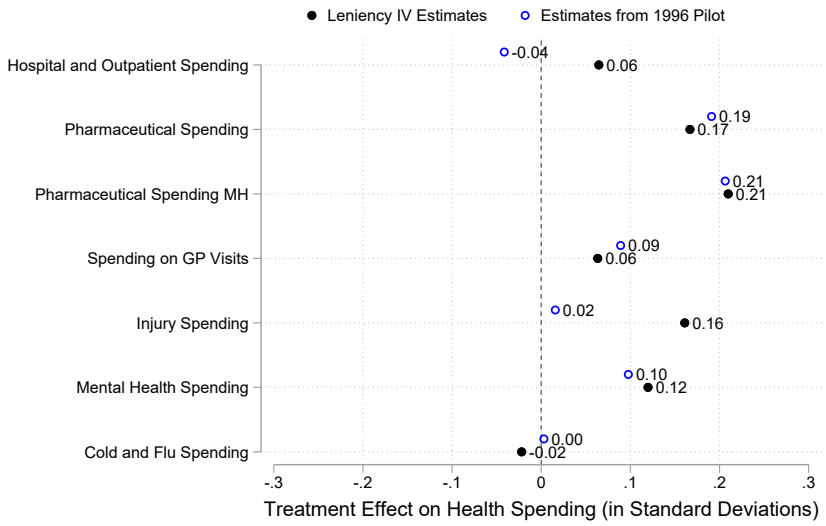
This paper explores the relationship between health and welfare. On the one hand, I find that health degrades sharply in the run-up to a welfare application: total hospital and outpatient spending doubles in the months before application. These spikes mostly dissipate over time, consistent with them being relatively short-term conditions. The implication is that traditional cash welfare insures

Table V: **Effects in First Post-Application Year from 1996 Pilot Experiment**

	OLS		IV				
				Analytical CIs		Boostrap CIs	
	B	SE	B	Min 95	Max 95	Min 95	Max 95
Dollar Amount of IA	2980.39	62.61	3078.69	1077.03	5080.34	1066.03	5315.57
Hospital and Outpatient Spending	0.05	0.01	-0.04	-0.24	0.16	-0.23	0.14
Total Drug Spending	0.06	0.01	0.19	-0.05	0.43	-0.03	0.48
Mental Health Drug Spending	0.05	0.01	0.21	-0.07	0.49	-0.05	0.54
Spending on GP Visits	0.10	0.02	0.09	-0.63	0.81	-0.59	0.94
Injury Spending	0.05	0.01	0.02	-0.26	0.29	-0.28	0.28
Mental Health Spending	0.05	0.01	0.10	-0.21	0.41	-0.19	0.50
Cold and Flu Spending	0.02	0.02	0.00	-0.48	0.49	-0.48	0.60

Note: This table contains estimated effects of application approval on outcomes in the first post-application year. The sample is applicants that applied during the pilot period (January 1996 to June 1996). The instrument is a dummy indicating the applicant applied at a treated office. Regressions control for month of application fixed effects. Standard errors are clustered at the office level. Spending variables are winsorized at the 99th percentile, and standardized to have standard deviation equal to 1. Analytical confidence intervals are constructed using regular cluster-robust standard errors. Bootstrapped CIs are wild-cluster bootstrapped confidence intervals with 499 repetitions.

Figure X: **Comparison of Baseline Estimates to Estimates from Pilot Experiment**



Note: This graph plots the estimated effects of being approved for welfare. The black circles demarcate the estimates from section 4.4. The blue circles show the effects from section 4.6 which uses as an instrument the initial pilot experiment of the EDP program in 1996. See text for details.

against short-term health shocks that might not be insured through other means. Stepner (2019) makes a similar point (using Canadian data) for the tax and transfer system in general. He finds that disability and unemployment insurance constitute only 25% of the total income replacement

after a hospitalization event. The remaining 75% of income replacement comes from progressive taxes and transfers, of which cash welfare is one component.

On the other hand, I show that approval for welfare has, at most, modest positive effects on universally-insured health spending. If such small positive effects exist, they are consistent welfare recipients being connected with the health care system through case officers and with recipients having more time to seek out treatment due to not having the pressure of working full time. But the largest spending effects are among pharmaceuticals which are not universally insured and for which welfare recipients receive heavy subsidization.

From a policy perspective, the results affirm welfare as an insurance mechanism for income losses stemming from temporary health shocks, that does not generate significant health spillovers. The sizable effects on pharmaceutical spending suggest that a lack of insurance coverage is holding back medication access among populations on the margin of welfare access. The other major subsidy that welfare recipients receive is for non-emergency dental care. While I do not observe private dental claims in this paper, it is plausible that there are similar spending effects on dental treatment. This finding is relevant to recent policy developments and debates on expanding government-provided dental (Robson, Schirle and Lindsay, 2022) and pharmaceutical insurance (Health Canada, 2019).

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Appendix for Online Publication Only

Jeffrey Hicks

A Data Details

A.1 Building a Dataset of Applications Data

To build a data set of applications, I use two administrative data sets. The first is the “pre-application" file. Persons who entered a field office seeking information about applying would be met by a front-line worker who would offer a preliminary assessment of whether the person may be eligible. Starting an application at this stage appears in the “pre-application" records. I consider the individual to be an applicant if they make it to this stage, regardless of whether they finish the application, and indeed many do not. They may find the hassle costs of gathering documentation too high. They may find work while they are proceeding with the application. They may realize that they are ineligible. If the person completes the application, it becomes eligible for review by a VO. Because families apply jointly for IA, I consider all adults listed on the application as applicants.

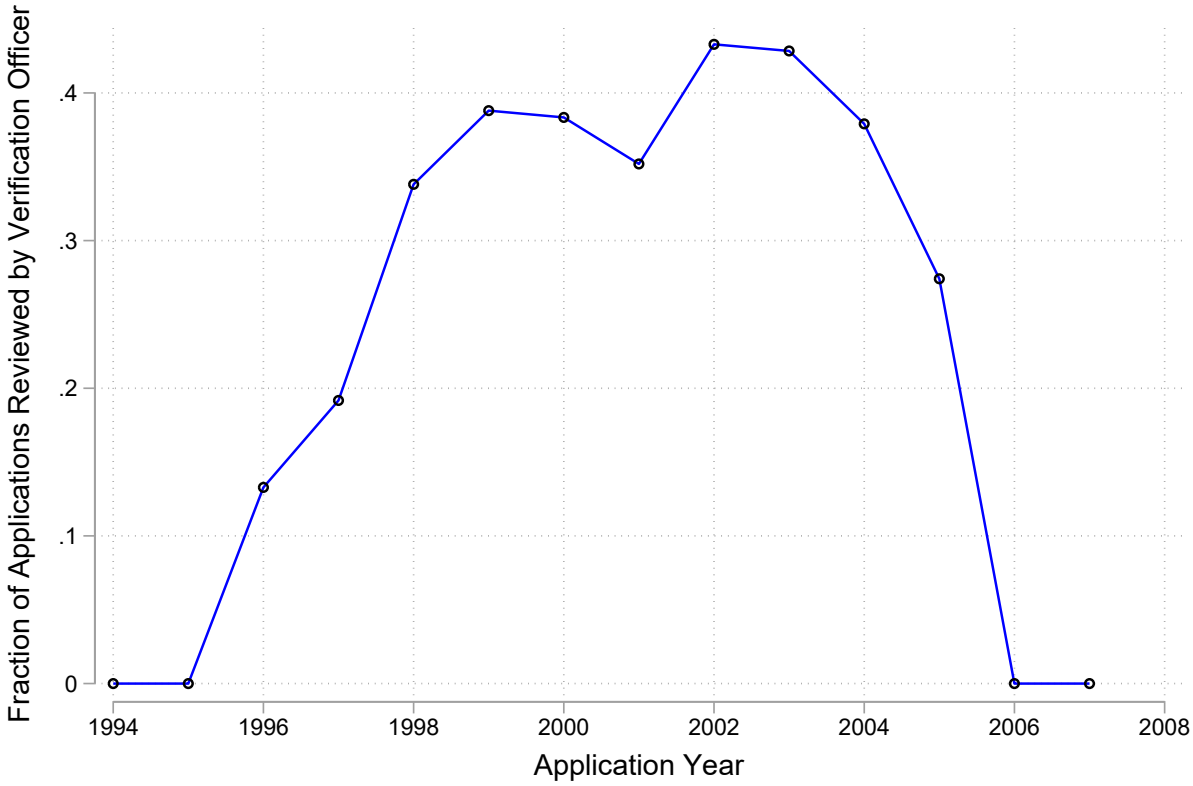
A.2 Costing of Hospital-Based Services

Each hospital visit is assigned a Resource Intensity Weight (RIW) based on the patient’s case mix. The RIW is then multiplied by a “Cost per Weighted Case", or CPWC, to derive that visit’s dollar value cost. The sum of RIW×CPWC within the provinces equates exactly to total hospital expenditure in the province, although for any given visit, RIW×CPWC may over- or under-estimate the true cost.

B Supplemental Results

B.1 Descriptive Statistics

Figure B.1: **Fraction of Applications Reviewed under the Early Detection Program**



Note: This figure plots the fraction of applications that were reviewed by a Verification Officer (adjudicators) under the Early Detection Program (EDP) starting in 1996.

Table B.1: **Descriptive Statistics: All Reviewed Applications and Subset in the Analysis Sample**

	All Reviewed Applications				Analysis Sample of Reviewed Applications			
	All	$D = 1$	$D = 0$	ρ	All	$D = 1$	$D = 0$	ρ
Approved	0.76	1	0		0.76	1	0	
Parent	0.48	0.49	0.48	0.08	0.47	0.47	0.47	0.19
Female	0.44	0.44	0.43	0	0.43	0.43	0.43	0.11
Age	34.96	34.91	35.12	0	34.69	34.57	35.07	0
No IA History	0.23	0.2	0.31	0	0.24	0.21	0.31	0
Employer Paid	0.12	0.11	0.14	0	0.12	0.12	0.14	0
Any MSP or Hospital Treatment	0.72	0.71	0.74	0	0.72	0.71	0.74	0
Treated in Hospital	0.12	0.12	0.12	0.02	0.11	0.11	0.12	0.03
Outpatient (MSP) Spending	469.34	459.36	501	0	466.02	452.93	506.66	0
Hospital and Outpatient Spending	1048.3	992.6	1224.94	0	1046.1	982.62	1243.21	0
Visited GP	0.66	0.66	0.69	0	0.66	0.65	0.69	0
GP Visits Expenditure	124.98	123.86	128.52	0	122.54	120.4	129.18	0
Treated for Injury	0.26	0.26	0.27	0	0.26	0.25	0.27	0
Injury Expenditure	34.91	34.3	36.84	0.02	34.57	33.92	36.6	0.05
Treated for Colds	0.18	0.17	0.18	0	0.18	0.17	0.18	0
Colds Expenditure	9.15	9	9.64	0.07	8.94	8.73	9.57	0.07
Treated for Mental Health (MH)	0.3	0.3	0.3	0.19	0.3	0.3	0.3	0.14
MH Expenditure	86.64	83.35	97.09	0	87.12	83.31	98.93	0
Received Pharmaceuticals	0.63	0.63	0.63	0.01	0.62	0.62	0.63	0
Pharmaceutical Expenditure	200.87	193.94	222.84	0	193.57	184.32	222.3	0
Share Drug Cost Uninsured	0.66	0.65	0.71	0	0.67	0.66	0.72	0
Day Supplied	166.12	163.51	174.41	0	158.69	155.06	169.97	0
Received MH Pharmaceuticals	0.25	0.25	0.24	0	0.24	0.24	0.24	0.15
MH Pharma Expenditure	66.92	65.12	72.62	0	63.82	60.87	72.96	0
4-Year Mortality Rate	0.03	0.03	0.03	0.55	0.03	0.03	0.03	0.94
N adult applicants	118476	96102	32922	1	79878	63788	21593	1
N	156819	119224	37595	1	98953	74849	24104	1
N adjudicators	919	884	709	1	350	350	349	1
N adjudicator years	1885	1826	0.	1	666	666	665	1

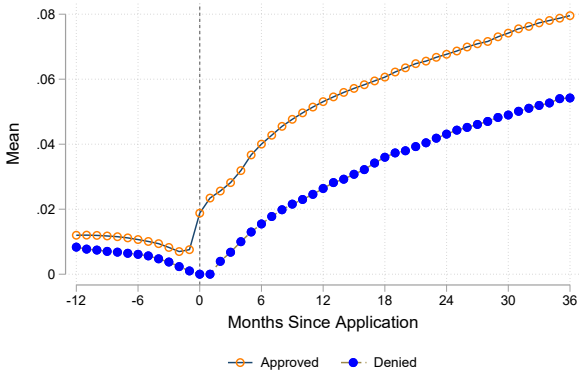
Note: This table shows average characteristics for the full set of applications reviewed under the EDP and the subset that are used in the analysis throughout the paper. $D = 1$ and $D = 0$ denote approved and denied applicants respectively, while "All" denotes both combined. ρ is the p-value testing the difference in means between approved and denied applicants. Applicant demographic characteristics are observed as of the application. Health outcomes are measured over the 12 months prior to application.

B.2 Disability Insurance and Excused from Work Search on Medical Grounds

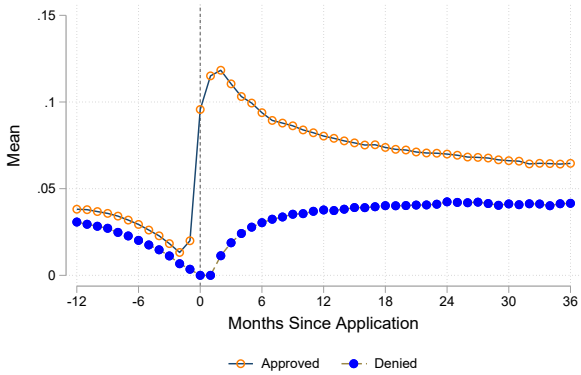
Some applicants for Income Assistance eventually transition onto either full disability insurance or categories of Income Assistance in which they are excused from searching for work due to "persistent barriers" or medical reasons. In the figure below, I plot the fraction of applicants receiving either type of benefits.

Figure B.2: **Disability Insurance and Excused from Work Benefits**

(a) **Disability Insurance Receipt**



(b) **Income Assistance Receipt, Excused from Work Search**



Note: This figure plots average outcomes around the time of application ($t = 0$) among the set of EDP-reviewed applications, separately for applicants that were approved and those that were denied. The outcome in panel (a) is an indicator for receipt of disability insurance. The outcome in panel (b) is whether the person received non-disability insurance income assistance, but in which they were exempt from searching for work due to medical reasons/persistent barriers to work.

B.3 Adjusting for Residence

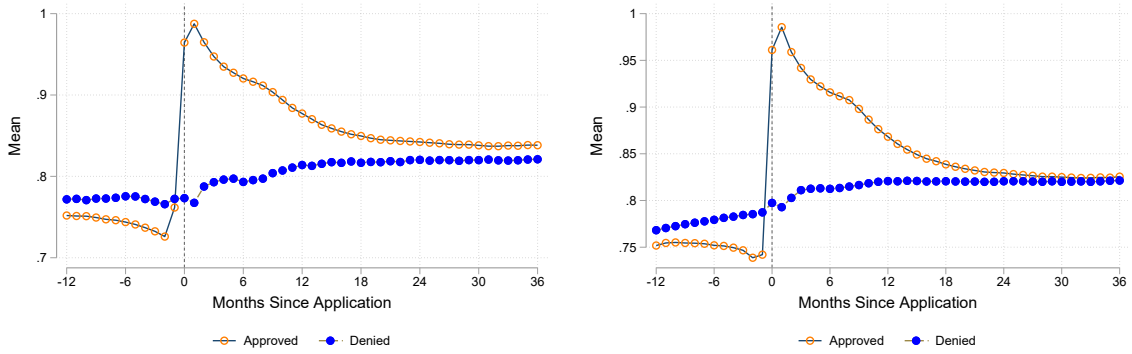
I do not observe an official indicator of residence in the province. I can, however, indirectly infer whether an individual is residing in the province as whether they appear in any of the administrative data sets that I access. The Medical Services Plan (MSP) registry is the most useful of these. All residents of the province are required by law to register for MSP and pay premiums. With full compliance, the MSP registry would therefore contain all residents in the province. The problem is that not all residents comply and pay, especially demographics on the margin of IA receipt. Additionally, IA recipients receive their MSP premiums paid for them by the IA ministry, implying that all IA recipients will appear in the registry. These two facts mean that low-income non-IA-recipients will appear to have lower resident rates than IA recipients, even if true resident rates are equivalent.

I combine these records with health care treatment records to generate an indicator for whether an applicant was observed in *any* of the data sets in a given month, my best proxy of residence. In Figure B.3, I plot the fraction of applicants observed as residents. At its lowest point, the residency rate is 75%. Unsurprisingly, the residency rate among approved applicants spikes after benefit approval, since approved applicants are recorded in IA records. As a result, the pattern of residency among approved and denied applicants strongly mimics the pattern of IA receipt in panel (a) of Figure I, indicating that inferred residency patterns are driven by the appearance in the IA data.

Figure B.4 shows the treatment effects of benefit approval on the proxy for residency, in each of the three years following application. There is a significant positive effect on inferred residency, consistent with approved applicants appearing as “residents" due to appearing in IA records. By year 2, the estimated treatment effect is approximately -2 percentage points and statistically insignificant. This suggests that benefit approval has no, or very limited, effects on the probability of remaining in the province, and thus the treatment effects estimates of approval on health outcomes are unlikely to be attenuated by selective outmigration.

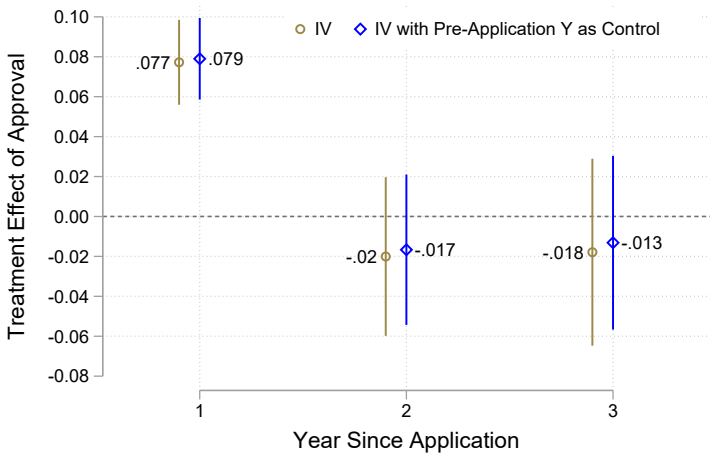
Figure B.3: **Residence Around Time of Application**

(a) **Full Application Data Set** (b) **Reviewed Applications**



Note: This figure plots the fraction of the sample that appear in any of the data sets, and thus inferred to be residing in the province, around the time of application. Panel (a) shows the full sample of applicants, panel (b) shows the sample of applicants that were reviewed by a Verification Officer.

Figure B.4: **Treatment Effects on Indicator for Being in Administrative Data (Residence)**

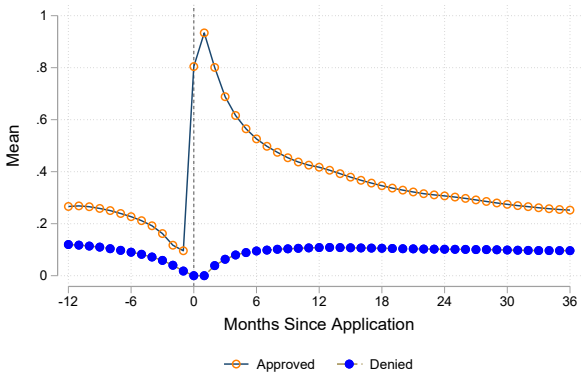


Note: This figure plots treatment effects of benefit approval on an indicator for whether the applicant appears in any of the administrative files, which serves as a proxy for residence. The estimators are described in section 4. 90% confidence intervals are shown. Standard errors are clustered as the adjudicator-year level.

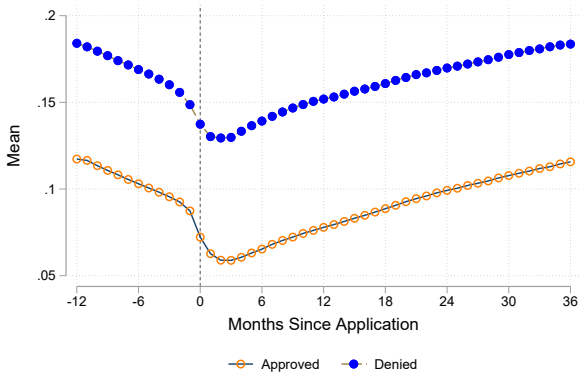
B.4 Trends for All Applications Rather than VO-Reviewed Applications

Figure B.5: **Income Assistance and Employment Around Application for IA, Full Sample**

(a) **IA Use**

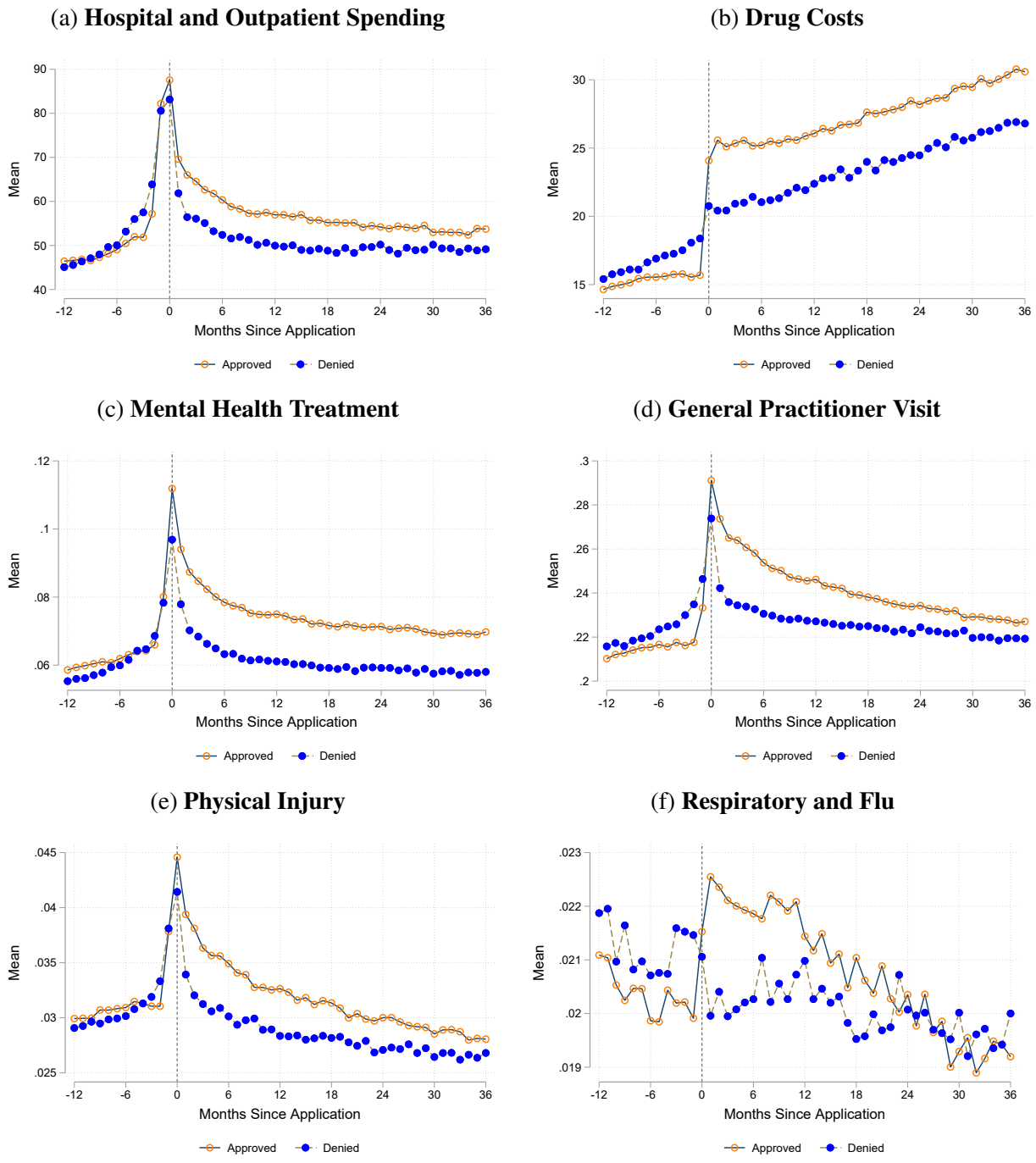


(b) **Employer Paid Premiums**



Note: This figure plots average outcomes around the time of application ($t = 0$) among the full set of applications, as opposed to only the EDP-reviewed applications used in the main text, separately for applicants that were approved and those that were denied. The outcome in panel (a) is welfare receipt. The outcome in panel (b) is whether the person had their health insurance premiums paid by an employer.

Figure B.6: **Health Outcomes Around Time of Application, Full Sample**



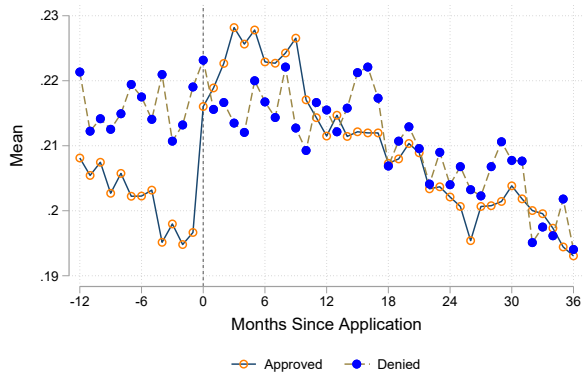
Note: This figure plots average outcomes around the time of application ($t = 0$) among the full set of applications, as opposed to only the EDP-reviewed applications used in the main text, separately for applicants that were approved and those that were denied. The outcomes in panels (a) and (b) are in 2002 CAD. The outcomes in the remaining panels are binary: panel (c) is receiving mental health treatment from a physician; panel (d) is visiting a general practitioner; panel (e) is being treated for a physical injury; panel (f) is being treated for a flu or respiratory illness.

B.5 Trends for Children

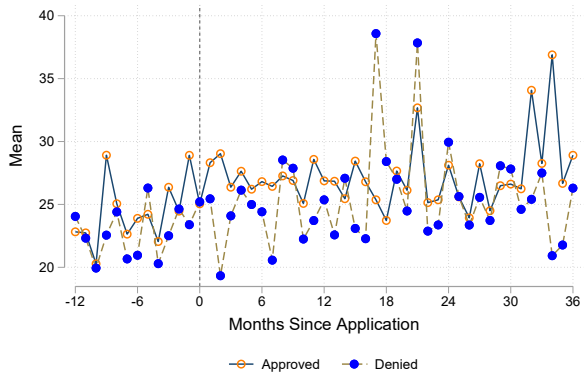
In this section I plot trends in outcomes around the time of application for children aged 2 to 17 years at the time that their parent(s) applied for Income Assistance. A child is linked to their parents if the child is listed on the application. Unlike their parents, there is no pre-application spike in health spending. However, among children whose parents were approved for Income Assistance, pharmaceutical spending and GP visits increase, consistent with the pharmaceutical subsidy for Income Assistance driving increased consumption.

Figure B.7: **Universally-Insured Health Spending Around Time of Application, Children**

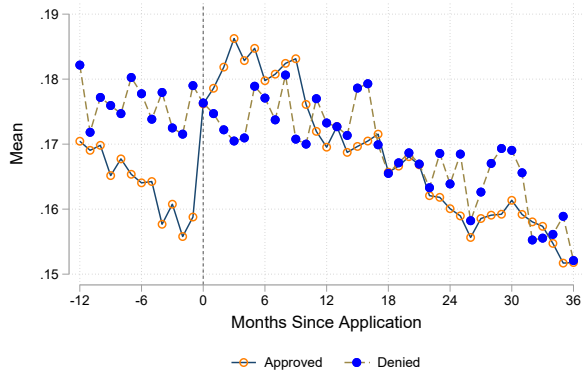
(a) **Probability of Receiving Medical Care**



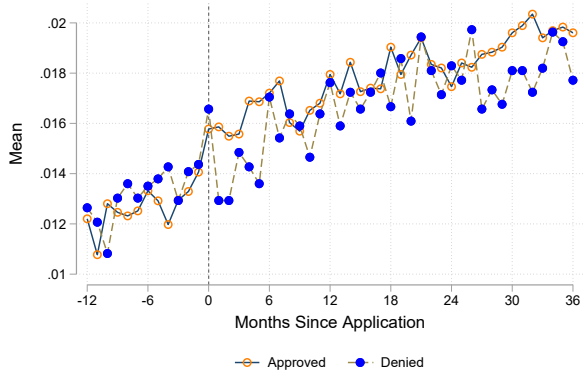
(b) **Hospital and Outpatient Spending**



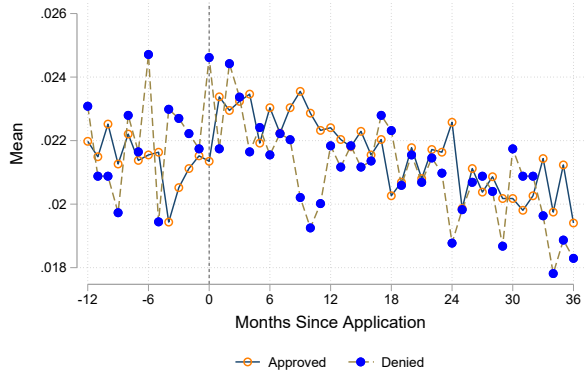
(c) **General Practitioner (GP) Visit**



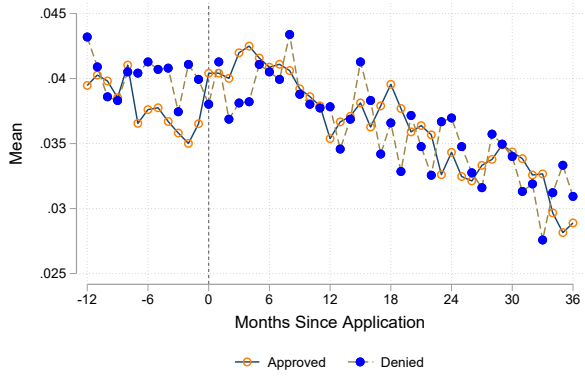
(d) **Mental Health Treatment**



(e) **Physical Injury**



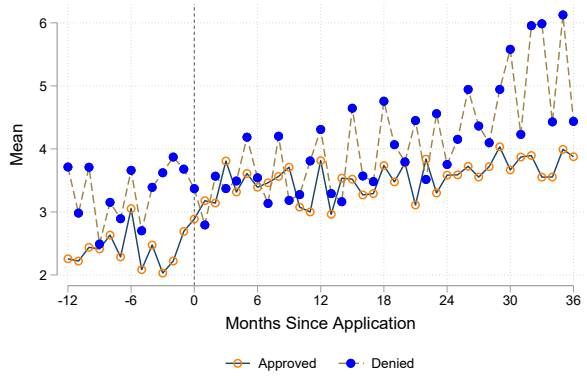
(f) **Respiratory and Flu**



Note: This figure plots average outcomes around the time of application ($t = 0$) among the set of EDP-reviewed applications, separately for applicants that were approved and those that were denied. The sample is children link to their parents application. The outcomes in panels (a) and (b) are in 2002 CAD. The outcomes in the remaining panels are binary: panel (c) is receiving mental health treatment from a physician; panel (d) is visiting a general practitioner; panel (e) is being treated for a physical injury; panel (f) is being treated for flu or respiratory illness.

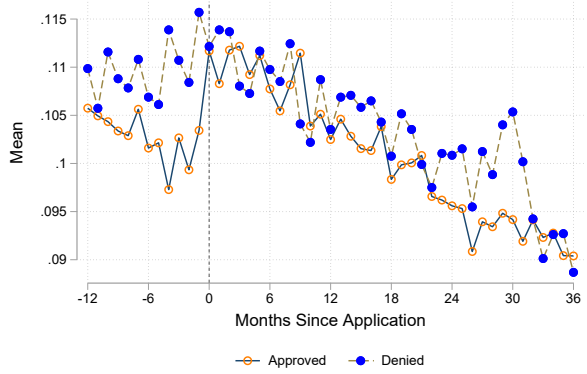
Figure B.8: **Pharmaceutical Spending Around Time of Application, Children**

(a) **Drug Costs**



(b) **Visited GP and Received Pharmaceutical**

(c) **Visited GP, Did Not Receive Pharmaceutical**



Note: This figure plots average outcomes around the time of application ($t = 0$) among the set of EDP-reviewed applications, separately for applicants that were approved and those that were denied. The sample is children link to their parents application. The outcomes, in order, are: average drug spending, whether the child visited a GP in the same month they filled a pharmaceutical prescription, and whether the child visited a GP without filling a prescription in that month.

B.6 Additional Results for the Causal Effects using VO Assignment

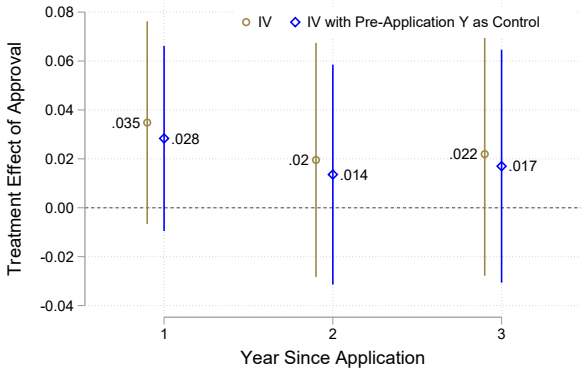
Table B.2: **Characterizing Compliers' Pre-Application Characteristics**

	Full Sample Mean	Complier Mean	Ratio	Z Value
Baseline Characteristics				
Age	34.69	33.53	0.97	1.01
Female	0.43	0.36	0.83	1.79
Childless Adult	0.53	0.65	1.22	2.41
Inferred Resident Before Application	0.89	0.90	1.01	0.38
No IA History	0.24	0.16	0.68	2.35
Spending Increase Before Application				
Received Any Medical Care 10-12 Months Before Application	0.41	0.33	0.80	1.92
Received Any Medical Care 1-2 Months Before Application	0.45	0.39	0.87	1.32
Medical Spending 10-12 Months Before Application	91.50	102.41	1.12	0.54
Medical Spending 1-2 Months Before Application	183.22	296.66	1.62	1.28
Cumulative Spending in 12 Months Before Application				
Total Non-Pharmaceutical Medical Spending	824.45	883.09	1.07	0.27
Mental Health Spending	74.71	78.09	1.05	0.16
Respiratory Illness Spending	7.72	5.31	0.69	1.39
Injury Spending	27.57	33.58	1.22	0.93
Spending on GP Visits	119.21	106.68	0.89	0.98
Pharmaceutical Spending	169.49	142.22	0.84	0.70
Pharmaceutical Spending Mental Health	53.68	43.90	0.82	0.59
Had Employer that Paid Insurance Premiums	0.12	0.08	0.67	1.50

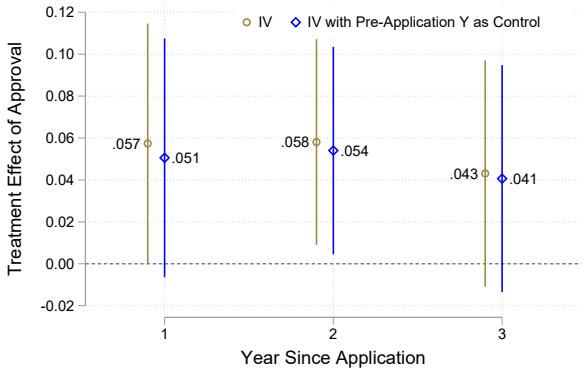
Note: This table shows the average characteristics of the full sample in column (1), of compliers in column (2), their ratio in column (3), and the Z-statistic from the two-sided test of whether their ratio deviates from 1 in column (4). Spending variables are winsorized at the 99th percentile. Standard errors clustered at the adjudicator-year level.

Figure B.9: **Dynamic Treatment Effects on Disability Insurance and Excused from Work Benefits**

(a) **Disability Insurance Receipt**



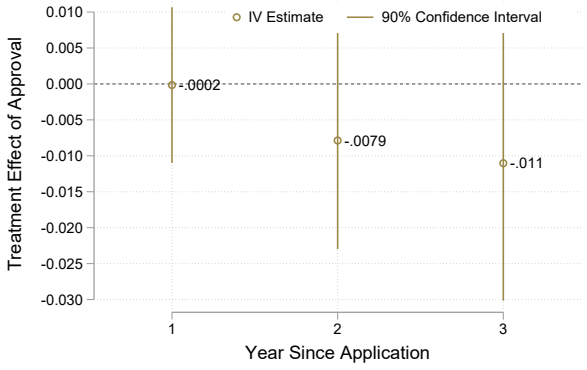
(b) **Income Assistance Receipt, Excused from Work Search**



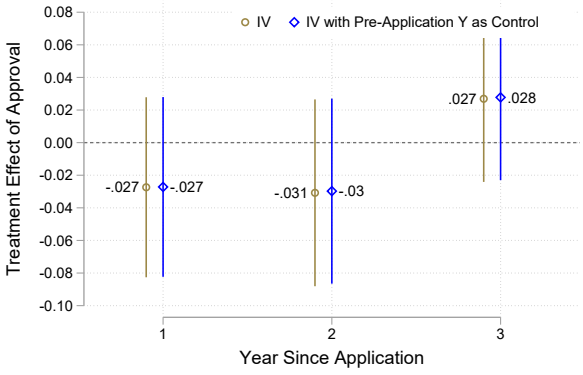
Note: TThese figures plot the estimates of the effect of application approval on the likelihood of receive disability insurance (panel(a)) and income assistance benefits in which the person is excused from work search requirements (panel(b)). 90% confidence intervals are shown. See main text for details.

Figure B.10: **Dynamic Treatment Effects on Mortality and Fertility**

(a) **Cumulative Death Probability**



(b) **Had Birth in Year**



Note: These figures plot the estimates of the effect of application approval on cumaltive mortality and an indicator for giving birth in the year. The sample for the latter is restricted to women. 90% confidence intervals are shown. See main text for details.

B.7 Additional Results from the 1996 Pilot

Table B.3: **Application Descriptive Statistics, 1996 Pilot Experiment**

	Control Offices	Treated Offices	ρ	Other Non-Treated Offices
Approved	0.72	0.61	0	0.72
Parent	0.44	0.41	0.52	0.5
N children	0.57	0.49	0.45	0.73
Female	0.39	0.37	0.17	0.41
Age 19 to 24	0.26	0.27	0.7	0.26
Age 25 to 35	0.37	0.37	0.85	0.36
Age over 34	0.36	0.35	0.62	0.37
First Time	0.34	0.35	0.65	0.33
Resident	0.86	0.85	0.29	0.87
On IA	0.46	0.45	0.73	0.46
Employer Paid Months	0.2	0.2	0.88	0.21
Treated in Hospital	0.09	0.09	0.58	0.1
Total Costs	414.91	403.14	0.63	412.07
Hospital Costs	24.2	25.95	0.67	23.18
Outpatient (MSP) Spending	329.87	318.08	0.52	333.29
Visited GP	0.59	0.58	0.53	0.6
GP Visits Expenditure	81.3	81.28	1	81.33
Treated for Injury	0.22	0.21	0.64	0.23
Injury Expenditure	18.69	18.52	0.94	20.8
Treated for Colds	0.17	0.17	0.93	0.17
Colds Expenditure	7.42	7.43	0.98	7.65
Treated for Mental Health (MH)	0.21	0.2	0.27	0.2
Mental Health Expenditure	40.25	38.57	0.62	34.54
Pharmaceutical Prescription	0.25	0.27	0.35	0.25
Total Pharmaceutical Expense	39.71	38.27	0.76	37.02
Pharmaceutical Copay	0.63	0.67	0.29	0.64
Days Supplied	14.54	17.58	0.24	15.5
Pharmaceutical Prescription MH	0.08	0.08	0.86	0.07
Total Pharmaceutical Expense MH	10.07	9.45	0.68	8.7
Number of Applicants	9469	7273		63256
N offices	15	21		157

Note: This table shows average characteristics for the set of applications used in the 1996 pilot analysis, as described in the main text. ρ is the p-value testing the difference in means between applications at treated and control offices. Applicant demographic characteristics are observed at the time of the application. Health outcomes are measured over the 12 months prior to application.

C Details of Estimators

I follow the notes and notation of Kolesar (2013) in outlining the unbiased jacknife instrumental variable (UJIVE) estimator, and its comparison to 2SLS and the jacknife instrumental variable (JIVE). The model is:

$$Y_i = T_i\beta + W_i'\gamma + \epsilon_i \tag{C.1}$$

Where Y_i is the outcome, T_i is the endogenous variable, and W_i is a vector of controls. In my case, T_i an indicator for person i's application being approved, and W_i contains office-by-year indicators.

Denote Z_i as a vector of instruments. In my case, Z_i is a $kx1$ vector of dummy variables equal to 1 if the application was reviewed by a given adjudicator, and k is the number of adjudicators. The 2SLS, JIVE, and UJIVE estimators can all be expressed in the same form:

$$\hat{\beta} = \frac{\hat{P}'Y}{\hat{P}'T} \tag{C.2}$$

where the difference lies in $\hat{P}'$:

$$\begin{aligned}
\hat{P}_{2SLS} &= H_{Z\perp}T \\
\hat{P}_{JIVE} &= M_W(I_n - D_{Z,W})^{-1}(H_{Z,W} - D_{Z,W})T \\
\hat{P}_{UJIVE} &= \Big[(I_n - D_{Z,W})^{-1}(H_{Z,W} - D_{Z,W}) - (I_n - D_W)^{-1}(H_W - D_W)\Big]T
\end{aligned}$$

The matrix H_Z is the projection matrix for Z, $Z(Z'Z)^{-1}Z'$, and $H_{Z\perp} = M_W H_Z$ where M_W is the annihilator matrix $I_n - H_W$. The matrix $H_{Z,W}$ is the projection matrix for the reduced form. Finally, $D_{Z,W}$ and D_W are diagonal matrices where the (i,i)th elements are the (i,i)th elements of $H_{Z,W}$ and H_Z, respectively.

β_{UJIVE} reduces bias relative to β_{JIVE} when the dimension of W is similar to the dimension of Z.

$\hat{P}$ in the three estimators are just different ways of projecting T onto the set of instruments. The 2SLS projects T onto adjudicator dummies after partialing out W. The JIVE does this on a leave-out-i basis. The UJIVE (i) projects T onto Z and W, (ii) projects T onto just W, then (iii) subtracts (ii) from (i), all on a leave-out-i basis.

My implementation deviates in one respect — rather than using T (the application approval outcome) in $\hat{P}$, I use the adjudicators' recommendation (R_i). See the main text for justification.