UCD CENTRE FOR ECONOMIC RESEARCH

WORKING PAPER SERIES

2019

Correcting for Transitory Effects in RCTs: Application to the RAND Health Insurance Experiment

Kevin Devereux, University College Dublin
Mona Balesh Abadi, University of Toronto
Farah Omran, DC Howe Institute

WP19/10

April 2019

UCD SCHOOL OF ECONOMICS
UNIVERSITY COLLEGE DUBLIN
BELFIELD DUBLIN 4

Correcting for Transitory Effects in RCTs: Application to the RAND Health Insurance Experiment*

Mona Balesh Abadi† Kevin Devereux‡ Farah Omran§

April 7, 2019

Abstract

The long run price elasticity of healthcare spending is critically important to estimating the cost of provision. However, temporary randomized controlled trials may be confounded by transitory effects. This paper shows evidence of a 'deadline effect' – a spike in spending in the final year of the program – among participants of the RAND Health Insurance Experiment, long considered the definitive RCT in the field. The deadline effect is economically and statistically significant, with power to identify coming from random allocation to three- or five-year enrollment terms. The deadline effect interacts with the price elasticity: participants who face lower coinsurance rates show larger spending spikes. Crucially, controlling for the price-deadline interaction yields significantly smaller estimates of the price elasticity in non-deadline years, which we argue is a better approximation for the long run elasticity. This has important implications for public finance and the design of private/temporary subsidy programs.

JEL codes: C93, D91, H31, H42, H51, I12, I13

Keywords: Health insurance, moral hazard, public health, RCTs.

*Thanks to Christian Dippel, Grant Gibson, Boriana Miloucheva, Paul Devereux, Michelle Colbert, and the seminar participants at University College Dublin, the Canadian Public Economics Group, and the Irish Behavioural Science and Policy Network. Remaining errors are ours.

†Ontario Treasury Board Secretariat, 8th Floor, Ferguson Block 77 Wellesley Street West Toronto, Ontario M7A 1N3, Canada.

‡University College Dublin, Deparment of Economics, John Henry Newman Building, Stillorgan Rd, Belfield, Dublin 4, Ireland. Tel: (353) 873959789. Email: kevin.devereux@ucd.ie.

§C.D. Howe Institute, 67 Yonge St #300, Toronto, Ontario M5E 1J8, Canada.

1 Introduction

The price elasticity of demand for healthcare crucially informs the costing of both public provision systems and subsidized private insurance. To determine the causal effect of prices on spending, the US government commissioned the RAND Health Insurance Experiment (HIE) during the 1970s-1980s. This was a large scale randomized controlled trial that is seen as the gold standard of evidence in the literature and whose findings have been largely confirmed in the subsequent non-experimental literature.[1] However, the experiment was temporary. As the original designers were aware, temporary provision programs may lead to transitorily higher demand, and naive interpretation of the treatment effect as the long run price elasticity may lead to overestimation (Arrow 1975).

This paper documents a phenomenon that has been largely overlooked in the HIE data: a spike in spending in the final year of the experiment, which we call the *deadline effect*. Since the deadline year is randomly assigned at the beginning of the experiment, we interpret the deadline effect as causal. We reason that with no deadline, there would be no accompanying spike in spending, and that the steady state level of spending is lower than a pooled average suggests.

Moreover, the deadline effect varies by price, with lower price (lower coinsurance rate) plans showing greater deadline effects. The *price-deadline interaction* biases price elasticity estimates away from zero in a sample pooled across all contract years. We find that controlling for the price-deadline interaction significantly reduces the magnitude of elasticity estimates for outpatient, drug, and supplies spending.[2]

We identify economically large and statistically significant deadline and price-deadline interaction effects by exploiting two sources of variation. First is variation in the start year. This allows us to control for calendar year, ruling out increasing healthcare prices as the cause of the deadline effect. More importantly, we exploit variation in enrollment term. At the beginning of the experiment, participants were randomly assigned to three- or five-year terms, in addition to being randomly assigned to a coinsurance plan. This was done by design, giving the experiment power to identify transitory effects (Arrow 1975). Effectively the deadline year is randomized. This provides power to separately identify the deadline effect from increasing trends of spending during program years.[3]

[1] See Aron-Dine et al. (2013), who deem the HIE results to be robust to modern statistical techniques.

[2] We also find evidence of a spending spike in the initial year for some subcategories of health spending. Spending spikes in the initial year of the program are larger for lower coinsurance rate groups as well, but controlling for these does not significantly affect elasticity estimates.

[3] If participants become more comfortable filing returns over time, they may increase spending towards

We conservatively estimate the long run price elasticity as that exhibited in non-deadline years. This significantly reduces the magnitude of price elasticity estimates for certain spending categories. Our prices elasticity estimates of drug spending are significantly smaller in magnitude across all specifications, in the order of two percentage points. Elasticities of outpatient, and medical supplies are lower in magnitude – also by around two percentage points – for specifications that treat the coinsurance rate as a continuous variable.

Figure 1 plots unconditional average total spending by contract year (the number of years for which a participant has been enrolled) for the three- and five-year cohorts of the HIE. Spending spikes in the final years of the experiment. Note in particular that spending in the third year of the three-year cohort – their final year of coverage – is higher than in the third year of the five-year cohort. This finding is robust to controlling for a host of observables, and excluding outliers from the sample. In the lower panel we plot average spending separately for the participants receiving free care from those in all other plans (who face positive coinsurance rates). In addition to the level effect, transitory effects appear more pronounced in the free care group.

The deadline effect has been somewhat overlooked in previous analysis of the HIE. Notably, Manning et al. (1987) states that there are no transitory effects in medical spending, citing the accompanying RAND technical report (Manning et al. 1988). This technical report compared unconditional averages of aggregated spending categories for initial years, deadline years, and all other years. We find that it is necessary to disaggregate by spending category in order to uncover significant transitory effects. This allows precise measurement of the coinsurance rate faced for coverage plans that provide different coinsurance rates for different categories of healthcare spending (see table 2).[4]

We acknowledge to the above the notable exception of Manning et al. (1985), who observe a spike in dental spending in the final year of the HIE for low coinsurance rate plans.[5] Lohr et al. (1986) drop the first and last year observations to remove transitory effects, but do not document a deadline effect. We contribute to the literature by documenting a price-deadline interaction for other spending categories, and demonstrating quantitatively the sensitivity of long run price elasticity estimates transitory effects.

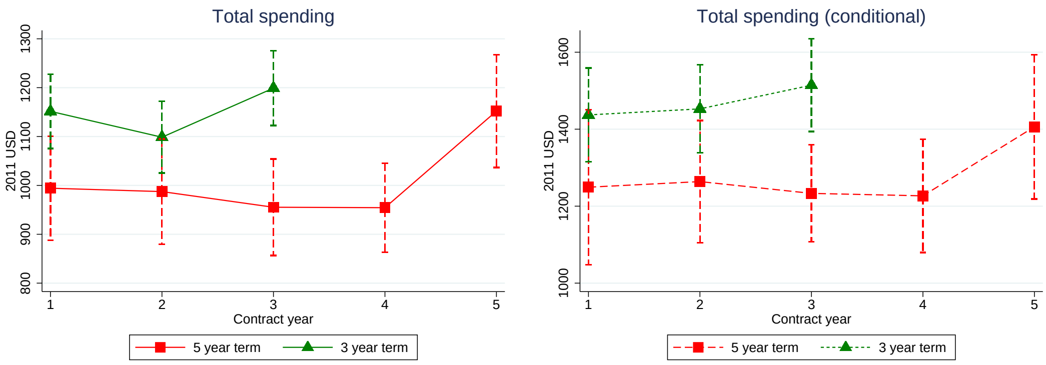
---

the end of the program. Enrollment term variation allows us to separately identify the deadline effect.

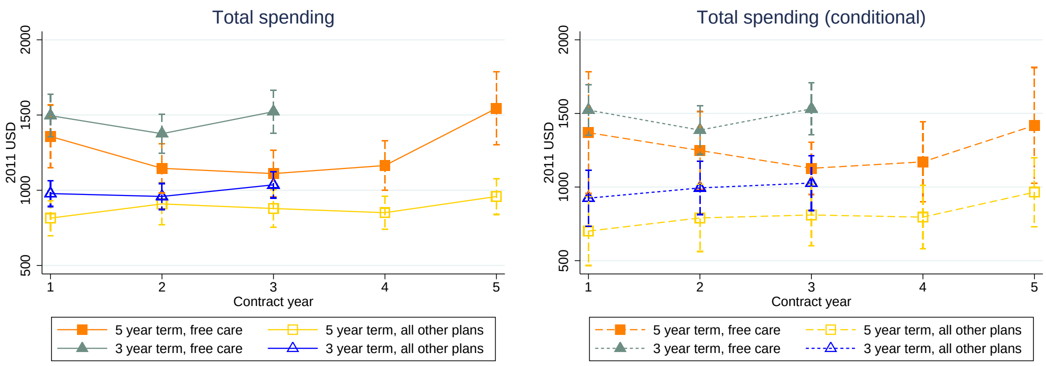
[4]The deadline effect is robust to an extensive set of controls, including sex-specific age fixed effects. In section 6 we show that the deadline effect is not an artifact of any other dynamic pattern.

[5]Manning et al. (1985) also finds a spike in dental spending in the initial year of the program. We replicate this finding in section 6, and find a significant initial year effect also for supplies spending.

Figure 1: Average spending by contract year (2011 USD)



Average total spending by contract length and year, with 95% confidence intervals. The left hand side graph plots unconditional averages, while the right plots fixed effects recovered from model (7), partial to the complete set of controls. Both exclude outliers.



Average spending plotted separately for participants in the free care plan, and that for participants in all other plans. The left plots unconditional averages, while the right uses the full set of controls decribed in section 3. Confidence bands indicate the 95% confidence interval.

Our revised estimates are significantly smaller in magnitude, implying a lower cost of healthcare provision. A back-of-the-envelope calculation says that our estimated two percentage point smaller-in-magnitude elasticity estimate predicts a cost of provision lower by just over two billion in 2011 USD for a country of 300 million, for outpatient care alone (using as a baseline average outpatient spending for the free care plan, which is $342.5 in 2011 USD). The cost of drug provision would be smaller by $576 million.

In addition to the revised long run elasticity estimates, the deadline effect is of critical importance to estimating the cost of provision for temporary coinsurance plans. Participants facing the imminent termination of subsidized healthcare demonstrate a deadline×price elasticity that significantly raises spending in the final year, to a greater extent for higher coinsurance rate plans. Our constant elasticity of substitution specification estimates a price elasticity greater by 7% in the deadline year for outpatient spending, and by around 5% for drugs and supplies spending.

Our results complement a recent literature on intertemporal substitution in healthcare spending. Lin and Sacks (2016) finds that participants in the HIE respond dynamically to the nonlinear price schedule, spending more in the final months of an enrollment year once that maximum out-of-pocket deductible has already been hit. Chandra et al. (2010), Aron-Dine et al. (2015), and Einav et al. (2015) report similar findings for Medicare users. Cabral (2016) shows that participants in a private insurance program strategically delay dental care. We complement this literature by quantifying the importance of netting out intertemporal susbtitution behaviour when estimating the long run price elasticity of healthcare spending.

Beyond the current application, we contribute a simple and transparent method, rooted in economic theory,[6] to adjust the treatment effects estimated by RCTs to bear closer resemblance to the underlying parameters of interest. This is of crucial importance to mapping the results of temporary experiments into long term policies in any context.[7]

The following section describes the HIE data. Section 3 presents the empirical specification and sections 4 and 5 the main results – documenting the deadline effect, the price-deadline interaction, and comparing baseline estimates of the treatment effect to those controlling for the deadline effect. Section 6 demonstrates robustness of our findings to a variety of specifications and sampling decisions. Section 7 concludes.

[6] Arrow (1975) shows that a deadline effect can result from a model of healthcare as a durable good. In appendix C, we present a salience cost model with a qualitatively similar effect.

[7] Metcalf (1973) makes this point in the context of a negative income tax.

2 Data & Background

The RAND Health Insurance Experiment (HIE) was carried out in six cities around the United States from 1974 to 1981. Those eligible for Medicare (older than age 62), families with income over $25000 (In 1973 USD), and institutionalized or military or former military personnel were excluded. Individuals meeting the eligibility criteria were allocated to coinsurance plans by stratified random sampling.[8]

At the beginning of the experiment each family was randomly allocated to an enrollment term and a coinsurance plan. Plans varied somewhat in how different categories of medical care were covered. The disaggregated categories consist of inpatient, outpatient, drug, supplementary, dental, and mental health spending.

The structure of the coinsurance plans was as follows. The coinsurance rate – the percentage of healthcare spending that must be paid by the household – varied from 0% to 95% (and up to 100% in the first year, for certain cities). This rate applied up until the individual or household reached the deductible (the maximum out-of-pocket amount), above which the plan paid 100%. The deductible was calculated as the minimum of 5, 10, or 15% of the family income prior to the experiment, or $1000 (or $750 in some cases). We follow Aron-Dine et al. (2013) in grouping together plans by coinsurance rate, so that our estimates capture the average effect. Table 2 provides details on coinsurance rates for different categories of spending by plan. Our main results focus on disaggregated care categories; for these results we measure price as the coinsurance rate faced before the deductible. For participants who exceed the deductible, this underestimates the effective coinsurance rate, which biases our elasticity estimates towards zero. To analyze total spending, we use fixed effects for each of the six plans in table 2, measuring price as the category-weighted average pre-deductible coinsurance rate (see appendix D for details).

We limit the sample to non-attriters, and consider specifications including controls for pre-randomization covariates. In addition to attriters, we exclude infants born during the experiment, who enter the experiment as part of the family unit. This ensures a balanced panel.[9] Additionally, we are concerned about sensitivity to outliers. Our main results exclude all person-year observations for which total spending in any year is greater than three standard deviations above the mean. This does not change our estimates substantially.

[8] Aron-Dine et al. (2013) show that this closely approximated pure random allocation.

[9] Aron-Dine et al. (2013) provide a series of formal bias corrections to address attrition and issues, and find that the quantitative results do not vary much when comparing the free care plan to plans with positive coinsurance rates. For a thorough discussion of the issues we refer the reader to their paper.

Table 1: Summary Statistics

	Mean	Median	Stdev	Min	Max	N
Inpatient	297.71	0	1222.09	0	12885.11	17825
Outpatient	276.87	119.53	455.96	0	7397.97	17825
Drugs	69.22	9.25	167.09	0	3492.3	17825
Supplies	38.54	0	112.23	0	6067.49	17825
Dental	370.42	63.84	1150.35	0	26676.54	17825
Mental health	39.13	0	340.44	0	8139.31	17825
Male	.48	0	.5	0	1	17825
Age	24.58	23	16.16	0	62	17825
Income	11648.75	10843.09	6510.56	-107.17	49848.2	17825

Table 2: Coinsurance rates by disaggregated care category and plan

	Disaggregated care category					
	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental h.
Free care	0	0	0	0	0	0
25% plan	.25	.25	.25	.25	.25	.25
Mixed coins.	.25	.25	.25	.25	.5	.5
50% plan	.5	.5	.5	.5	.5	.5
Indiv. deductible	0	.95	.95	.95	.95	.95
95% plan	.95	.95	.95	.95	.95	.95

This table reports the coinsurance rate paid by the participant for a given category of care, for a given plan, before reaching the deductible. After reaching the deductible – the maximum out-of-pocket amount – care becomes free.

3 Empirical specification

This section introduces our empirical specification. We introduce a set of plan-deadline year fixed effects to the empirical specification of Aron-Dine et al. (2013).

As a baseline, first consider their specification:

$$y_{it} = \lambda_p + \tau_t + \alpha_{lm} + \varepsilon_{it} \tag{1}$$

which estimates a set of plan p effects given by λ on spending outcomes y for individual i in calendar year t. Calendar year controls τ partial out any price trend. Plan assignment is random conditional on the location l and start month m, and so should be interpreted as causing spending outcomes.

Now define by D_i the deadline calendar year of the experiment for individual i. We introduce a dummy variable to capture the spending effect in the deadline year, and additional controls, in the following equation:

$$y_{it} = \lambda_p + \delta \times \mathbb{1}(t = D_i) + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{2}$$

where the parameter δ captures the conditional average spending in deadline years compared to non-deadline years. We call the estimated parameter $\hat{\delta}$ the flat deadline effect. We also introduce demographic controls. These include age, gender, and income pre-experiment. Age in particular may affect the dynamics of spending, as health care requirements increase over the course of the life cycle; we include a set of age-gender interaction effects to control for life cycle trends. Income is given as the log of the average of family income in the two years before commencement of the experiment, in constant 2011 USD. We also include a dummy variable for enrollment term.[10]

Our preferred specification allows the deadline effect to vary by plan. Consider the following model which includes a set of plan-deadline interaction terms:

$$y_{it} = \lambda_p + \delta_p \times \mathbb{1}(t = D_i) + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{3}$$

where y_{it} denotes a spending outcome for individual i in calendar year t. The set of fixed effects λ_p gives conditional average spending by plan p in non-deadline years, and δ_p gives the additional effect of plan p in the deadline year. We cluster all standard errors at the family (treatment) level.

[10] The dummy for enrollment term e is given by $\mathbb{1}(e = 5)$, where $e \in \{3, 5\}$. Enrollment term was randomly assigned, and may be of additional interest. A robust and systematic difference between the three- and five-year enrollment groups would provide further evidence of transitory effects; however, we find little evidence of such an effect.

4 Results

This section presents evidence of a spike in spending in the final year of the RAND HIE, which we call the deadline effect. We find that participants randomly assigned to coinsurance plans with lower prices exhibit larger spikes in spending in the deadline year. We call this the plan-deadline interaction effect. Since the timing of the deadline year is randomly assigned at the beginning of the program – being determined by random assignment to three- or five-year enrollment terms – we interpret the deadline spike as causally determined by proximity to the deadline.

We estimate two sets of plan effects: those in deadline years and those in non-deadline years. We find the price elasticity of demand to be greater in magnitude in deadline years. Because of this, controlling for price-deadline interaction fixed effects leads to elasticity estimates lower in magnitude than the restricted model without price-deadline interaction effects. We argue that the randomly assigned deadline year should not be included if the goal is to estimate a long run elasticity relevant to a permanent public provision policy.

4.1 Deadline effects by plan

Table 3 presents results for models (1) to (6) with various sets of controls. The first panel compares the restricted models (1) and (2) to the unrestricted model (6) for the set of controls used by Aron-Dine et al. (2013), while the second panel does the same for our full set of controls. The first column of each panel reports plan fixed effects for a baseline model without a deadline effect. These results are comparable to those from past studies. The second column of each introduces a flat deadline effect, averaged across plans. This is positive and significant; note that plan fixed effects are extremely stable across models (1) and (2). The third column of either panel gives results for the unrestricted model (6) which controls for plan-deadline interaction effects. Note that the magnitude of estimated plan effects falls once these controls are introduced.

4.2 Deadline effects by price

Table 4 shows results for the unrestriced model (6) for base spending category outcomes (total spending being the sum of the six disaggregated categories). Within each disaggregated category there is a single price (coinsurance rate). For this reason, we group together participants who face the same coinsurance rate for a given care category so

Table 3: Introducing Deadline and Plan-Deadline Fixed Effects

	Total spending (2011 USD)					
	(1)	(2)	(6)	(1)	(2)	(6)
	b/se	b/se	b/se	b/se	b/se	b/se
Constant (free care)	1381.6***	1329.2***	1309.3***	1390.0***	1358.9***	1339.3***
	(37.9)	(39.6)	(40.9)	(35.1)	(37.6)	(39.5)
25% plan	-372.6***	-372.9***	-356.5***	-379.2***	-379.2***	-365.3***
	(68.5)	(68.4)	(75.6)	(65.6)	(65.6)	(73.6)
Mixed coins.	-426.1***	-424.0***	-353.4***	-395.9***	-395.8***	-322.0***
	(79.6)	(79.3)	(88.3)	(73.6)	(73.6)	(84.1)
50% plan	-474.3***	-473.4***	-456.0***	-476.6***	-476.7***	-459.3***
	(86.1)	(85.7)	(88.2)	(83.7)	(83.7)	(86.0)
Indiv. deductible	-341.6***	-343.4***	-321.5***	-385.8***	-385.8***	-363.0***
	(56.7)	(56.6)	(60.2)	(53.9)	(53.9)	(58.0)
95% plan	-570.0***	-570.9***	-534.3***	-574.3***	-574.3***	-538.6***
	(59.3)	(59.2)	(62.3)	(56.3)	(56.3)	(60.3)
T.year		189.4***	260.5***		111.6**	181.9**
		(50.7)	(73.9)		(51.5)	(75.5)
T.year×25% plan			-58.8			-49.2
			(112.5)			(114.7)
T.year× Mixed c.			-251.5**			-262.9**
			(118.0)			(117.3)
T.year×50% plan			-62.7			-62.9
			(121.1)			(122.6)
T.year× Indiv. d.			-77.8			-81.0
			(93.4)			(93.1)
T.year×95% plan			-131.0			-127.8
			(92.2)			(91.8)
Enrol. term = 5				-140.8***	-96.8*	-97.6*
				(53.1)	(57.0)	(57.0)
Site × enrol.	Yes	Yes	Yes	Yes	Yes	Yes
Cal. years	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	No	No	Yes	Yes	Yes
N	17825	17825	17825	17825	17825	17825
R^2	0.02	0.02	0.02	0.09	0.09	0.09

The constant indicates average spending in the free care plan during non-deadline years. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 4: Price-Deadline Fixed Effects by Spending Category

	Spending by care category (2011 USD)					
	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental H.
	b/se	b/se	b/se	b/se	b/se	b/se
Constant (free care)	327.1***	333.9***	92.1***	45.4***	477.6***	42.9***
	(18.3)	(10.1)	(4.63)	(2.47)	(23.2)	(7.96)
25% coinsurance	-38.4	-70.2***	-32.6***	-10.8***	-160.2***	-7.82
	(31.3)	(15.7)	(6.63)	(2.96)	(37.8)	(10.9)
50% coinsurance	-89.7**	-89.2***	-46.7***	-8.39	-190.2***	-3.05
	(42.4)	(22.2)	(8.29)	(5.28)	(39.2)	(13.6)
95% coinsurance	-70.6**	-109.8***	-35.9***	-18.2***	-207.9***	-5.14
	(29.5)	(12.4)	(5.66)	(2.77)	(29.2)	(10.6)
T.year	-1.82	42.7**	14.6***	13.3***	128.5**	8.93
	(39.9)	(18.0)	(3.96)	(4.15)	(50.3)	(11.9)
T.year×25% coins.	-57.3	-27.6	-14.7***	-0.22	-42.9	-7.77
	(48.7)	(22.8)	(5.45)	(5.38)	(68.8)	(16.1)
T.year×50% coins.	49.1	-33.6	-11.3*	-6.22	-53.8	-25.0*
	(85.6)	(28.7)	(6.07)	(6.75)	(61.5)	(14.0)
T.year×95% coins.	17.8	-28.6	-17.2***	-4.96	-82.9	-13.6
	(49.6)	(19.3)	(4.60)	(3.83)	(54.4)	(13.4)
Enrol. term = 5	-46.3	-33.1**	-6.64	-1.58	-0.74	-8.30
	(32.6)	(14.5)	(5.56)	(3.13)	(31.8)	(12.0)
Site × enrol.	Yes	Yes	Yes	Yes	Yes	Yes
Cal. years	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
N	17825	17825	17825	17825	17825	17825
R^2	0.04	0.08	0.1	0.08	0.04	0.03

The constant indicates average spending in the free care plan during non-deadline years. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

as to increase precision. This yields four groups: those with free care, and those facing 25%, 50%, and 95% coinsurance rates.[11]

The deadline effect is positive and significant for participants with free care in the outpatient, drug, supplies, and dental spending categories, but not for inpatient or mental health spending.[12] Note that these categories that exhibit a deadline effect also tend to be more price-elastic. These deadline-elastic categories tend to have negative signs on the price-deadline interaction indicators – indicating that the deadline effect is smaller for higher coinsurance rate plans – but this is statistically significant only for drug spending.[13]

4.3 Elasticity estimates

Figure 2 plots average spending for each care category by price, once each for the total sample, the deadline year sample, and for the total sample excluding deadline years. Since price is randomly assigned by the experiment these should be interpreted as inverse demand curves. A steeper slop indicates a greater elasticity in absolute terms. Note first that the demand curves tend to be well behaved – that is, monotonically downward sloping – for all categories except inpatient and mental health care. These are the same two categories that do not exhibit a deadline effect. Secondly, note that the well behaved categories tend to be more price elastic in the deadline year. This implies that the elasticity in the non-deadline years is smaller in absolute terms.

We proceed to evaluate the statistical significance of these trends in tables 5 and 6. Table 5 shows estimated differences in average spending within a price group between the restricted model (1) and the unrestricted model (6), the latter of which controls for the price-deadline interaction. These correspond to the vertical distance beween the inverse demand curves plotted in figure 2 for the pooled average and non-deadline year samples.[14] For the free care group, the unrestricted model estimates significantly higher

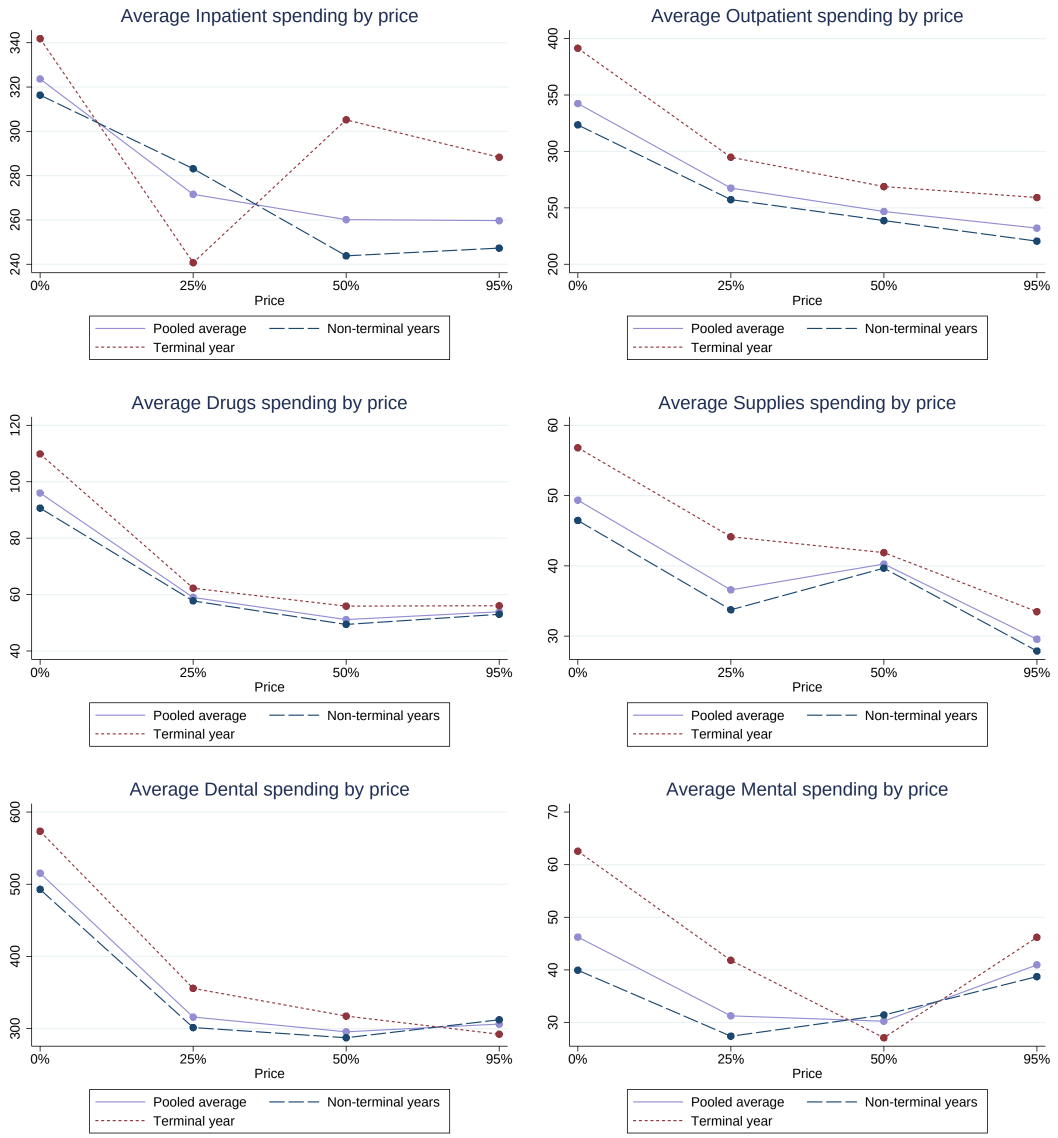
[11] Note that the mixed coinsurance and individual deductible plans assign different coinsurance rates for different care categories (see table 2). This means that a given participant maybe be in one price group for a certain category of care, and a different price group for another. For example, we assign a participant in the individual deductible plan to the free care group for inpatient care, but the 95% group for other care categories.

[12] These care categories that do not respond to the deadline tend to be long-term, price-inelastic, and non-elective.

[13] Similar patterns hold for log spending outcomes – see appendix D).

[14] For details on how these differences are estimated, see appendix A. We elect to report difference between average spending across pooled and deadline-excluding samples rather than confidence intervals

Figure 2: Inverse Demand Curves by Care Category



Inverse demand curves plot quantity as a function of price. A steeper slope indicates greater price elasticity of demand in absolute terms.

Table 5: Sensitivity of Average Spending by Price to Price-Deadline Interaction Effects

	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental h.
Free care	-.59	11.83**	4.08***	3.7***	35.71***	2.49
25% coinsurance	-16.16	4.28	0	3.64**	23.91	.3
50% coinsurance	12.57	2.78	.96	2.04	20.95	-4.41
95% coinsurance	3.91	3.77	-.76	2.3***	12.34	-1.31
Joint test	.65	.22	.00	.01	.07	.47

This table reports differences of average spending estimates by plan between models (1) and (6). A positive sign indicates that the conditional average is larger for model (1), which excludes price-deadline interaction effects.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 6: Sensitivity of Arc Elasticity Estimates to Price-deadline Interaction Effects

	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental h.
Model (1)	-.088	-.154***	-.157***	-.208***	-.209***	-.052
	(.058)	(.026)	(.043)	(.014)	(.043)	(.126)
Model (6)	-.106***	-.149***	-.137***	-.206***	-.194***	-.024
	(.036)	(.032)	(.043)	(.015)	(.059)	(.14)
Difference	.019	-.005	-.019***	-.002	-.015	-.028
	(.03)	(.013)	(.007)	(.014)	(.033)	(.062)

This table reports arc elasticity estimates for models (1) and (6), as well as their difference. A negative sign indicates that (1), which excludes the price-deadline interaction, produces a lower elasticity estimate. These arc elasticity estimates are weighted averages of the pairwise arc elasticity estimates presented in appendix D.
*** $p < .01$, ** $p < .05$, * $p < .1$

average spending for those plans that exhibit a deadline effect. The magnitude of the difference tends to decrease for higher price groups, but remains positive only in the supplies spending category.

The above results tell us that controlling for the price-deadline interaction significantly lowers estimated average spending for cheaper price groups. This translates into different elasticity estimates. Table 6 reports estimated average arc elasticities by care category for the restricted and unrestricted models, as well as their difference.[15] Elasticity estimates are smaller in magnitude for all categories except inpatient care. However, this difference in only significant for drug spending. The magnitude of the price elasticity of drug spending decreases by two percentage points when we control for price-deadline interaction effects.

4.4 Discussion

As noted in section 2, the enrollment term of three or five years is randomly assigned at the beginning of the experiment. Therefore we interpret differences in spending dynamics throughout the duration of the experiment as caused by this random assignment. Namely, the spike in spending in the deadline year is caused by the randomly assigned deadline. Arrow (1975) shows that such a pattern could result from intertemporal substitution in healthcare consumption; that is, healthcare consumption being a durable good. In appendix C we show that a qualitatively similar pattern arises from a model with a salience cost to healthcare spending, even without durable good effects. Regardless of the underlying model, we attribute the spike in spending to the randomly assigned deadline, implying that in the absence of a deadline there would be no spike. Therefore long run average spending within any given price group is better approximated by a sample that excludes deadline years. Because the deadline effect interacts with the price, the pooled sample yields price elasticity estimates greater in magnitude than the sample excluding deadline years. The sample excluding deadline years yields a better approximation of the long run price elasticity.

for each level, because while non-overlapping confidence intervals imply averages that are significantly different, the reverse is not true.

[15]The arc elasticity of y with respect to x ϵ_{yx} is the percentage change in y for a percentage change in x, calculated relative to the midpoints; $\epsilon_{yx} \equiv \frac{y_2-y_1}{(y_2+y_1)\frac{1}{2}} / \frac{x_2-x_1}{(x_2+x_1)\frac{1}{2}}$. Because it is calculated relative to the midpoint, the arc elasticity is defined for a base price category of zero, while a standard elasticity is not. For each price group, we calculate pairwise arc elasticities. We then calculate observation-weighted averages of these pairwise elasticities, which are reported in table 6. Standard errors are calculated based on 500 bootstrap replications.

5 Constant Price Elasticity of Demand

In this section we measure the price (coinsurance rate) of a health care category as a continuous variable (rather than a fixed effect, as in the previous section). This imposes on the utility function the assumption of constant price elasticity of demand, with the advantage that participants from all plans are pooled rather than being separated into various fixed effect groups, improving statistical power.

Consider a baseline parameterization that introduces the copayment rate log-linearly on right hand side:

$$y_{it} = \lambda copay_p + \tau_t + d\mathbb{1}(t = D_i) + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{4}$$

where λ gives the spending response to the coinsurance rate. The parameter d gives the effect on spending of being in the deadline year, partial to the coinsurance rate. Note that coinsurance rate is defined as the share of expenditure that the participant must pay for, so a higher coinsurance rate is equivalent to a higher price.

Now consider an augmented model that includes an interaction term between the coinsurance rate and the deadline year:

$$y_{it} = \lambda copay_p + \delta\left[copay_p \times \mathbb{1}(t = D_i)\right] + d\mathbb{1}(t = D_i) + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{5}$$

where δ gives the additional spending response to the coinsurance rate in deadline years. In this model, λ gives the spending response in non-deadline years. A negative and significant estimate of δ indicates a price elasticity that interacts with deadline year, which would imply a reduction in magnitude of λ compared to that estimated by model (4).

Table 7 presents regression results from equation (4) for log spending outcomes, by health care category. Outpatient and supplies spending have significant flat deadline effects, with participants spending significantly more in deadline years regardless of the coinsurance rate they face. All categories have a statistically significant price elasticity of demand, but the estimates are economically insignificant for inpatient and mental health spending. Moreover, the remaining categories of outpatient, drug, and supplies dental spending – which exhibit economically large price elasticities – show a significantly higher price elasticity in the deadline year.[16]

Table 8 reports estimates of the estimated price elasticity from a restricted model that excludes the price-deadline interaction to that recovered from the unrestricted model

[16] Dental spending has an economically large price elasticity but its interaction term is insignificant.

Table 7: Interaction between deadline year and coinsurance rate

	Log spending by care category (2011 USD)					
	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental h.
	b/se	b/se	b/se	b/se	b/se	b/se
Constant	0.66***	4.54***	2.72***	1.23***	3.89***	0.25***
	(0.038)	(0.059)	(0.062)	(0.043)	(0.073)	(0.031)
D.year	0.084	0.27***	0.091	0.46***	0.24**	0.0047
	(0.067)	(0.076)	(0.063)	(0.075)	(0.11)	(0.041)
Log(coins.)	-0.024**	-0.23***	-0.20***	-0.098***	-0.30***	-0.017**
	(0.010)	(0.019)	(0.018)	(0.012)	(0.022)	(0.0087)
Log(coins.)× D.year	-0.019	-0.069***	-0.047***	-0.052***	-0.017	-0.0047
	(0.016)	(0.018)	(0.015)	(0.017)	(0.025)	(0.0097)
Enrol. term = 5	-0.024	-0.0037	-0.013	0.063	0.068	-0.027
	(0.056)	(0.090)	(0.088)	(0.061)	(0.099)	(0.047)
Site × enrol.	Yes	Yes	Yes	Yes	Yes	Yes
Cal. years	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
N	17825	17825	17825	17825	17825	17825
R^2	0.04	0.1	0.2	0.1	0.1	0.03

The constant indicates log spending in the free care plan in non-deadline years. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 8: Stability of Price Elasticity to the Price-deadline Interaction Term

	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental H.
Model (4)	-.027	-.253	-.215	-.113	-.301	-.018
	(.008)	(.009)	(.008)	(.007)	(.010)	(.004)
Model (5)	-.024	-.233	-.202	-.097	-.297	-.017
	(.008)	(.010)	(.009)	(.009)	(.012)	(.005)
P-value difference	.242	0	.001	.002	.499	.626

This table reports p-values for the hypothesis test that $\hat{\lambda} = \tilde{\lambda}$, where $\hat{\lambda}$ is an estimate of price elasticity in a restricted model without price-deadline interaction effects, and $\tilde{\lambda}$ estimates plan effects from the model with interaction effects, given by equation (4). We report results for spending outcomes in logs, with price measured as the log of the coinsurance rate plus one (so as not to exclude the free care group).

Table 9: Sensitivity of Arc Elasticity Estimates to Price-deadline and Price-initial year Interaction Effects

	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental h.
Model (1)	-.087	-.153***	-.157***	-.208***	-.209***	-.052
	(.058)	(.026)	(.043)	(.013)	(.043)	(.126)
Model (6)	-.104***	-.149***	-.137***	-.225***	-.204***	-.019
	(.034)	(.033)	(.044)	(.025)	(.06)	(.125)
Difference	.016	-.004	-.02***	.017	-.005	-.033
	(.03)	(.012)	(.007)	(.022)	(.036)	(.058)

This table reports differences in arc elasticity estimates between the restricted model (excludes any price-time interactions), and a model controlling for both the price-deadline and the price-initial year interaction effects.
*** $p < .01$, ** $p < .05$, * $p < .1$

(4). For outpatient, drug, and supplies spending, the unrestricted model yields significantly different estimates of the price elasticity that are lower in magnitude by a couple percentage points.

This section has taken an alternative approach to modelling the spending response to random assignment to plan, assuming a constant price elasticity of demand in order to improve statistical power. This yields a single price elasticity of demand for each category of healthcare. With the exception of dental care, we find the estimated elasticities of price elastic categories of spending to be significantly smaller in magnitude when the price-deadline interaction term is included.

6 Spending Dynamics

This section investigates the dynamics of spending during the RAND HIE beyond a spending spike in the deadline year. Past literature on other datasets has investigated and shown evidence of 'pent-up demand' – the tendency upon entering a subsidized insurance regime to temporarily increase spending by a large amount, only for the effect to fade out within a few years – see Card et al. (2008), Cabral (2016), Franc et al. (2016). Indeed, the designers of the HIE were aware of such a possibility (Arrow 1975), and Lohr et al. (1986) excludes the initial year of the experiment as a precaution. Manning et al. (1987) compares unconditional average spending in initial years to middle years (excluding deadline years), and does not find a significant difference.

6.1 Controlling for initial year effects

Table 9 reports differences in arc elasticity estimates between a restricted model that does not allow any price-time interaction and a model that allows for the effect of price on spending to vary both in deadline years and in the initial year of program, given below. Letting I_i indicate the initial calendar year of the experiment for subject i,

$$y_{it} = \lambda_p + \delta_p \times \mathbb{1}(t = D_i) + \iota_p \times \mathbb{1}(t = I_i) + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{6}$$

where ι_p gives the additional effect of price in the initial year. The results are virtually identical to those generated by model 6, which controls only for the price-deadline interaction. Although the price-initial year interaction effect is not large enough to affect elasticity estimates, it is individually significant and positive for supplies and dental spending. Additionally, there is a modest negative initial year effect for drug spending and a larger one for mental health spending.

Table 10: Price-Deadline Fixed Effects by Spending Category

	Spending by care category (2011 USD)					
	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental H.
	b/se	b/se	b/se	b/se	b/se	b/se
Constant (free care)	335.5***	336.7***	94.9***	40.4***	415.5***	48.1***
	(21.2)	(11.5)	(4.98)	(3.21)	(24.4)	(9.12)
25% coinsurance	-31.3	-68.3***	-35.9***	-7.46*	-64.1	-9.24
	(36.7)	(18.4)	(7.42)	(3.91)	(45.3)	(13.4)
50% coinsurance	-81.7	-94.1***	-48.5***	-3.37	-128.7***	-1.40
	(50.7)	(24.0)	(9.38)	(6.24)	(44.4)	(17.5)
95% coinsurance	-79.3**	-117.3***	-40.6***	-14.5***	-140.7***	-8.92
	(35.4)	(14.4)	(6.21)	(3.70)	(33.0)	(12.7)
D.year	6.34	40.1**	12.2***	13.7***	165.8***	11.0
	(41.1)	(18.6)	(4.29)	(4.54)	(51.4)	(12.2)
D.year×25% coins.	-62.9	-29.4	-11.4**	-3.84	-142.0*	-5.41
	(52.8)	(24.2)	(5.63)	(6.17)	(73.5)	(16.1)
D.year×50% coins.	45.3	-28.8	-9.43	-12.3*	-117.2*	-26.1
	(86.8)	(30.0)	(6.57)	(7.37)	(63.6)	(17.0)
D.year×95% coins.	27.5	-21.1	-12.6**	-8.57*	-149.7***	-9.94
	(53.4)	(20.3)	(4.95)	(4.73)	(56.3)	(13.7)
I.year	-38.6	-7.56	-7.40*	17.8***	185.2***	-20.8**
	(40.0)	(15.8)	(4.02)	(4.85)	(55.3)	(9.90)
I.year×25% coins.	-20.1	-5.08	8.59	-8.37	-251.3***	2.69
	(51.5)	(19.4)	(5.49)	(5.54)	(70.2)	(13.2)
I.year×50% coins.	-25.2	13.0	4.54	-12.5	-157.9**	-5.00
	(78.0)	(22.1)	(6.82)	(8.98)	(68.3)	(16.9)
I.year×95% coins.	20.6	19.1	11.8***	-9.59*	-173.0***	9.76
	(57.1)	(16.5)	(4.38)	(5.14)	(56.0)	(11.4)
Enrol. term = 5	-38.2	-33.0**	-6.49	-3.78	-12.7	-4.88
	(33.2)	(14.7)	(5.63)	(3.04)	(33.3)	(12.3)
Site × enrol.	Yes	Yes	Yes	Yes	Yes	Yes
Cal. years	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
N	17825	17825	17825	17825	17825	17825
R^2	0.04	0.08	0.1	0.08	0.04	0.03

The constant indicates average spending in the free care plan during non-deadline years. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

6.2 Counting forwards by contract year

We begin by introducing *contract year* fixed effects into the estimation framework of Aron-Dine et al. (2013). In the first year of the plan the contract year is equal to one, and so on, until the plan is terminated after either three or five years. Denoting contract year by c, we analyze the dynamics of spending using the following equation:

$$y_{it} = \gamma_c + \lambda_p + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{7}$$

which considers the effects of plan p and contract year c on the outcome y of individual i in calendar year t. A plan corresponds more or less to price (coinsurance rate).[17] Plan effects can be interpreted as causal because participants are randomly assigned to a plan by the experiment. We use the full set of controls detailed in section 4.

The experiment randomly allocates participants to three- or five-year enrolment term cohorts. Because we expect participants to react to the deadline for contract termination, when estimating equation (7) we consider the three- and five-year cohorts separately. The results for total spending are reported in table 11. The first column of each panel introduces contract year into the specification of Aron-Dine et al. (2013). The second column of each introduces the full set of control variables, and the third column drops outliers, as detailed in section 2. Participants spend several hundred dollars more in the final year of the experiment. The effect is statistically significant in the three-year sample for all specifications, but in the five-year sample only when calendar year controls are excluded.[18] However, the flat deadline effect is not robust to the exclusion of outliers even for the three-year sample. The estimated coefficients on plan type are stable across specifications.

6.3 Counting backwards from deadline year

In order to increase power we now pool the three- and five-year samples. To consider them simultaneously, consider an alternative model that counts backward from the final year of the program. Recall that we denote the term of enrollment by $e_i \in \{3, 5\}$. Defining $d_{it} \equiv e_i - c_{it}$ as the years left until the deadline,

$$y_{it} = \kappa_{d_{it}} + \phi \mathbb{1}(e_i = 5) + \lambda_p + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{8}$$

[17] Coinsurance rate is the fraction of expenditure paid by the participant, with the remainder covered by the insurance plan. Some plans offer different coinsurance rates for different categories of spending. See section 2 for more.

[18] As discussed in section 2, this is due to colinearity with calendar year – there being less disjoint variation between calendar year and contract year in the five-year sample. See figure 1 for a visualization.

Table 11: Counting forwards from plan commencement

	Total spending					
	5 year cohort			3 year cohort		
	b/se	b/se	b/se	b/se	b/se	b/se
25% plan	-333.7***	-336.3***	-336.4***	-381.6***	-394.0***	-393.9***
	(106.0)	(99.8)	(99.9)	(89.3)	(87.4)	(87.4)
Mixed copay	-345.6***	-330.1***	-330.2***	-475.7***	-467.9***	-467.8***
	(127.1)	(113.4)	(113.5)	(102.5)	(93.3)	(93.4)
50% plan	-482.3***	-464.5***	-464.3***	-486.7***	-476.6***	-476.5***
	(106.3)	(101.4)	(101.5)	(132.9)	(134.3)	(134.3)
Indiv. deductible	-329.5***	-381.8***	-381.8***	-373.9***	-404.2***	-404.1***
	(89.9)	(83.4)	(83.4)	(74.1)	(70.4)	(70.4)
95% plan	-439.9***	-465.5***	-465.5***	-647.4***	-627.7***	-627.8***
	(91.5)	(87.3)	(87.3)	(76.9)	(72.8)	(72.8)
Cont. year 2	-6.93	-18.7	14.6	-53.0	-65.9	15.7
	(69.2)	(69.9)	(80.4)	(44.7)	(45.3)	(63.1)
Cont. year 3	-38.9	-64.4	-16.0	47.5	-1.80	77.3
	(66.1)	(67.3)	(103.5)	(46.0)	(47.1)	(71.4)
Cont. year 4	-39.8	-88.0	-22.4	0	0	0
	(62.9)	(65.6)	(130.3)	(.)	(.)	(.)
Cont. year 5	157.7**	98.0	156.9	0	0	0
	(68.9)	(71.6)	(155.4)	(.)	(.)	(.)
Constant	1250.3***	1174.3***	1139.5***	1466.6***	1460.3***	1399.5***
	(72.3)	(69.5)	(98.2)	(57.5)	(59.0)	(72.7)
Site × enrol.	No	Yes	Yes	No	Yes	Yes
Cal. years	No	No	Yes	No	No	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
N	7310	7310	7310	10515	10515	10515
R^2	0.01	0.10	0.10	0.01	0.10	0.10

The base category for plan is free care, and the base category for contract year is the initial year of the experiment. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 12: Counting backward from plan termination

	Combined cohorts					
	Inpatient	Outpatient	Drugs	Supplies	Dental	Mental
	b/se	b/se	b/se	b/se	b/se	b/se
25% plan	-77.1**	-78.2***	-36.8***	-11.1***	-169.6***	-13.5
	(36.7)	(18.2)	(8.9)	(3.3)	(33.2)	(12.2)
Mixed plan	-69.3*	-79.0***	-37.7***	-10.1***	-198.9***	-8.5
	(37.2)	(19.7)	(7.8)	(3.4)	(38.3)	(19.2)
50% plan	-93.1**	-101.5***	-52.2***	-11.3**	-208.7***	-13.6
	(43.1)	(21.3)	(8.2)	(5.0)	(54.4)	(13.7)
Indiv. deductible	-44.5	-96.2***	-34.0***	-15.5***	-212.8***	5.9
	(28.8)	(14.6)	(6.9)	(2.8)	(31.2)	(14.4)
95% plan	-88.1***	-143.3***	-47.3***	-23.9***	-245.5***	-27.4***
	(29.5)	(14.8)	(6.4)	(2.9)	(28.6)	(9.7)
T.year	-48.6	45.4**	7.4	-0.8	77.1	-0.4
	(53.5)	(19.0)	(4.6)	(4.3)	(57.5)	(8.5)
2 years left	-8.9	10.8	1.7	-9.1***	-10.5	-4.2
	(39.8)	(12.1)	(2.7)	(3.4)	(43.8)	(6.4)
4 years left	9.1	8.8	-0.9	-3.0	38.0	7.5
	(54.8)	(18.3)	(4.1)	(4.6)	(49.4)	(11.4)
5 years left	24.7	-21.9	-3.6	10.4**	-40.3	-7.3
	(55.5)	(19.9)	(4.6)	(4.2)	(64.2)	(9.7)
Enrol. term = 5	-65.8	-14.7	-2.1	-12.3***	0.6	-8.2
	(57.7)	(22.7)	(6.9)	(4.7)	(58.4)	(11.7)
Constant	140.5	-170.7	-87.0**	-29.8	76.7	-14.9
	(218.6)	(105.2)	(44.2)	(22.7)	(197.4)	(82.6)
Enrol. date	Yes	Yes	Yes	Yes	Yes	Yes
Site × enrol.	Yes	Yes	Yes	Yes	Yes	Yes
Cal. years	Yes	Yes	Yes	Yes	Yes	Yes
Site × cal.year	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
N	17825	17825	17825	17825	17825	17825
R^2	0.04	0.09	0.2	0.09	0.05	0.04

As before the base category for plan is free care. The base category for years left is three years left, and the base category for cohort length is $C = 3$. All standard errors are clustered at the family (treatment) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

where $\kappa_{d_{it}}$ captures the effect on spending of being further from the deadline. The additional assumption is that effect on spending of the remaining time before the deadline does not vary by enrollment term e_i. To relax this, we include and indicator dummy for cohort duration $\mathbb{1}(e_i = 5)$. These specifications allow simultaneous consideration of three- and five-year cohorts, allowing for larger sample size in plan groups p as well as time-to-deadline groups d_{it}. We use year $c = 3$ as the base category.

Table 12 estimates model (8) on the disaggregated categories of care. Participants spend significantly more on outpatient care in the final year of the program.

As noted in section 4, the flat deadline effect is less robust than the interactive deadline effect because it pools together participants in plans who exhibit a spike in spending with those who do not. In the following subsection we consider a more flexible model that allows for semiparametric interaction between years until deadline and the coinsurance rate.

6.4 Price×Years Remaining

Because the price interacts with the deadline effect, omission of the price-deadline interaction term leads to underestimation of the deadline effect. To see this consider that participants facing the lowest price – the free care group – exhibit the highest spending spike. Estimating a non-interactive deadline effect pools experimental groups with smaller spending spikes (high copayment rates) together with groups that exhibit larger spikes, weakening the magnitude and statistical significance of the effect.

Consider a model allowing for interaction effects between plan and years until the deadline:

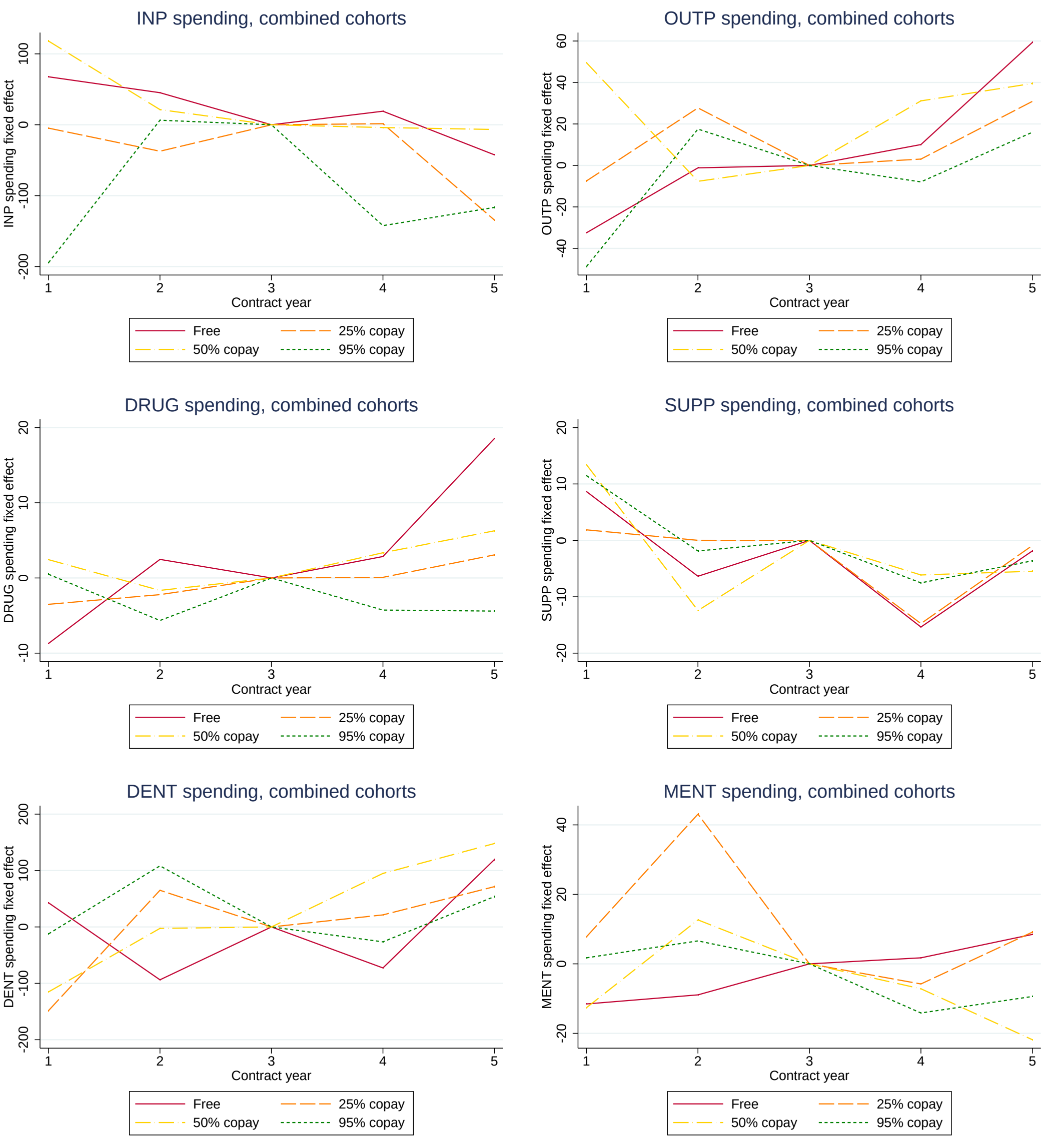
$$y_{it} = \delta_{pd_{it}} + \tau_t + \alpha_{lm} + X_{it}\beta + \varepsilon_{it} \tag{9}$$

which relaxes the assumption of separability between plan and contract year effects. This nests model (8), and we use it to investigate nonseparable interactions between plan p and years until deadline d_{it}, with plan given by coinsurance rate $p \in \{0, .25, .5, .95\}$.[19]

Figure 3 plots the price-years-to-deadline fixed effects for the combined sample. For outpatient and drug spending, participants with free care show the largest spikes in the deadline year, with higher coinsurance groups showing progressively smaller spikes. This

[19] This means that for the mixed coinsurance group, participants are included in the 50% copayment group for dental and mental health spending, and the 25% group for all other spending categories. Those on the individual deductible plan fall under free care for inpatient spending and 95% coinsurance for all other categories.

Figure 3: Average medical spending by continuation year and copayment rate



pattern holds approximately for dental spending, but is less clear for the other spending categories. These roughly correspond to the spending categories for which we found significant negative deadline year elasticities in sections 4 and 5.

7 Conclusion

We find that there is a statistically significant and economically large spike in spending in the deadline year of the RAND Health Insurance Experiment. This 'deadline effect' interacts with the price elasticity of spending, with lower coinsurance rates generating larger spikes. Controlling for the interaction between price and deadline significantly lowers estimates of the price elasticity in non-deadline years. We argue that the price elasticity in non-deadline years is a more plausible estimate of the long run elasticity that is relevant to costing a permanent subsidy program; without a randomly assigned deadline year, there is not deadline spike in spending. This implies that the cost of public provision – for example, free care – is lower than previously estimated. The deadline effect itself may also be instructive in situations where health insurance is to be cut off, such as temporary/employer provided programs.

More broadly, the presence of a deadline effect warns against a naive interpretation of treatment effects in RCTs. Because the necessary design limitation of the experiment – that subsidized care could not be provided permanently for enrollees – the treatment effect of coinsurance plan pooled across all years does not give the long run treatment effect. This was in fact anticipated while the experiment was being designed (Arrow 1975). This foresight lead to the randomly-assigned variation in enrollment term that we use to identify the deadline effect. Previous literature on the RAND HIE has largely overlooked transitory demand effects, with the exception of Manning et al. (1985), which finds a deadline effect for low coinsurance rate plans for dental spending. Lohr et al. (1986) drops the first and last year observations to avoid transitory effects, but does not document a deadline effect.

While this paper has looked at spending outcomes only, further work can analyze the relationship between deadline spending and health outcomes. Previous literature has found that increased medical spending during the HIE does not have strong positive effects on health outcomes; this may be due to 'over spending' during the deadline year.

Bibliography

Akerlof, G. A. (1991). Procrastination and obedience. *The American Economic Review*, 81(2):1–19.

Aron-Dine, A., Einav, L., and Finkelstein, A. (2013). The rand health insurance experiment, three decades later. *Journal of Economic Perspectives*, 27(1):197–222.

Aron-Dine, A., Einav, L., Finkelstein, A., and Cullen, M. (2015). Moral hazard in health insurance: Do dynamic incentives matter? *Review of Economics and Statistics*, 97(4):725–741.

Arrow, K. (1975). Two notes on inferring long run behavior from social experiments.

Bradford, D., Courtemanche, C., Heutel, G., Mcalvanah, P., and Ruhm, C. (2017). Time preferences and consumer behavior. *Journal of Risk and Uncertainty*, 55(2-3):119–145.

Cabral, M. (2016). Claim timing and ex post adverse selection. *The Review of Economic Studies*, 84(1):1–44.

Card, D., Dobkin, C., and Maestas, N. (2008). The impact of nearly universal insurance coverage on health care utilization: evidence from medicare. *American Economic Review*, 98(5):2242–58.

Chandra, A., Gruber, J., and McKnight, R. (2010). Patient cost-sharing and hospitalization offsets in the elderly. *American Economic Review*, 100(1):193–213.

Della-Vigna, S. and Malmendier, U. (2006). Paying not to go to the gym. *American Economic Review*, 96(3):694–719.

Einav, L., Finkelstein, A., and Schrimpf, P. (2015). The response of drug expenditure to nonlinear contract design: evidence from medicare part d. *The quarterly journal of economics*, 130(2):841–899.

Franc, C., Perronnin, M., and Pierre, A. (2016). Supplemental health insurance and healthcare consumption—a dynamic approach to moral hazard. *Health economics*, 25(12):1582–1598.

Kiil, A. and Houlberg, K. (2013). How does copayment for health care services affect demand, health and redistribution? a systematic review of the empirical evidence from 1990 to 2011. *The European Journal of Health Economics*, 15(8):813–828.

Lin, H. and Sacks, D. W. (2016). Intertemporal substitution in health care demand: Evidence from the rand health insurance experiment. Technical report, National Bureau of Economic Research.

Lohr, K. N., Brook, R. H., Kamberg, C. J., Goldberg, G. A., Leibowitz, A., Keesey, J., Reboussin, D., and Newhouse, J. P. (1986). Use of medical care in the rand health insurance experiment: diagnosis-and service-specific analyses in a randomized controlled trial. *Medical care*, 24(9):S1–S87.

Manning, W., Bailit, H., Benjamin, B., and Newhouse, J. (1985). The demand for dental care: evidence from a randomized trial in health insurance. *Journal of the American Dental Association (1939)*, 110(6):895.

Manning, W. G., Newhouse, J. P., Duan, N., Keeler, E., Benjamin, B., Leibowitz, A., Marquis, M. S., and Zwanziger, J. (1988). Health insurance and the demand for medical care. *USA: The Rand Corporation (supported by flagrant from the US Department of Health and Human Services).*

Manning, W. G., Newhouse, J. P., Duan, N., Keeler, E. B., and Leibowitz, A. (1987). Health insurance and the demand for medical care: evidence from a randomized experiment. *The American economic review*, pages 251–277.

Metcalf, C. E. (1973). Making inferences from controlled income maintenance experiments. *The American Economic Review*, 63(3):478–483.

O'Donoghue, T. and Rabin, M. (1999). Doing it now or later. *American Economic Review*, 89(1):103–124.

Ringel, J. S., Mahnovski, S., Hosek, S. D., and Vollaard, B. A. (2002). *The elasticity of demand for health care: a review of the literature and its application to the Military Health System*. RAND.

Ruhm, C. J. (2012). Understanding overeating and obesity. *Journal of Health Economics*, 31(6):781–796.

A Sensitivity of Plan Effects to Controlling for Plan-Deadline Interaction

In section 4 we began by showing the significance of the plan-deadline interaction. While this behavioural phenomenon may be of economic interest, a more policy relevant question is whether inclusion of the plan-deadline effects significantly affects estimates of the long-run price elasticity – which we take to be the price elasticity measured in non-deadline years. For that reason we proceeded to test the latter question in table **??**. In the current section we detail the hypothesis test of the equality of plan effects between the restricted and unrestricted models (equations 1 and 6 respectively).

For a sample of size N, let the $N \times 1$ vector of spending outcomes be given by Y. There are P randomly allocated coinsurance plans (with $P = 6$ in our preferred specification; see section 2). Let L denote an $N \times P$ matrix of plan indicators, and D an $N \times P$ matrix of plan-deadline year interaction indicators.[20] For notational convenience, denote by X the complete set of control variables (including calendar year×location effects and location×start-month effects). Then we can rewrite equation (1) in matrix form as

$$Y = L\lambda + XB + e$$

where e is a mean-zero vector of errors and β gives the coefficients on all control variables.[21]. The parameters of interest are the plan effects given in the $P \times 1$ vector λ, which correspond to the conditional means of spending across all person-years for a plan $p \in \{1, ..., P\}$. We rewrite the unrestricted model given by equation (6) as

$$Y = L\lambda' + D\delta + XB' + e$$

where $P \times 1$ parameter vector δ gives the plan-deadline year interaction effects, now denoting by λ' the conditional averages by plan of spending in non-deadline person-years. The coefficient on the matrix of controls may also differ. We can combine the two

[20] That is, for deadline years $t \in \mathbb{T}$ – where set $\mathbb{T}$ is the set of observations for which the participant is in the deadline year – element $L_{it} = D_{it}$ for $t \in \mathbb{T}$, and element $D_{it} = 0$ for $t \notin \mathbb{T}$.

[21] This is with some abuse of notation; in section 4 we used β to signify only the coefficients on demographic controls, with calendar year×location given by τ and location×start month controls given by α. Here we collect all these terms in β. Additionally, in equation

models into a single seemingly-unrelated regression framework given by

$$\begin{bmatrix} Y \\ Y \end{bmatrix} = \begin{bmatrix} L & 0 & 0 & X & 0 \\ 0 & L & D & 0 & X \end{bmatrix} \begin{bmatrix} \lambda \\ \lambda' \\ \delta \\ B \\ B' \end{bmatrix} + e.$$

For each element of estimated parameter vectors $\hat{\lambda}$ and $\hat{\lambda}'$, we want to test the null hypothesis that $\hat{\lambda}_p = \hat{\lambda}'_p$. We want also to conduct a joint hypothesis test that $\hat{\lambda}_p = \hat{\lambda}'_p$ $\forall p \in \{1, ..., P\}$. This can be reframed as a test of equality to zero as follows:

$$\begin{bmatrix} Y \\ Y \end{bmatrix} = \begin{bmatrix} L & 0 & 0 & X & 0 \\ L & L & D & 0 & X \end{bmatrix} \begin{bmatrix} \lambda \\ d\lambda \\ \delta \\ B \\ B' \end{bmatrix} + e.$$

where $d\lambda \equiv \lambda' - \lambda$. To determine whether the effect on spending of plan p is significantly different when deadline effects are included in the model, test the null hypothesis $d\hat{\lambda}_p = 0$.

B Literature review

The RAND Health Insurance Experiment (HIE) was an unprecedented study of consumer response to changes in health care prices. Nearly 6,000 individuals in the United States were randomly assigned to health insurance plans with co-pay rates ranging from 0-95%. The term of the contracts were either 3 or 5 years, during which the participants' health care spending was tracked. The study observed that the lower the cost faced by the individual, the higher their consumption of health care services, which resulted in their famous elasticity estimate of -0.2. More recently, this estimate was confirmed by Aron-Dine et al. (2013) who re-examined the RAND HIE data using the latest statistical methods and correcting for potential biases and threats to validity.

The price elasticity of demand for health care services and, health care consumption in the face of differing insurance plans has been widely studied and debated since the HIE was published. A review study by Ringel et al. (2002) finds a relatively wide range of estimates centered on -0.17. They further find that price elasticities differ across different classes of health care services, with preventative care and pharmacy benefits being among the most price sensitive. The authors argue that this is due to the higher number of available substitutes (such as exercise and a healthy diet), preventative medicine being more of a luxury rather than a necessity good and the opportunity cost of preventative care being higher than that of curative care. They further argue that the higher price elasticity of preventative care may reflect the fact that the benefits of such care accrue over the long term and are therefore more heavily discounted.

In line with these findings, a more recent review study by Kiil and Houlberg (2013) confirms that the introduction of co-pay reduces the consumption of out-patient care, while having little effect on in-patient care. More specifically, the studies reviewed by Kiil and Houlberg (2013) find a decrease in the use of medical consultations with general practitioners and specialists, prescription medication and ambulatory care as a result of the introduction of or rise in co-pay rates. No significant change was observed in hospitalization rates.

Ringel et al. (2002) further discuss the importance of knowing the context in which price elasticity of health care is measured as many factors, such as coinsurance rates and deductibles, influence the price and consequently consumer behavior. For example, one can expect the spending behavior of someone who has reached their deductible amount for the year to be different from a similar individual who has not yet reached their deductible. The authors further discuss the time-price of health care consumption and how, even in the face of an out-of-pocket price of zero, the time-price may affect an individual's consumption of health care services.

It is precisely this time-price and other non-monetary costs of health care consumption that this paper seeks to incorporate into the estimation of price elasticity of demand. We argue that while the resulting long term health outcomes are desirable, non-monetary costs to health care consumption such as time-cost and pain and discomfort associated with procedures, make the consumption of such services a "bad" in the short term. Therefore, in so far as the sum of the monetary and non-monetary cost of obtaining care outweighs the discounted future benefit of its consumption, the individual will postpone treatment.

There is ample evidence of procrastination resulting from present-bias and hyperbolic discounting in the behavioral economics literature. Present-bias has been shown to affect health related behaviors such as eating (Ruhm 2012) and going to the gym (Della-Vigna and Malmendier 2006) to retirement savings (**?**) and many others. A recent study by Bradford et al. (2017) showed that subjects who discount the future more heavily tend to have worse health outcomes and are less likely to have self-procured health insurance than their counterparts with lower discount rates. They further find that individuals with higher discount rates exercise less and snack, smoke and binge drink more than their counterparts.

C Model

This section presents an illustrative three-period model of intertemporal consumption with procrastination and price changes. The model gives a basis for understanding two stylized facts present in the RAND health data: a) participants spend more in their final year of the program than in prior years; and b) the aforementioned effect is greater for participants with higher subsidization rates.[22]

The first two periods correspond to years in which the participant receives subsidization in healthcare spending. In the third period the subsidy ends, and the price of healthcare jumps. Agents anticipate the price increase.

The key feature of the model is a 'salience cost' parameter that makes consumption in the current period relatively less attractive.[23] This is equivalent to the quasi-hyperbolic discounting model proposed by Laibson (1997), but with a bias *against* the present rather than for it. Intuitively: consumers put off healthcare spending because they would rather get around to it later. For simplicity we abstract from the exponential component of discounting, setting the exponential discount factor to unity.

The salience cost introduces time-inconsistency into the dynamic optimization problem: during the first period, the agent has a different relative valuation of periods two and three than that of her future self in period two. The agent is a 'sophisticated' quasi-hyperbolic discounter who anticipates this change in preferences. We model the dynamic optimization problem as a sequential game with two players, and solve using the concept of subgame perfect equilibrium. We label a given individual the 'first-period agent' in period one; this same individual becomes the 'second-period agent' in period two, and so on in period three. This terminology highlights the fact that the individual has different time preferences, and therefore a different lifetime utility function, in different periods.

The price change and the anti-present bias interact nontrivially. Time-inconsistency

[22] Arrow (1975) shows that there is a spike in spending in the deadline year if healthcare is a durable good. Participants anticipate the price increase after plan termination and 'stock up' on healthcare. The current section shows that a salience cost to healthcare consumption generates the same prediction, even if healthcare is nondurable.

[23] See Akerlof (1991) for a discussion. O'Donoghue and Rabin (1999) also consider anti-present biases.

means that the first period agent assigns relative valuations of consumption in periods two and three that differ from how she will value them tomorrow, in period two. Namely, the first period agent exhibits anti-present bias against periods two and three equally; if she could commit to a consumption path, she would allocate consumption between periods two and three in proportion to the change in price between these periods. In contrast, the second period agent has a bias against that period, and therefore allocates consumption between periods two and three in proportion to the anti-present bias as well as the relative prices. Since the first and second period agents agree on the relative allocation of consumption between periods two and three to the extent that this is incentivized by changes in the price of consumption, but disagree to the extent that it is determined by anti-present bias, the first period agent is willing to leave more resources to the second period agent the more that the second period agent's allocation is influenced by price differences – that is, the greater is the price difference between the second and third periods.[24]

In our finite-horizon setting, a time-consistent agent could choose a consumption profile that increases from period one to two, but falls in period three. This follows trivially from a parameter space with an exponential discount factor greater than one and a relative price of nonsubsidized healthcare of greater than the discount factor. However, such an approach presents two problems. First, exponential discounting with a discount factor greater than one is unworkable in longer/infinite-horizon settings. Second, the relative magnitude of the spike in consumption in the last year of subsidization (compared to the previous year) does not depend on the subsidization rate for the time-consistent agent, as it does for the time-inconsistent one – so the time-consistent model fails to predict our second key stylized fact.

C.1 Subgame perfect equilibrium

Agents consume over three periods. We normalize price to unity outside of the subsidization program, with relative price $p < 1$ during periods one and two. The exponential discount factor is set to one, and there is perfect storage with no return to saving. The felicity function $u(.)$ is increasing and concave; that is, $u' > 0$ and $u'' < 0$.

Agents cannot commit to a consumption profile, so we solve the model backwards in

[24] This mechanism requires that the first period agent be a sophisticated time-inconsistent agent: one who recognizes that she will have a different preference structure in the following period. A myopic agent would not realize that her relative valuation of the final two periods would change when she wakes up tomorrow.

time, starting from period three. The consumer's problem in the final period is trivial: she consumes all remaining income y_3 at the current price of unity. This means that the second period consumer effectively chooses the consumption profile for the second and third periods, from which the third period consumer is unable to deviate. It is sufficient to characterize the second period agent's problem as the decision of how much income to consume in the current period at price p and how much to store for consumption in the following period. Taking the income endowment y_2 as given, the value function of the second period consumer is then given by

$$V_2 = \max_{y_3} \left\{ u\left(\frac{y_2 - y_3}{p}\right) + \beta u(y_3) \right\} \tag{10}$$

where $\beta > 1$ captures the agent's preference to postpone consumption. This yields the first-order condition

$$\frac{u'\left(\frac{y_2 - y_3^*}{p}\right)}{u'\left(y_3^*\right)} = \beta p \tag{11}$$

that says the ratio of the marginal utilities of consumtion between the second and third period is proportional to the multiple of the dis-immediacy preference $\beta > 1$ and the price discount $p < 1$.[25]

Since the first period agent cannot commit to a consumption profile, she must take into account the second period agent's optimization condition. Constrained by this behaviour, the first period agent decides how much income to consume and how much to leave to her future self, who she knows will allocate spending differently than would she. For this reason it is instruction to consider how the second period agent's saving decision changes in response to a larger or smaller endowment (since the size of that endowment is chosen by the first period agent). Rearranging (11) and taking the total derivative yields the second-order condition

$$\frac{\partial y_3^*}{\partial y_2} = \frac{u''\left(\frac{y_2 - y_3^*}{p}\right)}{u''\left(\frac{y_2 - y_3^*}{p}\right) + \beta p^2 u''\left(y_3^*\right)} \tag{12}$$

which says that the second period agent saves extra income at a rate proportional to ratio of the second derivative of period-two utility to the sum of the second derivatives of second and third period utility, with the latter discounted by the multiple of the time-preference and the price discount.

[25]With longer time horizons the salience cost cancels out, leaving the ratio proportional only to the price discount.

Lemma 1. *Given a concave felicity function $u(.)$ and parameter values $\beta > 1$ and $p \in (0,1)$, it follows that*

$$\frac{\partial y_3^*}{\partial y_2} \in (0,1).$$

Proof. Since the second derivative of $u(.)$ is globally negative, the numerator is negative. Since $\beta p^2 > 0$, the denominator is also negative, and greater in magnitude. Therefore their ratio is positive, and less than one. □

Now consider the value function of the first period agent, given by

$$V_1 = \max_{y_2} \left\{ u\left(\frac{y_1 - y_2}{p}\right) + \beta u\left(\frac{y_2 - y_3^*}{p}\right) + \beta u(y_3^*) \right\} \tag{13}$$

which gives the following first-order condition

$$\frac{u'\left(\frac{y_1 - y_2}{p}\right)}{u'\left(\frac{y_2 - y_3^*}{p}\right)} = \beta\left(1 - \frac{\partial y_3^*}{\partial y_2}\right) + \frac{\partial y_3^*}{\partial y_2} \tag{14}$$

which says that ratio of the relative marginal utilities of consumption in the first and second periods is proportional to a weighted average of the salience factor β and unity. The weight on unity is given by the second period agent's marginal propensity to channel income into the third period, with the weight on β being the remainder. The interpretation is that from the perspective of the first period agent, the second period agent values third period consumption properly and so has already optimally weighted it by the salience factor β. However, the second period agent does not value second period consumption proportional to β while the first period agent does; thus the first period agent applies β to the second period agent's propensity to channel a higher endowment into savings. In other words, she disapproves of the second period agent's aversion to consuming in that period, and so withholds savings in proportion to how strong is her future self's inclination to procrastinate.

Theorem 1. *It follows from Lemma 1 and $\beta > 1$ that the ratio of the marginal utilities of first to second period consumption is greater than one, implying then by concavity of the utility function that second period consumption is greater than that in the first period.*

Proof. Lemma 1 and $\beta > 1$ imply that the right-hand side of equation (14) is positive. This implies that the marginal utility of consumption in the first period is greater than that in the second, which by concavity of the utility function implies that first period consumption is lower than second. □

Now consider how the first period agent's optimal decision of how much wealth to leave to the second period agent changes in response to a change in price. That is, take the derivative of (12) with respect to price. For tractability, we assume $u(c) = c^{\alpha}$, which implies constant price elasticity of demand (as in section 5). This yields

$$\frac{\partial^2 y_3^*}{\partial p \partial y_2} = \frac{\alpha}{1-\alpha} \frac{1}{2 + \beta^{\frac{1}{1-\alpha}} p^{\frac{1}{1-\alpha}} + \beta^{\frac{-1}{1-\alpha}} p^{\frac{-\alpha}{1-\alpha}}} > 0 \tag{15}$$

which says that the second-period agent tends to divert income to the third period to a greater extent the smaller grows the subsidy (that is, when $p < 1$, the closer is p to the unsubsidized price level of unity). Now define $\Phi \equiv \frac{\partial^2 y_3^*}{\partial p \partial y_2}$. The rate at which the ratio of marginal utilities of first and second period consumption changes in response to a price change is then given by

$$\frac{\partial}{\partial p} \frac{u'\left(\frac{y_1 - y_2}{p}\right)}{u'\left(\frac{y_2 - y_3^*}{p}\right)} = \Phi(1 - \beta) \tag{16}$$

which takes account of the second-period agent's inclination to divert differential amounts of savings to the third period in response to changes in price.

Theorem 2. *The relative spike in last period consumption compared to the preceding period is larger for larger subsidies (smaller p).*

Proof. It follows from equation (15) and $\beta < 0$ that $\Phi(1 - \beta) < 0$, meaning that the ratio of marginal utilities of first and second period consumption falls as the price p increases towards unity. Then by concavity of the utility function, the ratio of first period consumption to second period consumption increases as the subsidy decreases (that is, price p increases towards one). □

The predictions of theorem 1 and 2 rationalize the patterns of consumption found in the empirical section as optimal behaviour of a reference-dependent (time-inconsistent) consumer with a salience cost to present consumption. In qualitative terms, these predictions are observationally equivalent to those of Arrow (1975), who presents a model of health care as a durable good.

D Supplementary Tables and Figures

In this appendix we provide some additional tables and figures in order to clarify identification and demonstrate the robustness of our results to different measurement decisions.

Figure 4 plots the years for which three- and five-year enrollment term cohorts were active in different cities. This shows where the statistical power comes from to separately identify contract year and calendar year fixed effects. Within the five-year term sample there is not much variation, with substantially more variation in the three-year sample. Combining both subsamples substantially increases power not only because of increased sample size, but because of additional variation in the timing of deadline years.

Figures 5 and 6 plot average spending by contract year and enrollment term, reproducing figure 1 for the disaggregated categories of healthcare spending. The deadline effect is significant for all categories except inpatient and mental health spending, while the postive initial year effect is significant for supplies spending only. Mental health spending exhibits a negative initial year effect.

Tables 13 to 15 report the pairwise arc elasticity estimates that are used to calculate average arc elasticity estimates for the main results. Table 16 calculates pairwise arc elasticities for total spending, which necessitates pooling price groups in plans. The differences in average arc elasticity estimates between the restricted model 1 that omits plan-deadline interaction effects and out preferred specification 6 that controls for these effects is reported in the final panel. These differences are not significant as usual levels. Finally, table 17 reports differences in estimated plan effects between the two specifications. These are not significant at usual levels.

Figure 4: Calendar years for which the HIE was active, by city

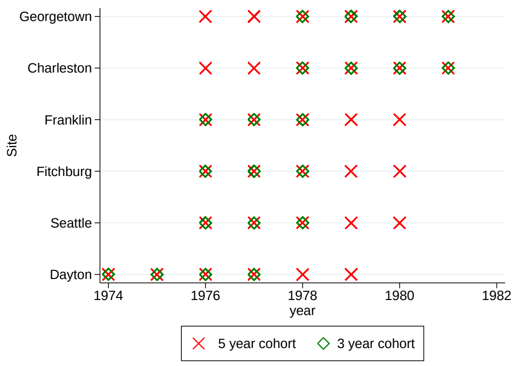
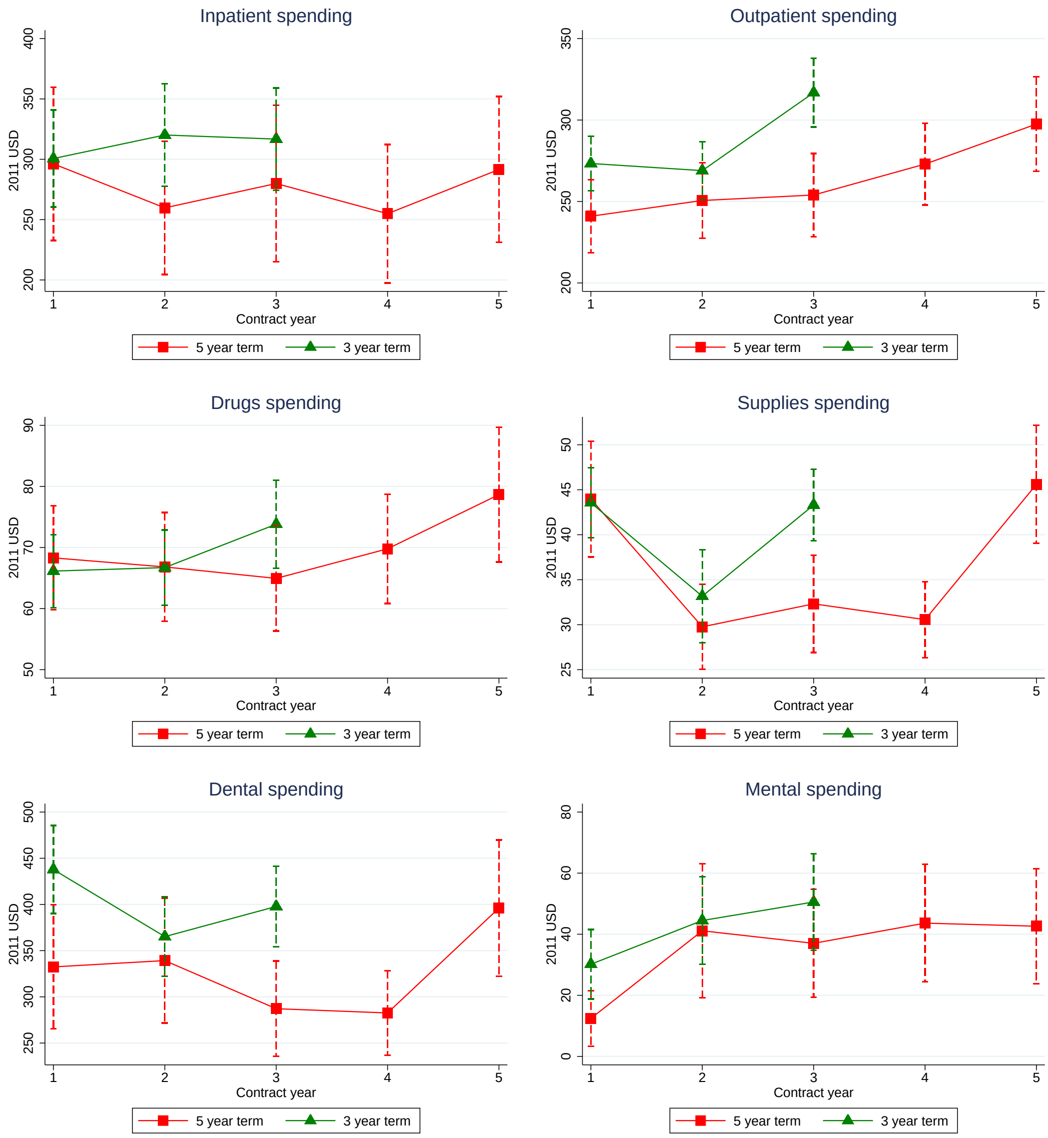
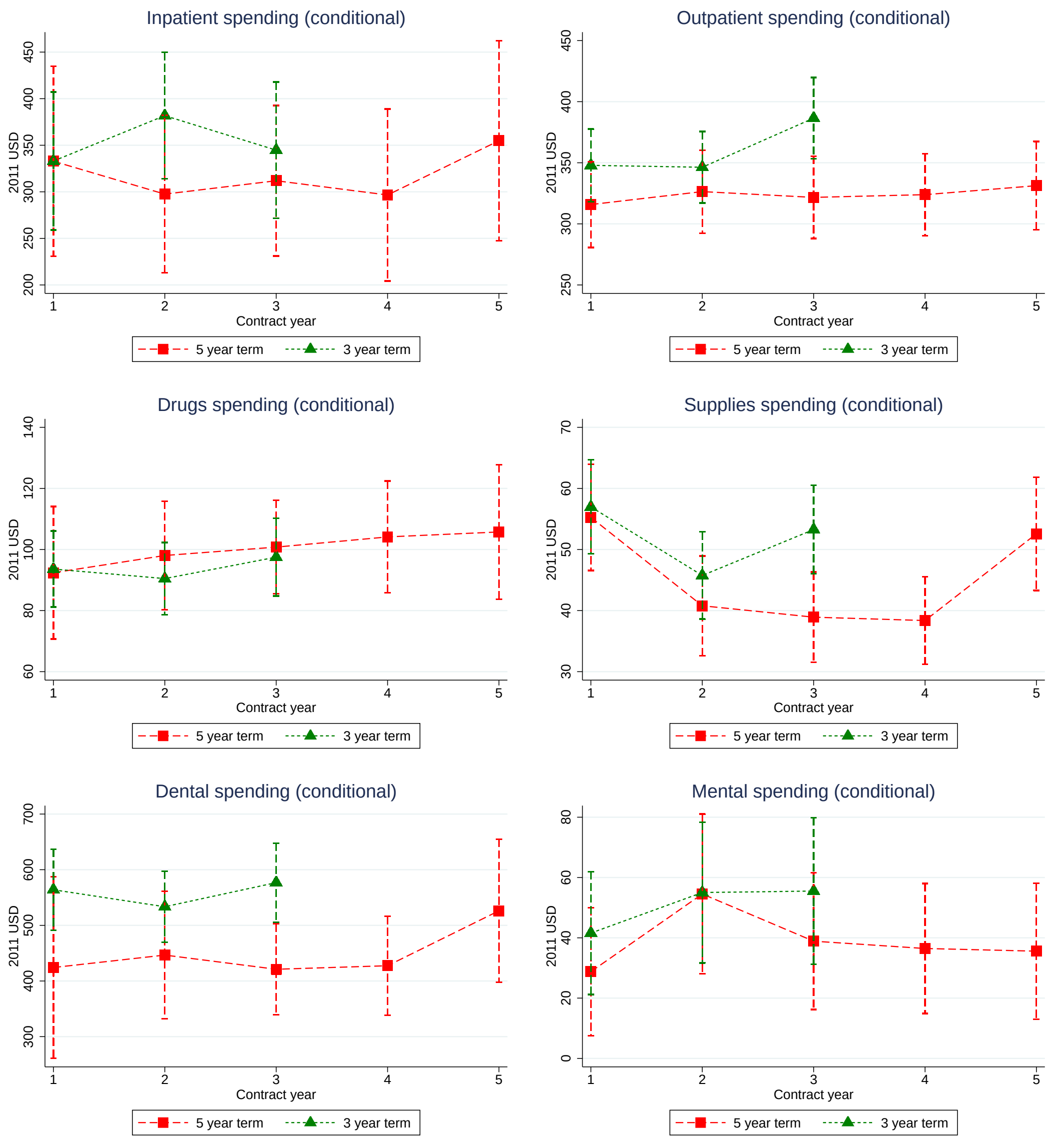


Figure 5: Unconditional Average Spending by Care Category



Plotted are series of uncoditional average spending by year for three- and five-year enrollment groups, constant 2011 USD. All figures are calculated for the restricted sample excluding outliers.

Figure 6: Spending Fixed Effects by Care Category



This figure plots fixed effects given by $\hat{\gamma}_c$ recovered from model (7), conditional on the full set of controls. All figures are calculated for the restricted sample excluding outliers.

Table 13: Sensitivity of Arc Elasticity Estimates to Price-deadline Interaction Effects

	Inpatient care				Outpatient care		
	Model (1)				Model (1)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.09	-.133	-.113	Free care	-.127	-.166	-.205
	(.042)	(.1)	(.067)		(.032)	(.038)	(.023)
25% coins.		-.13	-.039	25% coins.		-.12	-.139
		(.245)	(.171)			(.121)	(.063)
50% coins.			.066	50% coins.			-.132
			(.389)				(.081)
	Model (6)				Model (6)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.062	-.159	-.121	Free care	-.117	-.154	-.197
	(.062)	(.093)	(.049)		(.031)	(.044)	(.027)
25% coins.		-.293	-.101	25% coins.		-.112	-.139
		(.169)	(.157)			(.153)	(.077)
50% coins.			.125	50% coins.			-.141
			(.389)				(.069)
	Difference model (1) - (6)				Difference model (1) - (6)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.028	.026	.008	Free care	-.009	-.011	-.009
	(.024)	(.038)	(.03)		(.012)	(.013)	(.009)
25% coins.		.163	.062	25% coins.		-.007	0
		(.162)	(.068)			(.076)	(.031)
50% coins.			-.059	50% coins.			.009
			(.087)				(.042)

This table reports pairwise arc elasticity estimates recovered from models (1) and (6), using the full set of controls, and the difference between estimates across models. Arc elasticity is the percentage change in y for a percentage change in x, calculated relative to the midpoints; $\epsilon_{yx} \equiv \frac{y_2-y_1}{(y_2+y_1)\frac{1}{2}} / \frac{x_2-x_1}{(x_2+x_1)\frac{1}{2}}$. Standard errors are bootstrapped based on 500 replications.

Table 14: Sensitivity of Arc Elasticity Estimates to Price-deadline Interaction Effects

	Drugs				Supplies		
	Model (1)				Model (1)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.235	-.35	-.269	Free care	-.124	-.114	-.25
	(.027)	(.124)	(.033)		(.032)	(.117)	(.032)
25% coins.		-.374	-.062	25% coins.		.031	-.222
		(.491)	(.071)			(.353)	(.033)
50% coins.			.287	50% coins.			-.45
			(.527)				(.318)
	Model (6)				Model (6)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.215	-.34	-.242	Free care	-.135	-.102	-.251
	(.032)	(.131)	(.034)		(.036)	(.117)	(.034)
25% coins.		-.404	-.05	25% coins.		.1	-.206
		(.505)	(.077)			(.304)	(.031)
50% coins.			.342	50% coins.			-.493
			(.531)				(.292)
	Difference model (1) - (6)				Difference model (1) - (6)		
	25% coins.	50% coins.	95% coins.		25% coins.	50% coins.	95% coins.
Free care	-.021	-.01	-.027	Free care	.011	-.012	.001
	(.007)	(.012)	(.005)		(.021)	(.026)	(.014)
25% coins.		.031	-.012	25% coins.		-.069	-.016
		(.033)	(.017)			(.076)	(.032)
50% coins.			-.055	50% coins.			.043
			(.04)				(.1)

This table reports pairwise arc elasticity estimates recovered from models (1) and (6), using the full set of controls, and the difference between estimates across models. Arc elasticity is the percentage change in y for a percentage change in x, calculated relative to the midpoints; $\epsilon_{yx} \equiv \frac{y_2 - y_1}{(y_2+y_1)\frac{1}{2}} / \frac{x_2 - x_1}{(x_2+x_1)\frac{1}{2}}$. Standard errors are bootstrapped based on 500 replications.

Table 15: Sensitivity of Arc Elasticity Estimates to Price-deadline Interaction Effects

Dental care			
	Model (1)		
	25% coins.	50% coins.	95% coins.
Free care	-.201	-.249	-.291
	(.024)	(.029)	(.035)
25% coins.		-.152	-.163
		(.164)	(.088)
50% coins.			-.144
			(.117)
	Model (6)		
	25% coins.	50% coins.	95% coins.
Free care	-.201	-.249	-.278
	(.027)	(.045)	(.048)
25% coins.		-.149	-.139
		(.192)	(.12)
50% coins.			-.102
			(.142)
	Difference model (1) - (6)		
	25% coins.	50% coins.	95% coins.
Free care	0	-.001	-.013
	(.021)	(.018)	(.022)
25% coins.		-.003	-.024
		(.051)	(.069)
50% coins.			-.041
			(.095)

Mental health			
	Model (1)		
	25% coins.	50% coins.	95% coins.
Free care	-.124	-.123	-.109
	(.214)	(.27)	(.105)
25% coins.		.002	.025
		(.623)	(.314)
50% coins.			.046
			(.904)
	Model (6)		
	25% coins.	50% coins.	95% coins.
Free care	-.1	-.037	-.064
	(.191)	(.311)	(.109)
25% coins.		.191	.063
		(.712)	(.291)
50% coins.			-.087
			(1.091)
	Difference model (1) - (6)		
	25% coins.	50% coins.	95% coins.
Free care	-.024	-.086	-.046
	(.046)	(.065)	(.042)
25% coins.		-.189	-.038
		(.138)	(.131)
50% coins.			.132
			(.266)

This table reports pairwise arc elasticity estimates recovered from models (1) and (6), using the full set of controls, and the difference between estimates across models. Arc elasticity is the percentage change in y for a percentage change in x, calculated relative to the midpoints; $\epsilon_{yx} \equiv \frac{y_2-y_1}{(y_2+y_1)\frac{1}{2}} / \frac{x_2-x_1}{(x_2+x_1)\frac{1}{2}}$. Standard errors are bootstrapped based on 500 replications.

Table 16: Sensitivity of Arc Elasticity Estimates to Plan-deadline Interaction Effects

Panel A: Pairwise plan elasticities of total spending – model (1)					
	25% plan	Mixed c.	50% plan	Indiv. d.	95% plan
Free care	-.158	-.166	-.207	-.161	-.26
	(.042)	(.018)	(.049)	(.024)	(.02)
25% plan		-.073	-.152	-.009	-.183
		(.426)	(.17)	(.153)	(.093)
Mixed c.			-.186	.018	-.196
			(.181)	(.061)	(.072)
50% plan				.965	-.182
				(1.118)	(.217)
Indiv. d.					-.39
					(.121)

Panel B: Pairwise plan elasticities of total spending – model (6)					
	25% plan	Mixed c.	50% plan	Indiv. d.	95% plan
Free care	-.158	-.137	-.207	-.157	-.252
	(.057)	(.017)	(.054)	(.021)	(.015)
25% plan		.191	-.152	.003	-.167
		(.542)	(.155)	(.188)	(.106)
Mixed c.			-.317	-.075	-.237
			(.198)	(.061)	(.063)
50% plan				1.058	-.152
				(1.132)	(.225)
Indiv. d.					-.373
					(.114)

Panel C: Observation-weighted arc elasticities of total spending			
	Model (1)	Model (6)	Difference
All plans	-.111	-.098	-.013
	(.086)	(.096)	(.012)
Excluding free care	-.04	-.023	-.017
	(.17)	(.195)	(.03)
Excluding free care & indiv. d.	-.165	-.14	-.025
	(.097)	(.113)	(.044)

Panels A and B report pairwise arc elasticities, calculated based on plan effects from table 3 (using the full set of controls). For plans with multiple coinsurance rates, we take average coinsurance rates weighted by spending within a category at the plan level. This gives an average coinsurance rate of 31% and 55% for the mixed and individual deductible plans respectively. Panel C reports observation-weighted averages of the elasticities in panels A and B, as well as their difference. Standard errors are bootstrapped based on 500 replications.

Table 17: Testing Robustness of Plan Effects to Inclusion of Plan-Deadline Interaction Effects

25% plan	-13.87	.65
Mixed c.	-73.89	.02
50% plan	-17.29	.59
Indiv. d.	-22.83	.38
95% plan	-35.7	.16
Joint test		.33
Free care	50.75	.01
25% plan	36.88	.18
Mixed c.	-23.14	.45
50% plan	33.46	.25
Indiv. d.	27.92	.21
95% plan	15.05	.48
Joint test		.13

This table reports p-values for the hypothesis test that $\hat{\lambda}_p = \tilde{\lambda}_p$, where $\hat{\lambda}_p$ is an estimate of a plan fixed effect in the baseline model without plan-deadline interaction effects, given by equation (1), and $\tilde{\lambda}_p$ estimates plan effects from the model with interaction effects, given by equation (6). The bottom row reports the p-value for the joint test of equality between each corresponding element of $\hat{\lambda}_p$ and $\tilde{\lambda}_p$.

UCD CENTRE FOR ECONOMIC RESEARCH – RECENT WORKING PAPERS

WP18/10 Morgan Kelly and Cormac Ó Gráda: 'Gravity and Migration before Railways: Evidence from Parisian Prostitutes and Revolutionaries' June 2018
WP18/11 Kevin Denny: 'Basic Stata Graphics for Economics Students' July 2018
WP18/12 Ronald B Davies and Joseph Francois: 'Irexit: Making the Worst of a Bad Situation' July 2018
WP18/13 Ronald B Davies: 'From China with Love: The Role of FDI from Third Countries on EU Competition and R&D Activities' July 2018
WP18/14 Arnaud Chevalier, Benjamin Elsner, Andreas Lichter and Nico Pestel: 'Immigrant Voters, Taxation and the Size of the Welfare State' August 2018
WP18/15 Michael Spagat and Stijn van Weezel: 'On the Decline of War' August 2018
WP18/16 Stijn van Weezel: 'Apocalypse Now? - Climate Change and War in Africa' August 2018
WP18/17 FM Kiernan: 'The Great Recession and Mental Health: the Effect of Income Loss on the Psychological Health of Young Mothers' October 2018
WP18/18 Ronald B. Davies and Neill Killeen: 'The Effect of Tax Treaties on Market Based Finance: Evidence using Firm-Level Data' October 2018
WP18/19 George Sorg-Langhans, Clemens Struck, and Adnan Velic: Solving Leontief's Paradox with Endogenous Growth Theory
WP18/20 Alan Fernihough, Cormac Ó Gráda: Population and Poverty in Ireland on the Eve of the Great Famine
WP18/21 Cormac Ó Gráda: The Next World and the New World: Relief, Migration, and the Great Irish Famine
WP18/22 Lisa Ryan, Sarah La Monaca, Linda Mastrandrea and Petr Spodniak: 'Harnessing Electricity Retail Tariffs to Support Climate Change Policy' December 2018
WP18/23 Ciarán Mac Domhnaill and Lisa Ryan: 'Towards Renewable Electricity in Europe: An Empirical Analysis of the Determinants of Renewable Electricity Development in the European Union' December 2018
WP19/01 Ellen Ryan and Karl Whelan: 'Quantitative Easing and the Hot Potato Effect: Evidence from Euro Area Banks' January 2019
WP19/02 Kevin Denny: 'Upper Bounds on Risk Aversion under Mean-variance Utility' February 2019
WP19/03 Kanika Kapur: 'Private Health Insurance in Ireland: Trends and Determinants' February 2019
WP19/04 Sandra E Black, Paul J Devereux, Petter Lundborg and Kaveh Majlesi: 'Understanding Intergenerational Mobility: The Role of Nature versus Nurture in Wealth and Other Economic Outcomes and Behaviors' February 2019
WP19/05 Judith M Delaney and Paul J Devereux: 'It's not just for boys! Understanding Gender Differences in STEM' February 2019
WP19/06 Enoch Cheng and Clemens Struck: 'Time-Series Momentum: A Monte-Carlo Approach' March 2019
WP19/07 Matteo Gomellini and Cormac Ó Gráda: 'Brain Drain and Brain Gain in Italy and Ireland in the Age of Mass Migration' March 2019
WP19/08 Anna Aizer, Paul J Devereux and Kjell G Salvanes: 'Grandparents, Mothers, or Fathers? - Why Children of Teen Mothers do Worse in Life' March 2019
WP19/09 Competing Gains From Trade

UCD Centre for Economic Research Email economics@ucd.ie