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Basic Income as a Policy Lever: A Case Study of Crime in Alaska

Richard Dorsett
Westminster Business School, University of Westminster, UK

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BASIC INCOME AS A POLICY LEVER: A CASE STUDY OF CRIME IN ALASKA

Richard Dorsett*

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Abstract

Since 1982, Alaska's Permanent Fund has provided an annual dividend to all state residents, the world's longest-running example of a basic income. Initially universal, from 1989 onwards eligibility was withdrawn from an increasing proportion of those in prison. This paper uses two evaluation approaches – synthetic control and Bayesian structural time series – to evaluate the impact of this payment on crime, in particular property crime. Neither approach can detect a significant effect, either before or after the change to eligibility. Despite this, the results provide evidence that the size of the payment is relevant, with larger amounts significantly reducing property crime. There is no evidence that this effect is reinforced by the change to the rules governing eligibility. The results demonstrate the potential for a basic income to encourage positive outcomes and lend support to payment being universal rather than conditional.

JEL Codes: H24, I38, J18, K42
Key Words: Crime, Alaska Permanent Fund, basic income, conditionality

*University of Westminster, 35 Marylebone Road, London NW1 5LS, United Kingdom. Email: `r.dorsett@westminster.ac.uk`

1 Introduction

The idea of a universal basic income has a long history. The *Basic Income Earth Network* (BIEN)[1] attributes the first recorded mention to More (1516 [1963]) and the first detailed description of a guaranteed minimum income to Vives (1526 [1990]). Over subsequent centuries, the notion has intermittently come into intellectual focus and recent decades have seen renewed interest in the idea. U.S. presidential candidate Hillary Clinton, for example, considered (but rejected) including a basic income proposal as part of her 2016 campaign Clinton (2017). U.N. Secretary-General António Guterres suggested in 2018 to the General Assembly that governments may have to consider a universal basic income.[2] In Britain, the opposition Labour party has announced that it will include a basic income in its next general election manifesto[3] and a recent report considers how an initial pilot may operate (Standing 2019).

Advocates highlight its simplicity; in principle, a universal payment could replace – at least in part – a complex system of welfare payments (Murray 2016). Furthermore, they view the payment as providing a means of addressing problems of poverty and inequality. A basic income provides a means of ensuring that the proceeds of growth, increasingly driven by technology and automation rather than labour, can be more evenly distributed and can therefore offset the long-term increase in earnings inequality (Autor 2014). Critics, on the other hand, worry about the income effect of a basic income on incentives to work. Furthermore they argue that any ambition to replace welfare systems

[1] `https://basicincome.org`

[2] General debate of the 73rd Session of the General Assembly of the UN (New York, 25 September - 01 October 2018)
`http://webtv.un.org/watch/secretary-general-addresses-general-debate-73rd-session/5839802857001/`

[3] `https://www.businessinsider.com/labour-to-include-universal-basic-income-pilot-next-manifesto r=US&IR=T`

with a basic income would cost so much as to be non-viable. Hoynes & Rothstein (2019) show that to make available in the U.S. a universal payment sufficiently large to live on without other earnings would be enormously expensive.

Of course, the impact of a basic income depends on how it is defined and implemented. One definition with some level of recognition is ‘a periodic cash payment unconditionally delivered to all on an individual basis, without means-test or work requirement’.[4] This leaves unstated the size of the payment or, more generally, the standard of living it should be sufficient to support. It is useful to distinguish between a full basic income, designed to free individuals from poverty and allow full social inclusion, and a partial basic income which falls short of that and is more of an income supplement. A full basic income could replace existing state benefits. Under a partial basic income, the interaction with the tax and welfare system becomes particularly relevant and provides a link to the negative income tax experiments carried out between 1968 and 1982 in the U.S. and Canada (Robins 1985, Hum & Simpson 1993).

Recently, trials in Namibia (Haarmann 2009) and India (Bharat & UNICEF 2014) have demonstrated the poverty-reduction potential of a universal basic income in a developing country context, and this evidence base will be augmented by the ongoing *Give Directly* trial in Kenya.[5] The most prominent recent example in a developed country is Finland which tested a basic income for recipients of unemployment benefits aged 25-58.[6] No effect was found on employment but there was a positive effect on well-being and attitudes such as trust in institutions (Kangas et al. 2019). Canada was trialling a negative income tax for low-income people but this was cut short in March

[4] `https://basicincome.org/basic-income/`
[5] `https://www.givedirectly.org/basic-income`
[6] `https://www.kela.fi/web/en/basic-income-experiment-2017-2018`

2019.[7] In the U.S., a small trial is underway in Stockton, California[8] and a larger experiment is planned.[9]

The best example of a universal and unconditional basic income in the developed world is the Alaska Permanent Fund Dividend. When introduced in 1982, it provided a payment to all citizens of Alaska. Van Parijs (2004) describes Alaska as 'the only political unit that has ever introduced a genuine basic income'. As such, it provides unique opportunities for learning about long-term impacts.

However, even in the Alaskan case, changes over time in the eligibility criteria may call into question the basis for its claim to be universal. From 1989, individuals who had been in prison on felony charges at some point in the year were no longer able to receive payment for that year (this rule was later broadened to include misdemeanants). Van Parijs (2004) describes as 'obvious' that inmates should not receive a payment, on the grounds that incarceration costs exceed basic income costs. However, it is relevant to observe that there are other subgroups within the population whose net economic contribution is non-positive (children, pensioners) yet who remain eligible. Furthermore, the change to eligibility raises issues of a legal nature, particularly around criminals' position in the political community Griffin (2012).

This paper explores the effect of the Dividend on crime. For the first few years of its existence, when the Dividend was truly universal, one might expect the increase in individuals' income to reduce incentives to engage in criminal activity, perhaps reinforced by the societal reduction in inequality. From 1989 onwards, one might further expect that the the penalty associated with incarceration would act as a deterrent to offending.

[7] https://www.ontario.ca/page/ontario-basic-income-pilot
[8] https://www.stocktondemonstration.org/
[9] https://basicincome.ycr.org/our-plan

The analysis in the paper distinguishes between these two phases and therefore offers evidence on the broader question of how income and income penalties affect crime.

The primary outcome considered is property crime. This choice of focus has the simple rationale that it is with property crime that the most direct economic incentive exists. Sub-categories of property crime (burglary, larceny, vehicle theft) are also examined in order to provide additional insight. Furthermore, violent crime and its sub-categories are likewise included in the analysis. This is not just for completeness (although this does mean the analysis uses all outcomes for which data are available) but because robbery, despite being classified as a violent crime, has a clear economic motivation.

A small number of papers have looked at various effects of the Alaskan Dividend. Hsieh (2003) examined individuals' consumption responses, Evans & Moore (2011) looked at mortality rates around the time of receipt and Goldsmith (2012) considered possible economic and social effects more broadly, while noting the absence of robust evidence. More recently, Jones & Marinescu (2018) examined the impact on employment, finding no effect.[10]

Only one paper has looked at the effect of the Dividend on crime. Watson et al. (2019) used daily police reporting data in Anchorage between 2000 and 2016 to examine how crime changes around the time of Dividend payment. They showed an increase in substance abuse incidents on the day following receipt of the Dividend payment which, since the precise timing of payment is quasi-random, can be interpreted as causal. No such 'day-after' effect was found for property crime but a significant reduction over the

[10]This is consistent with Akee et al. (2010) who examined the effect of an unconditional transfer from casino profits to Cherokee Indians.

two weeks following payment day was detected.

While Watson et al. (2019) considered the timing effect of Dividend payment, they did not estimate the effect of the Dividend itself. In their analysis, the Dividend exists both before and after the day of receipt so the effect of the Dividend – as opposed to the effect of payment receipt – cannot be estimated.[11] The aim of this paper is to estimate how the *existence* of the Dividend and the change to eligibility affects crime, in particular, property crime. It does this by comparing observed crime to an estimate of crime in the counterfactual scenario of an Alaska without the Dividend. This is the first time such an analysis has been attempted.

To preview the results, the analysis does not find evidence that the introduction of the Dividend, nor the later introduction of conditionality, significantly reduced property crime. This is true for both empirical strategies used: the synthetic control approach of Abadie & Gardeazabal (2003) and the Bayesian structural time series approach of Brodersen et al. (2015). The estimates are highly imprecise. This is particularly the case with the Bayesian results and reflects the fact that those estimates allow for model uncertainty and are arguably to be preferred for that reason. Of course, with both approaches consistent in finding non-significant impacts, this preference is of little practical relevance. However, an advantage of the Bayesian approach is that it provides not just one time series of counterfactual outcomes but multiple such time series, and therefore multiple time series of estimated impacts. Looking across all such series reveals a significant effect of Dividend size on estimated impact. Before the change to eligibility for prisoners, a one-off increase in the Dividend would reduce property crime for at least two years. After the change to the eligibility rule, the impact was smaller and possibly

[11] Furthermore, their data does not extend to pre-1982 when the Dividend was introduced.

shorter-lived.

The findings of this paper offer novel evidence on the impact of a basic income on crime. In so doing, they highlight a potential benefit of basic income that has not so far been fully explored. Furthermore, the results appear, if anything, to be stronger without the conditionality that was introduced by the 1989 change to eligibility for prisoners. The results provide no support for the intuition that such conditionality should deter crime but rather provide an argument for retaining the principle of universality.

The remainder of the paper has the following structure. In Section 2, the Alaska Permanent Fund is described in more detail. Trends in Alaskan crime are presented and compared to the other states and the U.S. as a whole. Section 3 sets out the theoretical framework and provides a formal articulation of the expected effect of introducing conditionality. Section 4 describes how impacts were estimated. The estimated impacts are presented and further analysed in Section 5. Section 6 concludes.

2 Background - the Alaska Permanent Fund and crime in Alaska

2.1 The Alaska Permanent Fund

In 1976, the Alaska Permanent Fund was established through an amendment to the Alaska state constitution. Each year, a proportion of Alaskan oil revenues is diverted into the Fund. Since 1982, the Fund has paid a Dividend to every resident Alaskan.

The size of the Dividend varies each year. The total available for distribution is calculated as the Fund balance at the time of calculation, plus Fund earnings averaged over the previous five years, less appropriations and reductions. This is then divided by the estimated number of eligible applicants to give the size of the Dividend. The formula is public knowledge but relies on some elements that are unrealised until the time of calculation and one, the number of eligible applicants, that must be estimated. The Fund Commissioner must announce the value of the Dividend by October 1 of each year. Citizens have a rough sense of the likely size of the payment before the announcement since the Fund Corporation provides estimates some months in advance.

Figure 1 shows how the nominal value of the Dividend has varied since its introduction in 1982. It was initially set at $1,000 but was considerably smaller in 1983, after which it followed a mostly upward trend until 2000 ($1,963). Its value has fluctuated since then, but reached its record level in 2008, when the Dividend of $2,069 was boosted by a one-time 'Resource Rebate' supplement of $1,200, bringing the total amount that year to $3,269. In considering the size of the Dividend, it should be noted that parents can claim on behalf of their children. Consequently, families with children will receive multiple Dividends each year.

While initially a universal payment, eligibility restrictions for those in prison were introduced in 1989 and strengthened thereafter. Since 1989, those incarcerated on felony charges during the year to which the Dividend relates (the reference year) have been ineligible. In 1996, misdemeanants in the reference year who had two prior crimes became ineligible.[12] In 2002, misdemeanants in the reference year who had one prior felony or two prior misdemeanours also became ineligible. It should be noted that the children of ineligible adults themselves become ineligible. Consequently, the financial

[12] A felony is a crime for which a sentence of imprisonment for a term of more than one year is authorised. A misdemeanour is a crime for which a sentence of imprisonment for a term of more than one year may not be imposed.

penalty deriving from the change to eligibility may be greater where the sentenced or incarcerated individual has dependent children.

[Figure 1 about here.]

2.2 Crime and imprisonment in Alaska

State-level crime data is available from the Federal Bureau of Investigation Uniform Crime Reports, covering the period from 1960 to 2017.[13] In all cases, crimes are expressed relative to the state population; the number of reported crimes per 100,000 residents. Fuller details on data sources are given in Appendix A.

The principle focus in this paper is on property crime rather than violent crime since this is more likely to be economically-driven and therefore responsive to the threat of reduced income. Property crime itself covers burglary, larceny-theft and motor vehicle theft. Results for violent crime – murder, rape, aggravated assault and robbery – are also shown. There are two reasons for this. First, while not so readily accommodated within a model of incentives as property crime, violent crime is nevertheless liable to the influence of socio-economic conditions and circumstances. Second, robbery falls within the category of violent crime despite sharing the defining characteristic of a property crime.

Figure 2 shows crime trends over the period 1960 to 2017, displaying both the Alaskan crime rates, the rates for the U.S. as a whole and the rates for individual states. For reference, vertical lines are included at 1982 (the year of the first Dividend) and 1989 (the year conditionality was introduced for criminals). Property crime as a whole grew

[13] Note that data are missing for New York state between 1960 and 1964, so these years are excluded from the impact analysis.

in Alaska until the early 1980s and then steadily declined. This was broadly the pattern seen in the U.S. as a whole, although the rates in Alaska were relatively elevated during the peak years around 1980. Alaskan trends were within the range seen across other states. Looking at types of property crime allows a more detailed insight and highlights differences between Alaska and the U.S. as a whole. Alaska has tended to have lower rates of burglary and higher rates of larceny. Vehicle theft was initially higher in Alaska but this difference mostly disappeared after the mid 1980s.

With regard to violent crime, this grew in both Alaska and the U.S. until the early 1990s. There was then a reversal in both cases but this was temporary in the case of Alaska where, after 2000, violent crime reverted to its upward trend, thereby diverging from the U.S. as a whole. For much of this period, Alaska did not look notably different from other states. Since about 2000, though, its ranking has grown to the point where it had the highest rate of violent crime of any U.S. state in 2017. In more detail, murder was initially relatively high in Alaska but converged to roughly the U.S. rate from the mid-1980s. Rape, on the other hand, has consistently been higher in Alaska than the U.S., and this difference has increased over time.[14] Aggravated assault grew roughly in line with the national trend but the sustained decline that began in the early 1990s in the U.S. was not matched in Alaska. It is this category that dominates the trends in violent crime in the U.S. as a whole. Lastly, rates of robbery have mostly been considerably lower in Alaska than in the U.S. as a whole, although again that has changed in more recent years.

[Figure 2 about here.]

[14] The definition of rape used in this paper changed in 2016. See Appendix A for details.

3 The expected effect of the Dividend

Becker (1968) introduced the first economic theory of crime. He conceptualised individuals' offending behaviour as a function of the probability of conviction, the penalty associated with conviction and a range of other background factors such as education, civic values, income and so on. Individuals make a decision regarding whether to engage in criminal activity based on whether the expected benefits exceed the expected costs.

Lee & McCrary (2009) make explicit the dynamic nature of decision-making, allowing individuals' decisions to be based on a comparison of the relative expected utility flows from criminal and non-criminal activity over an extended period. Analogous to the reservation wage in a standard job search model (McCall 1970), they posit a "reservation threshold"; criminal opportunities offering a payback above this level will be taken. It follows from their model that the attraction of crime is lower among individuals for whom the per-period utility cost is higher and individuals with higher discount factors (for those who value the future more, lengthy punishment imposes a higher cost). It is also negatively related to sentence length and the probability of being caught.

In the Alaskan case, the introduction of the Dividend served to increase individuals' income and reduce inequality. It would therefore be expected, if anything, to reduce property crime (see, for instance, Blakeslee & Fishman (2018) and Choe (2008)). The 1989 change to rules governing Dividend eligibility increased the financial penalty associated with conviction. Consistent with the theoretical models, this might therefore be expected to reduce crime. The extent to which this will hold depends on the Dividend amount. To make this explicit, equation (1) gives the objective function for an individual deciding whether to commit a crime

$$G = \begin{cases} (1-p(s))\,B(s) - p(s)\,(C+D) & \text{if } \delta = 0 \\ (1-p(s))\,B(s) - p(s)\,(C+D)\,y(s) & \text{if } \delta = 1 \\ (1-p(s))\,B(s) - p(s)\,(C+D)\left(\frac{1-\delta^{y(s)}}{1-\delta}\right) & \text{if } 0 < \delta < 1. \end{cases} \quad (1)$$

Here, G is the expected gain from crime, $p(s)$ is the probability of being convicted for the crime, $B(s)$ is the reward if the crime is successful, C is the cost to the individual of incarceration, D is the Dividend amount, δ is the discount factor and $y(s)$ is the length of sentence. Hence, the penalty if imprisoned includes the (monetised) disutility from the incarceration itself plus the lost Dividend payments for the number of years imprisoned.

The severity of the crime (or perhaps the number of crimes) is denoted by s and is allowed to influence the reward, the probability of conviction and the length of sentence. The first derivatives are assumed to be positive. The intuition behind this assumption in the case of property crime is as follows. Larger thefts (higher value of s) will have a higher payback if successful. Hence, $B'(s) > 0$. They may also be more likely to result in arrest than smaller thefts, if limited resources demand that police efforts be concentrated on the most significant cases. Hence, $p'(s) > 0$. If convicted, larger thefts are likely to attract longer sentences than smaller thefts.[15] Hence, $y'(s) > 0$.

For a given value of the Dividend, the level of s that maximises G is the solution to the first order condition

[15] Alaskan law classifies theft of between \$500 and \$25,000 as a class C felony, attracting a prison sentence of up to five years and a fine of up to \$50,000 (Alaska Statute §11.46.130) while larger thefts are treated as class A felonies, attracting a sentence of up to 10 years and a fine of up to \$100,000 (§11.46.120). Smaller thefts are regarded as misdemeanours. Thefts of less than \$50 represent a class B misdemeanour, carrying a term of imprisonment of up to 90 days, plus a fine of up to \$2,000 (§12.55.135(b), §12.55.035(b)(6)). Thefts of between \$50 and \$500 can receive a prison term of up to 1 year and a fine of up to \$10,000 ((§12.55.135(a), §12.55.035(b)(5)).

$$\frac{dG}{ds} = 0. \tag{2}$$

The maximising level of s varies with D. By the implicit function theorem,

$$\frac{ds}{dD} = -\frac{d^2G/dsdD}{d^2G/ds^2}. \tag{3}$$

Since the objective function is maximised, its second derivative (the denominator on the right hand side of equation (3)) is negative so ds/dD has the same sign as $d^2G/dsdD$. Writing this in full,

$$\frac{d^2G}{dsdD} = \begin{cases} -\frac{dp(s)}{ds} & \text{if } \delta = 0 \\ -\frac{dp(s)}{ds}y(s) - p(s)\frac{dy(s)}{ds} & \text{if } \delta = 1 \\ -\frac{1}{1-\delta}\left(\frac{dp(s)}{ds}\left(1-\delta^{y(s)}\right) - p(s)\frac{dy(s)}{ds}\delta^{y(s)}\ln\delta\right) & \text{if } 0 < \delta < 1 \end{cases} \tag{4}$$

Given the assumptions about the signs of the first derivatives, equation (4) is negative and so equation (3) is also negative. When $\delta \in \{0, 1\}$, this negativity is obvious. While these scenarios are special cases, they have quite plausible interpretations. For instance, $\delta = 0$ corresponds to the case where the individual is focused purely on the short-term. The $\delta = 1$ scenario, on the other hand, implies future income is valued just as highly as current income. This may be a reasonable approximation to the decision parameter sub-consciously used by an individual sensitive in a vague way to the longer-term consequences of conviction. For the more general case of $0 < \delta < 1$, the negative sign follows from the fact that $\ln\delta < 0$.

Overall, the negative sign of equation (3) makes intuitive sense; when the Dividend amount is higher, agents stand to lose more from being convicted of a crime and so are likely to be deterred from participating in criminal activity. In principle then, making criminals ineligible for the Dividend provides a lever to policy-makers concerned with reducing crime.

4 Estimation approach

To estimate the impact of the Dividend on crime requires an estimate of how criminal activity would have evolved in Alaska had it not been introduced. A comparison of actual outcomes in Alaska with these estimated counterfactual outcomes can provide an unbiased estimate of the impact of the Dividend. Two different strategies are used to estimate the counterfactual: the synthetic control approach of Abadie & Gardeazabal (2003) and Abadie et al. (2010) and the Bayesian structural time series approach of Brodersen et al. (2015). The key features of these approaches are set out below. This is intended to provide an appreciation of their suitability for this analysis rather than to reproduce their full detail, for which the referenced sources are appropriate.

4.1 Synthetic control

The synthetic control approach operates by constructing a control region – in this case, a synthetic Alaska – as a weighted sum of other U.S. states. The synthetic control is not affected by the Dividend (since it does not operate in any of its component states), so it can be used to provide an estimated counterfactual outcome.

Following the notation of Abadie et al. (2010), write the outcome that would prevail in state i at time t in the absence of the intervention (that is, in the absence of the

Dividend) as Y_{it}^N. There are T time periods and T_0 pre-intervention periods, $1 \leq T_0 < T$. Denote by Y_{it}^I the outcome that would prevail in state i at time t were the intervention introduced at time $T_0 + 1$. It is assumed that $Y_{it}^I = Y_{it}^N$, $\forall i$ for $t \in \{1, ..., T_0\}$. Impacts are written $\alpha_{it} = Y_{it}^I - Y_{it}^N$. Setting the intervention state (Alaska) to be $i = 1$, Y_{1t}^I is observed and the impact is $\alpha_{1t} = Y_{1t} - Y_{1t}^N$.

Estimating α_{1t} requires an estimate of the counterfactual outcome Y_{1t}^N, $t > T_0$. This assumes an underlying model

$$Y_{it}^N = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \tag{5}$$

where δ_t is an unknown common factor, Z_i is a $(r \times 1)$ vector of observed covariates (not affected by the intervention), θ_t is a $(1 \times r)$ vector of unknown parameters, λ_t is a $(1 \times F)$ vector of unobserved common factors, μ_i is an $(F \times 1)$ vector of factor loadings and ε_{it} is a zero-mean error term.

Assume there are J states other than Alaska. The synthetic control is constructed as a weighted average of these other states, defined according to a $(J \times 1)$ vector of non-negative weights that sum to 1, $W = (w_1, ..., w_J)$, where w_j represents the weight attached to state j. Let X_0 be a $(K \times J)$ matrix which contains the values of predictors and pre-intervention outcomes at specified points in time for the J possible control states. Let V be a diagonal matrix with non-negative components reflecting the relative importance of the predictors and pre-intervention outcomes. The vector of weights W^* that minimises $(X_1 - X_0W)'V(X_1 - X_0W)$ defines the synthetic control used to provide counterfactual (no-treatment) Alaskan outcomes. In line with Abadie & Gardeazabal (2003), the preferred matrix V is chosen such that it minimises the mean squared prediction error in the pre-intervention period.

The synthetic control approach is widely-used in case study analyses of the type considered in this paper. It provides a means of estimating counterfactual outcomes – and thereby impacts – that generalises the standard fixed-effects difference-in-differences approach in that it allows the influence of unobserved confounders to vary over time. Two features need to be taken into account when assessing its suitability in a particular application. First, the restriction that weights be non-negative limits its usefulness when the treated unit is an outlier (lies outside the 'convex hull'). An attraction of the approach is that such cases can be quite readily identified through basic descriptive statistics. Specifically, it is reassuring when it can be shown that the synthetic control resembles the treated unit with regard to observed characteristics and pre-treatment trends. Second, identification relies on the number of pre-treatment periods being large relative to the scale of the transitory shocks, ε_{it}. Inspecting pre-intervention trends may help inform the judgement of whether this is achieved in practice but in doing so it should be noted that, as shown in Appendix B of Abadie et al. (2010), the potential bias derives from the volatility of transitory shocks in the J non-treated units rather than the treated unit itself.

The synthetic control analysis was implemented using the Stata program *synth*, available from `www.mit.edu/~jhainm/software.htm`.

4.2 Bayesian structural time series

The Bayesian structural time series (BSTS) approach provides an alternative means of estimating counterfactual outcomes, using a state-space model. The observation equation is

$$y_t = \mu_t + \beta' \mathbf{x}_t + \varepsilon_t \tag{6}$$

where $\mathbf{x}_t$ is a $(1 \times J)$ vector of contemporaneous outcomes in other states and $\varepsilon_t \sim \mathcal{N}(0, \sigma^2)$. The transition equation[16] shows how the trend term evolves over time as a random walk

$$\mu_{t+1} = \mu_t + \eta_t \tag{7}$$

where $\eta_t \sim \mathcal{N}(0, \tau^2)$.

As before, states other than Alaska are assumed not to be affected by the intervention. This allows the counterfactual outcomes to be estimated as predicted values from the model for $t > T_0$.

Inference involves first simulating draws of the model parameters given observed outcomes during the pre-intervention period. This is implemented using a Gibbs sampler. Second, these simulations are then used to simulate from the posterior predictive distribution of counterfactual time series over the post-intervention period. It is important to note that these posterior predictive simulations are coherent in the sense that they are defined as a joint distribution over all time periods. That is, for each draw, the counterfactual for any period is related to that of any other period through the shared time series properties unique to that draw. This allows inference for summary statistics that relate to multiple points in time, such as cumulative effects. The posterior predictive samples are used to compute the posterior distribution of impacts.

Inference requires specification of priors for σ^2 and τ^2. For the β coefficients, a

[16] More conventionally, this is the state equation. It is referred to here as the transition equation so that the word 'state' can be reserved for referring to a political unit (U.S. state).

'spike-and-slab' prior is used (Scott & Varian 2014). This data-driven approach sets to zero some elements of β such that the associated states do not contribute to the counterfactual. Regularising in this way offers protection against the risk of over-fitting. The set of states used to form the counterfactual will vary with each draw. The prior probability, π_j, of state j of J being included in the model must be specified, as must the prior expectation about β and the expected R^2.

The preferred specification uses sample variance over the pre-intervention period, s^2, as the prior for σ^2, and $0.01s^2$ as the prior for τ^2. The latter is described in the program used to fit the model as a typical choice for well-behaved and stable datasets with low residual volatility after regressing out known predictors. The prior probability of inclusion, π_j, is set to J_0/J, $\forall j$, where J_0 is the number of states used to construct the synthetic Alaska following the Abadie & Gardeazabal (2003) approach. The expected R^2 is set to the R^2 in the regression of Y_{1t}^N on $\hat{Y}_{1t}^N$, $t \in \{1, ..., T_0\}$, where $\hat{Y}_{1t}^N$ is the counterfactual series in the pre-intervention period estimated following the Abadie & Gardeazabal (2003) approach.

The BSTS analysis was implemented using the R program *CausalImpact*, available from `http://google.github.io/CausalImpact`.

4.3 A comparison of the two approaches

While the synthetic control approach is well-established in the programme evaluation literature and is perhaps the method of choice for case study analyses, the BSTS approach is less well-known. Consequently, it is useful to highlight those characteristics of BSTS that particularly lend themselves to this study.

One attractive feature relative to the synthetic control approach is that it avoids the restriction of non-negative weights. Instead, the predicted counterfactual is constructed as an average of outcomes in other states using estimated coefficients from the state-space model as weights. These coefficients are not constrained to be non-negative so, in this application, other states can contribute negatively to the predicted counterfactual. The flexibility allowed by this property is useful when the treated unit has characteristics that are outside the range seen in other units. Since some characteristics of Alaska differ markedly from other states the limitation of non-negative weights may well have empirical relevance to the extent that it does not permit a sufficiently similar synthetic Alaska to be formed.

A second feature is that, being Bayesian, the BSTS estimates take fuller account of model uncertainty. With the synthetic control approach, no account is taken of the fact that the weights used to construct the counterfactual are themselves subject to uncertainty.[17] BSTS allows fully Bayesian inference based on draws from the posterior distribution of simulated coefficients. Each such draw is used to derive counterfactual outcomes over the post-intervention period. Subtracting these from the observed outcomes in the treated unit provides a distribution of simulated impacts, each with draw-specific time series properties. This feature is particularly useful in this study where it is exploited to examine how impacts vary with the size of the Dividend.

The synthetic control approach has its own strengths. First, it avoids the need to specify priors. An analysis of sensitivity of the BSTS results to the choice of priors is reported below in order to provide some reassurance that those results are not unduly

[17] Inference under the synthetic control approach relies on permutation tests. While theoretically exact, these will have low power when the number of untreated units is small.

dependent on this. However, it remains the case that the basis for some priors required for BSTS – in particular, the prior on τ^2 – may be difficult to evidence. Second, the synthetic control approach allows the inclusion of characteristics that are not observed in every time period, and need not be observed at all in the post-intervention period. This reduces demands on the data.

5 Estimation results

This section presents the estimated results under the two approaches outlined above. In both cases, the years 1965-1981 constitute the pre-treatment period. This implies that impacts may be seen from 1982 onwards. A distinction is drawn between the 1982-1988 period and the years from 1989 onwards. This reflects the fact that the conditionality relating to criminals was introduced in 1989. Consequently, during the earlier (1982-1988) period the results capture the impact on crime of the Dividend whereas from 1989 onwards, the results capture the combined impact of the Dividend and the conditionality whereby certain prisoners are ineligible.[18]

5.1 Synthetic control estimates

The choice of predictors to incorporate in this approach involves an element of judgement. This is particularly the case when the theoretically-relevant predictors are not available in the data, or when there is no strong theory guiding the selection of predictors.

The theoretical framework discussed and developed in section 3 provides important

[18] It is conceivable that the 1989 change to eligibility may have had an anticipatory impact in 1988. As evidence of this, note the 1988 legal challenge to the constitutionality of the change(`https://law.justia.com/cases/alaska/supreme-court/1991/s-3650-4.html`).

guidance. In line with this, the following state-level predictors are included:[19]

- income – mean per capita income, 1965-1981
- inequality – mean Gini coefficient, 1965-1981
- unemployment – mean unemployment rate, 1976-1981
- education - mean proportion of the population with at least a high school degree, 1995-1981
- conviction rates – this is unobserved but is proxied by a measure calculated as the ratio of the number of prison admissions to the number of recorded crimes (mean, 1978-1982)
- state-level crime rates 1, 3 and 5 years prior to 1982.

Estimated impacts are summarised in Table 1 and shown in detail in Figure 3. Before considering these, it is informative to inspect model diagnostics. These are of two types. First, there are those that indicate how comparable Alaska is with its synthetic counterpart in respect of crime outcomes in the period prior to 1982. Figure 3 shows these differences graphically. For each category of crime, the black line traces out this difference. Prior to 1982, the line should ideally be close to the x-axis; this indicates that the synthetic control successfully estimates the observed Alaskan crime rate in the pre-intervention period. If not the case, this implies that the estimated counterfactual rate of crime in this period is not similar to that in Alaska, which calls into question the credibility of regarding post-1982 differences as capturing causal impacts.

[19]Some series are not available for the full period. Where this applies, predictors were averaged over those years for which they could be observed.

Of course, it would be surprising if the line lay exactly on the x-axis. To give a sense of the prediction error, the charts also depict using thin grey lines the results of separate placebo tests for each of the other 49 U.S. states. These are estimated in the same way as for Alaska, the only difference being that Alaska itself is not included among the pool of states that could possibly make up the synthetic control. The purpose of these placebo tests is to provide an indication of how likely it is that differences of the size estimated for Alaska could arise by chance. Statistical significance is indicated by markers. Where the actual-counterfactual difference in the Alaskan case is outside the top or bottom 2.5% of the distribution of placebo actual-counterfactual differences, it is marked with a diamond. This indicates statistical significance at the 95% level.

From Figure 3 (a), it appears that the the rate of property crime in Alaska prior to 1982 was mostly not significantly different from that in the synthetic Alaska. However, this was not uniformly true. Perhaps of particular concern is the significant difference in 1980. Violent crime shows a broadly similar pattern (Figure 3 (e)), again with a significant difference in 1980.

Looking within these broad groupings, it is clear that there is variation across types of crime in how well the approach manages to replicate observed trends. Within property crime, burglary does not show any significant differences pre-1982. The picture for larceny mirrors that for property crime as a whole, while that for motor vehicle theft looks poor. Within violent crime, murder, rape and aggravated assault all are revealed as problematic. Robbery is less so; it shows significant differences in two years pre-1982 but these are small.

This graphical evidence can be summarised by calculating the root mean squared prediction error (RMSPE) over the pre-intervention period. This is shown in Table 1, expressed as a percentage of the estimated counterfactual. It suggests that, overall, the fit is better for property crime than violent crime. Burglary and larceny show a better fit than motor theft. For all sub-categories of violent crime the fit is substantially worse.

The second type of diagnostic available is a check of how similar Alaska looks to its synthetic counterpart in respect of predictor variables. From Table 1 the impression is of mixed success. For all categories of crime, unemployment and income are both higher in Alaska than in the synthetic Alaskas. With the other predictors – conviction, education, inequality – the differences are less marked. It can also be informative to inspect which states make up the counterfactual. Across all crimes, the number of states ranges from 3 to 6, except in the case of rape where the synthetic Alaska is simply Nevada.

Overall, the diagnostics suggest the synthetic control approach has worked best for property crime, in particular burglary and larceny.

Returning to Figure 3, the differences shown by the thick black lines are interpretable as impacts from 1982 onwards. The overall impression is of a lack of impact on property crime. The exceptions are in 1984 and the last two years, in all of which cases the estimated impacts are positive. The results for burglary are non-significant throughout, as is the case for larceny aside from in the very last year. The vehicle theft impacts go from being negative at roughly the time conditionality was introduced to positive in the final two years. Given the performance diagnostics discussed above, these results should perhaps be viewed with caution. This applies similarly to violent crime and its sub-categories.

The estimated impacts are reported in Table 1. Over the full post-intervention period

(1982 onwards), the number of property crimes is estimated to have been increased by 418 per 100,000 residents per year, or 13.8% (column 1). This impact was greater (14.8%) in the post-1989 period (after conditionality was introduced) than in the period 1982-1988 (9.5%). Burglary and larceny – the sub-categories of property crime for which the approach appears to have worked best – both suggest impacts that were, if anything, stronger in the 1982-1988 period (columns 2 and 3, respectively). However, while these patterns are of interest, the inference from Figure 3 is that none is statistically significant.

[Figure 3 about here.]

[Table 1 about here.]

5.2 Bayesian structural time series results

The BSTS results can also be shown graphically. In Figure 4 the thick black lines again trace out the estimated impacts over time. The dashed lines are 95% credible intervals. Looking across all categories, there are fewer instances where the impacts appear statistically significant (in the sense of lying outside the credible interval) than was the case with the synthetic control estimates. The only occasional exception to this is with crimes of murder, which do suggest a significant negative effect. For all other crimes, there is no evidence of impact.

Table 2 summarises these impacts. It also provides a RMSPE diagnostic which, similar to the case with the synthetic control approach, suggests the results for property crime and the burglary and larceny sub-categories in particular are likely to be the most reliable. However, relative to the synthetic control approach, the RMSPE is considerably smaller for every category of crime, indicating a superior fit. To avoid extreme draws disproportionately influencing results, impacts are summarised by the median impact across simulations, rather than the mean. For property crime, there was a median impact of -3.6% over the period as a whole. The impact in the years 1982-88 was positive while the impact for 1989 onwards was negative. With burglary, larceny and vehicle theft the overall impacts were -16.4%, -3.7% and -1.8%, respectively. As with property crime as a whole, these effects took hold in the later years.

In comparison with the synthetic control approach, the BSTS results are more in line with theoretical expectations to the extent that they show, if anything, a negative impact on crime. However, the results are imprecise and clearly cannot be regarded as statistically significant.

[Figure 4 about here.]

[Table 2 about here.]

5.3 Modelling the relationship between estimated impact and Dividend Size

The results presented above suggest that the impact of the Dividend is too small to be detected using either of the two approaches used. One of the reasons behind this may be that the factors influencing crime in Alaska during the years before the Dividend later changed in a way not seen in other states. Such a change would present a challenge for either approach since they both construct counterfactual outcomes on the basis of relationships prevailing during the pre-Dividend years. It follows that, if those relationships change, estimates of counterfactual outcomes (and therefore impacts) will become more prone to error.

This consideration applies, of course, to all states. However, it is perhaps especially pertinent for Alaska which experienced a period of substantial economic and population change during the 1970s as a result of the construction of the Trans-Alaska Pipeline System (TAPS). This took place between 1974 and 1977 and was at the time the biggest privately-financed construction project in history. Carrington (1996) describes the TAPS as the "largest localized demand shock in postwar U.S. history". He shows that earnings and employment rose and fell substantially over the 1968-84 period, tracking the changes in TAPS-related activity. As another example of a change that affected Alaska alone, in 1980 the state eliminated income tax.

As discussed earlier, the synthetic control approach operates by constructing a synthetic Alaska that, in the pre-Dividend years, looks similar with regard to those covariates thought to influence crime. Where those covariates look very different in Alaska post-1982, it is no longer clear that the synthetic Alaska can accurately represent counterfactual outcomes since, in important regards, it may no longer resemble the true Alaska. The BSTS approach is less directly affected by this since it does not control for employment, earnings, income and so on. Nevertheless, since such factors may influence crime, the precision of the BSTS estimates is reduced since these take no account of any change in these factors post-1982.

An advantage of the BSTS approach relative to the synthetic control approach is that it allows for model uncertainty. The BSTS impact estimates have wide credible intervals, as apparent from Figure 4. These intervals summarise uncertainty in the estimates and correspond to the top and bottom 2.5% of estimates at any point in time. What is not visible from Figure 4 is the relationship between impacts in different years. The BSTS simulation approach provides multiple time series of impact estimates. While each may vary in its level, leading to wide intervals for point-in-time impact estimates, it is possible that there is more consistency across time series in how changes in estimated impacts vary with changes in the size of the Dividend. Put differently, the imprecision of the impact estimates presented above may be due to draw-specific effects; controlling for these makes it possible to focus directly on the relationship between crime and Dividend amount.

In this section, a separate autoregressive distributed lag (ARDL) model is estimated for every impact time series generated through the BSTS approach. The results across all simulated time series are then summarised and used to illustrate the impact on crime of a one-off increase in the Dividend. Pesaran & Shin (1998) show that ARDL models can be appropriate for the examination of long-run relationships regardless of the time series properties of the individual regressors. In contrast to tests for cointegration, 'pre-testing' to establish the order of integration of the regressors is not required. This is attractive given that unit root tests are known to have low power. With a sufficiently flexible specification, an ARDL model can capture the data generating process and so have a causal interpretation.

A generic ARDL(P,Q) model can be written

$$\Delta_t = \phi + \sum_{p=1}^{P} \gamma_p \Delta_{t-p} + \sum_{q=0}^{Q} \kappa_q D_{t-q} + \upsilon_t \tag{8}$$

where, in this application, Δ_t is the estimated impact of the Dividend on crime at time t, D_t is the (real) Dividend amount at time t, and ϕ, γ_t and κ_t are parameters to be estimated. The disturbances, υ_t ,are assumed to be independently and identically distributed, with zero mean. They are also assumed to be distributed independently of the regressors.

In considering this assumption, it is relevant to highlight that D_t is not influenced by contemporaneous or lagged values of Δ_t. Such a scenario could arise if the Dividend amount were calculated by simply dividing Fund earnings by the size of the eligible population. In that case, a negative impact on crime at time t would be likely to reduce the number of people sentenced or incarcerated, thereby reducing the denominator in the calculation and leading to a subsequent increase in the Dividend. Instead, the Dividend is calculated by dividing Fund earnings *less appropriations* and reductions by the size of the eligible population. Rather than being shared out among eligible applicants, the Dividends that would otherwise have been paid to those ineligible on criminal grounds are retained by the state and used to contribute to some of the costs associated with incarceration and probation, to provide victim support and to fund grants for domestic violence and sexual assault programmes. Consequently, the numerator and denominator in the calculation of the Dividend amount are both affected with the consequence that there is no such feedback.

In any event, while this institutional feature provides reassurance, it should be noted that the ARDL approach can be rendered valid even when the regressors are not strictly exogenous. On the assumption that D_t follows a finite order auto-regressive process, the inclusion of additional lags of the regressors in the ARDL model makes consistent estimation possible Pesaran (1997).

Equation 8 makes clear how a significant relationship between crime and Dividend amount might exist despite the earlier finding of no overall impact of the Dividend.

For each category of crime, equation 8 can be estimated for every time series of impact estimates resulting from the BSTS simulation. Each BSTS estimates is based on 10,000 simulated draws, the first 500 of which are discarded ('burn-in'), meaning that 9,500 ARDL models are estimated for each category of crime. These estimated coefficients will be different for every time series but looking across all time series allows the distribution of estimated γ_p and κ_q coefficients to be summarised. Intuitively, under the assumed model of equation 8, the lack of statistical significance of the impacts presented already may correspond to wide variation in the estimates of ϕ despite possibly precise estimates of γ_p and κ_q.

A preliminary analysis was carried out to examine the required order of the ARDL model. ARDL(i,j) models were estimated with $i = 1, .., 5, j = 1, .., 5$ for each draw from the posterior distribution for property crime impacts. The preferred ARDL(P,Q) specification for each draw was chosen as having P, Q equal to the i, j combination that achieved the smallest Bayesian Information Criterion (BIC) for that draw. The joint distribution of P and Q is shown in Table 3. This suggests an ARDL(2,1) specification is sufficiently flexible in 84% of cases.

[Table 3 about here.]

The results of estimating equation 8 for each draw from the posterior are summarised in Table 4. Each reported coefficient is the median across 9,500 ARDL estimates, and the 95% interval corresponds to the lowest and highest 2.5% of estimates. The results in the left panel are based on the BSTS property crime impact estimates presented already. Four ARDL(P,Q) models are shown, each differing in its values of P and Q (1 or 2). Across these four specifications, the results consistently show a significantly negative κ_0

coefficient. This indicates that increasing the Dividend amount reduces property crime. However, the direct effect does not last beyond the year of increase, as indicated by the lack of significance for the κ_1 and κ_2 coefficients. Instead, the contemporaneous effect can persist beyond the year of increase through its effect on γ_1.

In addition to providing evidence of the robustness of the results to the choice of ARDL specification, Table 4 also provides some reassurance that the results are not unduly sensitive to the choice of priors assumed by the BSTS approach. The results in the right panel are based on a prior of 3 for J_0, 0.8 for R^2 (these are the default priors in the *CausalImpact* program) and $0.1s^2$ for τ^2. This last prior implies more variability in the trend term relative to the *CausalImpact* default of $0.01s^2$ (used in the preferred specification). The impact estimates based on these priors are less precise than the preferred results (the RMSPE is 5.4 rather than 3.8), yet the estimated relationship with the Dividend amount is much the same as before; an estimated κ_0 of 0.5, or thereabouts. Hence, introducing more imprecision into the impact estimates does not appear to alter the finding that increasing the Dividend reduces property crime.

[Table 4 about here.]

Since the preliminary analysis suggests the ARDL(2,1) specification is preferred on the basis of the BIC in the majority of cases and given that the results seem robust to the choice of specification, an ARDL(2,1) specification is used in the remainder of this paper.

To allow for the relationship between impacts and Dividend size to change following the introduction of conditionality, a generalised version of equation 8 is estimated

$$\Delta_t = \phi + \sum_{p=1}^{P} \gamma_p \Delta_{t-p} + \sum_{q=0}^{Q} \kappa_q D_{t-q} + \sum_{q=0}^{Q} \kappa_q^C D_{t-q} C_{t-q} + \upsilon_t \tag{9}$$

where $D_t = 1$ if $t \geq 1989$, 0 otherwise such that κ_q^C captures the extent to which the impact of the Dividend changes under conditionality. This was estimated over the period 1982-2017. While this amounts to only 36 years of data, Monte Carlo results provided by Pesaran & Shin (1998) suggest good small-sample performance of ARDL estimators.

The results are presented in Table 5. For property crime (column 1), there is a statistically significant negative contemporaneous effect of Dividend size (the κ_0 coefficient) but the lagged effect is not significant. With the introduction of conditionality, the contemporaneous effect changes by κ_0^C. This is smaller in absolute size than κ_0 but is significant and positive, suggesting that under conditionality the impact of Dividend size is reduced. Again, the lagged effect is not significant. Looking within sub-categories of property crime, no significant effect of the Dividend is seen. The same is true for violent crime and all its sub-categories.

The results in Table 5 provide the econometric underpinning of the estimated relationships between crime and Dividend amount. However, it is helpful to present this information in a way that shows the evolution over time of the estimated effects. Figures 5 and 6 do this using impulse response functions (IRFs). For each type of crime, they plot the effect of a one-off $100 Dividend increase, contemporaneously and over the subsequent 10 years. As with the results above, these figures are based on 9,500 ARDL models. Each ARDL model gives rise to its own IRF. Figures 5 and 6 summarise these 9,500 IRFs, plotting the median, along with 95% intervals.

Figure 5 relates to the case where there is no conditionality (that is, it ignores the estimates of κ^C). It is readily apparent that the strongest effects are for property crime. Consistent with the regression results in Table 5, the immediate effect of the $100 increase is to reduce by 193 the number of property crimes per 100,000 population. One year later, the reduction is 184 and the following year it is 157. Beyond that point, the effect slowly decays such that 10 years later the estimated impact is a reduction of 46. The impact appears driven mainly by the reduction in larcenies. For all other categories, the results suggest impacts that are not significant and are close to zero by the end of the 10-year period.

Figure 6 shows the IRFs under conditionality. Again the strongest results are found for property crime. However, the reduction in crime resulting from the Dividend increase is smaller, and less significant, when conditionality is in place than with it is not in place (Figure 5). Substantively, there is no evidence from this analysis that denying eligibility to offenders reduces crime.

[Table 5 about here.]

[Figure 5 about here.]

[Figure 6 about here.]

5.4 Assessing robustness of the BSTS results

To provide reassurance that the result for Alaska is not spurious, the analysis was repeated for all other states, with property crime as the outcome. For each state, the BSTS impact was estimated using all other states (excluding Alaska) as the basis for estimating counterfactual outcomes, and the ARDL(2,1) model of equation 9 was estimated for each draw of the posterior distribution. The resulting placebo IRFs are plotted in Figure 7 for the no-conditionality case and Figure 8 for the conditionality state.

Since the Dividend is not paid in any states other than Alaska, the expectation is that these placebo IRFs should only ever appear statistically significant by chance. Consistent with this, Figures 7 and 8 confirm that the placebo IRFs are overwhelmingly not significant, as evident from the fact that the credible intervals mostly span the x-axis.

[Figure 7 about here.]

[Figure 8 about here.]

Figures 7 and 8 are intended to give a visual impression of statistical significance of placebo effects across other U.S. states. Another way of summarising these results, which allows a comparison of the size of effects, is to plot the simulated probability of the IRF being negative (as would be consistent with Dividend amount reducing property crime) for these states and compare them with the results for Alaska. The upper panel of Figure 9 relates to the no-conditionality case and plots the probability over time of the effect of a one-off Dividend increase of $100 being a reduction of more than 50 property crimes per 100,000 residents. Alaska is shown by the thick black line and it is readily apparent that a reduction of that scale is consistently more likely than it is in the other states (thin grey lines).

The bottom panel shows the results with conditionality. Here, in view of the smaller effects in this case, the lines trace out the probability over time that the one-off Dividend increase reduces property crime by more than 25 per 100,000 residents. Again, the probability is higher in Alaska than in other states.

Overall, these results show that the estimated impacts of a Dividend increase are stronger in Alaska than they are in other states. Clearly, since the Dividend only exists in Alaska, this is in line with expectations as the impacts estimated in other states are of a placebo. The fact that the results for Alaska stand out so prominently provides additional evidence that the main results are capturing a true relationship. The ARDL(2,1) results reported earlier have shown a statistically significant negative effect of Dividend amount on property crime. These results in Figure 9 are in the spirit of permutation tests and provide an alternative indication of statistical significance. Intuitively, the placebo results for other states provide the null distribution, and the results for Alaska can be seen to be quite distinct from that.

[Figure 9 about here.]

6 Conclusion

This paper has examined the impact of Alaska's Permanent Fund Dividend on crime. Two empirical approaches were used and neither found a statistically significant impact. Despite this, further analysis of the BSTS results provided evidence that an increase in the Dividend amount reduces property crime.

To reconcile these findings, note that the BSTS estimates of impacts at a point in time have low statistical power due to high variance. BSTS relies on the simulation of the posterior distribution of counterfactual time series, from which a posterior distribution of impacts can be readily constructed. The relationship between impact and Dividend amount can be estimated for each draw from this distribution, and itself summarised. Doing so abstracts from the posterior variation attributable to the draw fixed effect and thereby permits more precise estimation of the relationship of interest.

Substantively, the results imply that increasing the Dividend reduces property crime for an extended period. This may appear to contrast with Watson et al. (2019) who find the (negative) marginal effect on property crime of an increase in the Dividend to be non-significant. However, as noted already, their analysis addresses a different question – the effect of Dividend *receipt* rather than the Dividend *per se* – and sensitivity to the size of the Dividend is considered only in the week of receipt, rather than over a longer period. By contrast, the aim in this paper has been to estimate the counterfactual outcomes associated with the Dividend not existing; a no-Dividend Alaska. These results provide an estimate of overall impact and it is this that is found to be related to Dividend amount.

Two points about the estimated relationship should be emphasised. First, the posterior distribution of impact time series was constructed without using information on the Dividend amount. This is required if the relationship estimated by the ARDL models is to be meaningful in the sense of not merely reproducing assumed features of the underlying model. Instead, the results are more likely to be detecting a true effect. Second, it is valid to refer to the estimated relationship between impact and Dividend amount as an effect since ARDL models provide a basis for causal interpretation. This rests upon specifying an adequate lag structure. However, in this paper the basis for viewing the relationship as causal is further strengthened by the fact that Dividend amount is plausibly exogenous in its relationship to crime. Sensitivity analyses have served to support the main result.

With these points in mind, the findings demonstrate the potential of the Dividend to influence an important social outcome. As such, it provides evidence that may be considered alongside the other potential benefits of a basic income. In this case, a higher payment level results in a lower rate of property crime. Removing eligibility from criminals does not appear to reinforce this effect as one might expect if criminals are forward-looking. In view of this, the change to eligibility does not seem to act as a deterrent to crime.

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A Appendix: Data sources

This paper uses data drawn from a number of sources:

- Dividend amounts are taken from the Alaska Department of Revenue Permanent Fund Dividend Division, `http://pfd.alaska.gov/DivisionInfo/SummaryApplicationsPayments`. Deflated amounts were calculated using the Alaska Consumer Price Index for urban Alaska, `http://live.laborstats.alaska.gov/cpi/index.cfm`.

- Crime figures are taken from the Federal Bureau of Investigations Uniform Crime reports available online at `http://www.ucrdatatool.gov`. These provide state-level data on reported crimes from 1960 up until 2014. Figures for 2015 are taken from `https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-2` and figures for both 2016 and 2017 are taken from `https://ucr.fbi.gov/crime-in-the-u.s/2017/crime-in-the-u.s.-2017/topic-pages/tables/table-4`. Note that, in December 2011, the UCR Program adopted a revised definition of rape. To maximise consistency over time, the earlier definition of rape – referred to now by UCR as 'legacy rape' – is used for years 1960-2015. Legacy rape is defined as 'carnal knowledge of a female forcibly and against her will'. Rapes by force and attempts or assaults to rape, regardless of the age of the victim, are included. Statutory offences (no force used—victim under age of consent) are excluded. Statistics on legacy rape were discontinued after 2015, so this paper uses the revised definition for 2016 and 2017. The revised definition of rape is 'penetration, no matter how slight, of the vagina or anus with any body part or object, or oral penetration by a sex organ of another person, without the consent of the victim'. Attempts or assaults to commit rape are also included; however, statutory rape and incest are excluded.

- Crime clearance rates are not made available at the state level. This paper uses as a proxy for conviction rate the ratio of the number of prison admissions to the number of crimes. Admissions data were taken from the United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics. National Prisoner Statistics, 1978-2011. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2013-06-25. https://doi.org/10.3886/ICPSR34540.v1).

- State personal income per capita data originates from the U.S. Department of Commerce, Bureau of Economic Analysis (http://www.bea.gov) and was downloaded from the United States Regional Economic Analysis Project, https://united-states.reaproject.org.

- Annual unemployment rate was calculated as the unweighted average of seasonally-adjusted monthly rates), downloaded from https://fred.stlouisfed.org, which is sourced from http://data.bls.gov/cgi-bin/dsrv

- Educational attainment is represented by the proportion of the population with at least a high school diploma. The construction of this measure is described in Appendix B of Frank (2009) which notes that, although it would be more intuitive to consider only the population aged 25 or over, information on the size of that subgroup is not available at state level throughout the period considered. The data were downloaded from `http://www.shsu.edu/eco_mwf/Frank_Edu_v66.dta`.

- Inequality – annual state-level Gini coefficients from 1960 to 2015, as described in Frank (2009). The updated series were downloaded from `https://www.shsu.edu/eco_mwf/Frank_Gini_2015.dta`.

Tables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	AK	prop	burg	larc	motr	viol	murd	rape	asst	robb
Mean impact										
1982-2017		418.4	43.9	179.6	23.7	188.4	-0.9	29.2	122.8	5.0
1982-1988		443.7	97.8	257.0	-33.5	36.5	0.2	16.7	17.7	-5.9
1989-2017		412.3	30.8	160.9	37.6	225.1	-1.1	32.2	148.2	7.7
Mean impact, %										
1982-2017		13.8	9.1	6.9	37.4	50.5	-9.8	64.2	49.4	7.4
1982-1988		9.5	10.2	7.9	-3.9	7.1	0.6	30.1	5.7	-5.7
1989-2017		14.8	8.8	6.6	47.3	60.9	-12.3	72.4	60.0	10.6
RMSPE, %		7.8	8.7	8.6	18.4	13.7	21.0	32.3	26.0	23.1
Conviction rate	1.2	1.1	0.7	0.8	1.0	0.8	1.1	1.2	0.8	0.6
Unemployment rate	9.4	7.1	7.6	6.4	7.6	7.0	7.1	6.4	7.6	7.6
High school degree	32.5	32.6	32.5	31.4	32.1	32.9	33.1	36.6	32.4	33.5
Inequality (Gini)	46.3	47.5	46.4	46.7	47.9	46.8	47.8	47.7	47.0	45.9
PC Income ($000)	7.7	6.0	6.0	5.7	5.8	5.9	5.6	6.1	5.6	5.4
No. SCR states		4	5	3	4	4	5	1	4	5
- CT		0.445	0.366	0.242	0.375					0.03
- DE		0.301		0.585						0.081
- FL							0.307		0.331	
- GA							0.017			
- HI			0.242			0.163				0.189
- ME										0.25
- MD							0.136			
- MA					0.135					
- MI						0.003				
- NV		0.134				0.157	0.268	1	0.045	
- NH			0.03		0.095					
- NJ		0.12				0.677			0.593	
- NY					0.396		0.272			
- ND			0.146							
- PA			0.216							
- WA				0.173						0.449
- WY									0.031	

Table 1: *SCR impacts and diagnostics. Column 1 shows mean values of predictor variables in Alaska. Columns 2 to 7 show characteristics, estimated impacts and model diagnostics for the synthetic Alaska used to estimate impacts on : (2) property crime (3) violent crime (4) burglary (5) larceny (6) motor vehicle theft (7) murder (8) rape (9) aggravated assault (10) robbery.*

		Mean impact	95% interval	Median impact, %	95% interval	RMSPE
Property:	1982+	-237.9	[-2512.4, 1380.9]	-3.6	[-38.2, 72.9]	3.8
- $R_0^2 = 0.97$	1982-88	137.1	[-1093.9, 1254.6]	3.3	[-16.3, 33.2]	
- $J_0 = 5$	1989+	-328.5	[-2934.9, 1479.9]	-5.4	[-44.4, 85.0]	
Burglary:	1982+	-148.0	[-785.9, 385.9]	-16.4	[-55.9, 123.8]	3.5
- $R_0^2 = 0.95$	1982-88	-51.7	[-516.5, 366.5]	-2.6	[-30.2, 57.6]	
- $J_0 = 8$	1989+	-171.2	[-875.5, 409.8]	-19.9	[-63.4, 143.1]	
Larceny:	1982+	-233.8	[-1901.6, 1103.0]	-3.7	[-38.7, 83.4]	4.0
- $R_0^2 = 0.97$	1982-88	105.4	[-622.3, 869.1]	3.4	[-14.4, 35.1]	
- $J_0 = 6$	1989+	-315.7	[-2267.2, 1211.5]	-5.9	[-45.6, 99.4]	
Motor:	1982+	173.4	[-305.8, 610.6]	-1.8	[-1096.7, 1033.5]	7.4
- $R_0^2 = 0.78$	1982-88	-0.6	[-405.1, 318.5]	1.5	[-113.5, 192.0]	
- $J_0 = 5$	1989+	215.4	[-332.6, 707.1]	-2.7	[-1337.0, 1252.4]	
Violent:	1982+	85.7	[-236.1, 479.4]	11.0	[-319.8, 464.6]	5.7
- $R_0^2 = 0.93$	1982-88	27.3	[-135.3, 231.2]	4.1	[-18.9, 78.2]	
- $J_0 = 5$	1989+	99.8	[-280.7, 561.0]	12.5	[-404.9, 567.4]	
Murder:	1982+	-3.6	[-7.3, -1.1]	-32.4	[-49.1, -11.3]	14.0
- $R_0^2 = 0.36$	1982-88	0.0	[-2.1, 1.9]	-0.2	[-15.3, 20.8]	
- $J_0 = 5$	1989+	-4.5	[-8.7, -1.6]	-40.4	[-58.2, -16.2]	
Rape:	1982+	3.6	[-60.9, 66.6]	9.3	[-224.9, 302.5]	12.5
- $R_0^2 = 0.79$	1982-88	0.9	[-27.6, 23.5]	3.7	[-22.3, 46.2]	
- $J_0 = 2$	1989+	4.2	[-70.0, 82.5]	8.6	[-276.0, 373.2]	
Assault:	1982+	84.0	[-186.4, 421.8]	16.6	[-601.3, 728.4]	9.0
- $R_0^2 = 0.90$	1982-88	21.3	[-135.9, 238.9]	3.4	[-27.9, 216.8]	
- $J_0 = 4$	1989+	99.2	[-215.6, 475.3]	19.7	[-743.7, 870.8]	
Robbery:	1982+	11.0	[-58.2, 88.8]	11.6	[-385.4, 391.7]	10.4
- $R_0^2 = 0.84$	1982-88	12.2	[-38.9, 60.6]	14.0	[-37.4, 232.9]	
- $J_0 = 9$	1989+	10.7	[-69.9, 102.6]	10.1	[-460.6, 438.5]	

Table 2: *Mean and total impacts, estimated using BSTS, with crime-specific priors for R_0^2 and J_0 as shown, sample variance over the pre-intervention period, s^2, as the prior for σ^2 and $0.01s^2$ as the prior for τ^2.*

	Lags of D_t				
	1	2	3	4	5
Lags of Δ_t					
1	0.38	0.00	0.02	0.01	0.00
2	0.46	0.01	0.05	0.01	0.00
3	0.01	0.00	0.01	0.00	0.00
4	0.02	0.00	0.00	0.00	0.00
5	0.00	0.00	0.01	0.00	0.00

Table 3: *The distribution of P and Q values that minimise the BIC in 9,500 ADRL(i,j) estimates of equation 8 for property crime, with i and j allowed to vary between 1 and 5*

	Using priors given in Table 2				Default *CausalImpact* priors, except $\tau^2 = 0.1s^2$			
	ARDL(1,1)	ARDL(1,2)	ARDL(2,1)	ARDL(2,2)	ARDL(1,1)	ARDL(1,2)	ARDL(2,1)	ARDL(2,2)
γ_1	0.83	0.96	0.83	0.96	0.85	0.98	0.84	0.98
	(0.63, 0.93)	(0.70, 1.23)	(0.63, 0.94)	(0.69, 1.23)	(0.63, 0.96)	(0.62, 1.29)	(0.63, 0.96)	(0.61, 1.29)
γ_2		-0.18		-0.17		-0.18		-0.17
		(-0.44, 0.10)		(-0.45, 0.11)		(-0.50, 0.17)		(-0.50, 0.17)
κ_0	-0.47	-0.45	-0.45	-0.45	-0.50	-0.47	-0.46	-0.46
	(-0.83, -0.15)	(-0.77, -0.16)	(-0.80, -0.14)	(-0.78, -0.14)	(-0.86, -0.17)	(-0.82, -0.14)	(-0.83, -0.11)	(-0.83, -0.11)
κ_1	0.21	0.23	0.16	0.22	0.22	0.23	0.15	0.21
	(-0.14, 0.49)	(-0.16, 0.51)	(-0.19, 0.44)	(-0.19, 0.57)	(-0.09, 0.56)	(-0.11, 0.55)	(-0.26, 0.48)	(-0.24, 0.61)
κ_2			0.06	-0.00			0.10	0.03
			(-0.20, 0.34)	(-0.25, 0.30)			(-0.19, 0.43)	(-0.27, 0.41)
ϕ	179.36	140.31	156.16	142.55	88.55	44.29	39.56	28.56
	(-229.01, 559.54)	(-327.88, 540.69)	(-385.14, 571.99)	(-393.77, 584.97)	(-367.71, 510.86)	(-465.67, 490.24)	(-559.88, 507.08)	(-555.54, 518.32)

Table 4: *Results of estimating the ARDL(P,Q) model of equation 8 for property crime, allowing P and Q to be either 1 or 2. The results in the left panel are based on the preferred impact estimates, using priors described in Table 2. The results in the right panel use default* CausalImpact *priors apart from* $0.1s^2$ *as the prior for* τ^2 *rather than* $0.01s^2$*, imposing higher residual volatility. Each reported coefficient is the median across 9,500 estimates; the 95% interval shown in parentheses corresponds to the smallest and largest 2.5% of estimates.*

	(1) prop	(2) burg	(3) larc	(4) motr	(5) viol	(6) murd	(7) rape	(8) asst	(9) robb
γ_1	0.82	0.69	0.64	1.08	0.80	0.09	0.64	0.79	0.77
	(0.55, 1.08)	(0.28, 1.12)	(0.33, 0.97)	(0.67, 1.42)	(0.44, 1.16)	(-0.25, 0.47)	(0.16, 0.85)	(0.46, 1.16)	(0.42, 1.12)
γ_2	0.02	-0.05	0.11	-0.25	-0.00	0.47	0.03	-0.05	0.03
	(-0.28, 0.29)	(-0.41, 0.36)	(-0.17, 0.39)	(-0.56, 0.18)	(-0.33, 0.39)	(-0.09, 0.68)	(-0.24, 0.26)	(-0.40, 0.33)	(-0.30, 0.36)
κ_0	-1.93	-0.53	-1.14	-0.05	0.01	0.00	-0.01	-0.01	-0.03
	(-3.53, -0.68)	(-1.08, 0.06)	(-2.26, 0.04)	(-0.45, 0.66)	(-0.37, 0.26)	(-0.02, 0.01)	(-0.11, 0.03)	(-0.43, 0.25)	(-0.12, 0.03)
κ_1	-0.26	0.09	-0.28	0.02	-0.07	-0.00	-0.01	-0.06	-0.01
	(-1.05, 0.73)	(-0.20, 0.34)	(-0.86, 0.44)	(-0.24, 0.34)	(-0.26, 0.08)	(-0.01, 0.00)	(-0.06, 0.02)	(-0.20, 0.10)	(-0.07, 0.05)
ϕ^C	-703.24	-243.68	-498.49	129.09	26.35	0.79	5.10	9.17	-11.53
	(-1578.34, 90.10)	(-583.96, 136.53)	(-1272.89, 166.93)	(-155.84, 428.56)	(-192.16, 163.42)	(-14.69, 5.91)	(-84.12, 42.77)	(-147.40, 141.94)	(-60.20, 32.48)
κ_0^C	1.50	0.49	0.82	-0.05	-0.06	-0.00	-0.01	-0.01	0.03
	(0.35, 2.92)	(-0.08, 1.05)	(-0.23, 1.86)	(-0.86, 0.36)	(-0.29, 0.34)	(-0.01, 0.02)	(-0.05, 0.10)	(-0.26, 0.40)	(-0.03, 0.10)
κ_1^C	0.38	-0.04	0.35	0.00	0.09	0.00	0.02	0.05	0.01
	(-0.66, 1.14)	(-0.29, 0.23)	(-0.33, 0.92)	(-0.30, 0.31)	(-0.06, 0.24)	(-0.01, 0.00)	(-0.00, 0.06)	(-0.11, 0.17)	(-0.05, 0.06)
ϕ	956.99	169.74	666.94	-35.69	19.87	-1.61	0.77	27.75	20.12
	(184.49, 1809.68)	(-181.73, 439.89)	(11.82, 1321.27)	(-286.00, 266.76)	(-127.27, 246.37)	(-4.90, 5.13)	(-31.46, 55.13)	(-99.47, 278.11)	(-20.62, 74.34)

Table 5: *Results of estimating equation 9, specified as ARDL(2,1): (1) property crime (2) burglary (3) larceny (4) motor vehicle theft (5) violent crime (6) murder (7) rape (8) aggravated assault (9) robbery. Using priors given in Table 2. Each reported coefficient is the median across 9,500 estimates; the 95% interval shown in parentheses corresponds to the smallest and largest 2.5% of estimates.*

Figures

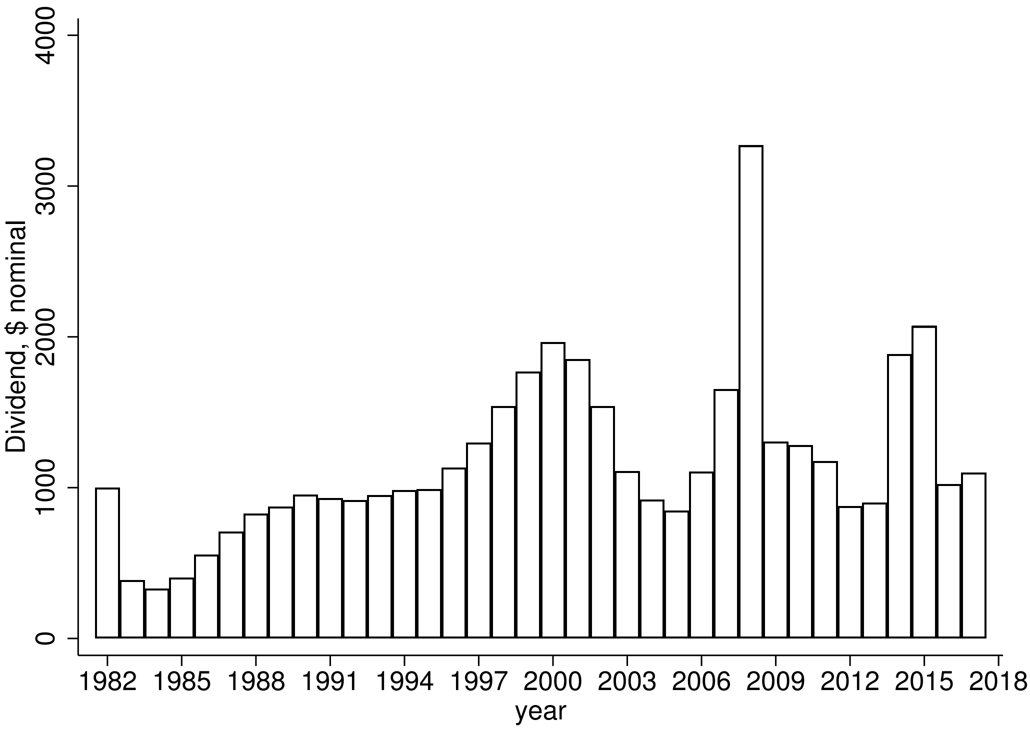
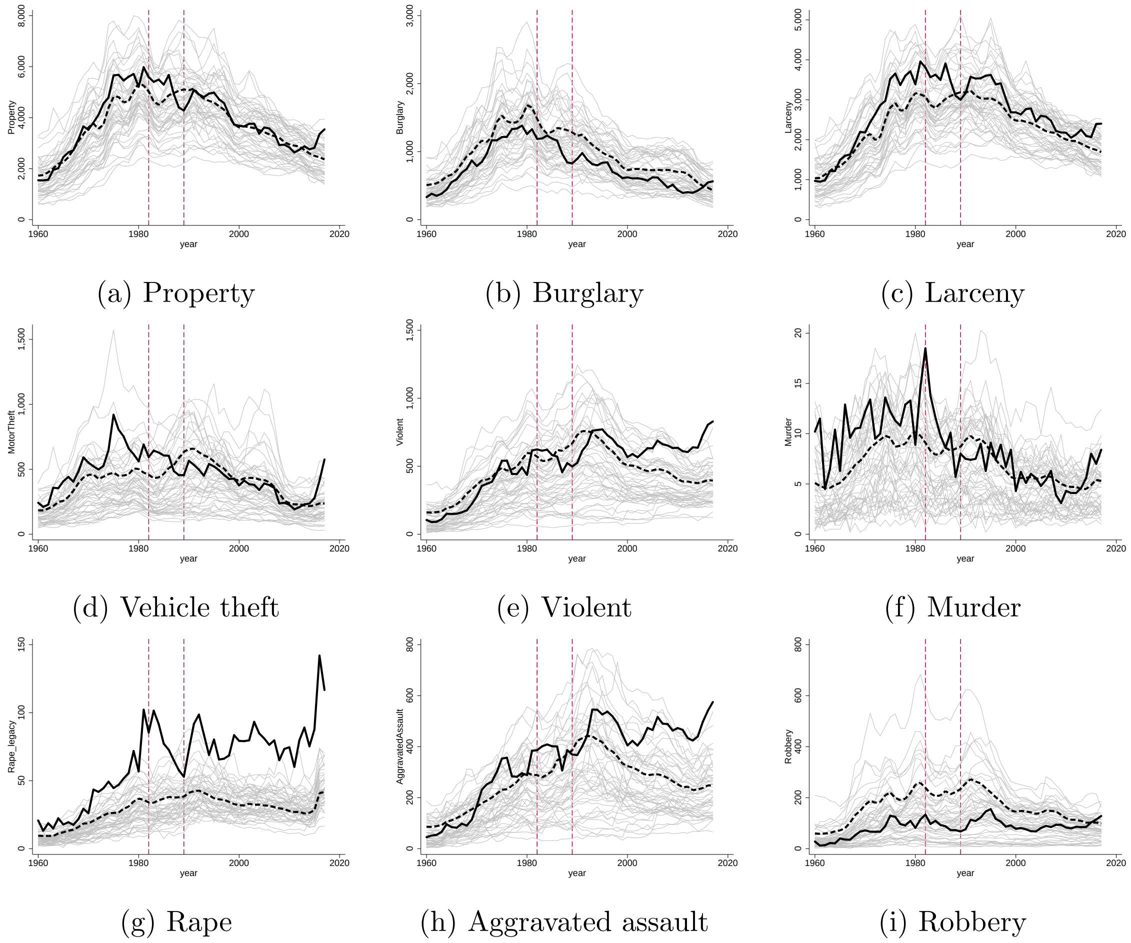


Figure 1: *Dividend amount, 1982-2017.*

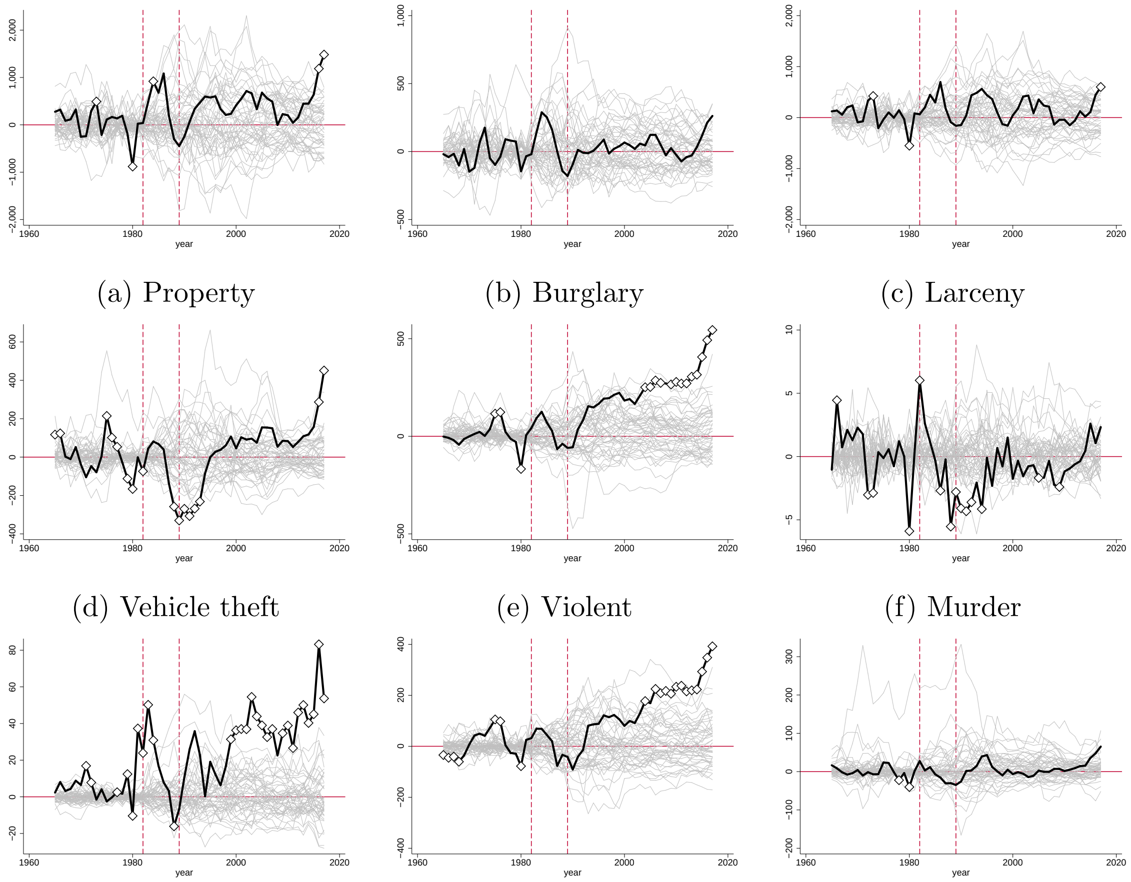


(a) Property (b) Burglary (c) Larceny

(d) Vehicle theft (e) Violent (f) Murder

(g) Rape (h) Aggravated assault (i) Robbery

Figure 2: *Crime trends in Alaska (solid black line), USA (dash black line) and all other states (grey lines)*

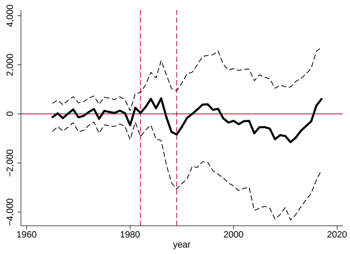


(a) Property (b) Burglary (c) Larceny

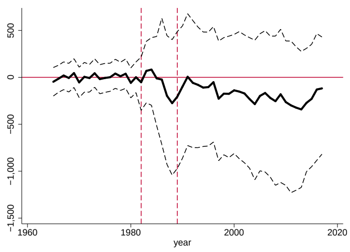
(d) Vehicle theft (e) Violent (f) Murder

(g) Rape (h) Aggravated assault (i) Robbery

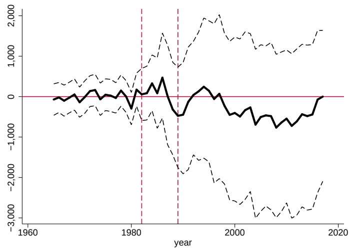
Figure 3: *Impacts of eligibility the dividend on crime in Alaska using a synthetic Alaska to represent counterfactual outcomes. In each case, the solid black line shows the difference between Alaska and the synthetic Alaska in the number of crimes per 100,000 population. The thin grey lines are placebo tests for other states. Markers indicate where the Alaskan difference is outside the top or bottom 2.5% of the placebo distribution. Estimates control for the mean pre-1982 rate of the crime in question, rates 1, 3 and 5 years pre-1982 and for state characteristics over those pre-1982 years for which data are available (these characteristics include ratio of convictions to crimes, unemployment rate, educational attainment and income inequality).*



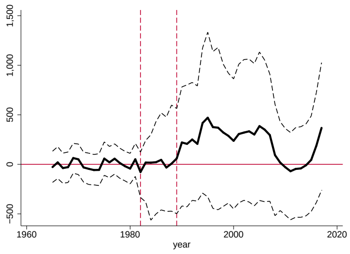
(a) Property



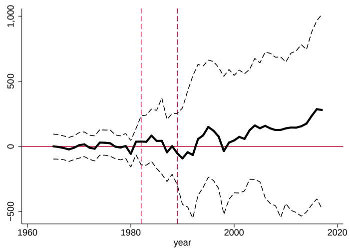
(b) Burglary



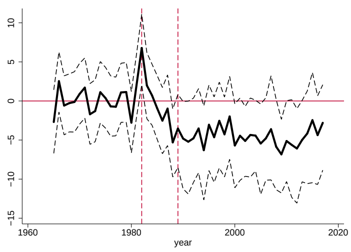
(c) Larceny



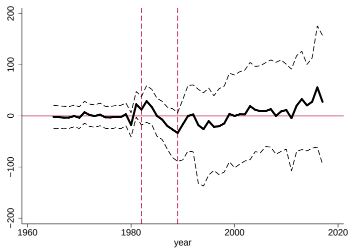
(d) Vehicle theft



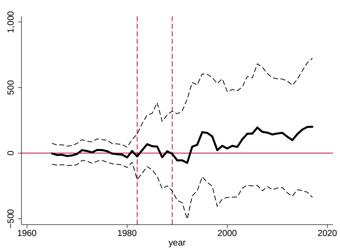
(e) Violent



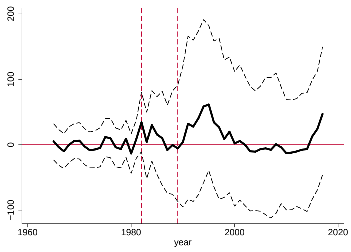
(f) Murder



(g) Rape

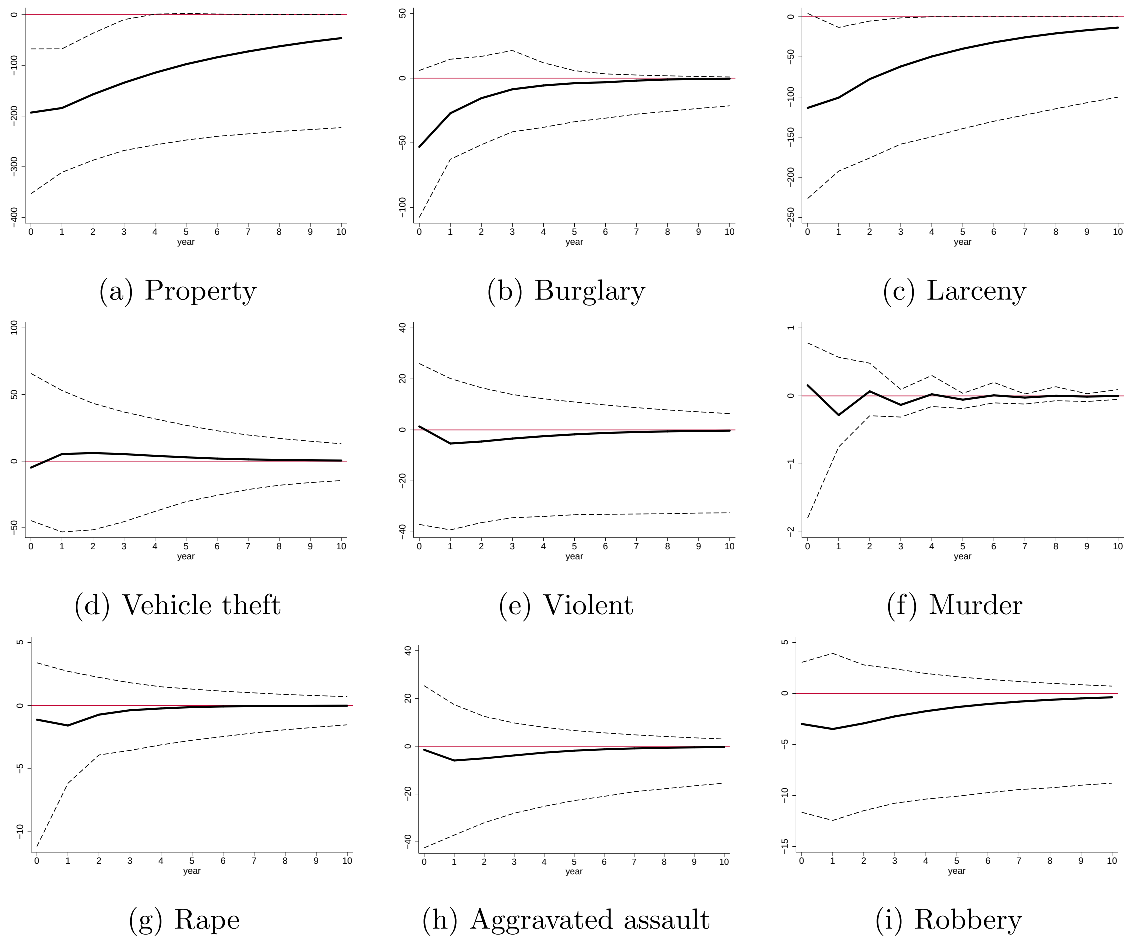


(h) Aggravated assault



(i) Robbery

Figure 4: *Impacts of the dividend on crime in Alaska using BSTS to construct counterfactual outcomes on the basis of crime trends in other states. In each case, the solid black line shows the impact on the number of crimes per 100,000 population, while the dashed lines are the 95% prediction intervals. Using priors given in Table 2.*

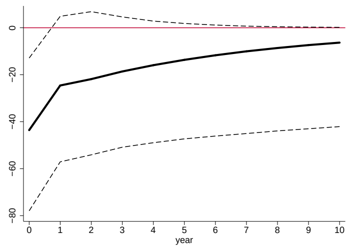


(a) Property (b) Burglary (c) Larceny

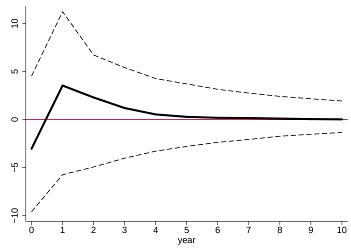
(d) Vehicle theft (e) Violent (f) Murder

(g) Rape (h) Aggravated assault (i) Robbery

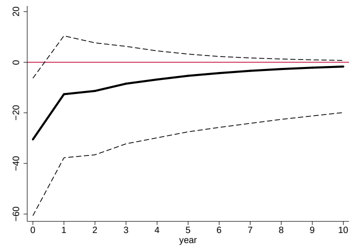
Figure 5: *Evolution of effect of one-off \$100 dividend increase (number of crimes per 100,000 population, with 95% prediction intervals), without conditionality. Estimated using ARDL(2,1) model.*



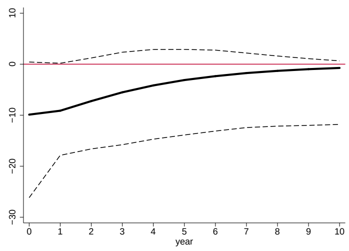
(a) Property



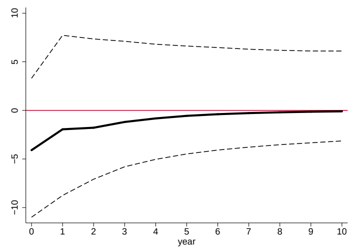
(b) Burglary



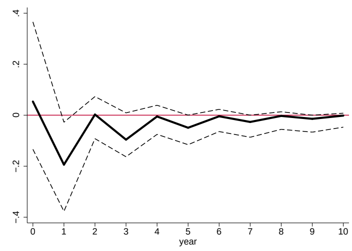
(c) Larceny



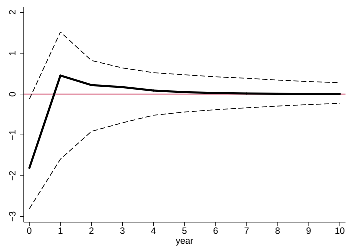
(d) Vehicle theft



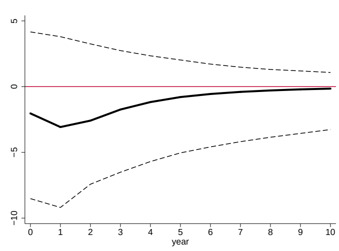
(e) Violent



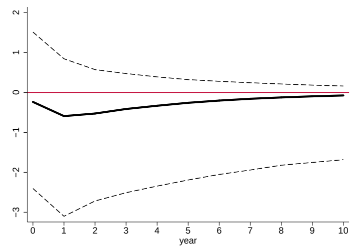
(f) Murder



(g) Rape



(h) Aggravated assault



(i) Robbery

Figure 6: *Evolution of effect of one-off $100 dividend increase (number of crimes per 100,000 population, with 95% prediction intervals), with conditionality. Estimated using ARDL(2,1) model.*

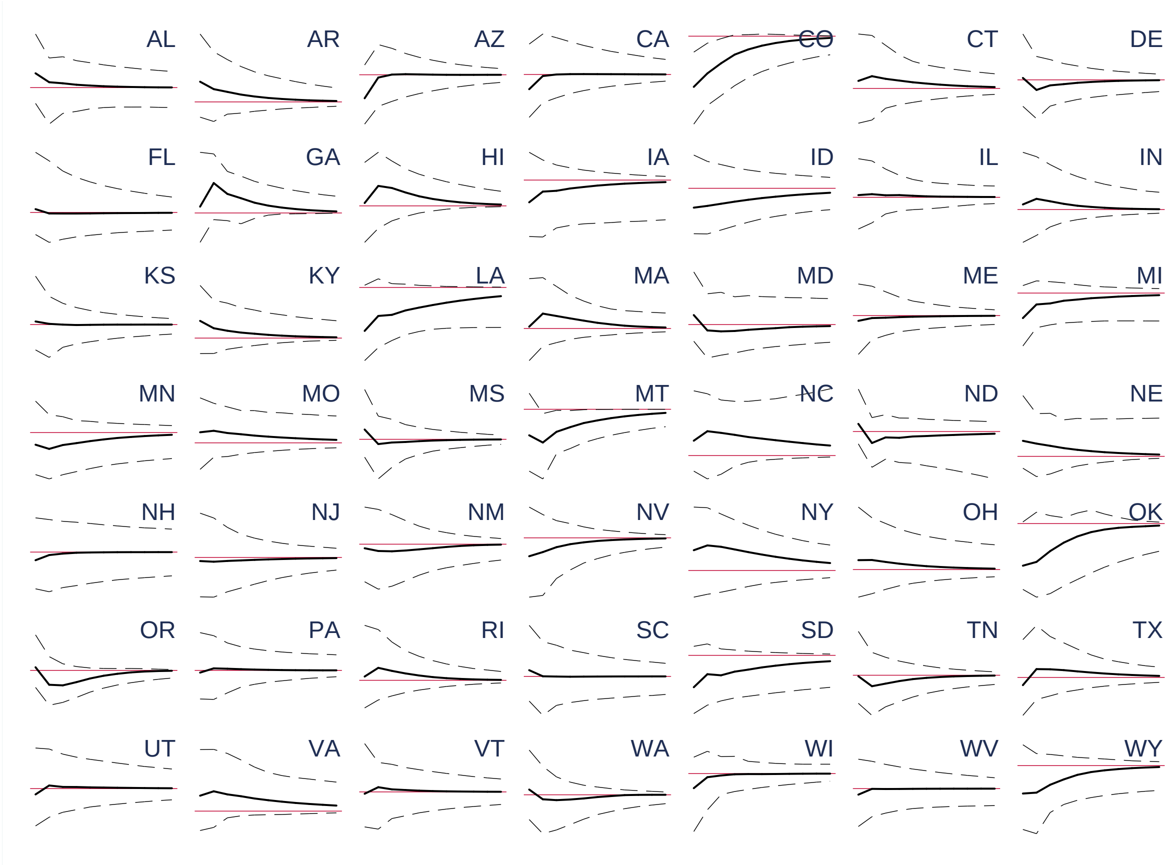


Figure 7: *Evolution of effect of one-off placebo Dividend increase on property crime (with 95% credible intervals), without conditionality by state. Estimated using ARDL(2,1) model. For each state, effects for 10 years since impulse are shown.*

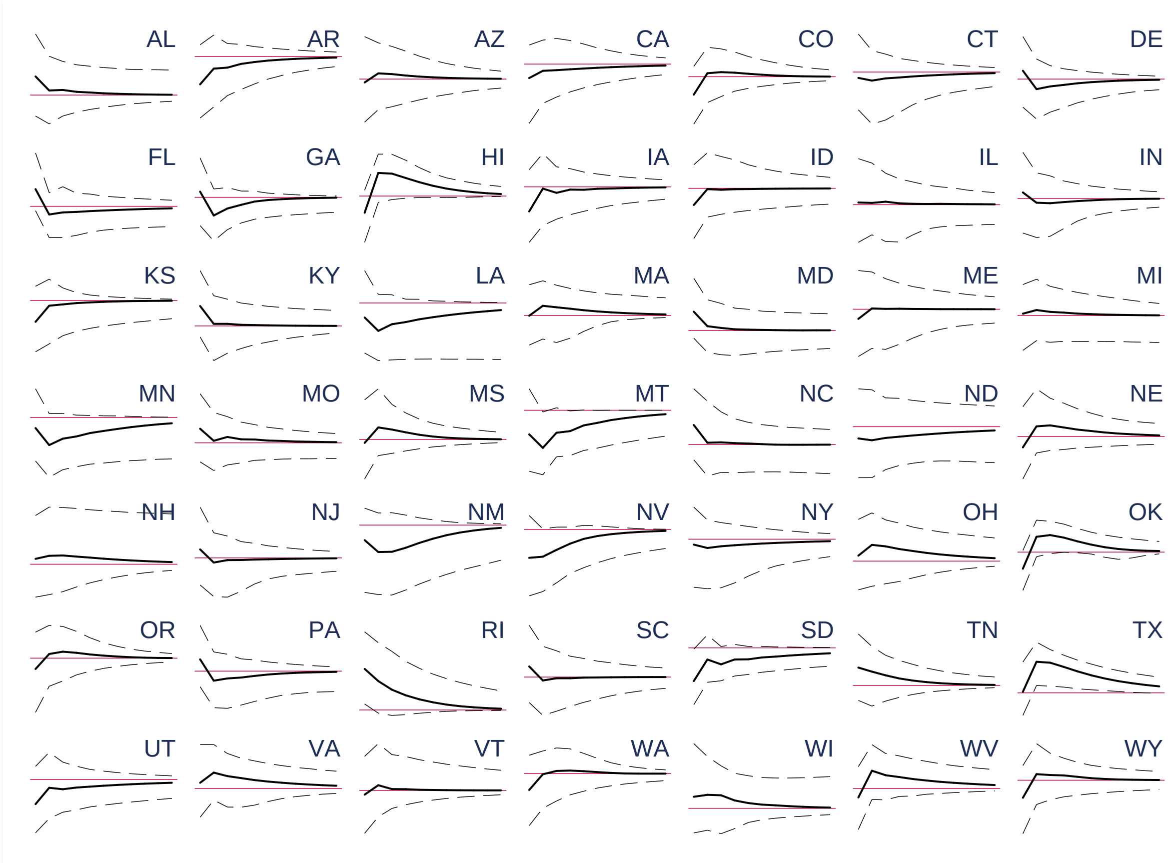
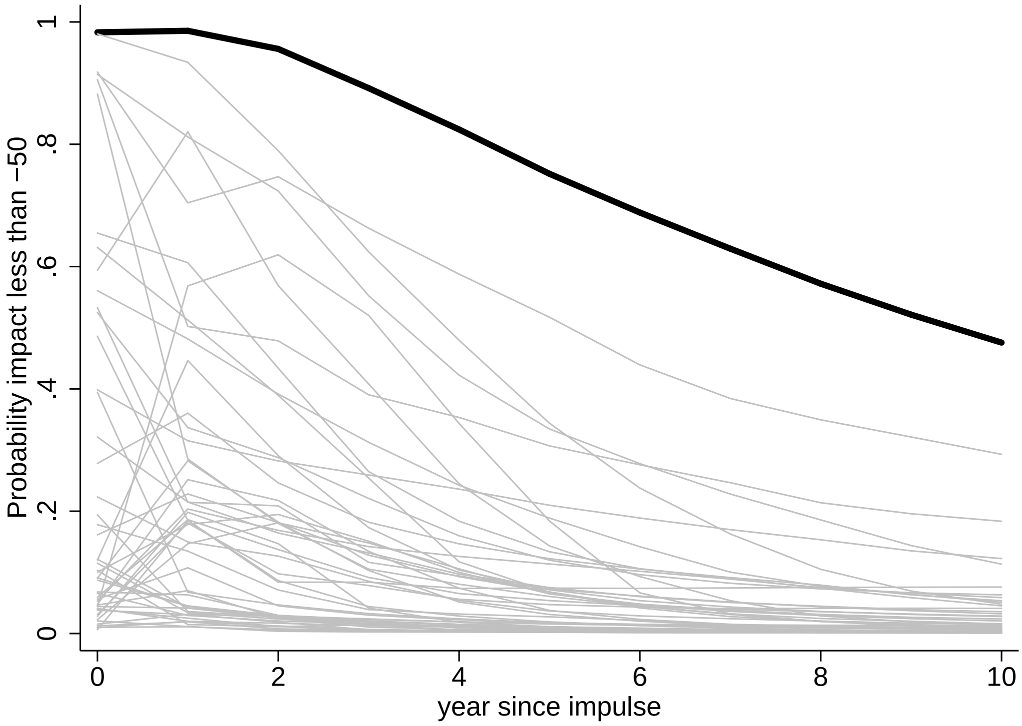
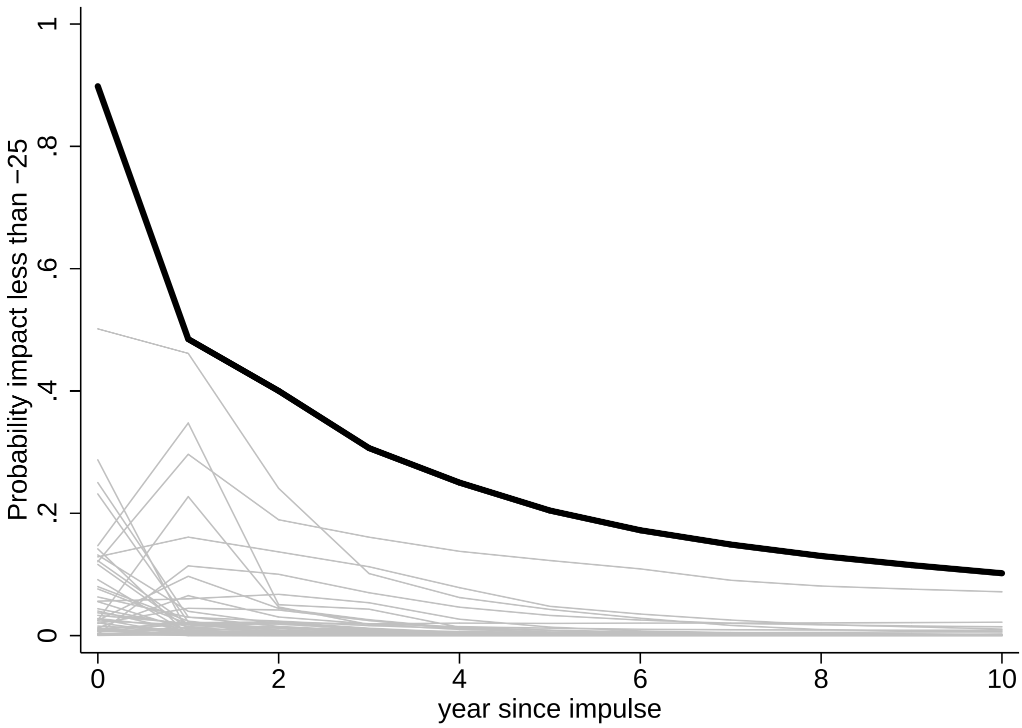


Figure 8: *Evolution of effect of one-off Dividend increase on property crime (with 95% credible intervals), with conditionality by state. Estimated using ARDL(2,1) model. For each state, effects for 10 years since impulse are shown.*



(a) Without conditionality - probability of reduction of 50 crimes per 100,000 residents



(b) With conditionality - probability of reduction of 25 crimes per 100,000 residents

Figure 9: *Probability of a one-off \$100 Dividend increase reducing property crime by at least 50 per 100,000 residents without conditionality (top) or 25 per 100,000 residents with conditionality (bottom). IRFs based on ARDL(2,1) specification for Alaska (thick black line) and all other states (thin grey lines).*