Controls, Not Shocks: Estimating Dynamic Causal Effects in the Face of Confounding Factors[*]

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Abstract

Much of empirical macroeconomics is concerned with first constructing series of 'shocks' to estimate causal effects. In settings with confounding factors, these shocks are typically constructed by orthogonalising causal variables of interest, before being used in a second stage. For a general set of estimators, we show that this two-step 'shock-first' approach can be problematic relative to a simple one-step procedure, which includes confounding factors as control variables directly. When one- and two-step estimators share a common population estimand, there are practical drawbacks to the two-step approach: standard errors will typically be mis-estimated, resulting in unnecessarily more conservative inference. When the estimators differ in population, there is omitted-variable bias in the two-step approach. In linear regression, this bias can arise when additional controls are included in the second stage. For other estimators, such as quantile regression, estimating the shocks first can be especially problematic since uncorrelatedness with omitted variables is insufficient for removing bias. We demonstrate these findings in an application to monetary policy, controlling for central-bank information. One-step estimates indicate that the dis-inflationary consequences of tighter US monetary policy are more robust than previously realised and not subject to a 'price puzzle' at the mean.

Key Words: Identification; Instrumental Variables; Local Projections; Omitted-Variable Bias; Vector Auto-regressions.

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1 Introduction

Identifying the causal impact of economic events or policy interventions is crucial in modern dynamic macroeconomics (Frisch, 1933). A key challenge in any study of causal inference is the presence of confounding factors that simultaneously drive the causal variable and outcome variable of interest. One way to 'partial out' the effect of confounding factors is to include them as control variables in regression analysis. However, the macroeconomics literature has typically taken an alternate route: first estimating 'shocks' by regressing the causal variable of interest on the set of confounding factors, and then saving the residuals.[1] Armed with these shocks, causal effects can be identified by estimating the association between the constructed shock and the outcome variable of interest, typically through a local projection (LP) or vector auto-regression (VAR).

This two-step 'shock-first' approach to controlling for confounding factors is widespread in macroeconomics. Indeed, the construction of a series of shocks is typically viewed as an necessary first step for any study of dynamic causal effects (e.g., Ramey, 2016; Nakamura and Steinsson, 2018). This approach has been used extensively in the monetary policy literature—most famously in Romer and Romer (2004) and subsequent studies that control for central-bank forecasts to identify shocks.[2] A similar two-step approach is also applied in studies that use high-frequency monetary policy 'surprises' and combine instrumental-variable (IV) and regression-control strategies (e.g., Miranda-Agrippino and Ricco, 2021; Bauer and Swanson, 2022; Karnaukh and Vokata, 2022). The approach has also been used in studies estimating the effects of other policies, including fiscal, macroprudential, and trade, as well as non-policy variables like oil-price shocks.[3] While many of these studies have typically relied on standard linear estimators (e.g., OLS and IV), similar approaches have more recently been applied to other estimators. For example, following the popularisation of quantile regression (QR) tools in macroeconomics (Adrian, Boyarchenko, and Giannone, 2019), the two-step 'shock-first' approach has proved popular for estimating the causal effects of various macroeconomic policies on conditional quantiles of outcome variables in the presence of confounding factors.[4]

In this paper, we argue that this popular two-step 'shock-first' approach to controlling for confounding factors is problematic. Instead, identification can be achieved by using a simple one-step multivariate regression that uses confounding factors as control variables. Across a range of applications, relative to this one-step approach, we show that the two-step approach

[1] In the literature estimating the effects of policy, this step is sometimes described as estimating a policy 'reaction function'. Alternatively, it can be interpreted as 'cleaning' or 'purging' the variable of endogeneity. This is also analogous to constructing a shock through Choleski-style identification in a vector auto-regression.

[2] See, for example: Coibion (2012); Cloyne and Hürtgen (2016); Tenreyro and Thwaites (2016); Coibion, Gorodnichenko, Kueng, and Silvia (2017); Champagne and Sekkel (2018); Barnichon and Mesters (2020); Cloyne, Ferreira Mayorga, and Surico (2020); Falck, Hoffmann, and Hürtgen (2021); Holm, Paul, and Tischbirek (2021).

[3] Examples for fiscal policy include: Auerbach and Gorodnichenko (2013); Miyamoto, Nguyen, and Sergeyev (2018). Macroprudential policy examples include: Forbes, Reinhardt, and Wieladek (2017); Ahnert, Forbes, Friedrich, and Reinhardt (2021); Chari, Dilts-Stedman, and Forbes (2022). Metiu (2021) applies the two-step approach to trade policy. Examples for oil-price shocks include: Kilian (2009); Chen, Ren, and Zha (2018); Herrera and Rangaraju (2020).

[4] See, for example: Linnemann and Winkler (2016); Brandão-Marques, Gelos, Narita, and Nier (2021); Gelos, Gornicka, Koepke, Sahay, and Sgherri (2022).

results in some combination of mis-estimation of standard errors, inefficiency and/or inconsistency. In OLS and IV settings, these issues largely relate to practical drawbacks of the two-step approach—including over-estimation of standard errors, and either a loss in efficiency or robustness. In other settings, including QR and other estimators (e.g., Logit and Probit), the issues are more severe. We show that controlling for confounding factors through a shock-first approach in a QR setting will generally fail to identify causal parameters of interest.

Omitted-Variable Bias (OVB) Result. Our key results are grounded purely in the properties of various estimators, without any assumptions about the true data-generating process or underlying causal structures. Our key insight is to note that the difference between estimated coefficients in the one- and two-step approach can always be expressed in terms of an omitted-variable bias (OVB) in the latter. This result is general, applying to a range of estimators which can be defined as the unique solution to an optimisation problem. Armed with this result, we demonstrate the implications across a range of settings covering OLS, IV and QR.

In a simple OLS setting, the Frisch-Waugh-Lovell Theorem (Frisch and Waugh, 1933; Lovell, 1963) provides our point of departure. When the outcome variable is directly regressed on the shock (without controls), the OVB is zero such that the one- and two-step approaches yield identical coefficient estimates. However, the two-step approach still has practical drawbacks. Most notably, the estimated standard errors from two-step estimation will be *over*-estimated when not properly accounting for the fact the constructed shock is a generated regressor.

When auxiliary controls are used in the second-stage—a common approach in LP and VAR settings—the Frisch-Waugh-Lovell benchmark no longer applies. In this setting, we show that OVB may be non-zero such that the one- and two-step coefficient estimates differ. The two-step coefficient estimate is either consistent, but inefficient—if the shock is exogenous with respect to auxiliary controls—or inconsistent—if the shock is correlated with auxiliary controls. We also show that these drawbacks of the two-step approach carry over to settings where the constructed shock is used as an 'external instrument' in an IV setting.

In more general settings, including non-linear estimators such as QR, OVB in the two-step approach is more problematic. We provide an explicit formula for this bias, and demonstrate that it will only be zero under assumptions that are unlikely to hold in practical applications. So any attempt to estimate causal effects in QR settings will generally lead to biased estimates from the two-step approach with observable confounding factors. We demonstrate this for QR, but the result applies to any other estimator for which uncorrelatedness with omitted variables is insufficient for removing OVB (e.g., Logit and Probit).

Implications. Our OVB result has important implications for a range of techniques used in the applied macroeconomics literature estimating dynamic causal effects.

First, our results contribute to the literature discussing estimation and identification via LPs. Recently, Plagborg-Møller and Wolf (2021) have challenged the conventional wisdom that LPs require a measure of a 'shock' for identification by demonstrating that popular structural VAR (SVAR) identification schemes can alternatively be achieved directly in a LP framework.

Our findings build on this by demonstrating that using a measure of a shock in a LP that has been constructed through an orthogonalisation procedure (including via a recursive VAR) is actively problematic relative to simply estimating a LP with appropriate controls.[5] Moreover, we show that adding control variables to a LP to 'mop up' any endogeneity in a generated shock yields inconsistent coefficient estimates, since doing so fails to ensure the shock is exogenous to the entire set of controls used in the first and second-stage regressions.

Second, our results have implications for estimation of dynamic causal effects via 'external instruments' (see Stock and Watson, 2018, for a review). We demonstrate that it is problematic to use constructed residuals as instruments, a common approach in the macroeconomics literature in cases where a potential instrument is believed to not be fully exogenous (e.g., Bauer and Swanson, 2022; Miranda-Agrippino and Ricco, 2023, in monetary policy settings). Our findings apply most directly to LP-IV regressions, but we also show how they carry over to estimation via VARs—in particular proxy SVARs (popularised by Mertens and Ravn, 2013; Gertler and Karadi, 2015). We highlight that the issues that can arise from using a constructed residual as an IV can be avoided by simply including appropriate controls as exogenous variables in LP-IV regressions or proxy SVARs.

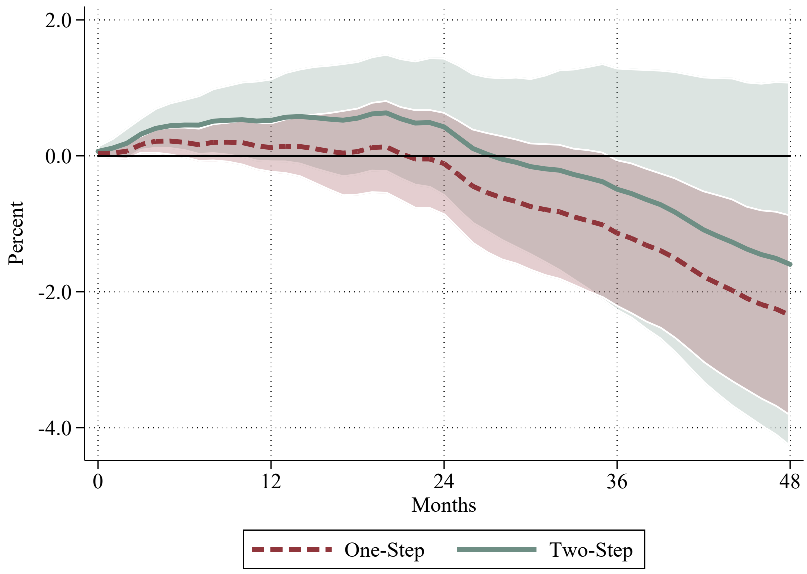
Third, our results have implications for recent literature that goes beyond standard OLS techniques to study the probability of financial crises and drivers of macroeconomic tail risk (Schularick and Taylor, 2012; Adrian et al., 2019). In particular, our findings are relevant for recent attempts to identify the causal effect of policies on tail risk (see, e.g., Linnemann and Winkler, 2016; Brandão-Marques et al., 2021; Gelos et al., 2022). These studies have relied on a two-step approach, which we highlight can generate an OVB relative to a one-step regression and so in general will fail to yield causally-interpretable coefficients.

Applications. We illustrate our results by revisiting popular approaches to estimate the dynamic effects of US monetary policy on US Consumer Price Index (CPI) inflation when controlling for central-bank information. We begin with the approach of Romer and Romer (2004): we construct monetary policy 'shocks' by regressing changes in the Federal Funds target rate on Greenbook forecasts and forecast revisions, and we then run an h-period-ahead LP of CPI on these shocks alongside lags of macroeconomic variables. We compare this to a one-step approach which directly regresses h-period ahead CPI on changes in the Federal Funds target rate alongside Greenbook forecasts, revisions and lagged macroeconomic variables as control variables. In OLS, coefficient estimates differ due to either inefficiency or bias in the two-step approach, and naïve two-step standard error estimates (i.e., without adjusting for generated regressors) can be significantly larger—as Figure 1 demonstrates. Correcting for this through a one-step approach removes a significant portion of the 'price puzzle' identified in previous studies when using this shock series (e.g., Ramey, 2016) and delivers results that are more

[5] We do not currently explicitly consider settings where VAR shocks are estimated using other identifying restrictions (e.g., sign restrictions, long-run restrictions or external instruments) and are then included in a LP, although similar issues around standard-error calculations, efficiency and identification are likely to arise since these shocks are also generated residuals.

robust than previously realised.

Figure 1: Estimated impulse response of US CPI to a US monetary policy shock



Notes: Estimated response of US $\ln(CPI)$ to US monetary policy shock using two-step shock-identification strategy in Romer and Romer (2004), as well as alternative one-step OLS estimator. Estimated using monthly data for the period 1972:01-2007:12. Shaded area denotes 90% confidence bands from Newey and West (1987) standard errors. For more details, see Section 4 and Appendix C.

We then turn to IV approaches using high-frequency monetary policy surprises (following Kuttner, 2001; Gürkaynak, Sack, and Swanson, 2005). Recent studies have noted how these surprises can be correlated with central-bank information within Greenbook forecasts (Miranda-Agrippino and Ricco, 2021) and economic news (Bauer and Swanson, 2022). In our application, we focus on the former of these concerns. For the two-step case, we construct instruments for monetary policy shock identification by regressing high-frequency Federal Funds Futures surprises on Greenbook forecasts and forecast revisions in the first step. In the second stage, we then use two-stage least squares (2SLS) to identify the dynamic effects of interest rate changes, instrumented by the orthogonalised surprise from step one, on CPI. In contrast, the one-step estimator applies 2SLS to a LP regression of CPI inflation on interest rate changes, Greenbook forecasts, revisions and macroeconomic controls—with interest rate changes instrumented with the high-frequency surprise. As in the OLS case, the IV coefficient estimates differ as a result of either inefficiency or bias, and naïve standard error formulas can deliver significantly wider confidence bands, in the two-step approach.

Finally, we consider a QR setting to study the potentially non-linear effects of monetary policy on the distribution of future inflation outturns. When the orthogonalised shocks are used in a second-stage QR, we show that point estimates can be significantly different to a simple one-step quantile regression, reflecting OVB in the two-step approach. In particular, the two-step approach misleadingly implies that changes in monetary policy significantly affect the right-tail of the inflation distribution, much more so than the median. Correcting for this through the one-step approach reveals that monetary policy acts as a 'location shifter' of the entire inflation distribution.

Literature. To the best of our knowledge, our paper is the first to explicitly highlight the drawbacks of the two-step approach popular in the macroeconomics literature. The starting point for our paper—that the two-step shock-first approach is equivalent to a regression-control strategy—follows from the Frisch-Waugh-Lovell theorem, although this point is rarely noted in the literature.[6] Our key contribution is to go beyond this equivalence result—which applies only to estimated coefficients and only in limited settings—to derive an explicit formula linking the one- and two-step approaches that can be applied across a range of settings. This permits a full comparison of the one- and two-step approach—both in-population and in-sample, and across a wide range of estimation techniques (OLS, IV and QR *inter alia*)—allowing us to clearly highlight the drawbacks of the two-step approach.

Our findings for standard-error estimation in OLS relate to Pagan (1984), who discusses inference in econometric models that use generated residuals as regressors. A key result is that OLS standard-error formulas provide a consistent estimator of true standard errors for coefficients on generated residuals in a model where the residual is included as a regressor alongside the fitted value from the first-stage estimation used to construct the residual (see Pagan, 1984, pp. 232-233). This result has since been taken to imply that, in general, standard errors need not be adjusted to account for generated regressors as long as one is interested in testing the null hypothesis that the coefficient on the generated regressor is zero regardless of whether fitted values are also included as controls.[7] Our results implies that this interpretation is invalid and that the typical approach to using generated regressors in macroeconomics—using constructed shocks directly in a LP without controlling for fitted values—leads to incorrect standard errors, and specifically to estimates of the variance that are biased upward. As a consequence, when using typical standard-error formulas in OLS, inference is unnecessarily conservative.

Outline. The remainder of the paper is structured as follows. Section 2 sets out our key insight in general form. Section 3 discusses the implications of the OVB in different settings. Section 4 presents an empirical application. Section 5 concludes.

[6]Notable exceptions to this include, e.g., Angrist and Kuersteiner (2011), Jordà and Taylor (2016), Angrist, Jordà, and Kuersteiner (2018), Barnichon and Brownlees (2019), Jordà, Schularick, and Taylor (2020) and Plagborg-Møller and Wolf (2021).

[7]For example, Coibion and Gorodnichenko (2012, p. 139) state that: "[W]hile we use generated regressors for shocks in our empirical specification, Pagan (1984) shows that under the null hypothesis that the coefficient on a generated regressor is zero, standard errors do not need to be adjusted for generated regressors."

2 Omitted-Variable Bias in the General 'Shock-First' Approach

In this section, after defining notation, we describe our general setup. Using this, we present our key insight: that the difference between the two- and one-step approach can always be expressed in terms of OVB in the former.

2.1 Notation and General Setup

We label y_t as the outcome variable of interest at time t and z_t is the causal variable (e.g., a policy indicator). Throughout, our central focus is on the causal effect of z_t on y_t.

We define $\mathbf{x}_t$ as a $(K_1 + K_2) \times 1$ vector of (non-perfectly-collinear) observable control variables that potentially drive y_t and z_t. We partition these controls into $\mathbf{x}_{1,t}$ and $\mathbf{x}_{2,t}$, which are $K_1 \times 1$ and $K_2 \times 1$ vectors, respectively. We allow $\mathbf{x}_{2,t}$ to be potentially empty (i.e., $K_2 \geq 0$), but restrict $\mathbf{x}_{1,t}$ to be non-empty (i.e., $K_1 > 0$). Our key results do not rely on any assumptions around the causal structure. But to aid with intuition, one can think of $\mathbf{x}_1$ as the 'confounding factors' (i.e., variables that simultaneously drive y_t and z_t). In contrast, $\mathbf{x}_2$ can be thought of as (exogenous) drivers of y_t, which do not simultaneously affect z_t.

We now introduce the widely used two-stage 'shock-first' approach to estimate the effect of z_t on y_t in its general form.

First, the model for z_t is an affine function $\mathbf{x}_{1,t}$:

$$z_t = \mathbf{x}_{1,t}'\boldsymbol{\delta} + \varepsilon_t \tag{1}$$

where $\boldsymbol{\delta}$ is a $K_1 \times 1$ vector of coefficients and ε_t is a population residual.

For now, we only impose the restriction that $\boldsymbol{\delta}$ is a vector of real numbers: $\boldsymbol{\delta} \in \mathbb{R}^{K_1}$. However, to help with intuition, one can note that the coefficients in equation (1) are typically defined as OLS coefficients in which case the following holds by construction:

$$\mathbb{E}\left[\varepsilon_t \mathbf{x}_{1,t}'\right] = 0$$

where, given this orthogonality condition, ε_t can be thought of as changes in z_t 'purged' of the confounding effect of $\mathbf{x}_{1,t}$ (i.e., as exogenous 'shocks' to z_t).

In the second step, a model for the outcome variable of interest y_t as a function of the shock ε_t and (potentially) controls $\mathbf{x}_{2,t}$ is estimated:

$$y_t = \mathbf{x}_{2,t}'\boldsymbol{\alpha} + \varepsilon_t \beta_{2S} + u_t^{2S} \tag{2}$$

where $\boldsymbol{\alpha}$ is a $K_2 \times 1$ vector of coefficients pertaining to the controls $\mathbf{x}_{2,t}$, and β_{2S} is a scalar.

We assume that the estimator underpinning equation (2) is defined as:

$$\{\boldsymbol{\alpha}, \beta\} = \arg\min_{\mathbf{a},b} \left\{ f\left(y_t - \mathbf{x}_{2,t}'\mathbf{a} - \varepsilon_t b\right)\right\} \tag{3}$$

where $f(x)$ summarises the objective function of the estimator. For example, for OLS $f(x) =$

$\mathbb{E}\left[x^2\right]$ and for quantile regression $f(x) = \mathbb{E}\left[\rho(x)\right]$, where $\rho(x)$ is the check function. For our purposes, the function $f(\cdot)$ must be well-defined and have a unique minimisation point. Beyond that, we place no further restrictions on $f(\cdot)$ in this section.

It is important to note that this setup is general. In addition to subsuming a range of estimation methods (i.e., different functional forms for $f(\cdot)$) this framework applies to a wide range of applications in macroeconomics. For instance, when the set of covariates $\mathbf{x}_{1,t}$ includes lags of z_t plus lagged (and some contemporaneous) values of other variables, then ε_t from equation (1) is equivalent to a shock from a recursively-identified SVAR model. Alternatively, when $\mathbf{x}_{1,t}$ contains forecasts made at time t then ε_t can be thought of as a shock purged of central-bank information as in, e.g., Romer and Romer (2004).

In addition, the outcome variable can be defined as h-period ahead values of y, in which case the second stage regression (2) amounts to a single h-specific regression from a LP model. Alternatively, when $\mathbf{x}_{2,t}$ includes p lagged values of y_t alongside p lags of other macroeconomic variables, then regression (2) can be thought of as a single equation from a VAR model where ε_t is used as an exogenous variable (as in, e.g., Paul, 2020) and β_{2S} captures the contemporaneous response of y_t to ε_t.[8]

The key question in this paper is whether this two-step shock-first approach to control for confounding factors is appropriate. Intuitively, in instances where all confounding factors are assumed to be observable, identification of the causal effect of interest can instead be achieved through a simpler one-step regression. And so, in the next sub-section, we propose a general formula for comparing β_{2S} to estimates derived from a one-step regression.

2.2 An Alternative One-Step Estimator

The alternative one-step approach to controlling for confounding factors involves regressing y_t on z_t and the full set of controls $\mathbf{x}_t$:

$$\begin{aligned} & y_t = \mathbf{x}_{1,t}'\boldsymbol{\theta_1} + \mathbf{x}_{2,t}'\boldsymbol{\theta_2} + z_t\beta_{1S} + u_t^{1S} \\ \iff \quad & y_t = \mathbf{x}_t'\boldsymbol{\theta} + z_t\beta_{1S} + u_t^{1S} \end{aligned} \tag{4}$$

where $\boldsymbol{\theta}$ is a $K \times 1$ vector of coefficients (where $K = K_1 + K_2$) and β_{1S} is a scalar. Combined, these estimators are defined by:

$$\{\boldsymbol{\theta}, \beta_{1S}\} = \arg\min_{\boldsymbol{\vartheta}, b} \left\{ f\left(y_t - \mathbf{x}_t'\boldsymbol{\vartheta} - z_t b\right)\right\} \tag{5}$$

where the function $f(\cdot)$ matches that used in the second stage of the shock-first estimator (3).

Intuitively, like β_{2S}, we seek to interpret estimates of β_{1S} as the effect of z_t after partialling out any confounding effects of $\mathbf{x_t}$. The following Proposition clarifies that the difference between the one- and two-step approaches can always be expressed in terms of an OVB that impacts the two-step estimator.

[8] We explicitly extend our setting to impulse-response estimation via SVARs in Appendix B.

Proposition 1. (OVB in the General Two-Step Approach) *Consider the following 'hybrid' model of y_t on ε_t and the full set of K controls $\mathbf{x}_t$:*

$$
\begin{aligned}
& y_t = \mathbf{x}_{1,t}'\boldsymbol{\phi}_\mathbf{1} + \mathbf{x}_{2,t}'\boldsymbol{\phi}_\mathbf{2} + \varepsilon_t\beta_{Hyb} + u_t^{Hyb} \\
\iff \quad & y_t = \mathbf{x}_t'\boldsymbol{\phi} + \varepsilon_t\beta_{Hyb} + u_t^{Hyb}
\end{aligned} \tag{6}
$$

where ε_t is defined as in equation (1) for any real vector of coefficients $\boldsymbol{\delta} \in \mathbb{R}^{K_1}$, where $K_1 \leq K$, and $\boldsymbol{\phi}$ is a $K \times 1$ vector of coefficients. Assume that β_{Hyb} satisfies:

$$
\{\boldsymbol{\phi}, \beta_{Hyb}\} = \arg\min_{\boldsymbol{\varphi}, b} \left\{ f\left(y_t - \mathbf{x}_t'\boldsymbol{\varphi} - \varepsilon_t b\right)\right\} \tag{7}
$$

where the objective function $f(\cdot)$ matches that used in two- and one-step estimators (3) and (5). Then the following holds:

$$
\beta_{Hyb} = \beta_{1S} \quad \textit{and} \quad \beta_{2S} = \beta_{1S} + \Omega
$$

where Ω is defined as omitted-variable bias from the exclusion of $\mathbf{x}_{1,t}$ in regression (2).

Proof: Substituting the definition of ε_t from equation (1) into the hybrid estimator (7), we have:

$$
\begin{aligned}
\{\boldsymbol{\phi}, \beta_{Hyb}\} &= \arg\min_{\boldsymbol{\varphi}, b} \left\{ f\left(y_t - \mathbf{x}_t'\boldsymbol{\varphi} - (z_t - \mathbf{x}_{\mathbf{1},\mathbf{t}}\boldsymbol{\delta})b\right)\right\} \\
&= \arg\min_{\boldsymbol{\varphi}, b} \left[f\left(y_t - \mathbf{x}_{1,t}'\boldsymbol{\varphi}_\mathbf{1} - \mathbf{x}_{2,t}'\boldsymbol{\varphi}_\mathbf{2} - (z_t - \mathbf{x}_{\mathbf{1},\mathbf{t}}\boldsymbol{\delta})b\right)\right\} \\
&= \arg\min_{\boldsymbol{\varphi}, b} \left\{ f\left(y_t - \mathbf{x}_{1,t}'(\boldsymbol{\varphi}_\mathbf{1} - b\boldsymbol{\delta}) - \mathbf{x}_{2,t}'\boldsymbol{\varphi}_\mathbf{2} - z_t b\right)\right\}
\end{aligned}
$$

When $f(\cdot)$ has a unique minimand, we know from the one-step estimator (5), which this is equivalent to, that the solution to the above minimisation problem is given by:[9]

$$
\begin{aligned}
\boldsymbol{\phi}_\mathbf{2} &= \boldsymbol{\theta}_\mathbf{2} \\
\boldsymbol{\phi}_\mathbf{1} - \beta_{Hyb}\boldsymbol{\delta} &= \boldsymbol{\theta}_\mathbf{1} \quad \implies \quad \boldsymbol{\phi}_\mathbf{1} = \boldsymbol{\theta}_\mathbf{1} + \beta_{Hyb}\boldsymbol{\delta} \\
\beta_{Hyb} &= \beta_{1S}
\end{aligned}
$$

Since regression (6) is the same as regression (2), albeit with additional covariates, the difference between coefficients can be expressed in terms of omitted-variable bias:

$$
\beta_{2S} = \beta_{Hyb} + \Omega \quad \implies \quad \beta_{2S} = \beta_{1S} + \Omega
$$

which completes the proof. □

In line with the setup described in Section 2.1, this Proposition is general. In equation (1), we only require $\boldsymbol{\delta}$ to be some vector of real numbers. And in the second stage, we only

[9] The result that $\beta_{Hyb} = \beta_{2S}$ holds for any estimator that is equivariant to reparametrisation of design. See, for example, Koenker and Bassett (1978) who show this property holds for QR.

require coefficients to be defined as a unique solution to some objective function $f(\cdot)$—covering estimation via OLS, maximum-likelihood estimation, QR etc. We also allow $f(\cdot)$ to be defined either as the population objective function—e.g., for OLS $f(x) = \mathbb{E}\left[x^2\right]$—or as the in-sample objective function—e.g., for OLS for some sample of length T, $f(x) = \frac{1}{T}\sum_{t=1}^{T}\left[x^2\right]$.

Note, that we follow convention and define OVB in Proposition 1 as the difference in coefficients between a 'long' regression and a 'short' regression that excludes some covariates.

3 Omitted-Variable Bias in the 'Shock-First' Approach for Specific Estimators

In this section, we set out the practical implications of the OVB from Proposition 1 for a range of commonly used estimators where the two-step shock-first approach has been widely applied, specifically: OLS, IV, and QR.[10] We argue that, under reasonable assumptions, in an OLS setting the OVB term in Proposition 1, Ω, will be zero such that β_{2S} and β_{1S} will be identical. In this case then, the two-step approach merely adds a layer of complexity to estimation: estimates of β_{2S} in equation (2) could instead be recovered from a single regression, in which case standard-error calculations would be simpler and, in some cases, there may be in-sample efficiency gains. In a QR, however, the two-step approach is more problematic as the OVB term will only be zero under assumptions that are unlikely to hold in practice. And so estimates of causal effects will be biased in the two-step approach if being used to assess effects on conditional quantiles in the face of confounding factors. These results rely purely on the properties of OLS and QR coefficients without any assumptions on the true data generating process.

3.1 Ordinary Least Squares (OLS) and the Conditional Mean

Consider a case where both regressions (1), (2) and (4) are estimated via OLS. From Proposition 1 and the formula for OVB in OLS, we can express the difference between estimators in the following Result:

Result 1. (OLS) *Define coefficients across regressions* (1), (2), (4) *and* (6) *as OLS coefficients. Then the following formula relates the two-step coefficient β_{2S} in equation* (2) *and the one-step coefficient β_{1S} in equation* (4)*:*

$$\begin{aligned}
\beta_{2S} &= \beta_{1S} + \Omega_{OLS} \\
&= \beta_{1S} + \left[\mathbf{A}\,\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{1,t}\right] + \mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right]\mathbf{B}\right]\boldsymbol{\phi}_1 \\
&= \beta_{1S} + \mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right]\mathbf{B}\,\boldsymbol{\phi}_1
\end{aligned}$$

where $\mathbf{B} \equiv \left[\mathbb{E}\left[\varepsilon_t^2\right]\mathbb{E}\left[\mathbf{x}_{2,t}\mathbf{x}'_{2,t}\right] - \mathbb{E}\left[\varepsilon_t \mathbf{x}_{2,t}\right]\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right]\right]^{-1}\mathbb{E}\left[\mathbf{x}_{2,t}\mathbf{x}'_{1,t}\right]$ *and $\boldsymbol{\phi}_1$ are coefficient loadings on $\mathbf{x_{1,t}}$ in equation* (6).

[10] In Appendix A, we explicitly turn to questions around causality and the 'true' parameter of interest.

Proof: The first line of this Result follows directly from Proposition 1. The second line then follows from the formula for OVB in OLS. The final line uses the the fact that since ε_t is an OLS population residual, by construction, $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{1,t}\right] = \mathbf{0}$. $\square$

We now discuss the implications of this result. We propose that in many practical OLS applications this OVB term is likely to be zero. Nevertheless, there are still notable practical drawbacks of the two-step approach. To do so, we consider two cases.

Case 1. (No Auxiliary Controls) *$\mathbf{x}_{2,t}$ is an empty vector such that $K_2 = 0$.*

We first consider a case where no auxiliary controls are included in the second-stage regression (2). This is a common approach in applied work since, if the first stage (1) is thought to adequately identify a 'shock', then no auxiliary controls are needed in the second stage to identify the causal effect of interest. In this case, there will be no OVB in the two-step estimator. The below Corollary formalises this:

Corollary 1. (General OLS Equivalence without Controls) *Under Case 1, $\beta_{2S} = \beta_{1S}$ and $\hat{\beta}_{2S} = \hat{\beta}_{1S}$.*

Proof: This follows directly from the Frisch-Waugh-Lovell Theorem.[11] $\square$

In this case, point estimates from the one- and two-step approaches will be exactly equivalent. But there are important practical advantages to the one-step approach for the calculation of standard errors. Estimating the standard errors of $\hat{\beta}_{1S}$ is more straightforward since, unlike for $\hat{\beta}_{2S}$, this calculation is not complicated by the generated regressor $\hat{\varepsilon}$, the sample residual from equation (1). One way to estimate standard errors to account for the generated regressor would be to run a bootstrap over both regressions (1) and (2), although applied work rarely follows this approach. Under naïve estimates (i.e., without correcting for first-stage uncertainty in the estimation of $\hat{\varepsilon}$), estimated standard errors will generally differ for $\hat{\beta}_{2S}$ and $\hat{\beta}_{1S}$, even though these are equivalent estimators (and so have equivalent finite-sample properties). Interestingly, this mis-estimation likely results in an *over*-estimation, rather than under-estimation, of standard errors in the two-step approach. The following Corollary formalises this:

Corollary 2. (OLS Standard Errors without Controls) *Under Case 1, when the number of observations T is large relative to the number of regressors K_1, for homoskedastic-only standard error formulas, the estimated variance of the two-step coefficient $\hat{\beta}_{2S}$ is weakly greater than the estimated variance of the one-step coefficient $\hat{\beta}_{1S}$:*

$$\widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{2S}) \geq \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{1S})$$

[11] Angrist and Pischke (2009) refer to this formulation of the Frisch-Waugh-Lovell Theorem as the 'Regression Anatomy Formula' (p. 27).

Proof: This follows from that fact that the regression anatomy formula carries over to estimated standard errors (see, e.g., Angrist and Pischke, 2014), and so:

$$\widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{2S}) \geq \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{1S}) \quad \Longleftrightarrow \quad \frac{1}{T-1}\mathbb{V}\mathrm{ar}(\hat{u}_t^{2S}) \geq \frac{1}{T-(K_1+1)}\mathbb{V}\mathrm{ar}(\hat{u}_t^{1S})$$

Note also that the sample residual from the hybrid regression (6), $\hat{u}_t^{Hyb}$, and the sample residual from the one-step regression (4), $\hat{u}_t^{1S}$, are the same, i.e. $\hat{u}_t^{Hyb} = \hat{u}_t^{1S}$. In addition, since adding covariates reduces sample residual variance in OLS, then the following must hold: $\mathbb{V}\mathrm{ar}(\hat{u}_t^{2S}) \geq \mathbb{V}\mathrm{ar}(\hat{u}_t^{Hyb})$. And so when T is large relative to K_1, then: $\widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{2S}) \geq \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta}_{1S})$. □

Intuitively, we can think of this mis-estimation of standard errors in terms of omitted variables. Excluding $\mathbf{x}_1$ from the second stage in the two-step approach excludes a variable that is, by construction, completely uncorrelated with the variable of interest ε_t, while (potentially) having explanatory power for y_t. So, adding this regressor into the second stage serves to lower the estimated standard error on the coefficient of interest. In which case, by Proposition 1, one may as well have run the whole regression as a one-step regression with z_t instead of ε_t.

Although Corollary 2 is written for a specific case, this intuition carries over to other standard-error formulas. Here, again, the estimated variance of the two-step coefficient will typically be over-estimated relative to the one-step coefficient since in effect the two-step approach excludes an explanatory variable that is uncorrelated with ε_t, while having explanatory power for y_t. In practical applications this over-estimation of standard errors may be large—for example, when $\mathbf{x}_{1,t}$ has significant explanatory power for y_t, then $\mathbb{V}\mathrm{ar}(\hat{u}_t^{2S}) \gg \mathbb{V}\mathrm{ar}(\hat{u}_t^{1S})$. This will likely be the case for example when $\mathbf{x_{1,t}}$ includes forecasts at time t of the outcome variable of interest (as in, e.g., Romer and Romer, 2004). We demonstrate this point with an application in Section 4.

Case 2. (Auxiliary Controls) $\mathbf{x}_{2,t}$ *is a non-empty vector such that* $K_2 > 0$.

We now consider an alternative case in which controls are included in the second stage, such that $\mathbf{x}_{2,t}$ is non-empty. In many practical applications, these second-stage controls are typically assumed to reflect other drivers of y_t that are included to reduce estimated standard errors and, either explicitly or implicitly are assumed to be uncorrelated with ε_t. In this case, the OVB can again be zero, as the below Corollary states:

Corollary 3. (Specific OLS Equivalence with Controls) *Under Case 2, if* $\mathbb{E}\left[\varepsilon_t \mathbf{x}_{2,t}'\right] = \mathbf{0}$, *then* $\beta_{2S} = \beta_{1S}$.

Proof: This follows directly from Result 1. □

This Corollary implies that, in practice, as long as the shock is constructed correctly in the first stage to be genuinely exogenous (i.e., uncorrelated with any other drivers of y), then the OVB at the population level will be zero. But there are still important practical drawbacks of the two-step approach in this case. As before, we have that $\mathbb{V}\text{ar}(\hat{u}_t^{2S}) \geq \mathbb{V}\text{ar}(\hat{u}_t^{1S})$ which mechanically inflates estimated standard errors in the two-step approach. In addition, in-sample estimated coefficients will likely differ across the two- and one-step approaches, even when the shock identified in the first stage is genuinely exogenous in population. Hence there may be finite-sample efficiency costs to using the two-step approach. Note for example, that under Gauss-Markov assumptions, the OLS-estimator $\hat{\beta}_{1S}$ is the most efficient among the class of linear unbiased estimators, suggesting an efficiency cost from the two-step approach whenever these assumptions hold. We demonstrate the size of this efficiency loss for an example data-generating process in Section D.

Suppose instead that the shock is not constructed correctly, such that $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right] \neq \mathbf{0}$. In this case, the one-step approach will be *as if* the shock was constructed to be orthogonal to $\mathbf{x}_{1,t}$ *and* $\mathbf{x}_{2,t}$ in the first-stage. In contrast the two-step approach will be biased since the shock from the first-stage is correlated with other drivers of the outcome variable, and Result 1 provides an exact formula for this bias. In this sense, the one-step approach is more robust since, unlike the two-step approach, it does not require correctly partitioning $\mathbf{x}_t$ into $\mathbf{x}_{1,t}$ and $\mathbf{x}_{2,t}$ such that: $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right] = \mathbf{0}$.

Taken together, our results imply that coefficient estimates on the causal driver of interest derived from a simpler one-step OLS regression (4) are preferable to those from the popular two-step approach typically applied in macroeconomic settings. In cases where no auxiliary controls are included in the second-stage regression, the one-step and two-step approaches will yield identical coefficient estimates, but the two-step approach will generally over-estimate standard errors. When auxiliary controls are included in the second-stage, the two approaches will generally yield different coefficient estimates. In this case, as well as complicating standard error calculations, the two-step approach may additionally be less efficient and less robust than the one-step approach.

3.2 Instrumental Variables (IV)

We now consider the implications of Proposition 1 and Result 1 for estimation via IV. In particular, we consider a setting where OLS residuals from a first-stage regression are then used as instruments in an IV regression for the outcome variable of interest y_t. This approach has been used in a variety of studies that use the Romer and Romer (2004) shock series as an instrument (e.g., Barnichon and Mesters, 2020). It also appears in studies that 'clean' high-frequency monetary policy surprises of their predictable content before using them in an IV setting (e.g., Miranda-Agrippino and Ricco, 2021; Bauer and Swanson, 2022). As before, we compare this to a simpler one-step IV regression with control variables. Since IV regression coefficients can be expressed as the ratio of OLS regression coefficients, the problems associated with using a generated residual in a second-stage OLS regression discussed in the previous sub-section carry over to IV.

In order to discuss the implications for IV we develop our general setting to distinguish two indicators. We continue to be interested in identifying the causal impact of z_t on y_t. But we now introduce an additional variable, m_t, as a potential instrument. For intuition, we can think of m_t as a valid instrument, satisfying exogeneity and relevance conditions, for z_t *only* after controlling for observable factors $\mathbf{x}_{1,t}$.[12] In this setting, the two-step estimation approach typically involves estimating the following model in a first step to orthogonalise the instrument with respect to observables in $\mathbf{x}_{1,t}$:

$$m_t = \mathbf{x}_{1,t}'\boldsymbol{\delta} + \varepsilon_t \tag{8}$$

where $\boldsymbol{\delta}$ is a $K_1 \times 1$ vector of coefficients. In the second stage, the outcome variable of interest is regressed on z_t and (potentially) controls $\mathbf{x}_{2,t}$, where ε_t is used as an instrument for z_t:

$$y_t = \mathbf{x}_{2,t}\boldsymbol{\alpha} + z_t\beta_{2S}^{IV} + u_{t,IV}^{2S} \tag{9}$$

where β_{2S}^{IV} represents the two-step IV coefficient. We are interested in comparing β_{2S}^{IV} to β_{1S}^{IV} from the following IV-regression:

$$y_t = z_t\beta_{1S}^{IV} + \mathbf{x}_t'\boldsymbol{\theta}^{IV} + u_{t,IV}^{1S} \tag{10}$$

with m_t as an instrument for z_t.

Because IV coefficients can be written as the ratio of OLS coefficients from a 'first-stage' and 'reduced-form' regression (see, e.g., Angrist and Pischke, 2009, p. 122), we can write:

$$\beta_{2S}^{IV} \equiv \frac{\beta^{RF}}{\beta^{FS}}$$

where β^{RF} and β^{FS} are defined from the following OLS regressions:

$$\begin{aligned} z_t =& \mathbf{x}_{2,t}\boldsymbol{\pi}^{FS} + \varepsilon_t\beta^{FS} + e_t^{FS} \\ y_t =& \mathbf{x}_{2,t}\boldsymbol{\pi}^{RF} + \varepsilon_t\beta^{RF} + e_t^{RF} \end{aligned}$$

Given this, the results from Section 3.1 carry over almost directly to this setting. Starting with Case 1, where auxiliary controls $\mathbf{x_{2,t}}$ are not included in the second-stage, we show that the two-step approach delivers identical coefficient estimates as the one-step approach, while over-estimating the degree of uncertainty around these estimates:

Corollary 4. (IV without Controls) *Under Case 1, then, if ε_t is defined as in equation (8), then: $\beta_{2S}^{IV} = \beta_{1S}^{IV}$ and $\hat{\beta}_{2S}^{IV} = \hat{\beta}_{1S}^{IV}$. In addition, when the number of observations T is large relative to the number of regressors $K1$, for homoskedastic-only standard error formulas, then:*

$$\begin{aligned} \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta_{2S}^{FS}}) \geq \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta_{1S}^{FS}}) \\ \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta_{2S}^{IV}}) \geq \widehat{\mathbb{V}\mathrm{ar}}(\hat{\beta_{1S}^{IV}}) \end{aligned}$$

[12]As before, we do not need this assumption for our results below, although this may help with intuition to understand the setting we are interested in.

Proof: The first result follows by applying Frisch-Waugh-Lovell Theorem to the numerator and denominator of the IV estimator, defined as the ratio of OLS coefficients. The second result follows directly from Corollary 2 as it refers to standard errors on OLS coefficients from a first-stage regression. We omit the final proof on IV standard errors as the logic is very similar to that for Corollary 2. □

These results are very similar to the OLS setting: the two- and one-step deliver identical coefficient estimates, but with larger estimated standard errors in the former. Unlike in OLS, there are now *two* implications of note related to standard error calculations. The first is that the two-step approach leads to an under-estimation of F-statistics from the first stage, implying a tendency to mistakenly reject 'strong' instruments as 'weak', while the second relates to an over-estimation of standard errors on the IV-coefficient of interest.

Likewise, it is straightforward to show that other results for OLS carry over to IV regression. If auxiliary controls $\mathbf{x}_{2,t}$ were included in the IV regression (9), as in Case 2, then β_{2S}^{IV} will suffer from OVB if $\mathbb{E}[\varepsilon_{\mathbf{t}}\mathbf{x_{2,t}}'] \neq \mathbf{0}$ and will be less efficient than the one-step IV with the full vector $\mathbf{x_t} = [\mathbf{x_{1,t}}', \mathbf{x_{2,t}}']'$ as controls when $\mathbb{E}[\varepsilon_t \mathbf{x}_{2,t}'] = \mathbf{0}$.[13]

We can further demonstrate that the results here carry-over to estimation via SVARs identified with external instruments. In Appendix B, we show that using generated residuals as an instrument in such a 'Proxy SVAR' generates similar issues to the IV setting, and that these can be avoided by simply including confounding factors as exogenous variables in the VAR (a 'Proxy SVARX').

Our results thus far suggest that, *vis-à-vis* a one-step approach, the two-step approach to controlling for confounding factors in an OLS—and, by extension, IV—can create a range of issues for the applied econometrician. Nevertheless, so long as the shock constructed in the first-stage is constructed so as to be exogenous with respect to other potential drivers of the outcome variable of interest, these issues are confined to mis-estimation of standard errors and inefficiency. In this 'exogeneity' case, the one-step and two-step share a common population estimand. We now show that the two-step approach is more problematic once we move beyond an OLS setting, since uncorrelatedness between the causal shock ε_t and the omitted variable $\mathbf{x}_{1,t}$ in these settings is not, in general, sufficient to ameliorate OVB in population.

3.3 Quantile Regression (QR) and Conditional Quantiles

To consider settings beyond the conditional-expectation function, in this sub-section we specifically focus on a case in which, in the first stage, the shock is constructed via OLS and is then used in second-stage *quantile* regression. This approach has been adopted to study the effects of various 'shocks' on conditional quantiles of outcome variables of interest. For example, Linnemann and Winkler (2016) use it to assess the effects of fiscal policy on the GDP distribu-

[13] In the former case, an explicit expression for OVB can be found by simply applying the formula from Result 1 to the numerator and denominator of the IV estimand. In the latter case, inefficiency follows from the fact the two approaches have the same estimand and that 2SLS is efficient under Gauss-Markov assumptions (Wooldridge, 2012, p. 553).

tion, Gelos et al. (2022) apply it to assess the effects of capital-flow measures on 'capital flows at risk', and Brandão-Marques et al. (2021) study the influence of macroprudential policy on growth-at-risk.

In this QR setting, we have the following expression for the difference between coefficients estimated under the two-step approach and a simple one-step quantile regression:

Result 2. (QR) *Define the coefficients in equation (1) as OLS coefficients, and the coefficients in equations (2), (4) and (6) as QR coefficients for some specific quantile $\tau \in (0,1)$, implying the following functional form for the objective function $f(\cdot)$ in equations (3), (5) and (7):*

$$f(x) = \mathbb{E}\left[\rho_\tau(x)\right]$$

where $\rho_\tau(x) = (\tau - \mathbb{1}(x \leq 0))$ is the check function. Then the following formula relates the two-step coefficient at the τ-th quantile $\beta_{2S}(\tau)$ in equation (2) and the one-step coefficient $\beta_{1S}(\tau)$ in equation (4):

$$\begin{aligned}\beta_{2S}(\tau) &= \beta_{1S}(\tau) + \Omega_{QR}(\tau) \\ &= \beta_{1S}(\tau) + \left[\mathbf{A}\,\mathbb{E}\left[w_\tau \epsilon_t \mathbf{x}'_{1,t}\right] + \mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}'_{2,t}\right]\mathbf{B}\right]\boldsymbol{\phi}_{\mathbf{1}}(\tau)\end{aligned}$$

where:

$$\begin{aligned}\mathbf{A} \equiv &[\mathbb{E}\left[w_\tau \varepsilon_t^2\right]^{-1} + \mathbb{E}\left[w_\tau \varepsilon_t^2\right]^{-2}\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}_{2,t}\right]\left[\mathbb{E}\left[w_\tau \mathbf{x}_{2,t}\mathbf{x}'_{2,t}\right]\right. \\ &\left.-\mathbb{E}\left[w_\tau \varepsilon_t^2\right]^{-1}\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}_{2,t}\right]\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}'_{2,t}\right]\right]^{-1}\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}_{2,t}\right] \\ \mathbf{B} \equiv &\left[\mathbb{E}\left[w_\tau \mathbf{x}_{2,t}\mathbf{x}'_{2,t}\right] - \mathbb{E}\left[w_\tau \varepsilon_t^2\right]^{-1}\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}_{2,t}\right]\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}'_{2,t}\right]\right]^{-1}\mathbb{E}\left[w_\tau \mathbf{x}_{2,t}\mathbf{x}'_{1,t}\right]\mathbb{E}\left[w_\tau \varepsilon_t^2\right]\end{aligned}$$

and $\boldsymbol{\phi}_1(\tau)$ are coefficient loadings on $\mathbf{x_{1,t}}$ in equation (6). In addition the 'weights' are defined as: $w_\tau = \int_0^1 f_{u_\tau^{Hyb}}(u\left[\mathbf{x}'_{2,t}\boldsymbol{\pi}(\tau) + \varepsilon_t \beta(\tau) - \mathbf{x}'_t \boldsymbol{\phi}(\tau) - \varepsilon_t \beta_{hyb}(\tau)\right]|\mathbf{x}_t, \varepsilon_t)\,\mathrm{d}u/2$.

Proof: The first line follows directly from Proposition 1 and the second line follows from the formula for OVB for QR from Angrist, Chernozhukov, and Fernández-Val (2006). □

We now argue that this OVB is likely to be more problematic for identification in QR setting than in OLS. Unlike for OLS, the assumptions needed for the OVB term in QR $\Omega_{QR}(\tau)$ to be zero are unlikely to hold in practical applications. As before, we first consider a simple setting where no auxiliary controls are included in the second-stage regression (i.e., Case 1).

In Case 1, unlike in OLS, there will still likely be omitted-variable bias in the two-step estimator. The below Corollary formalises this:

Corollary 5. (OVB in QR without Controls) *Under Case 1, the following relates* $\beta_{2S}(\tau)$ *and* $\beta_{1S}(\tau)$*:*

$$\begin{aligned}\beta_{2S}(\tau) &= \beta_{1S}(\tau) + \Omega_{QR}(\tau) \\ &= \beta_{1S}(\tau) + \boldsymbol{\phi}_{\mathbf{1}}(\tau)\frac{\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}'_{1,t}\right]}{\mathbb{E}\left[w_\tau \varepsilon_t^2\right]}\end{aligned}$$

where $w_\tau = \int_0^1 f_{u_\tau^{Hyb}}\left[u\ \left(\varepsilon_t\beta(\tau) - \mathbf{x}'_{1,t}\boldsymbol{\phi}_1(\tau) - \varepsilon_t\beta_{hyb}(\tau)\right)|\varepsilon_t, \mathbf{x}'_{1,t}\right]\ \mathrm{d}u/2.$

Proof: This follows from Result 2. □

Note that unlike in an OLS-setting, even though $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{1,t}\right] = \mathbf{0}$ by construction, this does not imply $\Omega_{QR}(\tau) = 0$. We can still have $\mathbb{E}\left[\omega_\tau \varepsilon_t \mathbf{x}'_{1,t}\right] \neq \mathbf{0}$.

In order for OVB to be zero, we need additional assumptions, which are unlikely to hold in practical applications. For example, if the weights w_τ are constant across $\left[\varepsilon_t, \mathbf{x}'_{1,t}\right]$ then it is straightforward to show that $\Omega_{QR}(\tau) = 0$. As Angrist et al. (2006) explain, this will be approximately true when the model for y_t is a pure location model. But this assumption is unlikely to hold in practice given the motivation for using quantile regression to estimate equation (2) rests on the idea that the covariates have differing effects across quantiles, which would be missed by estimation via OLS.

The situation is similar when moving to a setting where auxiliary controls are included in the second-stage regression (i.e., Case 2). In this case, unlike in OLS, we can still get OVB in a quantile regression setting when the shock is constructed correctly to be uncorrelated with other drivers of y_t:

$$\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right] = \mathbf{0} \quad \nRightarrow \quad \beta_{2S}(\tau) = \beta_{1S}(\tau)$$

This follows directly from the discussion above: uncorrelatedness is not sufficient to remove OVB in QR. The intuition for the result is that, while estimates of policy shocks attained from an OLS first-stage regression might be orthogonal to observables at the mean, they may not be independent across quantiles. The one-step estimator addresses this by ensuring the independence is achieved quantile-by-quantile, rather than solely at the mean.

4 Empirical Application

We illustrate our theoretical results with an empirical application, in which we empirically estimate the dynamic response of the US consumer price index (CPI) to a US monetary policy shock at the conditional mean and across conditional quantiles.[14]

We ground our empirical analysis into the dynamic effects of US monetary policy on US CPI in the specification of Romer and Romer (2004), who apply the two-stage shock-first approach in their estimation. We illustrate our theoretical results for each of the three estimators

[14]We present a complementary simulation exercise using a location-scale model in Appendix D.

discussed in Section 3 in turn: OLS, IV and QR.

Ordinary Least Squares. For OLS estimation, we apply the specification of Romer and Romer (2004) in the first stage, and estimate a US monetary policy shock $\hat{\varepsilon}_t^{mp}$ at monthly frequency for the period 1972:01-2007:12 using the change in the Federal funds target rate as the dependent variable, labelled Δr_t, and Greenbook forecasts and forecast revisions as explanatory variables that comprise $\mathbf{x}_{1,t}$. The exact functional form for this regression is presented in Appendix C, alongside the resulting estimated policy reaction function coefficients.

Using these shocks, we then estimate their dynamic effects on US CPI with the following second-stage LP regression:[15]

$$\ln(CPI_{t+h}) - \ln(CPI_{t-1}) = \alpha_0^h + \mathbf{x}_{2,t}'\boldsymbol{\alpha}^h + \hat{\varepsilon}_t^{mp}\beta_{2S}^h + u_{t+h}^{2S} \tag{11}$$

where $h = 0, 1, ..., 48$.[16] This is an analog to equation (2). We compare this to a one-step LP regression:

$$\ln(CPI_{t+h}) - \ln(CPI_{t-1}) = \alpha_0^h + \mathbf{x}_{1,t}'\boldsymbol{\theta}_1^h + \mathbf{x}_{2,t}'\boldsymbol{\theta}_2^h + \Delta r_t\beta_{1S}^h + u_{t+h}^{1S} \tag{12}$$

an analog to equation (4), as well as a hybrid regression, in which the estimated shocks are used alongside all controls:

$$\ln(CPI_{t+h}) - \ln(CPI_{t-1}) = \alpha_0^h + \mathbf{x}_{1,t}'\boldsymbol{\phi}_1^h + \mathbf{x}_{2,t}'\boldsymbol{\phi}_2^h + \hat{\varepsilon}_t^{mp}\beta_{Hyb}^h + u_{t+h}^{Hyb} \tag{13}$$

where this is the analog to equation (6).

As in our theoretical exposition, we consider two cases: Case 1, in which the second-stage controls $\mathbf{x}_{2,t}$ are empty such that $K_2 = 0$; and Case 2, in which $\mathbf{x}_{2,t}$ is non-empty (i.e. $K_2 > 0$). In this latter case, we use one-month lags of month-on-month industrial production, CPI and producer price index growth in $\mathbf{x}_{2,t}$.

Table 1 summarises our main theoretical findings from Section 3.1. Focusing on Case 1, in which the set of second-stage auxiliary controls $\mathbf{x}_{2,t}$ is empty, columns (1)-(3) present β_i estimates for $i = \{2S, 1S, Hyb\}$, respectively, and $h = 0, 12, 24, 36, 48$.[17] The coefficient point estimates shown here demonstrate that, in this case, point estimates from the one- and two-step estimators are identical, as stated in Corollary 1. The point estimates, unsurprisingly, indicate that a US monetary policy shock is associated with significantly negative lagged effects on US CPI. Comparing columns (2) and (3) further illustrates that both the point estimates and standard-error estimates from the one-step and hybrid estimators are identical in sample, a result stated in Proposition 1.

However, as Corollary 2 states, the estimated standard errors from the one- and two-step

[15] Unlike Romer and Romer (2004), who estimate a distributed-lag model in their second stage, we utilise the LP methodology of Jordà (2005) to estimate direct forecasts of US CPI across different horizons.

[16] Strictly, to ensure that the one-step regression is estimated using the same control data, we use data from 1972:01-2011:12 to estimate the forward lags of this regression.

[17] These impulse response functions are also presented in Figure 1.

Table 1: Response of $\ln(CPI)$ to US monetary policy shock across horizons ($h = 0, 12, 24, 36, 48$ months) estimated by OLS

		Case 1: $\mathbf{x}_{2,t}$ empty			**Case 2: $\mathbf{x}_{2,t}$ non-empty**		
		(1)	(2)	(3)	(4)	(5)	(6)
		Two-Step	One-Step	Hybrid	Two-Step	One-Step	Hybrid
$h = 0$		0.03	0.03	0.03	0.07*	0.04	0.04
	OLS s.e.	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
	N-W s.e.	(0.06)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
	Rob. s.e.	(0.09)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
$h = 12$		0.13	0.13	0.13	0.52*	0.12	0.12
	OLS s.e.	(0.50)	(0.22)	(0.22)	(0.39)	(0.23)	(0.23)
	N-W s.e.	(0.66)	(0.21)	(0.21)	(0.29)	(0.19)	(0.19)
	Rob. s.e.	(0.78)	(0.20)	(0.20)	(0.31)	(0.19)	(0.19)
$h = 24$		-0.15	-0.15	-0.15	0.43	-0.11	-0.11
	OLS s.e.	(0.90)	(0.41)	(0.41)	(0.75)	(0.42)	(0.42)
	N-W s.e.	(0.93)	(0.44)	(0.44)	(0.55)	(0.41)	(0.41)
	Rob. s.e.	(1.02)	(0.39)	(0.39)	(0.52)	(0.39)	(0.39)
$h = 36$		-1.20	-1.20*	-1.20*	-0.49	-1.14*	-1.14*
	OLS s.e.	(1.26)	(0.53)	(0.53)	(1.08)	(0.54)	(0.54)
	N-W s.e.	(1.20)	(0.67)	(0.67)	(0.97)	(0.64)	(0.64)
	Rob. s.e.	(1.04)	(0.55)	(0.55)	(0.91)	(0.55)	(0.55)
$h = 48$		-2.46	-2.46***	-2.46***	-1.60	-2.35**	-2.35**
	OLS s.e.	(1.57)	(0.62)	(0.62)	(1.38)	(0.63)	(0.63)
	N-W s.e.	(1.67)	(0.93)	(0.93)	(1.46)	(0.93)	(0.93)
	Rob. s.e.	(1.25)	(0.74)	(0.74)	(1.37)	(0.76)	(0.76)

Notes: Estimated response of US $\ln(CPI)$ to US monetary policy shock using Romer and Romer (2004) identification assumptions. Estimated using monthly data for the period 1972:01-2007:12. OLS, Newey and West (1987) and robust standard errors in parentheses. ***, ** and * denote significance at 1, 5 and 10% levels using Newey and West (1987) standard errors, respectively.

estimators are not the same. In all cases, as columns (1) and (2) show, the OLS standard errors, calculated assuming homoskedasticity are smaller for the one-step estimates relative to the two-step. This finding carries over to other standard errors too, including White (1980) robust standard errors—which admit heteroskedasticity—and Newey and West (1987) standard errors—which are robust to serial correlation. Because the naïve two-step standard errors are over-estimated, they imply that the dynamic effects of US monetary policy on US CPI are insignificant, even after four years. In contrast, the one-step estimates are significant at the 95%, at least, at the four-year horizon.

For Case 2, where $\mathbf{x}_{2,t}$ is non-empty and the conditions for Corollary 3 are not met, columns (4) and (5) demonstrate that coefficient estimates from the one- and two-step approaches do, in general, differ, confirming Result 1.[18] In this application, the one-step coefficient estimates suggest a limited near-term price puzzle, relative to the two-step estimates—which indicate that a US monetary policy tightening is, counterintuitively, associated with a marginally significant increase in prices in the near term. Further out, the lagged effects of monetary policy are only found to be significantly negative using the one-step (and hybrid) approach.

[18] Again, consistent with Proposition 1, the point estimates and standard-error estimates from one-step and hybrid approaches are identical.

While a comparison of columns (1) and (4) demonstrates that adding second-stage controls does generally reduce naïve standard-error estimates attained from the two-step procedure, the standard errors in column (4) remain greater than those from the one-step approach in column (5). This suggests that the implications of Corollary 2 carry over to the case with non-empty $\mathbf{x}_{2,t}$.

Instrumental Variables. For IV estimation, we use the specification of Miranda-Agrippino and Ricco (2021), which itself builds on that in Romer and Romer (2004). In the first stage, we take high-frequency monetary policy surprises—specifically the move in the third-month-ahead Federal funds futures rate in a 30-minute window around monetary policy announcements, as constructed by Gürkaynak et al. (2005)—at monthly frequency, using the series constructed by Gertler and Karadi (2015). Due to limitations on the availability of the high-frequency surprises, we start our sample in 1990:01. We regress these surprises m_t on Greenbook forecasts and forecast revisions $\mathbf{x}_{1,t}$. The exact functional form for this regression is presented in Appendix C, alongside resulting coefficients.

Using the shocks derived from this first stage, which we label $\hat{\varepsilon}_t^{FF4}$, we then estimate the dynamic effects on US CPI by estimating the following LP-IV regression:

$$\ln(CPI_{t+h}) - \ln(CPI_{t-1}) = \alpha_0^h + \Delta r_t \beta_{2S}^{IV,h} + u_{t+h,IV}^{2S} \tag{14}$$

where $\hat{\varepsilon}_t^{FF4}$ is used as an instrument for Δr_t. This is an analog to equation (9). We compare this to a one-step LP-IV regression:

$$\ln(CPI_{t+h}) - \ln(CPI_{t-1}) = \alpha_0^h + \mathbf{x}_{1,t}' \boldsymbol{\theta}_1^h \Delta r_t \beta_{1S}^{IV,h} + u_{t+h,IV}^{1S} \tag{15}$$

in which m_t is used as an instrument for Δr_t to make this the analog to equation (10).

Table 2 presents empirical counterparts to our main theoretical findings from Section 3.2. Focusing on Case 1, columns (1) and (2) present coefficient estimates from the two- and one-step IV approaches. As Corollary 4 explains, point estimates are identical at all horizons. But, as we discuss in Section 3.2, standard error estimates are different. And, in line with the logic underpinning Corollary 2, estimated standard errors are larger for the two-step regressions than the one-step.

Quantile Regression. To study the dynamic response of conditional quantiles of US CPI to US monetary policy shocks, we estimate QR analogs to equations (11) and (15). For the two-step approach, we use the same monetary policy shocks $\hat{\varepsilon}_t^{mp}$ used in equation (11), estimated for the period 1972:01-2007:12. The second-stage LP-QR is then given by the following conditional quantile function:

$$\mathbb{Q}_\tau \left[\ln(CPI_{t+h}) - \ln(CPI_{t-1}) | \mathbf{x}_{2,t}, \hat{\varepsilon}_t^{mp} \right] = \alpha_0^h(\tau) + \hat{\varepsilon}_t^{mp} \beta_{2S}^h(\tau) \tag{16}$$

Table 2: Response of $\ln(CPI)$ to US monetary policy surprise across horizons ($h = 0, 12, 24, 36, 48$ months) estimated by IV

	(1) Two-Step	(2) One-Step
$h = 0$	-0.33	-0.33
	(0.39)	(0.34)
$h = 12$	-0.77	-0.77
	(1.66)	(1.35)
$h = 24$	-0.32	-0.32
	(2.06)	(1.65)
$h = 36$	-1.35	-1.35
	(2.66)	(2.04)
$h = 48$	-2.98	-2.98
	(2.73)	(2.03)

Notes: Estimated response of US $\ln(CPI)$ to US monetary policy shock using Miranda-Agrippino and Ricco (2021) identification assumptions. Estimated using monthly data for the period 1990:01-2007:12. Two-stage least squares standard errors shown in parentheses.

and the one-step is:

$$\mathbb{Q}_\tau\left[\ln(CPI_{t+h}) - \ln(CPI_{t-1})|\mathbf{x}_{2,t}, \Delta r_t\right] = \alpha_0^h(\tau) + \mathbf{x}_{1,t}'\boldsymbol{\theta}_1^h(\tau) + \Delta r_t \beta_{1S}^h(\tau) \tag{17}$$

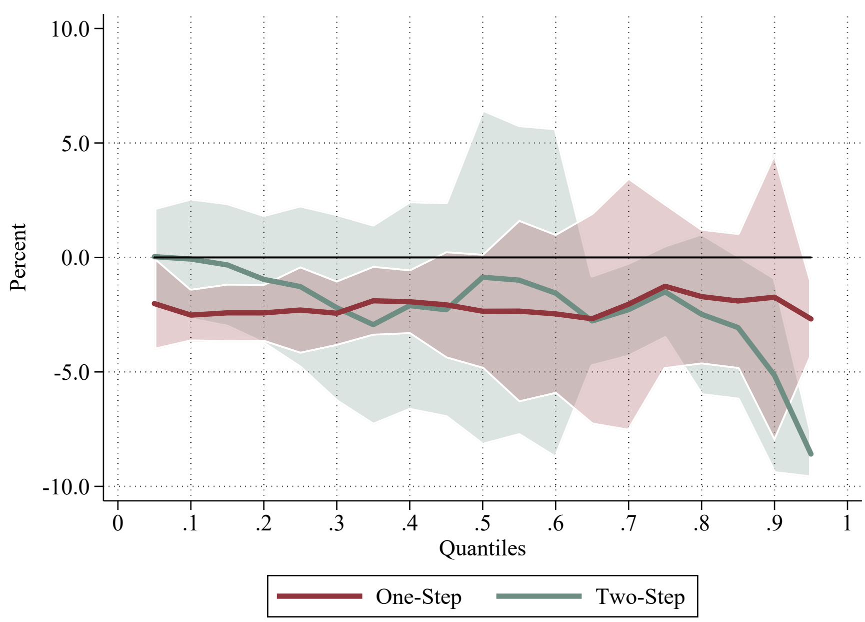
As discussed in Section 3.3, omitted-variable bias can arise in the two-step, even under C1. So we restrict our attention to this case here.[19]

Figure 2 plots the estimated response of conditional quantiles of US CPI to US monetary policy, after 4 years, from the one- and two-step approaches. The plot illustrates the key insights from Result 2 and Corollary 5. In particular, the point estimates from the two estimation procedures are different.

While, for all quantiles, a US monetary policy tightening is associated with a reduction in 4-year-ahead US CPI for both one- and two-step estimates, there are differences. Moreover, in this application, these differences can sometimes be significant. In particular, the coefficient estimates at $\tau = 0.95$ provide different policy implications. While the two-step estimate, which cannot be interpreted as causal, indicate that a US monetary policy tightening is associated with a more marked reduction in the right-tail of future inflation, the causally-interpretable one-step estimates are more in line with estimates at other quantiles. In essence, the one-step point estimates imply that tighter US monetary policy shifts the distribution of CPI outturns to the left in a parallel fashion, the two-step estimates would wrongly imply uneven effects of monetary policy across the inflation distribution.

[19] Additional empirical results for C2 are presented in Appendix C.

Figure 2: Response of conditional quantiles of $\ln(CPI)$ to US monetary policy shock across quantiles τ at the 4-year-ahead horizon



Notes: Estimated response of conditional quantiles at 4-year-horizon of US $\ln(CPI)$ to US monetary policy shock using two-step shock-identification strategy, as well as alternative one-step OLS estimator. Estimated using monthly data for the period 1972:01-2007:12. Shaded area denotes 90% confidence bands from bootstrapped standard errors.

5 Conclusions

To control for confounding factors, applied macroeconomists commonly orthogonalise causal variables of interest—estimating the 'shocks first'—before using the orthogonalised shocks in a second stage. As we have explained in this paper, this approach subsumes a range of identification approaches and has been applied in a wide range of settings. An alternate one-step approach would be to simply include confounding factors as control variables in a regression for the outcome variable.

We have shown, for a general set of estimators, that the two-step 'shock-first' approach can be problematic relative to the simple one-step approach. When the one- and two-step estimators share a common population estimand, there are practical drawbacks to the two-step approach, notably: incorrectly estimated standard errors and efficiency losses in sample. When the one- and two-step estimators differ in population, this can be interpreted as OVB in the two-step approach. For estimators that go beyond the conditional-expectation function, such as QR, estimating the shocks first can be particularly problematic since uncorrelatedness with omitted variables is not sufficient to remove the OVB in this setting. Moreover, as we have shown in practice, this bias can be substantive.

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Appendix

A Causal Identification With Confounding Factors

In this Appendix, we explicitly consider causality. Our argument is broadly as follows: β_{1S} can in general be interpreted as causal under a 'selection-on-observables' assumption. Given β_{1S} and β_{2S} generally align in population in an OLS setting, this implies the identifying assumptions underlying the two-step shock-first approach in this case similarly amounts to 'selection-on-observables'. In contrast, in a QR setting, β_{1S} and β_{2S} generally differ—and only β_{1S} maintains a causal interpretation under selection-on-observables. We formalise this argument using the potential outcomes (PO) framework.[20]

A.1 Causal Identification at the Conditional Mean

We start by considering the identification of *average* causal effects in the face of confounding factors. We define the potential outcome $y_t(z)$ as the value that the observed outcome variable y_t would have taken if $z_t = z$ for all z. Assuming causality can be defined in terms of counterfactuals, the average causal effect on y of setting z to some specific value z_1 in time t relative to some benchmark value z_0 is then defined as:

$$\mathbb{E}\left(y_t(z_1) - y_t(z_0)\right) \tag{18}$$

We never observe counterfactual outcomes and so the expectation in equation (18) cannot be estimated directly. The crucial assumption that permits estimation of (18) through use of regression control is:

Assumption 1. (Conditional-Mean Independence, CMI) $y_t(z) \perp z_t | \mathbf{x}_t$ *for all* z.

This assumption is typically referred to as 'selection-on-observables'. Intuitively, it captures the idea that, conditional on $\mathbf{x}_t$, z_t is as good as randomly assigned. We can now state the conditions under which both the one-step coefficient β_{1S} and the two-step coefficient β_{2S} have a causal interpretation in an OLS setting:

Result 3. (Identification in OLS) *Define coefficients across regressions* (1), (2), (4) *and* (6) *as OLS coefficients. Assume that the conditional expectation function* $\mathbb{E}\left(y_{t+h}|\mathbf{x}_t, z_t\right)$ *is linear and data is stationary, and that the 'shock'* ε_t *from regression* (1) *satisfies* $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right] = \mathbf{0}$. *Then under Assumption 1, the one-step coefficient* β_{1S} *and two-step coefficient* β_{2S} *are equal to the average causal effect of* z *on*

[20] The framework we use here is a simplified version of the PO framework for time series data set out in Angrist and Kuersteiner (2011).

y:

$$\mathbb{E}\left(y_t(z_1) - y_{t,h}(z_0)|\, z_t\right) = \beta_{1S}(z_1 - z_0)$$
$$= \beta_{2S}(z_1 - z_0)$$

Proof: First note that under Assumption 1 we can write the average causal effect in terms of observable conditional means:

$$\mathbb{E}\left(y_{t,h}(z_1) - y_{t,h}(z_0)|\, z_t\right) = \mathbb{E}\left(y_{t+h}|\mathbf{x}_t, z_t = z_1\right) - \mathbb{E}\left(y_{t+h}|\mathbf{x}_t, z_t = z_0\right)$$

Assuming further that the conditional expectation function is linear and data is stationary, the right-hand side of this equation can be directly estimated through an OLS-regression of y_t on $\mathbf{x}_t$ and z_t as in equation (4), implying:

$$\mathbb{E}\left(y_t(z_1) - y_{t,h}(z_0)|\, z_t\right) = \beta_{1S}(z_1 - z_0)$$

From Corollary 3, and assuming $\mathbb{E}\left[\varepsilon_t \mathbf{x}'_{2,t}\right] = \mathbf{0}$, we then have that $\beta_{1S} = \beta_{2S}$ and so:

$$\mathbb{E}\left(y_t(z_1) - y_{t,h}(z_0)|\, z_t\right) = \beta_{2S}(z_1 - z_0)$$

which completes the proof. □

Building on previous results, Result 3 explicitly shows the assumptions under which the one- and two-step approach yield estimates have a causal interpretation. In particular, under selection-on-observables, both the one- and two-step coefficients are interpretable as causal parameters *as long as* the shock constructed in the first stage of the two-step approach is genuinely exogenous to other variables assumed to affect y.

A.2 Causal Identification at Conditional Quantiles

We now consider whether it is reasonable to interpret either the one- or two-step coefficient as causal in a quantile-regression setting. We argue that, unlike in an OLS setting, only the one-step coefficient β_{1S} maintains a causal interpretation under 'selection-on-observables'.

Following the notation from the previous sub-section, we first define the causal effect on *conditional quantiles* of y of setting z_t at some value z_1 relative to some benchmark value z_0 as:

$$\mathbb{Q}_\tau\left(y_t(z_1)|\mathbf{x}_t, z_t\right) - \mathbb{Q}_\tau\left(y_t(z_0)|\mathbf{x}_t, z_t\right) \tag{19}$$

Consider now a slightly stronger version of the CMI assumption (Assumption 1):

Assumption 2. (Conditional Independence, CI) $y_{t,h}(z) \perp\!\!\!\perp z_t|\mathbf{x}_t$ *for all* z.

This states that potential outcomes are *fully* independent of policy conditional on $\mathbf{x}_t$. As the following result states, under this assumption the one-step coefficient β_{1S} has a causal interpretation in a QR setting:

Result 4. (Identification in QR) *Define the coefficients in equation (1) as OLS coefficients, and the coefficients in equations (2), (4) and (6) as quantile-regression coefficients for some specific quantile $\tau \in (0,1)$. Assume that the conditional quantile function $\mathbb{Q}\left(y_t|\mathbf{x}_t, z_t\right)$ is linear and data is stationary. Then under Assumption 2, the one-step coefficient $\beta_{1S}(\tau)$ is equal to the causal effect of z on conditional quantiles of y:*

$$\mathbb{Q}_\tau\left(y_t(z_1)|\mathbf{x}_t, z_t\right) - \mathbb{Q}_\tau\left(y_t(z_0)|\mathbf{x}_t, z_t\right) = \beta_{1S}(\tau)(z_1 - z_0)$$

Proof: First note that under Assumption 2 we can write the causal effect of interest in terms of observable conditional quantiles:

$$\mathbb{Q}_\tau\left(y_t(z_1)|\mathbf{x}_t, z_t\right) - \mathbb{Q}_\tau\left(y_t(z_0)|\mathbf{x}_t, z_t\right) = \mathbb{Q}\left(y_{t+h}|\mathbf{x}_t, z_t = z_1\right) - \mathbb{Q}\left(y_t|\mathbf{x}_t, z_t = z_0\right)$$

Assuming further that the conditional quantile function is linear and data is stationary, the right-hand side of this equation can be directly estimated through a quantile regression of y_t on $\mathbf{x}_t$ and z_t as in equation (4) implying:

$$\mathbb{Q}_\tau\left(y_t(z_1)|\mathbf{x}_t, z_t\right) - \mathbb{Q}_\tau\left(y_t(z_0)|\mathbf{x}_t, z_t\right) = \beta_{1S}(\tau)(z_1 - z_0)$$

which completes the proof. □

This shows that the one-step coefficient β_{1S} has a causal interpretation in a QR setting under selection-on-observables. In addition, given Result 2, under this same assumption the two-step coefficient β_{2S} is equal to the causal effect of interest plus some, generally non-zero, omitted-variable bias term.

So, in sum: in an OLS setting, the identification strategy underpinning the one-step and two-step approach can be understood as relying on 'selection-on-observables'. In contrast, in a quantile regression setting, only β_{1S} maintains a causal interpretation under selection-on-observables.

B Estimation via Proxy SVAR

In this Appendix, we demonstrate how our results for IV estimation carry over to SVAR settings in which identification is achieved through external instruments (i.e. a 'Proxy SVAR').

SVAR Setting. We are interested in estimating the effect of r_t on a $n \times 1$ vector of variables $\mathbf{Y_t}$ and propose doing so using proxy SVAR. We define $\mathbf{W_t} = [\mathbf{r_t}, \mathbf{Y_t}]$. We think m_t is a valid instrument satisfying Proxy SVAR relevance and exogeneity conditions, but only after controlling for $\mathbf{x_{1,t}}$. So we propose to instrument r_t with ε_t defined as in equation (8) as the residual from a regression of m_t on $\mathbf{x_{1,t}}$.

We can then follow Stock and Watson (2018) (p. 932) and estimate a proxy SVAR by first estimating the following IV-regression for each variable $w_{i,t}$ in $\mathbf{W_t}$:

$$w_{i,t} = r_t \beta_{0,i1}^{2S} + \mathbf{W}_{t-1}' \boldsymbol{\theta}^{2S}(L) + u_t^{2S} \tag{20}$$

with ε_t as an instrument for r_t, where L is lag operator. Note that this constitutes a 'two-step' approach since the instrument ε_t has been estimated separately from a first-stage OLS regression. The impulse response of the vector $\mathbf{W_t}$ to a shock to r_t at horizon h is then given by:

$$\boldsymbol{\phi}_{h,1}^{2S} = C_h \boldsymbol{\beta}_{0,1}^{2S} \tag{21}$$

where $\boldsymbol{\beta}_{0,1}^{2S}$ is a $[(n+1) \times 1]$ vector collecting each $\beta_{0,i1}$ and C_h is a horizon-specific coefficient matrix estimated via the reduced form-VAR:

$$\mathbf{W}_t = C(L)\mathbf{W}_{t-1} \tag{22}$$

Note that equation (20) is just a special case of equation (9) setting $\mathbf{x}_{2,t} \equiv \mathbf{W}_{t-1}'$. And so, following the logic of Section 3.2, $\beta_{0,i1}$ could instead be estimated through a one-step IV regression with $\mathbf{x}_{1,t}$ as controls:

$$w_{i,t} = r_t \beta_{0,i1}^{1S} + \mathbf{x}_{1,t} \boldsymbol{\pi}^{1S} + \mathbf{W}_{t-1}' \boldsymbol{\theta}^{1S}(L) + u_t^{1S} \tag{23}$$

with m_t as an instrument for r_t.

The entire impulse response can then be estimated as before using the same reduced-form VAR coefficients from equation (22) to project-out across horizons:

$$\boldsymbol{\phi}_{h,1}^{1S} = C_h \boldsymbol{\beta}_{0,1}^{1S} \tag{24}$$

SVAR Results. Comparing equations (24) and (21), impulse responses estimated under a one- and two-step approach differ only in their estimates of the contemporaneous coefficients $\beta_{0,i1}$.

By Corollary 3, when $\mathbb{E}\left[\varepsilon_t \mathbf{W}_{t-1}'\right] = \mathbf{0}$ then $\beta_{0,i1}^{1S} = \beta_{0,i1}^{2S}$, although there may be efficiency losses in sample from the two-step approach.

Also by Corollary 4, when $\mathbb{E}\left[\varepsilon_t \mathbf{W}'_{t-1}\right] \neq \mathbf{0}$ then coefficient estimates will differ where this difference can be expressed as OVB in the two-step approach. Intuitively this bias can be thought of as a failure of exogeneity conditions necessary for identification in an IV setting, as the instrument ε_t is in fact correlated with other variables $\mathbf{W}_{t-1}$ that affect the outcome variable $y_{i,t}$. Although these variables are included as controls in equation (20), this is not sufficient to ensure exogeneity with respect to the entire vector of controls $[\mathbf{x}_{1,t}, \mathbf{W}_{t-1}]$. As before, the one-step approach automatically avoids this bias and so can be thought of as more robust.

Romer-Romer Application. We finish this section by discussing a special case of Proxy-SVAR estimation. In particular, we are interested in the case where there is no external source of variation captured by m_t, but instead ε_t is constructed as the residual from a regression of r_t on $\mathbf{x_{1,t}}$ and then used as a instrument for r_t in a Proxy-SVAR. This is the approach followed in various applications employing Romer and Romer (2004) monetary shocks as an external instrument in a VAR (see, e.g., Nakamura and Steinsson, 2018). Note that in this case the 'one-step' approach involves the following IV-regression:

$$w_{i,t} = r_t \beta_{0,i1}^{1S} + \mathbf{x}_{1,t} \boldsymbol{\pi}^{1S} + \mathbf{W}'_{t-1} \boldsymbol{\theta}^{1S}(L) + u_t^{1S} \tag{25}$$

But since the instrument we use for r_t in this case is just r_t itself, the coefficients are trivially identical to those from estimating (25) using OLS. Note however, this 'Proxy SVAR' approach is distinct to simply including $\mathbf{x}_{1,t}$ as exogenous variables in a VAR since in this case $\mathbf{x}_{1,t}$ are included only to estimate contemporaneous coefficients and do not feature in the estimation of subsequent impulse responses.

C Empirical Application: Additional Results

In this Appendix, we provide more information underpinning our empirical application in Section 4.

Data Sources. As described in Section 4, we use monthly data for our empirical estimation. Our dependent variable, the US Consumer Price Index (CPI), is sources from *FRED*, and we also use additional macroeconomic controls—specifically seasonally-adjusted US industrial production and the US Producer Price Index—from the same source. These controls comprise $\mathbf{x}_{2,t}$ in our study.

To estimate Romer and Romer (2004) US monetary policy shocks, we use Federal Reserve Greenbook forecasts and forecast revisions. We draw on Wieland and Yang (2020) for this, who provide updated Greenbook forecast data up to, and beyond, the end of our sample period, 2007:12.

Finally, to carry out our IV exercise, we additionally use 3-month-ahead Federal Funds Futures surprises. For this, we use the monthly series of Gertler and Karadi (2015), which builds on the work of Gürkaynak et al. (2005).

Monetary Policy Shock Construction. To construct the Romer and Romer (2004) shocks, we make two changes relative to the original work. First, and most notably, we estimate the model for a different sample period—specifically 1972:01-2007:12, rather than 1969:01-1994:12. We start the sample a little later to avoid calendar months in which there was more than one FOMC meeting. And we end the sample later given data availability, stopping just before the effective lower bound was reached. Second, rather than estimating the model at meeting frequency, we estimate the shocks at monthly frequency. This constitutes a minimal difference, which we merely do to ensure direct comparability of conditioning data in the one- and two-step approaches.

Following Romer and Romer (2004), we construct the shock by regressing the change in the Federal Funds target rate Δr_t on the previous target rate r_{t-1}, as well as past and future Greenbook forecasts of GDP growth Δy_t^e, inflation π_t^e and unemployment u_t^e, as well as their revisions. Our specific functional form is:

$$\begin{aligned}\Delta r_t =& \delta_0 + \delta_1 r_{t-1} + \sum_{i=-1}^{2} \left[\delta_{2,i}\Delta y_{t,i}^e + \delta_{3,i}(\Delta y_{t,i}^e - \Delta y_{t-1,i}^e) + \delta_{4,i}\pi_{t,i}^e + \delta_{5,i}(\pi_{t,i}^e - \pi_{t-1,i}^e)\right] \\ &+ \delta_6 u_{t,0}^e + \varepsilon_t^{mp} \end{aligned} \tag{26}$$

Column (1) of Table 3 presents the estimated coefficients from this regression, estimated for the period 1972:01-2007:12.

Central Bank Information Channel. For our IV application, we follow Miranda-Agrippino and Ricco (2021) and regress monetary policy surprises m_t on Greenbook forecasts to con-

Table 3: First-Stage Regressions: The Romer-Romer Reaction Function and the Miranda-Agrippino and Ricco Central-Bank Information Channel

	(1) Romer-Romer Regression DEP. VAR.: Change FFR Target	(2) Miranda-Agrippino and Ricco Regression DEP. VAR.: Monthly FF4 Surprise
Old FFR Target	-0.014	-0.006
	(0.009)	(0.004)
Output forecasts		
$k = -1$	0.002	0.003
	(0.008)	(0.003)
$k = 0$	0.008	0.007**
	(0.013)	(0.003)
$k = 1$	0.016	0.004
	(0.020)	(0.006)
$k = 2$	0.019	-0.003
	(0.023)	(0.004)
Inflation forecasts		
$k = -1$	0.016	-0.001
	(0.015)	(0.005)
$k = 0$	-0.029	-0.000
	(0.021)	(0.005)
$k = 1$	0.019	-0.006
	(0.041)	(0.010)
$k = 2$	0.030	0.019*
	(0.048)	(0.010)
Unemployment forecast		
$k = 0$	-0.037***	-0.009
	(0.011)	(0.007)
Output forecast revisions		
$k = -1$	0.039	-0.008
	(0.045)	(0.010)
$k = 0$	0.129***	0.010
	(0.032)	(0.006)
$k = 1$	0.032	0.017*
	(0.044)	(0.009)
$k = 2$	0.011	-0.012
	(0.046)	(0.009)
Inflation forecast revisions		
$k = -1$	0.069	-0.005
	(0.045)	(0.010)
$k = 0$	-0.007	0.003
	(0.055)	(0.011)
$k = 1$	0.030	0.016
	(0.090)	(0.016)
$k = 2$	-0.054	-0.012
	(0.087)	(0.017)
R^2	0.274	0.289
Observations	432	216

Notes: Estimated policy reaction functions. Column (1) estimated using monthly data for the period 1972:01-2007:12; column (2) 1990:01-2007:12. Robust standard errors in parentheses. ***, ** and * denote significance at 1, 5 and 10% levels, respectively.

struct monetary policy shocks purged of the central bank information effect. To construct these shocks, we re-estimate equation (26), but make three changes. First, we replace the dependent variable, switching the change in the Federal Funds target rate Δr_t for the monthly frequency Federal Funds Futures surprise m_t. Second, we relabel the error term to ε_t^{FF4}. Third, owing to data availability, we estimate it for the period 1990:01-2007:12. The resulting coefficient estimates are presented in column (2) of Table 3.

D Simulation Exercise

In this Appendix, we provide Monte Carlo analysis to complement our theoretical results and to demonstrate the drawbacks of the two-step approach for a specific data-generating process. We consider an AR(1) process with minimal dimensionality for the outcome variable y_t:

$$y_t = \rho y_{t-1} + \theta z_t + \boldsymbol{\gamma}'\mathbf{x_t} + u_{y,t}$$

where $\mathbf{x_t} = [x_{1,t}, x_{2,t}]'$. We assume the following data-generating process for z_t and $\mathbf{x_t}$:

$$z_t = \delta \mathbf{x_t} + u_{z,t}$$
$$\mathbf{x_t} = \rho_{\mathbf{x}} \mathbf{x}_{t-1} + \mathbf{u}_{x_t}$$

where $\rho_{\mathbf{x}} = \text{diag}(\rho_1^x, \rho_2^x)$. We are interested in the dynamic causal effect of z_t on y_t and consider both a one-step and two-step local projection to estimate this effect.

OLS. We begin by considering estimation via OLS. For this application, we set $u_{z,t}$ and $u_{y,t}$ as i.i.d. standard-normal random variables and $\mathbf{u}_{x_t}$ as an i.i.d. multivariate-normal random variable $N \sim (0, \boldsymbol{\Sigma})$, with $\boldsymbol{\Sigma} = [1, 0.4; 0.4, 1]$. We set $\rho = \rho_{x,1} = \rho_{x,2} = 0.9$ to mimic the high persistence of macroeconomic data. We simulate $10,000$ time series for $[y_t, z_t, \mathbf{x_t}]$ each for a sample of $T = 500$. For each simulation, we estimate the following 'one-step' LP:

$$y_{t+h} = \alpha_h^{1S} + \rho_h^{1S} y_{t-1} + \theta_h^{1S} z_t + \boldsymbol{\pi}_h^{1S\prime} \mathbf{x}_t + u_{y,t,h}^{1S} \tag{27}$$

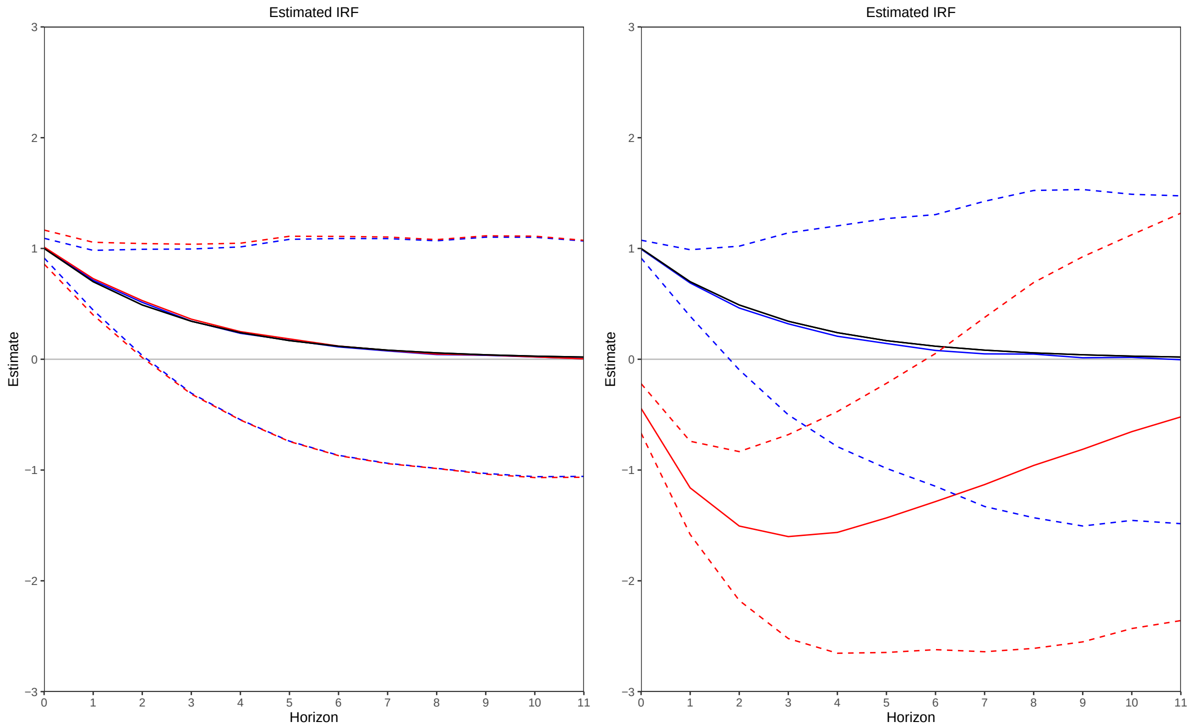
as well as the two-step LP:

$$y_{t+h} = \alpha_h^{2S} + \rho_h^{2S} y_{t-1} + \theta_h^{2S} \varepsilon_t + \pi_h^{2S} x_{2,t} + u_{y,t,h}^{2S} \tag{28}$$

where ε_t is estimated as the residual from an OLS regression of z_t on $x_{1,t}$.

We compare $\hat{\theta}_h^{2S}$ and $\hat{\theta}_h^{1S}$ to the true impulse response function where we are interested in both the bias and variance of the estimators. Figure 3 shows results for two cases: first we set the coefficient of x_2 on z_2 denoted by δ_2 equal to zero, and all remaining coefficients $[\theta, \pi, \delta_1]$ equal to 1. Shown on the left-hand side, this is equivalent to the 'exogeneity' case discussed in Section 3.1 since the excluded variables in the two-step regression x_2 do not feature in the model for z_2. By Corollary 3, the one- and two-step estimators are identical in population and so both provide unbiased estimates of the true IRF. However, the variance of the two-step estimator is higher, especially at near-term horizons. The right-hand side of Figure 3 shows results where δ_2 is now also set equal to 1. This is equivalent to the 'endogeneity' case discussed in Section 3.1 since ϵ_t is now correlated with $x_{2,t}$. By Result 1, the two-step approach suffers from OVB and so provides biased estimates of the true impulse response across horizons. This bias (and inconsistency) comes at the additional cost of higher variance.

Figure 3: Simulation-implied OLS coefficients: exogenous auxiliary controls (left-hand side) and endogenous auxiliary controls (right-hand side)



Quantile Regression. We now consider estimation of LP using QR. To do so, we consider a setting where the effect of z_t on conditional quantiles of y_t varies across quantiles. And so we assume the following for the error term and $u_{y,t}$:

$$u_{y,t} = \left(\pi_0 + \pi_1 z_t + \boldsymbol{\pi}_2' \mathbf{x}_t\right) \Psi_{u_t}$$

where Ψ_{u_t} is an i.i.d. standard-normal random variable.

In this setup, z_t and $\mathbf{x}_t$ can affect the location (through θ and γ) and scale (through π_1 and $\boldsymbol{\pi}_2$) of y_t.

We further assume $\Psi_{\varepsilon_t} \sim \mathcal{N}(0, V_{\varepsilon_t})$ and $\Psi_{u_t} \sim \mathcal{N}(0,1)$ and that $\gamma_0 + \gamma_1 x_t > 0$, $\pi_0 + \pi_1 z_t + \pi_2 x_t > 0$. In this case the conditional quantile function of y_{t+h} can be written as:

$$Q_\tau\left[y_{t+h}|x_t, z_t\right] = \left[\theta_0 + \pi_0 \Phi^{-1}(\tau)\right] + \left[\theta_2 + \pi_2 \Phi^{-1}(\tau)\right] x_t + \left[\theta_1 + \pi_1 \Phi^{-1}(\tau)\right] z_t$$

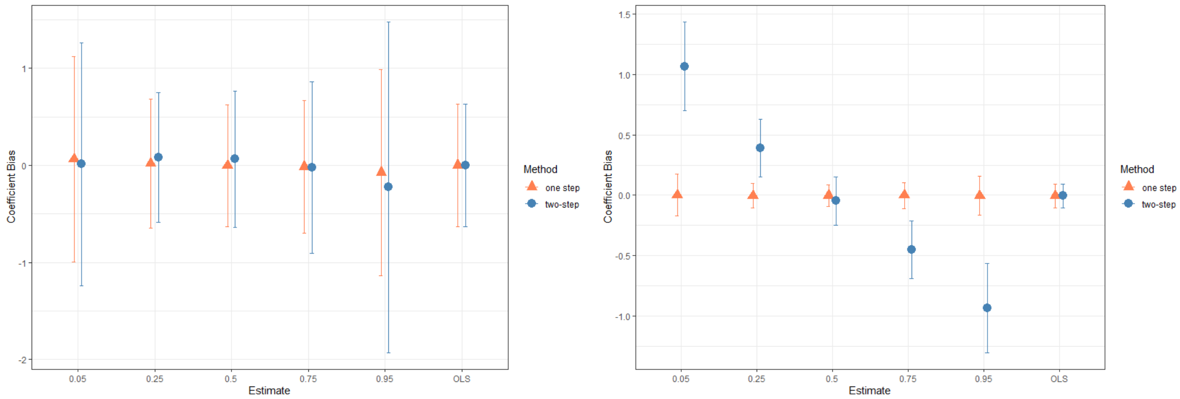
where the conditional quantile function of the normally-distributed disturbance $\Psi_{u_{t+h}}$ is denoted by $Q_\tau[\Psi_{u_t}] = \Phi^{-1}(\tau)$, which represents the inverse of the standard-normal conditional distribution function.

The variable of interest is the association between z_t and conditional quantiles of y_t defined as:

$$\beta_{1S}(\tau) \equiv \frac{\partial Q_\tau\left(y_t|x_t, z_t\right)}{\partial z_t} = \theta_1 + \pi_1 \Phi^{-1}(\tau)$$

where this can be directly estimated via a quantile regression of y_t on x_t and z_t.

Figure 4: Simulation-implied QR and OLS coefficients: model with limited bias (left-hand side) and model with substantial bias (right-hand side)



We compare this one-step approach to the alternate two-step shock-first estimator $\beta_{2S}^{h}(\tau)$ obtained via a first-step regression of z_t on x_t and then a second-step quantile regression of y_t on the residuals from the first step. Following Result 2 this can be expressed as:

$$\begin{aligned}\beta_{2S}^{h}(\tau) &= \beta_{1S}(\tau) + \Omega_{QR}^{h}(\tau) \\ &= \beta_{1S}(\tau) + \left[\theta_1\delta_1 + \theta_2 + (\pi_1\delta_1 + \pi_2)\Phi^{-1}(\tau)\right] \frac{\mathbb{E}\left[w_\tau(x)\varepsilon_t x_t\right]}{\mathbb{E}\left[w_\tau(x)\varepsilon_t^2\right]}\end{aligned}$$

Results. We carry out the simulation study by simulating the model $N = 1000$ times over samples with length $T = 10,000$ observations. Using the constructed data, we then compare one- and two-step estimates, for both OLS and QR.

Figure 4 presents results for two simulations, which differ in their calibration. In both studies, the OLS coefficient is identical—in line with the theoretical result in Corollary 1. However, the degree of OVB can differ in the QR, depending on the model specification. The left-hand figure presents a model with limited bias in practice. Point estimates from the one- and two-step estimators continue to differ—in line with Result 1. However, estimates are not statistically distinguishable in this setting. The right-hand figure depicts a model with substantial bias, especially in the tails. Here, point estimates are different, and standard errors imply that the estimates are significantly different. In particular, the two-step estimates are particularly biased in the tails, while the bias in the one-step approach is roughly zero across all quantiles.