The age for austerity? Population age structure and fiscal consolidation multipliers

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1. Introduction

Advanced economies are getting old. Aging boomers, rising life expectancy, and falling fertility are working to bring an unprecedented change in the population age structures of advanced economies. Just as these demographic tides are rising, policy response to the global financial crisis and Covid-19 have seen government debts soar. In this environment, it will be critical to understand how government multipliers evolve as economies undergo demographic change. I study the interaction between population age and narratively identified fiscal consolidation shocks for a panel of advanced economies. To do this, I implement a recently proposed Kitagawa–Blinder–Oaxaca1 (KBO) decomposition of state dependent effects of fiscal policy shocks. Both output and deficit responses to these fiscal contractions are highly sensitive to the underlying demographic structure.

Using narrative fiscal austerity measures from (Guajardo et al., 2014), I find that transmission of tax hikes and spending cuts both depend on age structure, but in different ways. For tax increases, concentrating population in early working-age (15–30) and older retirees (> 70) weakens the negative response of output to the policy shock, while strengthening the reduction in deficits. Shares of late-career age groups (45–65) have the opposite effect, reinforcing the output response to the tax hike while mitigating deficit response. The change in output following a spending cut displays no interaction with age in my baseline estimates, but the

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1 Developed for local projections by Cloyne et al. (2020) using well established methods in the micro literature by Kitagawa (1955), Blinder (1973) and Oaxaca (1973)
deficit transmission depends strongly on underlying demographics. For spending cuts, the deficit response weakens with young and middle aged workers (15–50), strengthening as shares aged > 55 increase.

What are the implications of these findings? Tax cuts had weaker output impact with the relatively young economies of the 1970–80s. Their transmission to deficits were stronger, making them effective tools for reducing primary balances without having large economic impact. As these populations transition to middle age in the 2000s the effect reverses, with tax cuts having very costly output effects and their impact on deficits shrinking, even reversing sign. For spending cuts, deficit reductions become stronger in response to policy as countries see increases in late career workers and retirees over the same period. Projecting demographics forward suggests a return to weak output response to tax increases and larger improvements of deficits for both consolidation measures.

To further explore the channels for demographic transmission of policy shocks, I study the age-dependent impact of these policy shocks on consumption, investment, and hours worked. Tax shocks have very large age interactions with consumption and investment, but none with hours worked. This may suggest that the demographic changes in the output response to tax shocks work through differences in marginal propensity to consume (MPC) over the life-cycle, impacting the size of consumption and savings responses of households. With spending cuts, these consumption and investment interactions with age are much weaker, and die out quickly. However, spending cuts display strong age effects in their transmission to hours worked, suggesting there may be some aspect of labor market response to these shocks that varies by age. This difference is supported by evidence that demographic interactions are stronger for spending (and weaker for taxes) when using working-age population to control for demographics.

Why should population age structure be important for fiscal multipliers? Recent work shows that age very plausibly correlates with MPC, which in turn is important for determining the size of fiscal impact. Using Norwegian lottery data, Fagereng et al. (2021) find a negative relationship between MPC and age. Similar negative age relationships arise in Italian data studied by Jappelli and Pistaferri (2014), who find relatively constant MPCs throughout working life, declining in retirement. They show that MPC has a strong negative correlation with cash-on-hand wealth, which explains a great deal of the observed heterogeneity. Heterogeneous agent life-cycle models generate large changes in cash-on-hand wealth as households age, particularly as they move toward retirement, motivating a theoretical channel that links aging to multipliers. Brinca et al. (2016) show that the size of fiscal multipliers in an OLG economy is highly sensitive to the amount of individuals facing liquidity constraints, which again may vary greatly with age. Basso and Rachedi (2021) show that labor supply differences between broad age groups account for variation detected in local fiscal spending multipliers for the United States.

Similar channels have been developed for changing monetary policy effectiveness, with (Auclert, 2019) highlighting the role changing MPCs due to differential effects of policy across earnings, balance sheets, and exposure of assets. Berg et al. (2021) show that such wealth accumulation channels make monetary policy sensitive to age structure, with older households responding much more strongly to shocks. Leahy and Thapar (2019) provide empirical evidence for the age dependence of monetary policy shocks. Notably for my findings, they report significant variation across the working life.

In Guajardo et al. (2014), narrative shocks are developed to estimate the effects of fiscal austerity. They identify changes to policy that were not motivated by the current state of the economy, suggesting that prior work such as Alesina and Ardagna (2010) suffered from biased results due to the endogeneity of policy choices. This paper fits in a large literature studying the state dependence of such fiscal shocks. There is evidence that economies are significantly more responsive to government spending and taxation in recessions than in expansions. While the idea goes back to Keynes, work such as Auerbach and Gorodnichenko (2012) has recently brought it empirical rigor. Jordà and Taylor (2016) use these austerity measures to show that this is the case with fiscal consolidations, which have larger negative impact in recessions than expansions.

A small number of recent papers have studied the state dependent effects of aging on multipliers directly. Basso and Rachedi (2021) estimate these effects in the context of the United States, making use of the local fiscal spending shocks of Nakamura and Steinsson (2014). Empirically, they find that young working age individuals increase the size of fiscal spending stimulus. They show in an overlapping generations model that much of the effect can be explained by differential labor adjustment, with young workers having much higher labor supply elasticity. Recent work by Ferraro and Fiori (2020) use narrative tax shocks in the United States to understand differential employment response across the age distribution. The labor response of the young is almost twice as large as that of older workers, opening a potentially meaningful channel for age to alter the efficacy of policy. Heer and Scharrer (2018) show that the method of financing fiscal projects interacts with the age distribution, putting a large burden on younger generations when fiscal spending or tax cuts are debt financed.

Most similar to this paper are Honda and Miyamoto (2021) and Miyamoto and Yoshino (2020). They pose a similar question with different identification of fiscal shocks, as well as different methods of controlling for population age. They show that high old-age dependency ratios shrink fiscal spending shocks. Moreover, their findings suggest that this relationship is particularly strong during recessions, but not detectable during economic booms. I contribute to their results along a number of dimensions. In addition to using different shocks, I show that important variation across the age distribution is missed when using only one control for age.

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2 Young for my sample of advanced economies from 1978–2006. Here I discuss broad trends consistent with the United States and many western European countries, though exact timing and magnitude varies.

3 Other work on aging by Wong (2019) studies the efficacy of monetary policy, showing that younger households more likely face binding borrowing constraints, and respond more to changes in interest rates as a result.

4 Along with life-cycle heterogeneity in portfolio choice, planning horizons, and labor supply.

5 A large body of work has found evidence in line with this result. See, for example: (Ilzetzki et al., 2013) and Ramey and Zabairy (2018).

6 They use the forecast error method of Auerbach and Gorodnichenko (2012)
I find similar results to theirs when using working-age population. Additionally, my work suggests that state dependence on both output and deficit will be important, with the latter having particularly large implications for the efficacy of policy.

In Section 2, I describe the data and estimating equations. In Section 3, I present results with two separate demographic controls: the working age population, and a polynomial fit of the entire age distribution. I explore potential channels for demographic transmission to fiscal shocks in Section 4. In Section 5 I discuss the robustness of my results and Section 6 concludes.

2. Data and methods

Narrative fiscal consolidation shocks come from Guajardo et al. (2014), who identify changes in fiscal policy in a panel of 17 OECD countries. From the historical record, they establish whether increases in discretionary taxes and decreases in spending measures are motivated by a response to current economic conditions and forecasts. Excluding policy changes motivated in this way aims to causally identify fiscal effects that would otherwise be biased by the cyclical nature of policy. Work such as Alesina and Ardagna (2010), who study changes in the cyclically adjusted primary balance (CAPB), are likely to generate biased coefficients given that policy changes are motivated by current economic conditions, which themselves are predictive of future output. Systematic reductions in taxation at the onset of recessions (or increases during booms) might imply a positive relationship between the variables, due simply to the policymakers' preference to engage in counter-cyclical activity. Guajardo et al. (2014) show that their narrative shocks are unrelated to real-time revisions of IMF forecasts, suggesting that they are not responses to economic news. Testing the CAPB in this way, they find a positive relationship between output revisions and changes in the primary balance.

The identification problems associated with measures such as the CAPB could be exaggerated in my framework, where the policy will be interacted with all other controls that are likely endogenous themselves. It is therefore important to have the policy change reasonably well identified, even if failure to causally identify changes in underlying demographics will make it difficult to put strong causal weight on my results. Unlike similar narrative instruments such as Romer and Romer (2010), who identify shocks that are motivated either to boost long run growth or to address government deficits, Guajardo et al. (2014) shocks focus specifically on policy changes that are motivated to reduce the budget deficit. I employ these specific austerity shocks for two reasons. The first is that the Guajardo et al. (2014) shocks cover 17 countries, allowing my analysis to leverage large cross-country variation in sizes of population age groups. Given the slow moving nature of age distributions, utilizing these differences is particularly important. Additionally, with looming debts, advanced economies may seek to engage in measures to reduce their primary deficits in the near future, making the effects of austerity policies relevant.

To provide a rich set of macroeconomic controls, I merge these fiscal shocks with the macrohistory database of Jordà et al. (2017). This contains a large number of macroeconomic variables. I also utilize interest rates from the Jordà et al. (2019) addition to this data. Their data contains 18 countries, but is missing Austria from the Guajardo et al. (2014) shocks. For my baseline specification I use control groups which add Austria to this data (using OECD data as well as replication files from Guajardo et al. (2014)), but I will present some robustness using controls where I could not extend the sample. Population age structure comes from the Human Mortality Database (2020). I use their population tables by age to generate shares of population across the entire age distribution for each country. From these, I will construct my main demographic controls, which I describe in detail below. Future population shares come from United Nations projections (UN, 2019).

The resulting data for my baseline estimation is a balanced panel of 17 countries from 1978 to 2009. Baseline regressions reported will be estimated from the 26 year window: 1980 to 2006. The smaller estimation window is in part due to the lag structure of my estimating equation, but also to ensure that all local projections (which use forward outcome variables) are estimated on the same sample. While there is not a large difference in estimations if these later years are included for shorter horizons of the local projection, I wish to avoid estimating responses to shocks where some horizons contain the global financial crisis in their estimation, while others do not.

2.1. Methodology: Kitagawa–Blinder–Oaxaca local projections

I estimate impulse response functions (IRFs) empirically using the local projection (LP) method of Jordà (2005). Specifically, I use the Kitagawa–Blinder–Oaxaca (KBO) decomposition developed in Cloyne et al. (2020), who study the state dependence of fiscal shocks on monetary policy accommodation. This approach makes state dependence quite easy to interpret and allows continuous controls to be interacted with continuous shocks. Many earlier papers studying state dependent fiscal policy instead split samples between discrete values (eg: recessions and expansions). Plagborg-Møller and Wolf (2021) show that the impulse responses generated from local projections are the same as those estimated in traditional vector autoregression approaches. Montiel Olea and Plagborg-Møller (2021) show that lag augmented local projection estimates provide valid inference when data is stationary and non-stationary and over long horizons. Existing literature on state dependent effects using the Guajardo et al. (2014) shocks show that LP estimates provide nearly identical results to those in the original paper, where a different VAR method was used.

Following Cloyne et al. (2020), the difference from my specification and a standard LP equation is that controls are demeaned and a set of interactions between treatment variables (here the fiscal shock) and all controls are included. Demeaning allows interpretation of interactions as deviations around an average fiscal effect, which should be comparable to those estimated in studies

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7 I also replicate their old-age dependence results directly in Appendix C.

8 Guajardo et al. (2014) instrument for the CAPB using their narrative shock series, and draw direct comparison to their work.
of the shock without state dependent effects. Interaction terms then provide estimates of state dependence from variation of the control set around their sample mean. Eq. (1) is my estimating equation for the IRFs using this KBO decomposition. For exposition, I have split out the demographic controls from the vector of additional controls, \( x_{i,t} \).

\[
y_{i,t+h} - y_{i,t-1} = f_{i,t} \beta^h + (x_{i,t} - \bar{x}) \gamma^h + f_{i,t} (x_{i,t} - \bar{x}) \theta^h + (D_{i,t} - \bar{D}) \psi^h_B + f_{i,t} (D_{i,t} - \bar{D}) \theta^h + \mu^h + \mu_i^h + \epsilon_{i,t+h}; \quad f \neq h = 0, \ldots, H
\]

In Eq. (1), \( y_{i,t+h} - y_{i,t-1} \) is the \( h \) horizon cumulative change in an outcome variable, \( y \), in country \( i \) at time \( t \). The two outcomes I will use calculate fiscal multipliers are the cumulative change in output and government deficits to GDP. The fiscal shock, \( f_{i,t} \), is the narrative fiscal consolidation shock. These are tax increases, spending cuts, or both, scaled as a percent of GDP. For all specifications, I include a vector of controls, \( x_{i,t} \), which includes two lags of the dependent variable and contemporaneous values plus two lags of: the deficit to GDP ratio, institutional investor rating, log change in real consumption per capita, log change in real investment per output, and change in short term interest rates. I also follow the literature\(^9\) estimating fiscal response to narrative shocks by including two lags of the policy shock variable, though inclusion has little bearing on my coefficients of interest.

The interpretation of the KBO specification in Eq. (1) is that \( \beta^h \) is the direct effect of the policy change, which is the average across all states in the estimation. This is the primary coefficient of interest in papers studying fiscal shocks without state dependence. Each control has a compositional effect estimated by \( \gamma^h \) and an indirect effect estimated through \( \theta^h \). The compositional effect reflects any change in the outcome due to changes in the underlying control set unrelated to policy. For example, demographics may have some effect on growth that is unrelated to changes in fiscal shocks. These are estimated through \( \gamma^h \). My primary object of interest is estimation of \( \theta^h \), the indirect effect of the fiscal shock dependent on demographic factors.

Many macroeconomic studies of demographics use a single statistic, such as the working-age population, to control for population age. This has a number of disadvantages. First, it requires an ex ante assumption of what ages are important, and where the important cutoffs in the age distribution take place. Second, because population age shares tend to have high correlations,\(^10\) it is not obvious whether effects estimated for one group are capturing their own impact or the effect of an excluded group. Therefore, my preferred specification controls for the entire age distribution. I divide the population into \( J = 15 \) age groups and construct a set of demographic controls using the method proposed in Fair and Domínguez (1991). They construct a finite set of controls that fit coefficients for the entire age distribution with a \( K \)th order polynomial as: \( a_j = \sum^K \gamma_k j^k \). To accomplish this, I construct the following demographic controls:

\[
D_{k,j,t} = \left[ \sum_j (p_{j,t} - \bar{p}_j)^k \right]
\]

Where \( p_{j,t} \) is the share of population in age group \( j \), for country \( i \) in time \( t \). As with my other controls, I use demeaned shares to interpret this KBO decomposition as described above. I show how Eq. (2) is derived in Appendix A, and that the estimated coefficients on the \( D_{k,j,t} \) terms in Eq. (1) are the \( \gamma_k \) terms of the polynomial for the \( j \) specific age coefficients. I use 15 age groups covering the population share younger than 10 years old, older than 75, and five year intervals between. I set \( K = 3 \) so that the estimated age specific coefficients are fitted by a third order polynomial. I also report results using the working-age population (WAP) for comparison. All population shares are constructed using (Human Mortality Database, 2020) data on population by age for both sexes.

To get estimates of fiscal multipliers, I not only estimate output response to the fiscal shock, but also the corresponding change in government deficit. Estimating impulse responses of the cumulative change in both output and government deficit, allows for the calculation of the associated multiplier as the ratio of these two changes, as in Cloyne et al. (2020). This follows work such as (Uhlig, 2010), who suggests that multipliers should be the cumulative net-present value ratio of these terms.\(^11\) Ramey (2016) discusses that taking the ratio of these estimated effects is equivalent to the 2SLS method used in Guajardo et al. (2014), where the narrative shocks are used as instruments for change in the CAPB. I show that this is not a trivial step in the context of population aging. In particular, when controlling for the entire age distribution, the state dependence of changes in deficits with respect to population age structure is large and has substantial impact on the size of multipliers.

The KBO methodology used here is not a cure for issues of identification. The fiscal shock series has some claim of exogeneity with respect to output. Underlying demographics do not. While these deficit measures were unmotivated by current economic conditions, they may have been motivated by demographic trends, particularly in the face of rising pension liabilities. This could be a source of bias if unobserved changes in output, due to demographics, are correlated with decisions to engage in austerity measures. What I hope to capture with these results are changes in the transmission of policy across different demographics. If demographics are predictive of the narrative shock series themselves, then I may rather capture changes in deficits and output which are endogenous to the policy choice themselves. To test this, Appendix E shows a series regressions of demographics on the shock series.

The findings in Appendix E, Table 12 show that my demographic variables D1–D3 and WAP are at best weakly correlated with changes in fiscal shocks. Regressing only demographics\(^12\) on fiscal shocks, WAP is unrelated to spending cuts while the full demographic variables from Eq. (2) show some significant relationship. With corresponding tax shocks the WAP is correlated while

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\(^9\)See for example: Guajardo et al. (2014), Mertens and Ravn (2013) or Cloyne (2013)

\(^10\)For example, in my sample the share of population aged 20–34 and those aged 50–64 have a \( -0.63 \) correlation coefficient.

\(^11\)See also: Drautzburg and Uhlig (2015) and Woodford (2011).

\(^12\)Along with country fixed effects
the full demographic variables are not. All of these entirely disappear when time fixed effects are included, and coefficients remain insignificant in specifications that include the full set of controls in Table 13. This provides some evidence that the results that follow capture transmission of policy to outcomes, and not the effect of population age on the policy decision itself. In Section 4, I show that for tax shocks these transmission channels appear to come through consumption/saving decisions, while for spending shocks may be more related to labor supply choices. However, I still caution that for a true causal claim, one needs to have an identification strategy for both the fiscal shock as well as for the interacted controls. This is an issue that plagues most state dependent estimates of fiscal shocks, as it is difficult to credibly overcome.

2.2. Variation in demographic variables

To better contextualize results that follow, I present the variation population age driving them. Fig. 1 plots the WAP for all seventeen countries, demeaned as in the following analysis. A few broad trends are apparent, as well as some notable differences across countries. Generally WAP rose with entry of the large boomer cohort. Differences in timing and size of this cohort, along with different fertility trends, cause this to peak at different times. Japan, for example, saw this peak early and fall quickly, while much of western Europe peaked early with slower adjustment to the mean. The United States, United Kingdom, and Canada saw a much later peak, while some very young countries (Ireland, Portugal, Spain, and Austria) had much later and larger fertility booms.

Fig. 2 plots a snapshot of population shares as deviations from their sample average for three countries near the start (1985) and end (2005) of the estimation sample, along with one future projection (2025). Since population shares in each period sum to one, these demeaned shares always sum to zero. For brevity the remaining fourteen countries are reported in Appendix F, Fig. 13. These are the shares used in constructing the demographic variables in Eq. (2). They demonstrate some common trends present in demographics, notably: declining fertility and rising longevity. What is highly variable across countries is the presence, timing, and severity of post-war fertility booms and busts, along with other idiosyncratic differences.

The countries in Fig. 2 provide a snapshot of the variation present in the data. Ireland and Japan had relatively smaller and shorter lived post-WWII baby booms as evidenced by their small shares for 20–34 year olds in 1985, compared to the United States and many others. Both experienced a larger fertility boom in the 1970s. They have little else demographically in common. Irish fertility remained quite high through the 1980s, along with dramatic changes migration, while Japanese fertility dropped precipitously.

The boomer cohort is seen in the middle-to-old working-aged population in 2005 when they occupy the age groups from 41–59. Differences in the boom, longevity improvements, and migration, makes the size of this group different across countries and time.

3. Results: Population age structure and fiscal shocks

I now report estimates of Eq. (1). I begin with my preferred specification, which controls for the entire age distribution using the (Fair and Dominguez, 1991) style controls defined in Eq. (2). As I will establish, there is strong variation across working-age and retirement that may be missed by single population share estimates. Section 3.1 describes the baseline results for tax increases,

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13 Especially the existence or size of an “echo” boom.

14 I choose these dates as they represent: the period after which the boomer cohort has fully entered the workforce, one where they are in late career, and finally when they have almost fully retired.

15 Large emigration in the 1980s, which can be seen to decimate their adult population in 1985, which rapidly reversed during the Celtic Tiger period.
3.1. Tax consolidations: Estimations with the full age distribution

My preferred estimates use demographic controls that are constructed from population age shares across the entire distribution using a low order polynomial, as in Fair and Dominguez (1991). Table 1 reports estimates for Eq. (1) using the three demographic variables to control for population age structure. I report the effect of taxes directly, demographics directly, and the interactions between the two. All other controls (and their interactions) are omitted from this table for space.

The point estimates for the direct effect of the tax shock in on age in Table 1 are $-1.42$ over the full four year horizon. This is slightly smaller than the original (Guajardo et al., 2014) finding of about $-2$. My object of interest is rather the indirect effects of the shock due to changes in underlying population age. These are the interaction terms between the tax and demographic controls,

Table 1
Output and Deficit response to tax shocks: Full age distribution.

| Horizon, $h$ | Cumulative IRF, Output | Cumulative IRF, Deficit |
|--------------|------------------------|------------------------|
|              | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  |
| Tax          | 0.07| $-1.02^{**}$ | $-1.23^{**}$ | $-1.42^{**}$ | $-0.24$ | $-0.24$ | $-0.35$ | $-0.36$ |
| (0.10)       | (0.40) | (0.60) | (0.58) | (0.25) | (0.37) | (0.40) | (0.48) |
| D1           | 0.06| $-0.00$ | $-0.03$ | $0.00$ | $0.02$ | $0.12$ | $0.22$ | $0.31$ |
| (0.04)       | (0.09) | (0.16) | (0.27) | (0.06) | (0.13) | (0.21) | (0.27) |
| D2           | $-0.11^*$ | 0.04 | 0.12 | 0.07 | $-0.02$ | $-0.19$ | $-0.32$ | $-0.44$ |
| (0.07)       | (0.18) | (0.31) | (0.49) | (0.11) | (0.21) | (0.33) | (0.45) |
| D3           | 0.04| $-0.04$ | $-0.09$ | $-0.08$ | $0.00$ | $0.08$ | $0.12$ | $0.16$ |
| (0.03)       | (0.09) | (0.15) | (0.23) | (0.05) | (0.10) | (0.15) | (0.20) |
| Tax $\times$ D1 | $-0.02$ | 0.40 | 0.90$^{**}$ | 0.94$^{*}$ | 0.10 | $-0.39^{**}$ | $-0.64^{**}$ | $-0.72^{*}$ |
| (0.08)       | (0.20) | (0.36) | (0.43) | (0.12) | (0.18) | (0.25) | (0.35) |
| Tax $\times$ D2 | 0.03 | $-0.75^*$ | $-1.58^{**}$ | $-1.61^*$ | $-0.12$ | 0.75$^{**}$ | 1.18$^{**}$ | 1.35$^{**}$ |
| (0.15)       | (0.38) | (0.68) | (0.78) | (0.21) | (0.30) | (0.42) | (0.59) |
| Tax $\times$ D3 | $-0.01$ | 0.36$^{*}$ | 0.73$^{**}$ | 0.74$^{*}$ | 0.04 | $-0.37^{**}$ | $-0.56^{**}$ | $-0.64^{**}$ |
| (0.07)       | (0.19) | (0.33) | (0.38) | (0.09) | (0.13) | (0.19) | (0.27) |

$X_i$ includes two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks. Variables D2 and D3 are scaled by 10 and 100 respectively for clarity.

with Section 3.2 reporting estimates using spending cuts. These have very different interactions with age. In Section 3.3, I show while using just the WAP, that demographic interactions are weaker, but still significant. The differences in estimates using WAP provide some insight into the different transmission channels between tax and spending shocks.
and are significant at all horizons after the contemporaneous year of the shock for both outcomes. One drawback of this approach to controlling for age structure is that these coefficients are not easy to interpret themselves. Since they are weights on the polynomial fit of the five-year age coefficients, their significance implies that there is enough variation in effect across ages to justify all three terms. A third order polynomial allows for up to two turning points in the coefficients across ages, which will be readily seen below.

Age specific coefficients $\alpha_j$ are backed out using the estimates from Table 1. These are by assumption: $\alpha_j = \sum_{k=0}^K \gamma_{k,j}$, where $\gamma_k$ is given by the estimates of $\hat{\gamma}_k(D)$ from Eq. (1). However, these composition effects of age are not generally significant in these specifications. More importantly, these direct effects of population aging on outcomes are not my object of interest. The age specific indirect effects, estimated through interaction between demographics and the fiscal tax shock, describe how fiscal transmission changes across age groups. These are calculated the same way but using $\hat{\theta}_{s,h}(D)$. Unlike composition effects, they are zero in the absence of a fiscal policy change and scaled by the size of the fiscal shock. I report the estimates of $\hat{\alpha}_h$, the five-year age-specific coefficient for the indirect effect of tax shocks, for both output and deficits in Fig. 3. These are reported for a 1 percent increase in tax relative to GDP. To aid in the interpretation of these figures, I also include the demeaned values of the population age shares in the United States for 1985, 2005, and projections to 2025.16

Demeaned values of population shares always sum to zero. While the output indirect effect coefficients are always positive (and deficit all negative), this does not mean that the demographic effect must itself be positive (or negative), as the net impact is the average of these age specific effects multiplied by the positive/negative population shares relative to their long run mean. For example, large positive shares age 20–30 in 1985 will combine with large, positive, indirect coefficients associated with that age for output to offset the average effect of the shock (which is negative). The large negative weights for middle ages have smaller indirect coefficients, and so do not fully offset these.

Table 2 shows how conditional responses are constructed for the full ($h = 3$) horizon. This shows how these age coefficients, weighted by population shares, sum up. The demographic effect is simply the sum of estimated interaction terms ($\hat{\theta}_D$) from Table 1.
multiplied by the constructed demographic variables. This is by definition equal to the sum of all five year coefficients ($\hat{\alpha}_j$), graphed in Fig. 3, times the corresponding demeaned five year population share. Fig. 4 plots the state dependent responses of output and deficit for the United States at all horizons for these three demographic structures. These are the changes in output and deficit from Table 2, including the earlier horizons.

In 1985, large population pressure from young working ages shrinks the absolute size of the negative output response, as this group has a relatively large, positive, indirect effect. The corresponding negative population weights are concentrated in late career, where their coefficients are small. This is offset slightly by a relatively small share of old age retirees. The net result is a positive, 1.16, contribution coming through population age. This offsets the negative direct effect of the tax shock, making it much smaller in absolute value than its $-1.42$ average. Meanwhile, the large share of young workers (and dependents) have a relatively large negative effect reinforcing the contraction of the deficit. Taking the multiplier as the ratio of these two terms, the age-dependent multiplier becomes quite small, with population age shrinking the numerator and increasing the denominator.

Boomers move to late-career age in 2005, where their estimated impact on the deficit shrinks substantially. At the same time, lower than average young workers and retirees offset the average (negative) effect. This turns the net deficit response positive. One could perhaps interpret underlying demographics as changing where the economy falls on a Laffer curve, with having large shares concentrated in 40–60 age groups pushing the economy to the downward sloping portion. While demographics strengthen the output response, making tax increases strongly contractionary, the multiplier becomes negative.

Projecting forward, retirees dominate these estimates, with the oldest age group having an outsized impact. Their coefficient estimates for retirees is strongly positive for output (offsetting the average effect) and negative for deficits (reinforcing the average effect), such that predicted multipliers shrink to near zero as aging continues. The estimated multiplier in 2005 is not expansionary austerity, despite being negative. This is actually the worst of both worlds for a policy-maker wishing to raise taxes. Output responds strongly and deficits do not improve. These projections in 2025, on the other hand, suggest a world where austerity measures are easier, with large deficit improvements and little negative impact on output.

The exercise from Table 2 can be repeated for all countries in the sample, with results shown in Fig. 5. Fig. 5(a) and Fig. 5(b) highlight the demographic impact on output alone, and estimates that allow joint age effects on output and the deficit responses. Fig. 5(a) shows the variation coming from demographics on output, holding deficit response to the average value in Table 1. For most countries the age-specific output response looks similar to the United States described above, though there are differences for the very young and very old economies.

Fig. 5(b) allows for state dependence on both dimensions. While the estimate for the average multiplier is quite large (about 4), the demographic impact on fiscal deficits almost always works to make the state dependent multiplier small. The relatively young and old economies of 1985 and 2025 respectively see weaker output responses due to demographics and stronger deficit responses. Since these both work to shrink multipliers, effects are generally much smaller in absolute value than the average effect. While the middle aged economies of 2005 mostly see output effects of these tax shocks strongly reinforced, the effect on deficit is weakened, often reversing the sign of the multiplier entirely. As we move to a world of high concentration of populations in old age retirees the large negative effects on deficits and large positive effects on output response tends to push all conditional multipliers toward

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17 I exclude from both panels in Fig. 5 extreme values (greater than 15) for readability.
zero. For all figures, I caution that this effect in 2025 for Fig. 5(b) is strongly dominated from the large point estimate of the effect of the oldest age group. While it is reasonable to try to understand the impact of unprecedented increases in this age group, the point estimates of a single age may be sensitive to model and specification.

What do the results in Fig. 5 suggest? Not only is aging potentially quite important for the pass through of fiscal tax shocks to output, but also incredibly important for deficits. Understanding how both series respond will be crucial to understand the effects of fiscal consolidation going forward. There is evidence from these empirical correlations that there may be strong downward pressure on multipliers arising from tax shocks in the future. Any research trying to understand the role of fiscal consolidations should consider that policy changes may have drastically different efficacy as underlying demographics change. This result may be welcome news to policy makers. If the United States, for example, faces pressure to reduce primary deficits through austerity, the results in Table 2 suggest that this will be much less painful in 2025 than it would have been in 2005. This is in spite of average multipliers that suggest an incredibly painful consolidation. Conversely, if one expects a symmetric response, tax cuts may have little output impact while being quite hard to finance due to their large impact on the deficit.

3.2. Spending consolidations: Estimations with the full age distribution

Table 3 reports results using narrative spending cuts. Unlike tax shocks, spending cuts display both weaker average output effects and no interaction between age and output. The smaller direct effect of spending is in line with (Guajardo et al., 2014), as well as others using their shocks. However, there again are significant demographic correlations with the effects of these spending cuts on reductions in the deficit. Interestingly, these display different indirect effects from those in Table 1, which can be seen from their different signs.

Fig. 6 shows the indirect effect for output and deficits with aging. These are calculated in the same way those in Fig. 3. As expected the output indirect effect is small and relatively flat. Although deficit coefficients have opposite sign from those in Fig. 3, they still exhibit stronger interaction with young workers, shrinking through middle age. Unlike tax coefficients, retirees show no rebound and have effects similar to late career workers. The large positive response of young workers weakens the negative effect of spending cuts on deficits, older workers and retirees show less of this offset, suggesting a larger deficit transmission with increased weights in older ages groups, broadly defined.

I once again plot the state dependent IRFs for the United States from spending shocks in Fig. 7. Output interactions in Fig. 7(a) are quantitatively small (in addition to being statistically insignificant). In Fig. 7(b), the magnitude of the interaction of age structure with deficits is considerable. The relatively young population of 1985 reinforced the negative deficit effect with tax shocks; here it counteracts it. The implication projecting forward is that these will have strong reinforcing effects on the deficit consolidation, with spending cuts having larger reductions in deficit-to-GDP. Output responses, though smaller on average than in responses to taxes, remain the same across age.

Given the lack of significant of output responses, I do not present multipliers across all countries from these coefficients. The strength of state dependent deficit response works across all countries in this sample to decrease fiscal spending multipliers over time. Because prior work such as Basso and Rachedi (2021) suggests that spending shocks are related to age through labor supply channels, I test in Section 3.3 whether using just WAP to control for demographics provides any relationship between fiscal shocks and age. In Section 4, I will provide further evidence that a labor supply channel is relevant for spending shocks, but that tax transmission work through different channels, explaining some of the differences between my baseline tax and spending results.
Table 3
Output and deficit response to tax shocks: Full age distribution.

| Horizon, h | Cumulative IRF, Output | Cumulative IRF, Deficit |
|------------|------------------------|------------------------|
|            | 0 1 2 3                | 0 1 2 3                |
| Spend      | −0.10 −0.51** −0.35 −0.63 | −0.52** −1.12*** −1.30*** −1.35*** |
|            | (0.14) (0.29) (0.35) (0.52) | (0.26) (0.35) (0.38) (0.37) |
| D1         | 0.06 0.09 0.20 0.37 | 0.04 0.12 0.22 0.24 |
|            | (0.05) (0.10) (0.21) (0.33) | (0.06) (0.12) (0.19) (0.24) |
| D2         | −0.11 −0.09 −0.21 −0.46 | −0.04 −0.16 −0.31 −0.33 |
|            | (0.08) (0.17) (0.35) (0.56) | (0.08) (0.19) (0.30) (0.42) |
| D3         | 0.05 0.01 0.03 0.12 | 0.01 0.05 0.11 0.11 |
|            | (0.03) (0.08) (0.15) (0.24) | (0.04) (0.08) (0.13) (0.20) |
| Spend × D1 | 0.03 0.05 −0.17 −0.32 | 0.30*** 0.59*** 0.55*** 0.78** |
|            | (0.07) (0.14) (0.23) (0.27) | (0.09) (0.16) (0.21) (0.22) |
| Spend × D2 | −0.04 −0.04 0.29 0.53 | −0.45*** −0.86*** −0.71*** −0.99*** |
|            | (0.11) (0.24) (0.35) (0.41) | (0.14) (0.27) (0.32) (0.32) |
| Spend × D3 | 0.01 0.01 −0.12 −0.23 | 0.19*** 0.35*** 0.25* 0.34** |
|            | (0.05) (0.11) (0.15) (0.18) | (0.06) (0.12) (0.13) (0.14) |
| Spend × 𝑋_𝑖,𝑡 | ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ |
| 𝑋_𝑖,𝑡 | ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ |
| TimeFE | ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ |
| CountryFE | ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ ✓ |
| R2         | 0.82 0.66 0.56 0.53 | 0.39 0.51 0.54 0.59 |
| N          | 442 442 442 442 | 442 442 442 442 |

Table reports estimations for IRFs of narrative spending cuts on two outcomes. Standard errors in parenthesis and clustered at the country level. 𝑋_𝑖,𝑡 includes two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks. Variables D2 and D3 are scaled by 10 and 100 respectively for clarity.

† 𝑝 < 0.15.
* 𝑝 < 0.10.
** 𝑝 < 0.05.
*** 𝑝 < 0.01.

Fig. 6. Indirect Effects at 𝑡 = 3 by Age (Spending Shock).

3.3. Fiscal multipliers and the working age population

I now study interactions between fiscal shocks and the WAP. While earlier results suggest that these estimates are missing key variation across age, particularly for tax shocks, they provide useful context for the large differences between the tax and spending results shown above. They also link my work to Honda and Miyamoto (2021), who use a similar control in related work that focuses on spending shocks. Results for state dependent IRFs are reported in Table 4. This table contains three separate impulse response functions for each of the two outcomes of interest (output and deficits). Total consolidations are simply the combination of spending cuts and tax hikes. I include in the table the composition effects of the working age population on outcomes directly, as well as their indirect effects coming through the fiscal shocks. As above, coefficient estimates for other controls and their interactions with the shocks are omitted for space.

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18 They use old age dependency ratios, for which I find similar results in Appendix C.
The indirect effects suggest similar point estimates across the three fiscal shock specifications in Table 4. These are somewhat significant at all horizons for the combined series, and are more significant for spending shocks than for taxes. The positive interaction coefficients suggests that increasing working age population weakens the effects of fiscal consolidation. Here I find very little in the way of a demographics transmission of fiscal shocks to deficits.

The indirect effects on output are large relative to the size of the average effects of the fiscal shock. In Fig. 8, I plot the conditional IRF when WAP is +/- one and one-half standard deviations around its sample mean, about 2 and 1 percentage points respectively.
Table 4  
Output and deficit response: Working age population.

| Horizon, $h$ | Cumulative IRF: Output | Cumulative IRF: Deficit |
|-------------|------------------------|------------------------|
|             | 0         | 1     | 2     | 3     | 0     | 1     | 2     | 3     |
| Total       | 0.06     | -0.28* | -0.25 | -0.37 | -0.15 | -0.43* | -0.64*** | -0.68*** |
| WAP         | -0.02    | 0.07   | 0.09   | 0.07   | 0.06   | 0.14   | 0.26   | 0.36   |
| TotalxWAP   | 0.09***  | 0.22*  | 0.34*  | 0.54*  | 0.09   | 0.11   | 0.09   | 0.07   |

| X_{it}      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| X_{it} × Total | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| TimeFE      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| CountryFE   | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |

| R2          | 0.83     | 0.67   | 0.56   | 0.53   | 0.39   | 0.49   | 0.53   | 0.55   |
| N           | 442      | 442    | 442    | 442    | 442    | 442    | 442    | 442    |

| Tax         | 0.09     | -0.69** | -0.69  | -0.76  | -0.25  | -0.63†  | -0.92**  | -1.05*** |
| WAP         | -0.03    | 0.07    | 0.08   | 0.07   | 0.06   | 0.16   | 0.28    | 0.37    |
| TaxxWAP     | 0.15**   | 0.11    | 0.32   | 0.55   | 0.04   | -0.06  | -0.16   | -0.19   |

| X_{it}      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| X_{it} × Tax | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| TimeFE      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| CountryFE   | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |

| R2          | 0.83     | 0.68   | 0.58   | 0.54   | 0.38   | 0.48   | 0.51   | 0.54   |
| N           | 442      | 442    | 442    | 442    | 442    | 442    | 442    | 442    |

| Spend       | -0.05    | -0.44*  | -0.45  | -0.82* | -0.32  | -0.71** | -0.88**  | -0.75†  |
| WAP         | -0.03    | 0.09    | 0.17   | 0.23   | 0.09   | 0.20   | 0.29    | 0.39    |
| SpendxWAP   | 0.16***  | 0.33*** | 0.26   | 0.39   | 0.08   | 0.20   | 0.23    | 0.22    |

| X_{it}      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| X_{it} × Spend | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| TimeFE      | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |
| CountryFE   | ✓        | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      | ✓      |

| R2          | 0.82     | 0.66   | 0.55   | 0.51   | 0.38   | 0.48   | 0.52    | 0.55    |
| N           | 442      | 442    | 442    | 442    | 442    | 442    | 442    | 442    |

Table reports separate estimations for IRFs of three fiscal shocks on two outcomes. $X_{it}$ includes two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks.

† $p < 0.15$.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Because the average effect of deficits are relatively close to one, and aging has little interaction effect, these output shocks are close to estimates of the multiplier, taken as the ratio of these two estimates. For the full horizon these are 0.72 and 1.09 for taxes and spending respectively. According to these estimates the fiscal consolidation multiplier for the United States should have been high at the start of the sample, low toward the end, and should be large again as WAP falls across the board. This trend would be true in many countries as seen in Fig. 1, with differences in timing. Such a result from 1985 to 2005 is in line with observed trends in spending multipliers documented in Perotti (2005).

Why are these results so different than those estimated using the full age distribution controls? It is clear in Fig. 3 and Fig. 6 that there were large differences in the population coefficients within working life, which could be masked by using WAP. Further, with the tax shocks, shares of young dependents and old dependents have very different effects, but are forced to act symmetrically when using WAP alone. Another consideration is that the effects of changing WAP might pick up variation in labor market tightness due to demographics, which may be poorly captured by the full population controls. This may have different and potentially offsetting transmission from other life-cycle transmission.

It seems plausible from this exercise that demographic transmission of spending shocks may work through different channels than transmission of tax shocks. If demographic spending shocks work through labor supply/demand channels, as suggested in Basso and Rachedi (2021), then it is reasonable that WAP better captures these effects. Meanwhile, if taxes display more impact on life-cycle
consumption/savings decisions, such as those that have been extensively explored in the context of monetary policy, then WAP may do a poor job capturing relevant demographic variation. In the next section, I further explore these channels.

4. Exploring potential channels: Response of consumption, investment, and hours worked

To further explore the potential demographic transmission channels of fiscal shocks, I estimate impulse responses as above for the log changes in: real consumption, real investment, and total hours worked. My hope is to understand whether aging affects the pass-through of policy to consumption/savings and labor supply channels. I also hope explain some of the differences in estimates between tax and spending interactions above. Results for these IRFs are shown in Table 5. I compress table notation for space, but all specifications use the same control set, lag structure, and full sets of interactions as the baseline specifications in Table 1 and Table 3, excluding contemporaneous controls of consumption/investment when used as outcomes.

The direct effects estimated for tax increases are the expected negative sign and broadly significant. Magnitudes are intuitive with standard macro facts: investment responds more than consumption, while labor response is slightly weaker. There is a large degree of age dependence for both consumption and investment, with effects strengthening over time. Tax shocks show no meaningful interaction with hours worked. I take this as suggestive evidence that tax shocks interact with age through life-cycle consumption/savings decisions.

Spending cuts reduce consumption on average. Over longer horizons they appear to increase investment, perhaps signaling some government crowd-out. The mean spending effect on hours is completely flat. While there are strong demographic interactions for spending transmission to investment in the initial periods of the policy, these fade over time. Further, though they have the same sign as those for tax cuts, their magnitude is almost four times smaller when comparing peak effects. Unlike tax shocks, spending has large and persistent age interactions with hours worked.

I plot the indirect effects across the age distribution for consumption and investment responses to tax shocks in Fig. 9 for the full (h=3) horizon. While the level values are quite different, the relative change by age groups follow similar patterns. The shape of these coefficients are similar to those in Fig. 3 for output, and so much of the same logic discussed there applies. In 1985 the United States has above average shares of all age groups under 40, with particularly large shares of individuals in their 20s. Coefficients for these ages are relatively large and positive, and negative weights are mostly concentrated in late career where coefficients are small. This implies that there will be strong positive pressure mitigating the negative effects of tax hikes when the population is distributed in this way. In 2005, when these young workers are scarce and mid-late career workers have much larger population shares, this flips, allowing negative responses to tax increases to grow in absolute value. Eventually the large coefficients in old age will bring these effects back down, with consumption having particularly strong reversal. This can be seen in the conditional IRFs for the United States in Fig. 10.

State dependence of consumption and investment for tax shocks may come about for a number of reasons. Late-career workers are likely to be more strongly impacted by progressive tax changes, perhaps driving some result through changes in their tax burden. Differences in MPC by age, as found in Fernández-Villaverde and Krueger (2007), may also affect their response to an equivalent tax shock. Quantitative estimates from life-cycle models, which may capture such an age relationship, would be useful to shed light on such mechanisms. While it is intuitive that retired households may be less exposed to these shocks, they are also more income constrained and therefore may react more strongly, as estimated in work (Ferraro and Fiori, 2020). Understanding how household decisions aggregate under plausible tax changes should help unpack discrepancies between those results and what I find here. Also useful would be investigating different forms of taxation. Recent work by Nguyen et al. (2021) shows that consumption taxes have much different impact than income. These may also have different age-specific transmission.

Unlike tax shocks there are large and significant spending interactions between demographics and working hours. While average coefficient on spending consolidations is zero, it is not entirely clear what the sign should be. A positive response would be sensible if reductions in spending, for example on social welfare programs, encourages individuals to work more, while a negative response may imply that effects coming through output reductions affect employment negatively. Fig. 11 plots the age specific effect at the h = 3 horizon for hours worked across the tax and spending specifications. I include the insignificant tax response as a reference, as they are found to be important in Ferraro and Fiori (2020), who suggest large employment responses to taxes driven by early career workers. These are the more important groups for both shocks, but are large and significant for spending cuts. Positive deviations in young workers generates a negative estimated effect of spending, implying large contractions in labor supply. This is mitigated by mid-to-late career workers and early retirement. The net effect is plotted in the IRFs in Fig. 12.

This interaction with hours is in line with findings of Basso and Rachedi (2021), who show that local fiscal spending shocks increase multiplier effects in the United States, primarily driven by young working aged individuals. They show in a new Keynesian model how labor supply and demand yields this result. The evidence here is consistent with such a channel being empirically relevant. The weakening I find of deficit effects with young populations is consistent with a labor supply contraction in response to a spending cut, as such a labor response should limit the effectiveness of the spending cut in reducing deficits through a reduction in the tax base. The lack of a spending interaction with output in Table 3 makes my results not fully consistent with Basso and Rachedi (2021). This may be due to asymmetry between spending cuts and increases in terms of output effect. The investment impacts of spending cuts in Table 5 may matter for this. They are similar to the tax estimates in Fig. 10(b) (though smaller and shrinking), which move in opposite directions from the hours estimates in Fig. 12(b). If these effects are offsetting then it is possible that the zero output impact in Table 3 is hiding conflicting demographic forces in response to spending on output. The (Fair and Domínguez, 1991) controls may also poorly capture the relevant age groups with statistical power.

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19. See, for example: Auclert (2019), Berg et al. (2021), and Leahy and Thapar (2019)

20. Hours worked is the only variable not present in the (Jordà et al., 2017) and come from (OECD, 2021).
Table 5: Consumption, Investment, Hours Impulse Response Functions.

| Horizon, $h$ | Cumulative IRF (Tax Shock) | Cumulative IRF (Spend Shock) |
|--------------|----------------------------|-----------------------------|
|              | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  |
| **Consumption** |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |
| Tax          | 0.04 | −0.85*** | −1.16** | −1.44*** | Spend | −0.06 | −0.26 | −0.44 | −0.91† |
|              | (0.16) | (0.23) | (0.43) | (0.50) | &nbsp; | (0.14) | (0.20) | (0.40) | (0.54) |
| Tax × D1    | −0.03 | 0.28 | 0.63* | 0.73* | Spend × D1 | −0.09* | −0.08 | −0.06 | −0.10 |
|              | (0.09) | (0.21) | (0.33) | (0.36) | &nbsp; | (0.05) | (0.08) | (0.16) | (0.21) |
| Tax × D2    | 0.08 | −0.56† | −1.21** | −1.39** | Spend × D2 | 0.13 | 0.07 | −0.01 | 0.05 |
|              | (0.16) | (0.35) | (0.57) | (0.63) | &nbsp; | (0.09) | (0.14) | (0.28) | (0.34) |
| Tax × D3    | −0.04 | 0.28* | 0.59** | 0.68*** | Spend × D3 | −0.05 | −0.01 | 0.04 | 0.02 |
|              | (0.07) | (0.16) | (0.26) | (0.30) | &nbsp; | (0.04) | (0.07) | (0.13) | (0.16) |
| R²          | 0.80 | 0.72 | 0.63 | 0.58 | 0.79 | 0.69 | 0.62 | 0.58 |
| N           | 442 | 442 | 442 | 441 | 442 | 442 | 442 | 442 |
| **Investment** |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |
| Tax         | −1.03** | −2.82*** | −2.78† | −3.22* | Spend | 0.19 | 0.60 | 1.45* | 1.51 |
|              | (0.40) | (0.87) | (1.73) | (1.63) | &nbsp; | (0.52) | (0.59) | (0.82) | (1.03) |
| Tax × D1    | 0.39 | 1.99*** | 2.74** | 3.15** | Spend × D1 | 0.60*** | 0.71** | 0.06 | −0.09 |
|              | (0.38) | (0.68) | (1.18) | (1.27) | &nbsp; | (0.17) | (0.32) | (0.54) | (0.58) |
| Tax × D2    | −0.71 | −3.39*** | −4.45** | −5.29** | Spend × D2 | −1.02*** | −1.35** | −0.60 | −0.44 |
|              | (0.66) | (1.16) | (2.08) | (2.20) | &nbsp; | (0.29) | (0.58) | (0.85) | (0.92) |
| Tax × D3    | 0.34 | 1.54*** | 1.96* | 2.38** | Spend × D3 | 0.46*** | 0.65** | 0.40 | 0.36 |
|              | (0.30) | (0.53) | (0.97) | (1.01) | &nbsp; | (0.14) | (0.28) | (0.38) | (0.41) |
| R²          | 0.69 | 0.73 | 0.68 | 0.64 | 0.70 | 0.72 | 0.67 | 0.62 |
| N           | 442 | 442 | 442 | 442 | 442 | 442 | 442 | 442 |
| **Hours**   |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |    |
| Tax         | −0.16 | −0.26 | −0.55* | −0.63* | Spend | 0.03 | −0.12 | −0.10 | −0.04 |
|              | (0.13) | (0.29) | (0.31) | (0.31) | &nbsp; | (0.16) | (0.26) | (0.27) | (0.24) |
| Tax × D1    | −0.08 | 0.07 | 0.08 | −0.13 | Spend × D1 | −0.12* | −0.25* | −0.36*** | −0.35** |
|              | (0.08) | (0.15) | (0.18) | (0.22) | &nbsp; | (0.07) | (0.13) | (0.15) | (0.16) |
| Tax × D2    | 0.15 | −0.10 | −0.13 | 0.18 | Spend × D2 | 0.16† | 0.34† | 0.53** | 0.56*** |
|              | (0.15) | (0.25) | (0.29) | (0.38) | &nbsp; | (0.11) | (0.20) | (0.22) | (0.25) |
| Tax × D3    | −0.07 | 0.04 | 0.06 | −0.06 | Spend × D3 | −0.06 | −0.13 | −0.21** | −0.24*** |
|              | (0.07) | (0.11) | (0.13) | (0.17) | &nbsp; | (0.05) | (0.09) | (0.10) | (0.11) |
| R²          | 0.28 | 0.29 | 0.32 | 0.34 | 0.32 | 0.35 | 0.39 | 0.39 |
| N           | 388 | 388 | 388 | 388 | 388 | 388 | 388 | 388 |

Control set same as baseline including two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks. Year and country fixed effects included. D2 and D3 scaled by 10 and 100 respectively for readability. Effects of population structure on outcomes directly are omitted for space.

†$p < 0.15$.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

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Fig. 9. Age Shares Indirect Effects on Tax Shocks at $h = 3$ by Age.
5. Robustness

In Appendix D, I run a series of robustness exercises, repeating my baseline estimates of tax and spending shocks under various model specifications. I include specifications that: omit other control variables, change lag structure, and add additional macroeconomic and financial controls. Additional controls used are: log change in cpi, changes in current account-to-gdp ratios, change in log real house prices, and the change in annual real per capita loans. These variables are missing for some country-years, and reduce the sample slightly. I also change lag structure considering zero and four lags of controls and dependent variables. For all tables in Appendix D, column 3 represents the same specification as those in the paper, including: baseline controls, country and time fixed effects, and two lags of dependent variables and controls.

In general I find little change to the point estimates of my results. For the full age polynomial controls I report estimates for horizons \( h = 2, 3 \), where results were most significant in the baseline case. For WAP I report estimates for horizons \( h = 0, 1 \), where the corresponding baseline specifications showed significant interaction effects. Estimates of the WAP appear to be more sensitive, though still broadly consistent and under some specifications gain missing significance at longer horizons. This reinforces my already strong preference for the (Fair and Dominguez, 1991) style controls for age structure.

In addition to these robustness checks using the same fiscal shock variable, I use the narrative UK tax shocks from Cloyne (2013) to test if my primary results on taxation in Table 1 hold up to a change in the fiscal shocks themselves. This narrative tax database differs from that of Guajardo et al. (2014) on a number of dimensions. For one, instead of focusing entirely on austerity measures there are both positive and negative changes in taxation. Tax changes are classified into endogenous and exogenous groups, with the exogenous categories containing: long-run economic reforms, ideological changes, external changes due to outside factors, and deficit consolidation. Notably the deficit consolidation measure is more conservative than that of Guajardo et al. (2014) who’s estimates...
Fig. 12. Hours Worked: Conditional IRFs United States. (Direct effect shows point estimates of average fiscal shock. Conditional IRFs adjusted for indirect effect through aging using (Fair and Dominguez, 1991) controls for age distribution in the specified year.).

for the UK span both the exogenous and endogenous shocks in the (Cloyne, 2013) data. It also covers a longer period. I estimate results over the period from 1945 to 2006, though a single country (the United Kingdom), which limits the sample substantially.

Table 6 reports estimates using the exogenous narrative shock series from Cloyne (2013). For the specification with controls I use contemporaneous and two lags of: log changes in consumption and investment, demeaned as in the baseline model. Both specifications contain two lags of output and one lag of the policy shock. With and without controls demographic interactions are quite strong at the cumulative horizon, though they lack statistical significance in the first three years of the shock. While they are larger in magnitude than the point estimates in Table 1, they have the same sign and scaled by a similar magnitude to the direct effects of the tax shocks, which have larger estimates here. This suggests that the demographic interactions found in my results are not artifacts of the specific narrative instrument chosen, and may be relevant for a broader set of fiscal policy shocks, than the austerity measures I report.

6. Conclusions

With public debts reaching their highest levels since World War II, and populations rapidly aging, it is important to understand the effect that age structures will have on fiscal decisions going forward. This paper provides context for understanding how age structure is related to the transmission of fiscal shocks. I find strong empirical evidence of such state dependence. Using narratively estimated fiscal consolidation shocks, a flexible methodology for state dependence, and controlling for population structure, it appears that the effects of taxes increases and spending cuts on outputs and deficits have changed dramatically over the last thirty years.

What takeaways are there for policymakers? For one, output responses to tax shocks look much stronger with middle-aged economies, as many advanced economies were in the mid-2000s, and much weaker in the relatively young and old economies of 1985 and 2025 respectively. These output responses to tax shocks appear to come predominantly through consumption and investment responses that are much stronger when there are larger shares of the population aged 45–65. However, middle aged economies have much weaker, even positive, deficit response to the tax hike such that an appropriately defined multiplier has been negative and potentially small in absolute value in the recent past. Spending cuts, which have a lower average output effect, appear to not have output transmission affected by age. The deficit response does change with age and has been steadily strengthening since the 1980s also shrinking multipliers. The labor supply channel, already documented in work such as Basso and Rachedi (2021), appears to be driving this.

It is important to keep in mind that while the narratively identified fiscal shocks can make some claim toward being well identified, underlying population age structure likely cannot, and that more work should be done to try to understand the potential mechanisms at play here. Additionally, it is likely that not all tax and spending related shocks interact with age structure in the same way. Consumption taxes may interact differently than income tax, while spending shocks that affect investment may work differently than those aimed at providing government services. More work can be done to understand these mechanisms in the macro data by

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21 The control set chosen is consistent with that used in Cloyne (2013).
Table 6
Output response: UK narrative instrument of Cloyne (2013).

| Horizon, $h$ | Cumulative IRF (Output) | Cumulative IRF (Output) |
|--------------|-------------------------|-------------------------|
|              | 0 1 2 3                 | 0 1 2 3                 |
| Tax          | $-1.63^*$               | $-0.88$                 |
|              | $(1.07)$                | $(0.84)$                |
| D1           | $-0.23$                 | $-0.17$                 |
|              | $(0.22)$                | $(0.16)$                |
| D2           | $0.26$                  | $0.19$                  |
|              | $(0.36)$                | $(0.22)$                |
| D3           | $-0.08$                 | $-0.06$                 |
|              | $(0.15)$                | $(0.09)$                |
| TaxD1        | $-0.21$                 | $-0.27$                 |
|              | $(0.48)$                | $(0.29)$                |
| TaxD2        | $-0.09$                 | $0.20$                  |
|              | $(0.82)$                | $(0.43)$                |
| TaxD3        | $0.13$                  | $-0.03$                 |
|              | $(0.35)$                | $(0.17)$                |
| $X_{i,t}$    | ✓ ✓ ✓ ✓                 | ✓ ✓ ✓ ✓                 |
| $X_{i,t} \times \text{Tax}$ | ✓ ✓ ✓ ✓ | ✓ ✓ ✓ ✓ |
| R2           | 0.33                    | 0.84                    |
| N            | 61 61 61 61             | 61 61 61 61             |

Table reports estimations for IRFs of tax shocks on output. Robust standard errors in parenthesis. $X_{i,t}$ includes two lags of the dependent variable, the tax shock, as well as contemporaneous values and two lags of: log change in real consumption per capita, log change in real investment per GDP, and the change in the short term interest rate.

continuing to classify plausibly exogenous fiscal shocks. Models with life-cycle implications for MPC should be turned toward this question to uncover specific mechanisms that in turn can be tested in micro data.

If these austerity shocks are symmetric, with effects of spending increases and tax cuts working the same way, then this paper might suggest that governments will see an important stimulus tool weaken as economies age. While my estimates using the UK narrative shock suggest similar effects using tax movements in both directions, more work should be done to establish the age transmission of stimulus. However, if output responses to fiscal consolidations weaken, while deficit responses are stronger, then these estimates paint a favorable portrait for government’s ability to pay down debts, which at present are extremely high. If policy makers are able to finance deficit reductions without a large economic contraction, then the age for austerity may arrive just in time to provide relief to countries looking to rein in debts in the coming years.

Data availability

Data will be made available on request.

Appendix A. Construction of demographic controls

My estimates for effects across the entire age distribution use a methodology first proposed in Fair and Dominguez (1991). Suppose one wanted to estimate the following relationship:

$$y_{i,t} = X_{i,t}\beta + \sum_{j} \alpha_j p_{j,i,t} + \mu_i + \nu + \epsilon_{i,t} \quad (3)$$

Where $y_{i,t}$ is any outcome of interest, $X_{i,t}$ is an arbitrary vector of controls, $\epsilon_{i,t}$ is error, $\mu_i$ country fixed effects, and $\nu$ a constant. The variables $p_{j,i,t}$ are shares of the population, divided into $J$ bins. This is inestimable due to the perfect colinearity of the population shares $p_{j,i,t}$. Additionally while one would prefer to take a granular approach to modeling population shares (allowing for a large number of, $J$, groups), these shares are highly colinear with one another, more so as their number increases. Finally in smaller samples it may be undesirable to fit such a large number of coefficients, particularly as in my case when interaction terms would also be needed. Fair and Dominguez (1991) proceeds by making the following two assumptions:

1. Letting $\alpha_j$ be the coefficient on population share $p_{j,i,t}$ of age group $j$ in country $i$ and time $t$. Assume that all of the effects of these coefficients across the age distribution sum to zero. In other words:

$$\sum_{j} \alpha_j = 0$$

$\alpha_j$ is the coefficient on population share $p_{j,i,t}$ of age group $j$ in country $i$ and time $t$. Assume that all of the effects of these coefficients across the age distribution sum to zero. In other words:

$$\sum_{j} \alpha_j = 0$$
2. Assume that the age coefficients \( a_j \) can be fitted with a \( K \) order polynomial. In other words:

\[
a_j = \sum_k^K y_k j^k
\]

(4)

This transforms the problem of estimating \( J \) coefficients into one of estimating \( K \). Second, the assumption that all age effects, \( a_j \), sum to zero makes them jointly estimable without having to drop the regression constant. Finally, by forcing the age effects to lie on a polynomial, the model requires that there be relatively smooth transitions from the effect of one age group to another. If one were to take only the first assumption (or omit the constant of the regression) it would be possible to estimate the effects to lie on a polynomial, the model requires that there be relatively smooth transitions from the effect of one age group to another.

\[
y_{i,t} = X_{i,t}\beta + \sum_j \left(y_0 + y_1 j + y_2 j^2 + y_3 j^3\right)p_{j,i,t} + \mu_t + \nu + \epsilon_{i,t}
\]

(5)

The summing over both sides of Eq. (4), and using the assumption that the sum of \( a_j \) must be zero, it can easily be shown that \( y_0 \) is equal to:

\[
y_0 = \frac{-1}{J} \left( y_1 \sum_j j + y_2 \sum_j j^2 + y_3 \sum_j j^3 \right)
\]

From here one simply plugs the above expression for \( y_0 \) into Eq. (5), given that the first term, \( \sum_j y_0 p_{j,i,t} = y_0 \) from the population parameters summing to one. Rearrange as:

\[
y_{i,t} = X_{i,t}\beta + y_1 \sum_j \left( p_{j,i,t} - \sum_j j/\sum_j \right) + y_2 \sum_j \left( p_{j,i,t}^2 - \sum_j j^2/\sum_j \right) + y_3 \sum_j \left( p_{j,i,t}^3 - \sum_j j^3/\sum_j \right) + \mu_t + \nu + \epsilon_{i,t}
\]

(6)

With the terms in parenthesis being the demographic variables. Because I used demeaned shares for the Kitagawa–Blinder–Oaxaca decomposition, I opt instead to estimate:

\[
y_{i,t} = X_{i,t}\beta + \sum_j \alpha_j (p_{j,i,t} - \bar{p}_{j,t}) + \mu_t + \nu + \epsilon_{i,t}
\]

(7)

There is slightly different rearrangement of terms to construct the correct demographic variables for estimation. This simplifies to:

\[
Dk_{i,t} = \left[ \sum_j \left( p_{j,i,t} - \bar{p}_{j,i,t} \right)^k - \sum_j j^k \sum_j \left( p_{j,i,t} - \bar{p}_{j,i,t} \right) \right]
\]

(8)

But only this first term is needed given that \( \sum_j y_0 (p_{j,i,t} - \bar{p}_{j,t}) = 0 \), and not \( y_0 \) as before. So rather the estimation becomes:

\[
y_{i,t} = X_{i,t}\beta + y_1 \sum_j \left( p_{j,i,t} - \bar{p}_{j,i,t} \right) + y_2 \sum_j \left( p_{j,i,t} - \bar{p}_{j,i,t} \right)^2 + y_3 \sum_j \left( p_{j,i,t} - \bar{p}_{j,i,t} \right)^3 + \mu_t + \nu + \epsilon_{i,t}
\]

(9)

Where these \( \sum_j (p_{j,i,t} - \bar{p}_{j,t})^k \) terms are the demographic controls used in the estimations in the paper.

Appendix B. Total consolidation shocks with (Fair and Dominguez, 1991) style controls

Here I present the regression estimates for total fiscal consolidation shocks for completeness. Interactions with population age are not significant, likely due to differences in taxation and spending transmission, as discussed in the text. Table 7 reports results using the baseline specification as in Table 1 and Table 3 in the paper.

Appendix C. Estimates using the old age population share

(Honda and Miyamoto, 2021) use old age dependency ratios in their estimations. Though they use a different identification of fiscal shocks, it is worthwhile to show that I can replicate their effects. In Table 8, I show estimates for four year impulse response functions for output and deficits for all three narrative consolidation shocks (Total, Tax, and Spending), but unlike above, I use old age dependency ratios to control for population age structure. Consistent with their broad finding I estimate positive interactions for each of these shocks, suggesting that aging decreases the size of fiscal multipliers. Their work does not report effects of fiscal deficits and instead adjust their output variable (scaling by previous year GDP) in an attempt to correctly arrive at fiscal multipliers, so while I find a large amount of state dependence of deficits, with old ages increasing the negative responses of all consolidation shocks, I cannot directly compare to theirs. In either case this would reinforce the shrinking of the multiplier effect as found in this paper.

These results may seem at odds with those from Table 4 in the main body of the paper. How can both working age population and old age dependents have positive interactions? The answer is that they are capturing different, but simultaneous trends. The
### Table 7
Output and deficit response: Fair and Dominguez (1991) style population controls.

| Horizon, $h$ | Cumulative IRF, Output | Cumulative IRF, Deficit |
|--------------|------------------------|------------------------|
|              | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  |
| Total        | 0.05 | -0.40** | -0.41 | -0.50 | -0.27* | -0.55** | -0.70*** | -0.75*** |
|              | (0.07) | (0.19) | (0.32) | (0.32) | (0.15) | (0.21) | (0.22) | (0.20) |
| D1           | 0.07* | 0.03 | 0.04 | 0.15 | -0.00 | 0.07 | 0.15 | 0.18 |
|              | (0.04) | (0.09) | (0.18) | (0.30) | (0.06) | (0.11) | (0.18) | (0.24) |
| D2           | -0.14* | 0.00 | 0.02 | -0.15 | -0.00 | -0.12 | -0.23 | -0.26 |
|              | (0.07) | (0.18) | (0.33) | (0.53) | (0.10) | (0.21) | (0.31) | (0.43) |
| D3           | 0.06* | -0.03 | -0.06 | 0.01 | -0.00 | 0.05 | 0.09 | 0.09 |
|              | (0.03) | (0.09) | (0.15) | (0.24) | (0.05) | (0.10) | (0.14) | (0.20) |
| Total $\times$ D1 | -0.02 | 0.12 | 0.15 | 0.10 | 0.18** | 0.21* | 0.19 | 0.29* |
|              | (0.04) | (0.12) | (0.21) | (0.22) | (0.07) | (0.11) | (0.13) | (0.16) |
| Total $\times$ D2 | 0.04 | -0.19 | -0.23 | -0.10 | -0.26** | -0.28* | -0.22 | -0.35 |
|              | (0.08) | (0.23) | (0.38) | (0.41) | (0.11) | (0.18) | (0.21) | (0.24) |
| Total $\times$ D3 | -0.02 | 0.09 | 0.10 | 0.03 | 0.10* | 0.10* | 0.06 | 0.10 |
|              | (0.04) | (0.11) | (0.18) | (0.19) | (0.05) | (0.08) | (0.09) | (0.10) |

$X_i, t$ includes two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks.

$^{†} p < 0.15$.

$^{*} p < 0.10$.

$^{**} p < 0.05$.

$^{***} p < 0.01$.

### Table 8
Output and deficit response and the old-age dependency ratio.

| Horizon, $h$ | Cumulative IRF: Output | Cumulative IRF Deficit |
|--------------|------------------------|------------------------|
|              | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  | 0  | 1  | 2  | 3  |
| Total        | 0.04 | -0.35** | -0.36 | -0.53 | -0.32** | -0.62** | -0.70*** | -0.66** |
|              | (0.09) | (0.14) | (0.27) | (0.33) | (0.12) | (0.15) | (0.18) | (0.24) |
| OldAge       | -0.08** | -0.29*** | -0.45*** | -0.65*** | -0.07 | -0.12 | -0.16 | -0.19 |
|              | (0.04) | (0.06) | (0.11) | (0.17) | (0.04) | (0.09) | (0.13) | (0.17) |
| Total $\times$ OldAge | 0.01 | 0.11* | 0.17** | 0.22*** | 0.02 | -0.01 | -0.11 | -0.22 |
|              | (0.02) | (0.05) | (0.07) | (0.07) | (0.05) | (0.09) | (0.11) | (0.15) |
| R2           | 0.83 | 0.67 | 0.57 | 0.54 | 0.40 | 0.50 | 0.54 | 0.57 |
| N            | 442 | 442 | 442 | 442 | 442 | 442 | 442 | 442 |

Control set same as above including two lags of the dependent variable as well as contemporaneous values and two lags of all controls and their interactions with policy shocks. Year and country fixed effects included.

$^{†} p < 0.10$.

$^{*} p < 0.05$.

$^{***} p < 0.01$. 
Table 9

Output and deficit response to tax shock, various models.

| Output Response | Horizon: h = 2 | Horizon: h = 3 |
|-----------------|----------------|----------------|
|                 | 1 2 3 4 5 6    | 1 2 3 4 5 6    |
| Tax             |               |               |
| -0.32           | -0.10         | -0.35         |
| (0.49)          | (0.32)        | (0.40)        |
| D1              | 0.08          | 0.22          |
| 0.22            | 0.22          | 0.55*         |
| (0.13)          | (0.16)        | (0.21)        |
| D2              | -0.16         | -0.35         |
| -0.32           | -1.02*        | -0.38         |
| (0.20)          | (0.29)        | (0.33)        |
| D3              | 0.07          | 0.15          |
| 0.12            | 0.45*         | 0.16          |
| (0.09)          | (0.13)        | (0.15)        |
| Tax × D1        | -0.531        | -0.64*        |
| -0.64*          | -0.64**       | -0.54*        |
| (0.33)          | (0.32)        | (0.28)        |
| Tax × D2        | 1.06*         | 1.19**        |
| 1.19**          | 1.18**        | 1.07**        |
| (0.58)          | (0.56)        | (0.42)        |
| Tax × D3        | -0.53*        | -0.57*        |
| -0.57*          | -0.56*        | -0.52*        |
| (0.28)          | (0.26)        | (0.19)        |
| BaseX           | ✓             | ✓             |
| BaseX × Tx      | ✓             | ✓             |
| AddX            | ✓             | ✓             |
| AddD × Tx       | ✓             | ✓             |
| TimeFE          | ✓             | ✓             |
| CountryFE       | ✓             | ✓             |
| Lags            | 0 0 2 2 0 4   | 0 0 2 2 0 4   |
| R2              | 0.09          | 0.32          |
| N               | 442 442        | 442 442        |

Deficit Response

| Horizon: h = 2 | Horizon: h = 3 |
|----------------|----------------|
|                 | 1 2 3 4 5 6    | 1 2 3 4 5 6    |
| Tax             |               |               |
| -0.32           | -0.10         | -0.35         |
| (0.49)          | (0.32)        | (0.40)        |
| D1              | 0.08          | 0.22          |
| 0.22            | 0.22          | 0.55*         |
| (0.13)          | (0.16)        | (0.21)        |
| D2              | -0.16         | -0.35         |
| -0.32           | -1.02*        | -0.38         |
| (0.20)          | (0.29)        | (0.33)        |
| D3              | 0.07          | 0.15          |
| 0.12            | 0.45*         | 0.16          |
| (0.09)          | (0.13)        | (0.15)        |
| Tax × D1        | -0.531        | -0.64*        |
| -0.64*          | -0.64**       | -0.54*        |
| (0.33)          | (0.32)        | (0.28)        |
| Tax × D2        | 1.06*         | 1.19**        |
| 1.19**          | 1.18**        | 1.07**        |
| (0.58)          | (0.56)        | (0.42)        |
| Tax × D3        | -0.53*        | -0.57*        |
| -0.57*          | -0.56*        | -0.52*        |
| (0.28)          | (0.26)        | (0.19)        |
| BaseX           | ✓             | ✓             |
| BaseX × Tx      | ✓             | ✓             |
| AddX            | ✓             | ✓             |
| AddD × Tx       | ✓             | ✓             |
| TimeFE          | ✓             | ✓             |
| CountryFE       | ✓             | ✓             |
| Lags            | 0 0 2 2 0 4   | 0 0 2 2 0 4   |
| R2              | 0.03          | 0.33          |
| N               | 442 442        | 442 442        |

Table reports the cumulative IRF two and three years after the initial year of the policy shock. Column 3 is identical to specification in Table 1. Cluster robust standard errors in parenthesis. Lags refer to number of lags on dependent variable, Xₜ, controls, and policy shocks. Baseline controls include demeaned values of: log change in real consumption per capita, log change in real investment per GDP, institutional investor rating, and the change in the short term interest rate. Additional controls include: log change in cpi, change in current account to gdp, change in long real house prices, and the change in annual real per capita loans.

¹p < 0.15.
²p < 0.10.
³p < 0.05.
⁴p < 0.01.
baby boomer cohort boosted working age population significantly as they entered the workforce from roughly 1964 to 1982. From there working age population remained elevated, with many countries in the sample showing slowed, but continued, growth turning at some point in the 1990s and 2000s. For the most part WAP remains above the sample mean at the end of my estimation period (2006), as can be seen in Fig. 1. This period was also characterized by large growth in old age dependents due to increasing longevity, why I prefer the methods used in Section 3.1 and Section 3.2. Use of information across the whole distribution allows that differences dramatically among adjacent groups.

**Appendix D. Robustness of main results**

In this Section 1 run a series of robustness exercises to show that my main results are not sensitive to changes in the control group, and lag structure. For reference column 3 in each table is the baseline specification used in the paper. Table 11 shows output responses for both tax and spending shocks with WAP as a control. Deficits are omitted as they were not significant in Table 4. Table 9 shows both output and deficit responses to tax shocks using my preferred demographic specification controlling for the full age distribution. In Table 10, I show the same results for the deficit response to spending cuts (again output is omitted as it was not significant in the baseline). The lags row refers to number of lags of both the outcome variable and of the control set. Additional controls (AddX) refers to inclusion of: log change in cpi, changes in current account-to-gdp ratios, change in log real house prices, and the change in annual real per capita loans.

### Table 10: Deficit response to spending shocks and full age distribution, various models.

| Horizon: h = 2 | 1 | 2 | 3 | 4 | 5 | 6 |
|---------------|---|---|---|---|---|---|
| Spend         | −1.98** | −1.27*** | −1.30** | −1.93*** | −1.58*** | −1.18** |
|               | (0.32)   | (0.31)   | (0.38)   | (0.41)   | (0.30)   | (0.49)   |
| D1            | 0.04     | 0.13     | 0.22     | 0.50*    | 0.12     | 0.20     |
|               | (0.12)   | (0.16)   | (0.19)   | (0.28)   | (0.16)   | (0.22)   |
| D2            | −0.10    | −0.24    | −0.31    | −0.87†   | −0.22    | −0.25    |
|               | (0.20)   | (0.30)   | (0.30)   | (0.55)   | (0.31)   | (0.41)   |
| D3            | 0.06     | 0.11     | 0.37     | 0.10     | 0.08     | −0.03    |
|               | (0.09)   | (0.14)   | (0.13)   | (0.26)   | (0.14)   | (0.19)   |
| Spd × D1      | 0.42**   | 0.37**   | 0.55**   | 0.32***  | 0.41*    | 0.51**   |
|               | (0.18)   | (0.18)   | (0.21)   | (0.11)   | (0.19)   | (0.22)   |
| Spd × D2      | −0.46†   | −0.40    | −0.71**  | −0.31†   | −0.49†   | −0.65*   |
|               | (0.28)   | (0.28)   | (0.32)   | (0.19)   | (0.30)   | (0.37)   |
| Spd × D3      | 0.13     | 0.10     | 0.25†    | 0.07     | 0.16     | 0.23     |
|               | (0.12)   | (0.12)   | (0.13)   | (0.13)   | (0.13)   | (0.17)   |

Table reports the cumulative IRF two and three years after the initial year of the policy shock. Column 3 is identical to specification in Table 1. Cluster robust standard errors in parenthesis. Lags refer to number of lags on dependent variable, $X_t$ controls, and policy shocks. Baseline controls include demeaned values of: log change in real consumption per capita, log change in real investment per GDP, institutional investor rating, and the change in the short term interest rate. Additional controls include: log change in cpi, change in current account to gdp, change in long real house prices, and the change in annual real per capita loans.

**Appendix E. Predictability of fiscal shocks from demographics**

In their paper introducing these narrative shocks, Guajardo et al. (2014), show that they are not predictive of news about the economy. This helps show that the shock is not reacting to current economic conditions, which may bias estimates of their effect on output. Here I wish to understand if my results are being driven by predictive power of the demographic variables on the narrative shocks themselves. To test this, I regress population shares on the narrative shock series. Table 12 does this with the WAP and
Table 11
Output response to tax and spending shocks: wap, various models.

|                | Horizon: h = 0 | 1 | 2 | 3 | 4 | 5 | 6 | Horizon: h = 1 | 1 | 2 | 3 | 4 | 5 | 6 |
|----------------|----------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|
| **Tax**        |                | −0.14            | −0.02            | 0.09             | 0.13             | 0.24             | −0.06            | −0.95            | −0.86            | −0.69**           | −0.99            | −0.40**           | −1.04**          |
|                |                | (0.56)           | (0.53)           | (0.12)           | (0.18)           | (0.15)           | (0.19)           | (0.64)           | (0.62)           | (0.29)           | (0.58)           | (0.15)           | (0.37)           |
| **WAP**        |                | 0.16**           | 0.19†            | −0.03            | −0.04            | −0.01            | −0.10†           | 0.27†            | 0.34             | 0.07             | 0.06             | 0.12             | −0.10            |
|                |                | (0.07)           | (0.12)           | (0.05)           | (0.05)           | (0.04)           | (0.06)           | (0.14)           | (0.24)           | (0.15)           | (0.16)           | (0.12)           | (0.16)           |
| **TaxxWAP**    |                | 0.38**           | 0.38**           | 0.15**           | 0.27***          | 0.09†            | 0.24***          | 0.46***          | 0.51***          | 0.11             | 0.42             | 0.02             | 0.29†            |
|                |                | (0.17)           | (0.17)           | (0.06)           | (0.09)           | (0.05)           | (0.07)           | (0.13)           | (0.13)           | (0.22)           | (0.33)           | (0.24)           | (0.19)           |
| **TimeFE**     | ✓              | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                |
| **CountryFE**  | ✓              | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                | ✓                |
| **Lags**       | 0              | 0                | 2                | 2                | 0                | 4                | 0                | 0                | 2                | 2                | 0                | 4                |
| **R2**         | 0.07           | 0.27             | 0.83             | 0.88             | 0.80             | 0.84             | 0.07             | 0.30             | 0.68             | 0.75             | 0.65             | 0.71             |
| **N**          | 442            | 442              | 442              | 391              | 442              | 408              | 442              | 442              | 391              | 442              | 408              |

Table reports the cumulative IRF two and three years after the initial year of the policy shock. Column 3 is identical to specification in Table 1. Cluster robust standard errors in parenthesis. Lags refer to number of lags on dependent variable, $X_i$, controls, and policy shocks. Baseline controls include demeaned values of: log change in real consumption per capita, log change in real investment per GDP, institutional investor rating, and the change in the short term interest rate. Additional controls include: log change in cpi, change in current account to gdp, change in long real house prices, and the change in annual real per capita loans.

$^†p < 0.15$.

$^{*}p < 0.10$.

$^{**}p < 0.05$.

$^{***}p < 0.01$.

Fair and Domínguez (1991) variables alone, including country and year fixed effects. Table 13 includes the full specification of controls in the paper. There is some correlation between working age population and total consolidations, mostly coming through tax shocks, and between the polynomial controls and spending shocks. It is comforting that these are the specifications where I find little output effect of the shocks in the model and my primary results, interactions of D1-D3 with taxes and WAP with spending, have little predictive power for the policy variable. Once year fixed effects are included these correlations are no longer significant. They remain broadly statistically insignificant when the control set from the baseline specification (excluding interactions) is used, with only some very weak relationship ($p < 0.15$) for the WAP variable on tax shocks.

These results are reassuring as they imply that population aging is not simply predicting the path of future policy, but rather that the KBO interactions are pointing toward some difference in the actual transmission of policy under different demographic structures. I discuss some suggestive evidence for what these channels may be in Section 4.

Appendix F. Variation in population age shares

Fig. 13 presents the remaining countries who’s five-year population age shares were not reported in the body of the paper. See Fig. 2 for the United States, Ireland, and Japan. As discussed, there are both broad trends apparent across countries, with respect to long run aging, as well as a large degree of variation in the relative sizes of age groups over time.
Table 12
Predictability of narrative shocks: Demographics.

| Variable   | Total  | Total  | Total  | Total  | Tax   | Tax   | Tax   | Tax   | Spend | Spend | Spend | Spend |
|------------|--------|--------|--------|--------|-------|-------|-------|-------|-------|-------|-------|-------|
| WAP        | −0.07* | −0.06† | −0.04* | −0.03  | −0.03 | −0.03 |       |       |       |       |       |       |
|            | (0.04) | (0.04) | (0.02) | (0.02) | (0.03)| (0.03)|       |       |       |       |       |       |
| D1         | 0.03   | 0.02   | −0.02  | −0.02  |       |       |       |       |       |       |       |       |
|            | (0.04) | (0.05) | (0.02) | (0.02) |       |       |       |       |       |       |       |       |
| D2         | −0.08  | −0.09  | 0.00   | 0.01   | −0.08*| −0.10 |       |       |       |       |       |       |
|            | (0.05) | (0.10) | (0.02) | (0.03) | (0.04) | (0.09) |       |       |       |       |       |       |
| D3         | 0.05*  | 0.05   | 0.01   | 0.00   | 0.04* | 0.05  |       |       |       |       |       |       |
|            | (0.02) | (0.05) | (0.01) | (0.01) | (0.02) | (0.04) |       |       |       |       |       |       |
| CountryFe  | ✓      | ✓      | ✓      | ✓      | ✓     | ✓     | ✓     | ✓     | ✓     | ✓     | ✓     | ✓     |
| YearFe     | ✓      | ✓      | ✓      | ✓      | ✓     | ✓     |       |       |       |       |       |       |
| R2         | 0.02   | 0.16   | 0.04   | 0.18   | 0.10  | 0.12  | 0.04  | 0.01  | 0.15  | 0.03  | 0.15  | 0.17  |
| N          | 442    | 442    | 442    | 442    | 442   | 442   | 442   | 442   | 442   | 442   | 442   | 442   |

† \( p < 0.15 \), * \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \).

Table 13
Predictability of narrative shocks: Full control set.

| Variable   | Total  | Total  | Tax   | Tax   | Spend | Spend |
|------------|--------|--------|-------|-------|-------|-------|
| WAP        | −0.19  |       | −0.28*|       | −0.04 |       |
|            | (0.28) |       | (0.16)|       | (0.15)|       |
| D1         | −0.43  |       | −0.35 |       | −0.15 |       |
|            | (0.62) |       | (0.43)|       | (0.31)|       |
| D2         | 0.67   |       | 0.46  |       | 0.32  |       |
|            | (1.09) |       | (0.78)|       | (0.51)|       |
| D3         | −0.31  |       | −0.18 |       | −0.18 |       |
|            | (0.50) |       | (0.37)|       | (0.22)|       |
| L.Total     | 0.28***|       | 0.28***|      |       |       |
|            | (0.08) |       | (0.08)|      |       |       |
| L2.Total    | 0.11   |       | 0.10  |       |       |       |
|            | (0.10) |       | (0.09)|      |       |       |
| L.Tax       | 0.02   |       | 0.02  |       |       |       |
|            | (0.07) |       | (0.06)|      |       |       |
| L2.Tax      | 0.07   |       | 0.05  |       |       |       |
|            | (0.13) |       | (0.12)|      |       |       |
| L.Spend     | 0.39***|       | 0.38***|      |       |       |
|            | (0.10) |       | (0.10)|      |       |       |
| L2.Spend    | 0.11†  |       | 0.10  |       |       |       |
|            | (0.07) |       | (0.07)|      |       |       |
| Deficit-to-GDP | −3.30  | −3.44†| −2.58***| −2.39**| −1.48| −1.74|
|            | (2.21) | (1.97)| (1.10) | (1.08) | (1.57)| (1.42) |
| log change cpi | 0.05* | 0.05* | −0.01 | −0.01 | 0.06***| 0.06***|
|            | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) | (0.02) |
| log change cons | −0.02 | −0.02 | −0.01 | −0.01 | −0.02† | −0.02†|
|            | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) | (0.02) |
| log change I/Y | −0.01 | −0.01 | −0.01*| −0.01**| −0.00 | −0.00 |
|            | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.01) |
| change short rate | −0.00 | 0.00 | 0.02* | 0.02 | −0.02 | −0.02 |
|            | (0.03) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| L(1/2).X    | ✓      | ✓      | ✓      | ✓      | ✓     | ✓     |

Control set same as baseline in paper including two lags of the shock variable as well as contemporaneous values and two lags of all controls. Year and country fixed effects included.

† \( p < 0.15 \), * \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \).
Fig. 13. Changes in Population Age Structure. (Each bar/line represents the deviation of the five-year age share within a country from the sample’s 1978–2006 average.)

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