Housing Market Responses to Transaction Taxes: Evidence From Notches and Stimulus in the UK*

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Abstract

Using administrative data on all property transactions in the UK from 2004-2012, we provide evidence on the distortionary effects of property transaction taxes ("stamp duty") on the housing market. Two sources of quasi-experimental variation allow us to obtain compelling non-parametric results: (i) notches created by discontinuous jumps in tax liability at threshold property prices, (ii) time variation created by permanent reforms and temporary stimulus in specific price brackets. We present two broad findings. First, transaction taxes are highly distortionary across a range of margins, causing large distortions to the price, volume and timing of property transactions. Second, temporary transaction tax cuts are an enormously effective form of fiscal stimulus. A temporary elimination of a 1% transaction tax increased housing market activity by 20% in the short run (due to both timing and extensive responses) and less than half of the stimulus effect was reversed after the tax was reintroduced (due to re-timing). Due to the complementarities between moving house and consumer spending, these stimulus effects translate into GDP effects of about 1 dollar per dollar of foregone revenue. We interpret our empirical findings in the context of a housing model with down-payment constraints in which leverage amplifies the effects of transaction taxes.

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1 Introduction

Transaction taxes on assets are widely discussed by economists and policy makers, but remain understudied (Campbell & Froot 1994; Poterba 2002; Matheson 2011; European Commission 2013). This debate has been particularly energetic in recent years as a result of the enormous turmoil in the financial and housing markets and their potential importance for the boom-bust cycle of the economy as a whole. In this paper we focus on the taxation of housing transactions—a policy that is ubiquitous throughout the world, and raises substantial amounts of revenue in many countries.¹ We analyze the UK property transaction tax, known as the Stamp Duty Land Tax (SDLT), which is substantial both in terms of revenue and the distortions it creates. Our analysis is based on first-time access to administrative stamp duty records covering the universe of property transactions combined with policy-induced quasi-experimental variation that allows us to obtain compelling and striking evidence on housing market responses to transaction taxes.

Our analysis delivers two broad findings. First, the transaction tax is highly distortionary across a range of margins, causing large distortions to the price, volume and timing of property transactions. This finding raises significant questions about the suitability of a property transaction tax as a long-run policy instrument. Second, temporarily eliminating the transaction tax as a stimulus policy during a recession is enormously effective. We find that such stimulus increases housing market activity dramatically, and that consumer expenditures complementary to moving house increase by roughly the amount of the tax cut. This finding is not specific to the elimination of a distortionary tax, but may be relevant more broadly for policies that reduce the cost transacting houses during recession (such as the US homebuyer tax credit introduced by the 2009 Stimulus Bill).

We exploit administrative tax data on the universe of property transactions in the UK from 2004–2012, about 10 million property transactions. Besides the quality of the data, two sources of quasi-experimental variation allow us to obtain compelling evidence on housing market responses to transaction taxes. First, the UK stamp duty features large discontinuities in tax liability—notches—at cutoff property prices. For example, the tax rate jumps from 1% to 3% of the entire transaction price at a cutoff of £250,000 (about \$400,000), creating an increase in tax liability of £5,000 (about \$8,000) as the house price crosses this cutoff. Such notches create strong incentives for reducing house prices in a region above the cutoff to a point just below the cutoff, thereby creating a hole in the price distribution on the high-tax side and excess bunching in the price distribution of house price to the notch. This allows for non-parametric identification of house price

¹As of 2012, 38 states in the US had a property transaction tax Lincoln Institute of Land Policy (2014). In 2010 in the OECD, Austria, Australia, Belgium, Canada, Chile, the Czech Republic, Denmark, Estonia, Finland, Germany, Greece, Hungary, Ireland, Iceland, Israel, Italy, Japan, the Netherlands, Norway, Poland, Portugal, South Korea, Spain, Sweden, Turkey, the UK, and the US imposed transaction taxes on property (Andrews *et al.* 2011). Beyond the OECD, Hong Kong, India, Pakistan, and Singapore also impose property transaction taxes.

responses to transaction taxes using a bunching approach (Saez 2010; Chetty *et al.* 2011; Kleven & Waseem 2013). Second, the UK stamp duty features subtantial time variation, including both permanent and temporary tax changes that affect specific price brackets but not others. For example, a stamp duty holiday lasting 16 months eliminated transaction taxes in a certain price range in order to provide stimulus to the housing market during the current recession. As we show, this provides an ideal setting for a difference-in-differences approach to evaluating both extensive responses (whether or not to buy a house) and timing responses (when to buy a house) to temporary stimulus.

To facilitate interpretation of the empirical results, we develop a theoretical model of the housing market with transaction taxes. A central aspect of housing transaction taxes is that they require a large cash payment upfront that cannot be financed by debt, which may be particularly harmful in a market where agents are liquidity constrained and highly leveraged (e.g., Mian & Sufi 2011, 2014). Motivated by this observation, we extend the classic housing model with down-payment constraints by Stein (1995) to study the effects of transaction taxes. In this model transaction taxes act as an extra down-payment requirement and exacerbate liquidity constraints, contributing to larger responses other things equal. We characterize the intensive and extensive margin responses to (notched) transaction taxes within this model and show how those responses interact with leverage.

Our empirical findings can be divided into four main categories. First, there is large and sharp bunching just below notch points combined with large holes above notch points in the distribution of house prices. Our bunching estimates imply that house prices respond by a factor of 2–5 times the size of the tax increase at the notch, with larger effects at the bottom than at the top of the price distribution. As shown by our conceptual framework, such bunching depends not only on the structural demand elasticity, but also on the amount of leverage among down-payment constrained buyers. For example, a down-payment constrained buyer with a loan-to-value ratio of 75% (the modal value in the UK) will respond by a factor of 4 times the tax increase.²

Second, we consider the dynamics of house price responses using both anticipated and unanticipated changes in the location of notches. The dynamic adjustment of bunching and holes to changes in notches is very fast, with a new steady state emerging in about 3–4 months for unanticipated changes and almost immediately for anticipated changes. The remarkable sharpness of our dynamic findings suggests that agents in the housing market are less affected by optimization frictions (inattention, inertia, etc.) than for example agents in the labor market (Chetty *et al.* 2011; Chetty 2012; Gelber *et al.* 2013; Kleven & Waseem 2013).

Third, we find strong evidence of short-term timing responses to pre-announced tax changes that create *time notches* at cutoff dates. In the two weeks leading up to an anticipated tax increase, activity levels in the housing market increased by around 150%. Our sharp, non-parametric evi-

²In addition, in a world with matching frictions and price bargaining, house price responses to transaction taxes may also be affected by search and bargaining effects (see e.g., Besley *et al.* 2014; Kopczuk & Munroe 2014). There may also be an element of tax evasion and avoidance as we discuss below. Hence, estimating house price responses to taxes is conceptually related to the large literature estimating taxable income responses (see Saez *et al.* 2012), which combines real labor supply, wage bargaining, evasion/avoidance, etc.

dence on timing responses in the housing market contributes to previous findings that short-term timing responses may far exceed medium- or long-run responses (Auerbach 1988; Burman & Randolph 1994; Goolsbee 2000; Einav *et al.* 2013).

Fourth, we estimate medium-term timing and extensive margin responses using temporary and permanent tax reforms.³ Temporary housing stimulus successfully boosts activity in the short run as transaction volumes in the treatment group clearly diverge from transaction volumes in a control group during the 16-month stamp duty holiday. A 1%-point cut in transaction taxes increases market activity by about 20% during the holiday. This effect combines a *timing effect* (intertemporal substitution by those who would have purchased a house anyway) and an *extensive margin effect* (house purchases that would not have taken place absent the tax holiday). We can separate the two effects by comparing treatments and controls following the removal of the stimulus policy. Consistent with a timing effect, activity levels in the treatment group drop by about 8% compared to the control group in the first year after the holiday, with no further reversal in the second year after the holiday. The total reversal effect due to re-timing is less than half of the total stimulus boost, in contrast to Mian & Sufi (2012) who find complete reversal within one year of a US stimulus program in the car market.

Our results have implications for the design of fiscal stimulus. Even though higher transaction levels in the housing market (for a given aggregate housing stock) do not add mechanically to real economic activity, house purchases have important real effects. Besides the implications of homeowner mobility for housing and labor markets, moving house is associated with substantial household spending on repairs, renovations, durable goods (domestic appliances, consumer electronics, furnishing, etc.), and commissions to agents and lawyers. Using UK consumption survey data, we estimate conservatively that a house transaction triggers extra spending of about 5% of the house price. Combined with our estimated increase in transaction volume (20%) and the size of the tax cut (1% of the house price), this implies that the amount of extra economic activity per dollar of tax cut is about 1. This captures only the immediate stimulus effect of larger spending; it does not include potential Keynesian multiplier effects or indirect effects of mobility. Compared to a large body of evidence on consumer responses to other forms of fiscal stimulus such as tax rebates (e.g. Shapiro & Slemrod 2003a,b; Johnson et al. 2006; Agarwal et al. 2007; Kreiner et al. 2012), our findings suggest that the spending impact of the UK housing stimulus program has been considerably larger. The large effect is due to the strong responsiveness of house purchases to transaction taxes along with the complementarities between moving house and consumer spending.

Why are responses to housing transaction taxes so large and long-lasting compared to standard recurrent taxes? A natural explanation and the one highlighted by our model turns on the role of down-payment constraints and leverage. While such a theory is consistent with the evidence presented here, we do not causally identify the effect of leverage on tax responsiveness, which would require linked tax-mortgage data as well as exogenous sources of variation in leverage.

³These are extensive responses for house *purchases* as opposed to house *ownership*. Hence, our estimates of extensive responses do not just capture movements between renting and owning, but also that existing homeowners make additional house purchases (and therefore move more) over their lifetime.

This is an interesting avenue for future research.

We contribute primarily to three literatures. First, a small body of recent and contemporaneous work studies the effects of property transaction taxes on house prices and homeowner mobility (van Ommeren & van Leuvensteijn 2005; Dachis *et al.* 2012; Besley *et al.* 2014; Kopczuk & Munroe 2014; Slemrod *et al.* 2014).⁴ In particular, the contemporaneous papers by Kopczuk & Munroe (2014) and Slemrod *et al.* (2014) study house price responses using bunching at US tax notches. Unlike these papers, we analyze the dynamics of house price responses, extensive margin responses, timing and stimulus, and so the only real overlap is in the static bunching analysis.⁵ Also contemporaneous with this paper, Besley *et al.* (2014) study the UK stamp duty holiday. While they focus on parametrically estimating price incidence in a matching frictions model, in this paper we take a non-parametric approach and focus on a wider set of behavioral margins as described above. None of the earlier literature considers the role of down-payment constraints for the effect of housing transaction taxes.

Second, a larger empirical literature has examined the impact of capital gains taxes on asset prices and asset transactions (e.g. Feldstein *et al.* 1980; Auerbach 1988; Burman & Randolph 1994) and some of this work has focused specifically on the taxation of housing capital gains (Cunningham & Engelhardt 2008; Shan 2011). Transaction taxes and capital gains taxes share the feature that tax liability is triggered by a transaction, with the key difference being that transaction taxes fall on the entire value of the asset and not just on the appreciation of the asset. This difference in tax base is particularly important in the presence of down-payment constraints and high leverage, making behavioral responses to transaction taxes an order of magnitude larger than responses to capital gains taxes, other things equal.

Finally, our paper is related to recent work in macroeconomics using micro data, including work on fiscal stimulus (e.g. Johnson *et al.* 2006; Agarwal *et al.* 2007; Mian & Sufi 2012) and on the housing market during the Great Recession (e.g. Mian & Sufi 2009; 2010; 2011). Our paper provides an example of how administrative data and quasi-experimental approaches from public economics can be used to address questions of interest to macroeconomics.

The paper proceeds as follows. Section 2 presents our theoretical model, section 3 describes the context and data, section 4 estimates house price responses using notches, section 5 estimates timing and extensive responses using stimulus and permanent reforms, and section 6 concludes.

⁴Besides these empirical papers, theoretical work by Lundborg & Skedinger (1999) has analyzed the implications of housing transaction taxes.

⁵The main point of Kopczuk & Munroe (2014) is that notches may cause markets to locally unravel as productive matches fail to occur at prices just above notch points. Motivated by the conceptual difficulties of precisely estimating missing mass above notch points (see also Kleven & Waseem 2013), we do not rely on missing mass above notches for any of our key empirical results (relying instead on bunching and difference-in-differences strategies that are robust to the size of missing mass).

2 Conceptual Framework

This section develops a simple model to analyze housing market responses to transaction taxes. We extend the classic housing model with down-payment constraints by Stein (1995) to incorporate transaction taxes. Accounting for the down-payment aspect of housing transactions is important, because transaction taxes represent a large upfront payment that cannot be financed by debt and therefore reinforce down-payment requirements. We start by analyzing the impact of a linear transaction tax at the intensive margin (which house to buy) and at the extensive margin (whether to buy a house), and then analyze the impact of a notched transaction tax. The framework is deliberately unrealistic in some dimensions as our goal is to build the most parsinomious model possible that is still general enough to demonstrate the key effects in our empirical application.⁶

2.1 A Housing Model with Down-Payment Constraints and Transaction Taxes

Households are endowed with 1 unit of housing and an outstanding debt of k (which we allow to be heterogeneous in the population) denoted in units of a numeraire consumption good. Individuals can trade houses at a price of p per unit of quality-adjusted housing h. If they move house, households sell their existing house and repay their debt, giving them liquid assets of p - k. To buy a house of value $h_v \equiv ph$, they must make a down-payment of at least γh_v financed from their liquid assets, and they can borrow the remainder at an interest rate normalized to 0. They must also pay a transaction tax financed from their liquid assets. As a baseline case, we first consider a linear transaction tax at rate t. These financing requirements imply that a household can buy a house as long as its value satisfies

$$h_{v} \leq \frac{p-k}{\gamma+t} \equiv \hat{h}_{v}\left(k,\gamma,t\right). \tag{1}$$

Households choose whether or not to move (extensive margin) and how much housing to buy conditional on moving (intensive margin). After making housing choices, they receive income 1 + k, repay all debts, and consume their remaining income. The fact that income equals 1 + k is just a convenient normalization. Letting *c* denote units of a numeraire consumption good, we consider the following parametrization of preferences

$$u(c,h) = c + \frac{A}{1+1/\varepsilon} \left(\frac{h}{A}\right)^{1+1/\varepsilon},$$
(2)

where A > 0, $\varepsilon < 0$ are parameters characterizing housing preferences. The quasi-linear utility function conveniently eliminates income effects on housing demand as we will focus purely on

⁶In this spirit, we analyze a competitive housing market, abstracting from the search frictions that some authors have argued are significant in the housing market (see, for example, Wheaton 1990; Krainer 2001; Piazzesi & Schneider 2009; Ngai & Tenreyro 2014), though we extend a version of this model without down-payment constraints to include matching frictions and price bargaining in Best & Kleven (2014) and demonstrate that the key empirical effects are also present in such a setting.

the price effect.⁷

Households thus maximise utility (2) subject to the down-payment constraint (1) and the budget constraint

$$c = 1 + M \cdot p \left[1 - (1+t) h \right], \tag{3}$$

where *M* is an indicator for moving. We allow for heterogeneity in all parameters captured by a smooth density distribution $f(A, \varepsilon, \gamma, k)$. Conditional on moving (*M* = 1), household optimization yields the following housing demand function

$$h^* = \min\left\{A\left(\left(1+t\right)p\right)^{\varepsilon}, \hat{h}_v\left(k,\gamma,t\right)/p\right\},\tag{4}$$

where ε is the unconstrained price elasticity of housing demand. We call a household downpayment constrained whenever (1) binds so that $h^* = \hat{h}_v (k, \gamma, t) / p$. This happens whenever they have insufficient liquid assets to afford their preferred house, i.e. when

$$k > p \left[1 - A \left[(1+t) \, p \right]^{\varepsilon} \left(\gamma + t \right) \right] \equiv \hat{k},\tag{5}$$

so that the share of individuals who are down-payment constrained is given by

$$C(p,\gamma,t) = \int_{A} \int_{\varepsilon} \int_{\gamma} \int_{\hat{k}}^{\infty} f(A,\varepsilon,\gamma,k) \, dk d\gamma d\varepsilon dA.$$
(6)

Households will choose to move as long as they can afford a house that gives them higher utility than their current house. This will be the case whenever debt $k \leq \tilde{k}$ where \tilde{k} satisfies

$$u\left(1+p-(1+t)\,\hat{h}_{v}\left(\tilde{k},\gamma,t\right),\hat{h}_{v}\left(\tilde{k},\gamma,t\right)/p\right) = u\left(1,1\right),\tag{7}$$

where we always have $\tilde{k} \geq \hat{k}$.

The model thus features three sorts of households depending on their initial debt levels. First, there are *unconstrained movers* whose initial debt level is small enough that they are not down-payment constrained ($k \le \hat{k}$). These households move to their preferred house. Second, there are *constrained movers* whose initial debt level makes them down-payment constrained, but who still find it worthwhile to move ($\hat{k} < k \le \tilde{k}$). These households move to the best house they can afford given their liquidity. Finally, *non-movers* have such high initial debt levels that the house they can afford to move to is worse than staying in their current house ($\tilde{k} < k$). Total housing demand is then given by

$$D\left(\left(1+t\right)p\right) = \int_{A} \int_{\varepsilon} \int_{\gamma} \int_{0}^{k} h^{*} f\left(A,\varepsilon,\gamma,k\right) dk d\gamma d\varepsilon dA.$$
(8)

⁷While we do not make use of this assumption in our empirical analysis, the presence of income effects implies that our bunching estimates should be interpreted as reflecting a combination of income and substitution effects (Saez 2010; Kleven & Waseem 2013).

2.2 Effects of a Linear Transaction Tax

 ε).

The effects of the transaction tax can be seen by considering the comparative statics of the model in the case of a linear tax. In particular, changes in the tax rate have three effects on the housing market summarised in the following proposition:

Proposition 1 (Effects of a Linear Transaction Tax). *A change in the transaction tax rate t has three effects on the housing market:*

(i) an intensive margin effect on housing demand as households demand less housing conditional on moving: $dh^*/d(1+t) < 0$;

(ii) a liquidity constraining effect as the share of individuals who are down-payment constrained increases: $dC(p, \gamma, t) / d(1+t) > 0$;

(iii) an extensive margin effect on housing demand as fewer households choose to move: $d\tilde{k}/d(1+t) < 0$.

Proof. (i) follows directly from differentiation of (4). (ii) follows from differentiation of (6) and (5), and (iii) from implicit differentiation of (7). \Box

It is interesting to consider how leverage (as captured by the loan-to-value ratio $1 - \gamma$) amplifies responses to transaction taxes, as characterized in the following proposition:

Proposition 2 (Interaction Between Leverage and Transaction Taxes). Leverage $1 - \gamma$ amplifies both intensive and extensive margin responses among down-payment constrained households. In particular, *(i)* Intensive margin responses are larger the more leveraged households are: $d^2\hat{h}_v/d(1+t) d(1-\gamma) < 0$. *(ii)* Extensive margin responses are larger the more leveraged households are: $d^2\tilde{k}/d(1+t) d(1-\gamma) < 0$.

Proof. To see (i), note that for down-payment constrained buyers, the house value response to a tax increase is $\frac{d\hbar_v}{d(1+t)} = \frac{-\hbar_v}{(1+t)-(1-\gamma)}$. Differentiating this yields $\frac{d^2\hbar_v}{d(1+t)d(1-\gamma)} = \frac{-2\hbar_v}{[(1+t)-(1-\gamma)]^2} < 0$. To see (ii), note that implicitly differentiating (7) yields $\frac{d\tilde{k}}{d(1+t)} = -\hat{h}_v \left[\frac{(1-\gamma)p - (\frac{\hbar}{A})^{1/\epsilon}}{(1+t)p - (\frac{\hbar}{A})^{1/\epsilon}} \right] < 0$ where $\hat{h} \equiv \hat{h}_v/p$. When signing this derivative we use that both the numerator and denominator in the bracketed term are negative for a down-payment constrained household whose housing consumption is below the unconstrained optimum. Differentiating again, we can obtain the following result $\frac{d^2\tilde{k}}{d(1+t)d(1-\gamma)} = \frac{1}{\gamma+t}\frac{d\tilde{k}}{d(1+t)} - \hat{h}_v p \frac{(1+t)p - (1+\frac{1}{\epsilon})(\frac{\hbar}{A})^{1/\epsilon}}{\left[(1+t)p - (\frac{\hbar}{A})^{1/\epsilon}\right]^2}$. The first term of this derivative is always negative, and the second term is also negative except for extreme values of the unconstrained demand elasticity ε (making the second term zero requires ε sufficiently below -1, in which case the full

Proposition 2 implies that small transaction taxes can generate large behavioral responses amongst leveraged households even if the unconstrained demand elasticity ε is modest. For example, the intensive margin elasticity of down-payment constrained households can be written as $-\frac{d\hat{h}_w/\hat{h}_v}{dt/(1+t)} = 1/\left[1 - \frac{1-\gamma}{1+t}\right]$, implying that a household with a loan-to-value ratio of 75% (the modal

derivative is still negative, so making the full derivative positive would require extreme values of

value in the UK in our sample period, see Financial Conduct Authority 2014) would have an elasticity of approximately 4, much larger than what might be reasonable values for the elasticity of an unconstrained household ε . Similarly, extensive margin responses are stronger for more leveraged buyers. For highly leveraged buyers, even small changes in the transaction tax are leveraged into large changes in the house that down-payment constrained movers can afford, and so can have large impacts on participation levels in the housing market.

To highlight the key effects in the simplest possible manner, we have presented a static version of the model. A multi-period generalization is feasible and would allow us to explicitly analyze the effects of the type of stimulus policy that we consider in the empirical application.⁸ In general, in a multi-period model, an unanticipated, temporary reduction in the transaction tax will have two conceptual effects on transaction volumes. First, the tax cut will have a *timing effect* as households that were close to indifferent between buying in the current period and buying in some future period are induced to move their house transaction forward to the current period. Second, the tax cut will have *extensive margin* effects amongst two sets of households. Those who were sufficiently close to indifference between buying in the current period and never buying are induced to buy in the period of the tax cut, i.e. movements between renting and owning. Moreover, those who were sufficiently close to indifference between buying in the current period as well as in some future period(s) and buying only in future period(s) are also induced to buy in the period of the tax cut, i.e. additional moves among homeowners. While the timing effect will be fully reversed by lower activity in future periods, the extensive margin effect will not be reversed in future periods and therefore represent a lasting effect of a temporary tax cut.

2.3 Effects of a Notched Transaction Tax

Now consider the introduction of a discrete jump Δt in the proportional transaction tax rate—a notch—at a cutoff property value. The notched tax schedule can be written as $T(h_v) = t \cdot h_v + \Delta t \cdot h_v \cdot I\{h_v > \overline{h}_v\}$ where \overline{h}_v is the cutoff and $I\{\cdot\}$ is an indicator for being above the cutoff. Figure 1 illustrates the implications of this notch in budget set diagrams for different levels of initial debt k (panels A–C) and a density distribution diagram (panel D). Each budget set diagram (depicted in (h_v, c) -space) illustrates intensive responses among individuals with heterogeneous housing preferences A, but a specific demand elasticity ε and specific values of k and γ . The notch creates bunching at the cutoff \overline{h}_v by all individuals in a preference range $(\overline{A}, \overline{A} + \Delta \overline{A})$, who would have bought houses on the segment $(\overline{h}_v, \overline{h}_v + \Delta \overline{h}_v)$ in the absence of the notch. The marginal bunching individual at $\overline{A} + \Delta \overline{A}$ is indifferent between the notch point \overline{h}_v and the best interior location \overline{h}_v^I . No individual is willing to locate between \overline{h}_v and \overline{h}_v^I , and hence this range is completely empty.⁹

⁸Best & Kleven (2014) present a dynamic extension in a setting without down-payment effects.

⁹Notice that the above characterization is based on a given price p per unit of housing. The tax-induced change in aggregate housing demand (from bunching as well as interior responses further up) will affect the equilibrium price, which by itself will shift indifference curves in Panel A (as they are depicted in (h_v, c) -space) and hence shift the density distribution of property values. The qualitative characterization above holds for any arbitrary price and therefore also for the new equilibrium price. The key insight is that, in this competitive model, price incidence occurs at the *market level* and therefore does not contribute to bunching and holes locally around notches. Appendix A.1 of Best & Kleven

The initial debt level affects whether or not the marginal bunching household is down-payment constrained in the counterfactual location $\bar{h}_v + \Delta \bar{h}_v$ without the notch, at the best interior location \bar{h}_v^I with the notch, or in both locations. This affects whether we interpret bunching at the notch as identifying the unconstrained demand elasticity ε or a combination of ε and the effect of down-payment constraints, in which case bunching only identifies bounds on ε (appendix A.1 analyzes this in detail). Panel A depicts households with low levels of k.¹⁰ The marginal bunching household in this scenario is never down-payment constrained, and so bunching can be used to identify the structural parameter ε . Panel B depicts households with an intermediate level of k.¹¹ Here the marginal bunching household is down-payment constrained at the best interior location \bar{h}_v^I in the presence of the notch, but not in the counterfactual scenario at $\bar{h}_v + \Delta \bar{h}_v$. As shown in appendix A.1, in this case bunching can be used to identify bounds on the structural parameter ε . Finally, when initial debt levels are high,¹² the marginal bunching household is down-payment constrained is down-payment constrained both at its best interior location \bar{h}_v^I and at the counterfactual location $\bar{h}_v + \Delta \bar{h}_v$ as depicted in panel C. In this case, bunching only identifies an upper bound on the parameter ε .

In addition to intensive responses, the notch creates extensive responses above the cutoff by households close to being indifferent between buying and not buying (those with $k \in (\tilde{k} - \Delta \tilde{k}, \tilde{k})$). However, such extensive responses will be negligible *just* above the cutoff, i.e. $\Delta \tilde{k} = 0$ when $h_v \approx \bar{h}_v$. This can be seen by considering a household that prefers a location on the segment $(\bar{h}_v, \bar{h}_v + \Delta \bar{h}_v)$ without the notch and therefore prefers the cutoff \bar{h}_v with the notch (conditional on moving). If such a household is initially close to the indifference point \tilde{k} , the introduction of the notch reduces the utility from moving by $\Delta u = u(\hat{c}, \hat{h}_v(\tilde{k}, \gamma, t) / p) - u(\bar{c}, \bar{h}_v / p)$ where $\bar{c}, \bar{h}_v / p$ is the consumption bundle obtained at the notch. As the preferred point absent the notch \hat{h}_v converges to the cutoff \bar{h}_v from above (and hence \hat{c} converges to \bar{c}), Δu converges to zero and hence the extension margin decision in unaffected. Intuitively, if in the absence of the notch a household would choose to buy a house slightly above \bar{h}_v , then in the presence of the notch, they will be better off buying a house at \bar{h}_v (which is almost as good) rather than not buying at all. Combining all the above reasoning implies that intensive and extensive responses affect the density distribution as illustrated in panel D in which we allow for heterogeneity in all the parameters $A, \varepsilon, \gamma, k$. These effects can be summarized in the following proposition

Proposition 3 (Effects of Transaction Tax Notches). A transaction tax featuring a notch at a property value \bar{h}_v at which the proportional tax rate jumps from t to $t + \Delta t$ induces

¹²that is, whenever $p\left[1 - Ap^{\varepsilon} (1+t)^{\varepsilon} (\gamma+t)\right] \leq k$.

⁽²⁰¹⁴⁾ considers a bargaining model where price incidence occurs at the *match level* in which case price incidence does create bunching and holes.

¹⁰More precisely, for these households we have k .

¹¹Specifically, for these households we have $p\left[1 - Ap^{\varepsilon}\left(1 + t + \Delta t\right)^{\varepsilon}\left(\gamma + t + \Delta t\right)\right] \leq k < p\left[1 - Ap^{\varepsilon}\left(1 + t\right)^{\varepsilon}\left(\gamma + t\right)\right]$. As long as $1 > \left(\frac{1+t}{1+t+\Delta t}\right)^{\varepsilon} \frac{\gamma+t}{\gamma+t+\Delta t}$, the marginal bunching household is down-payment constrained in the interior of the higher tax bracket whenever they are down-payment constrained in the counterfactual world absent the notch $(\varepsilon > -1$ is sufficient for this, and is consistent with all our empirical estimates) and so the above are the relevant bounds on k. If the inequality is violated, then the bounds are reversed and for intermediate levels of k the marginal buncher is down-payment constrained in the counterfactual but not in the interior of the higher tax bracket.

(i) an intensive margin response as agents in a house price range $(\overline{h}_v, \overline{h}_v + \Delta \overline{h}_v)$ bunch at the threshold \overline{h}_v , where the width of the bunching segment $\Delta \overline{h}_v$ depends on both the unconstrained demand elasticity ε and on down-payment constraints as determined by k, γ ; and

(ii) an extensive margin response as agents in the house price range (\bar{h}_v, ∞) who are sufficiently close to indifference between buying and not buying, $k \in (\tilde{k} - \Delta \tilde{k}, \tilde{k})$, no longer buy. The extensive response converges to zero just above the cutoff as $\Delta \tilde{k} \to 0$ for $h_v \to \bar{h}_v^+$.

These effects imply that at the cutoff at \bar{h}_v the density of house values that we observe will feature an excess mass of

$$B\left(\bar{h}_{v}\right) = \int_{\bar{h}_{v}}^{h_{v}+\Delta h_{v}} g_{0}\left(h_{v}\right) dh_{v} \approx g_{0}\left(\bar{h}_{v}\right) \Delta \bar{h}_{v},\tag{9}$$

where $B(\bar{h}_v)$ is excess mass at the cutoff and $g_0(h_v)$ is the counterfactual density of house values (i.e. the density that would prevail absent the notch). The approximation is accurate to the extent that the counterfactual is approximately uniform around the notch. Based on equation (9), it is possible to recover the house price response $\Delta \bar{h}_v$ based on estimates of the counterfactual distribution $g_0(h_v)$ and bunching $B(\bar{h}_v)$.

The relationship (9) implicitly assumes that there is just one bunching segment $(\bar{h}_v, \bar{h}_v + \Delta \bar{h}_v)$, which amounts to assuming that ε, γ and k are homogeneous in the population. Our conceptual framework allows for heterogeneity and we can also account for it in the empirical implementation. There will be a price response $\Delta \bar{h}_v(\varepsilon, \gamma, k)$ and a counterfactual density $\tilde{g}_0(h_v, \varepsilon, \gamma, k)$ associated with each type { ε, γ, k }. In this case, equation (9) can be generalized to

$$B(\bar{h}_v) = \int_{\varepsilon} \int_{\gamma} \int_k \int_{\bar{h}_v}^{\bar{h}_v + \Delta \bar{h}_v(\varepsilon)} \tilde{g}_0(h_v, \varepsilon, \gamma, k) \, dh_v dk d\gamma d\varepsilon \approx g_0(\bar{h}_v) \, E\left[\Delta \bar{h}_v\right], \tag{10}$$

where $E [\Delta \bar{h}_v]$ is the average price response across all ε , γ , k. As before, the approximation requires that the counterfactual density is locally uniform in house prices h_v (but not type ε , γ , k) around the notch point. Equation (10) shows that estimates of the counterfactual distribution and bunching allows us to recover the average house price response in the population.

3 Context and Data

3.1 The UK Property Transaction Tax: Notches and Reforms

The UK property transaction tax—Stamp Duty Land Tax (SDLT)—is imposed on the transaction value of land and any construction on the land, known as the "chargeable consideration".¹³ This is defined in the broadest possible terms to include anything of economic value given in exchange for land or property, including money, goods, works or services, and transfers of debts. The statutory

¹³The chargeable consideration includes the buildings and structures on the land as well as fixtures and fittings (such as in bathrooms and kitchens), but excludes freestanding furniture, carpets or curtains. If such extras are included in the sale, the buyer and seller are to agree on the market value of these extras and subtract it from the chargeable consideration. See http://www.hmrc.gov.uk/sdlt/calculate/value.htm for details.

incidence of the SDLT falls on the buyer, who is required to file a stamp duty return and remit tax liability to HMRC within a few weeks of the completed transaction. The SDLT is a significant source of government revenue in the UK, much more so than other wealth transfer taxes such as inheritance taxation and capital gains taxation. The SDLT has raised revenue of around 0.6% of GDP over recent years,¹⁴ and the political debate in the UK suggests that future rates (on highly priced properties) are more likely to go up than down.

A central aspect of the stamp duty is that it features discrete jumps in tax liability—notches—at threshold property prices. Tax liability is calculated as a proportional tax rate times the transacted property price, with different tax rates in different price brackets. Hence, as the purchase price crosses a bracket threshold, a higher tax rate applies to the entire amount and not just the portion that falls above the cutoff as in standard graduated schedules. Figure 2 illustrates the stamp duty schedule for residential property in tax year 2012–13.¹⁵ The schedule features five notches as the proportional tax rate jumps from zero to 1% at a price of £125,000, from 1% to 3% at a price of £250,000, from 3% to 4% at a price of £500,000, from 4% to 5% at a price of £1,000,000, and finally from 5% to 7% at a price of £2,000,000.¹⁶ The schedule is different for residential property in certain disadvantaged areas (where the first bracket threshold is at a higher price) as well as for non-residential property. It is important to note that the buyer cannot mortgage the SDLT liability, it must be financed from savings, and so we should expect the SDLT to have particularly large effects on down-payment constrained buyers (as shown by our conceptual framework). It should also be noted that stamp duty schedules are not indexed for inflation, which creates "bracket creep" as property price inflation pushes houses into higher stamp duty brackets.

Another important aspect of the stamp duty is that it has been subject to a great deal of policy experimentation over the years. As shown in Table 1, the main policy experiments during our data period have been (*i*) changes in the location of the lower notch and (*ii*) the introduction of new notches at £1,000,000 in April 2011 and at £2,000,000 in March 2012. It is worth describing the specific features of some of those policy changes as they will be important for the empirical analysis.

For the lower notch, the most salient change was the so-called *stamp duty holiday* between 3 September 2008 and 31 December 2009, which moved the first notch point from £125,000 to £175,000 and thereby eliminated stamp duty in a £50,000 range. The motivation of the program was to provide housing stimulus during the current recession.¹⁷ The following features of the stamp duty holiday are important for our analysis. First, the beginning of the holiday was *unanticipated* as it was announced suddenly by the then Chancellor Alistair Darling on the day before its introduction. Although there was some media speculation about the possibility of a stamp duty

¹⁴See http://www.hmrc.gov.uk/stats/tax_receipts/tax-receipts-and-taxpayers.pdf.

¹⁵The UK tax year for personal taxes runs from April 6 in one year to April 5 the next year.

¹⁶At the £2,000,000 notch, the stamp duty rate jumps to 15% if the residential dwelling is purchased by certain "non-natural persons" such as corporations and collective investment schemes.

¹⁷Another stimulus program was implemented specifically for first-time buyers between 25 March 2010 and 24 March 2012. This program temporarily abolished the notch at £125,000, thereby eliminating stamp duty in the range between £125,000 and £250,000 for first-time buyers.

holiday in the month leading up to the announcement, the details and start date of such a holiday were unknown. Second, the end of this holiday was *anticipated*. The initial announcement was that the holiday would last for one year (until September 2009), but in April 2009 this was extended until the end of 2009 and the government committed to no further extensions (and indeed did not grant any extensions). The sudden announcement of the stamp duty holiday and the preannounced committment to its end date allow us to compare the effects of expected and unexpected changes in tax policy. In particular, the pre-announced end date creates a *time notch* (a discrete jump in tax liability at a cutoff date) allowing us to analyze short-term timing effects. Finally, as the stamp duty holiday applied only to properties in a certain price range, we are able to study the stimulus effects of the policy and subsequent reversal (medium-term timing) using a difference-in-differences approach.

For the top notches, the introduction of a higher stamp duty rate above £1,000,000 was preannounced a full year in advance, while the higher stamp duty rate above £2,000,000 was confirmed just one day before it took effect. Hence, the introduction of the £1,000,000 price notch (but not the £2,000,000 price notch) also creates a time notch that can be used to study anticipatory behavior.

The UK stamp duty appears to be characterized by relatively high compliance. According to HMRC estimates, the so-called *tax gap*—the difference between taxes owed and taxes paid on a timely basis—is 4–5% of true stamp duty tax liability. This is lower than the tax gap estimates for most other taxes in the UK. As described above, the tax base is defined in an very comprehensive manner meaning that the scope for shifting or re-classification of specific features of the property to avoid the tax is limited. The one exception is the exclusion from the tax base of freestanding "extras" such as furniture and curtains. If such extras are part of the sale, the buyer and seller are to agree on the market value of these extras and subtract it from the chargeable consideration, which creates an opportunity to evade stamp duty by overvaluing such items (while undervaluing the rest of the property by the same amount). Most stamp duty evasion is likely to occur through this channel, although the extent of this is limited by the fact that large amounts of tax exempt extras, there must be monetary side payments between the buyer and the seller in order to cover the difference between reported and true house prices. Such side payments are associated with substantial risk and are therefore likely to be very rare.¹⁸ Still, since we cannot

¹⁸Monetary side payments are very risky for two reasons. First, almost all property transactions in the UK are facilitated by licensed real estate agencies, implying that side payments require collusion between a buyer, a seller and a real estate agency (typically with multiple employees). Such evasion collusion involving many agents is difficult to sustain (Kleven *et al.* 2009). Second, the scope for monetary side payments is further reduced by the existence of a considerable lag between agreeing on a house price and completing the contract (this lag is 2-3 months on average in the UK). If the house price reported to tax authorities is lower than the true house price, the buyer must make a side payment to the seller. If the buyer makes the side payment at the time of agreeing on the house price, the seller would be able to renege before completing the contract and it would be difficult for the buyer to recoup the payment. If instead the buyer promises to make the payment at the time of completing the contract, the seller would take his property off the market with no credible commitment from the buyer that he would not renege later when the bargaining position of the seller may be weaker. Hence, such side payments would be associated with substantial risk for either the buyer or the seller or both.

rule out evasion responses, our bunching estimates of price responses should be interpreted as combining real price responses with potential evasion responses analogously to the literature on taxable income responses (Saez *et al.* 2012). However, our main findings—regarding the dynamics of adjustment to tax changes, extensive margin responses, and the stimulus properties of the transaction tax cut—do not rely on the size of bunching and are therefore not affected by the possibility of evasion.

3.2 Data and Raw Time Series Evidence

The empirical analysis is based on administrative data covering the universe of stamp duty (SDLT) returns in the UK from November 2004 to October 2012. Since most property transactions require the filing of an SDLT return (the main filing exemption being for property transactions under \pounds 40,000), our data is close to the universe of property transactions in the UK. The full dataset contains about 10 million transactions. The dataset contains rich tax return information for each transaction, but currently very little information outside the tax return.

The housing market has seen substantial turmoil during the period we consider. Figure 3 shows the monthly number of house transactions (Panel A) and the monthly average property price (Panel B) in all of the UK and in London alone. The figure shows nominal prices (real prices give the same qualitative picture) and normalizes both the price and the number of transactions to one at the start of the period. We make the following observations. First, housing market activity collapses between late 2007 and early 2009 as the the number of transactions falls by around two-thirds. There has been some recent recovery, but activity is still very far from pre-recession levels. Second, property prices also fall between late 2007 and early 2009, but the price drop is less dramatic and the subsequent recovery much stronger. Third, property prices (though not activity) in London have evolved differently than in the rest of the UK during the recession. While UKwide property prices have recovered only partially in the past couple of years, London property prices are almost back on their pre-recession trend. Fourth, the recovery in house prices and activity throughout 2009 coincides with the stamp duty holiday, which has been used as an argument that the policy had the desired effect. We will take a quasi-experimental approach to evaluate how much of the recovery (if any) can indeed be explained by the stamp duty holiday. Finally, average house prices in London feature a sharp spike in early 2011 and a subsequent dip, which constitutes our first piece of evidence of a behavioral response to stamp duty incentives. This spike reflects excess trading of houses above £1,000,000 just before the pre-announced introduction of the £1,000K stamp duty notch on 6 April 2011 and the dip reflects missing trading of such houses just after the introduction of the notch—a short-term timing response to an anticipated tax change.

4 House Price Responses to Transaction Taxes: Notches

4.1 House Price Responses to Static Notches

This section presents static results using price notches during periods when they are stable. We consider residential property transactions that incur a stamp duty land tax liability.¹⁹ Figure 4 considers the two notches located at cutoff prices of £250,000 (Panel A) and £500,000 (Panel B), both of which have remained in place throughout the period of our data. Each panel shows the empirical distribution of house values (blue dots) as a histogram in £5,000 bins and an estimated counterfactual distribution (red line). Following Chetty *et al.* (2011) and Kleven & Waseem (2013), the counterfactual distribution is estimated by fitting a flexible polynomial to the empirical distribution, excluding data in a range around the notch, and allowing for round-number fixed effects to capture rounding in the price data.²⁰ The excluded range is demarcated by vertical dashed lines; the lower bound is set at the point where excess bunching starts and the upper bound is set at the point where the hole ends (where the empirical distribution above the cutoff changes slope from positive to negative).

Before reporting our results, it is worth qualifying the estimated "counterfactual distribution" and what it is supposed to achieve in this context. Two points are worth noting. First, because the estimation uses data above the notch that may be affected by extensive responses, the procedure does not yield the full counterfactual distribution that would prevail if the notch were eliminated. As discussed by Kleven & Waseem (2013), the estimation procedure intends to provide a "partial counterfactual" stripped of intensive responses, but not extensive responses. We may think of the partial counterfactual as the border of the light-gray area in Panel D of Figure 1.²¹ Second, even

$$c_{i} = \sum_{j=0}^{q} \beta_{j} (z_{i})^{j} + \sum_{r \in \mathcal{R}} \eta_{r} I\left\{\frac{\bar{h}_{v} + z_{i}}{r} \in \mathbb{N}\right\} + \sum_{k=\bar{h}_{v}^{-}}^{\bar{h}_{v}^{+}} \gamma_{k} I\left\{i=k\right\} + \mu_{i},$$
(11)

where c_i is the number of transactions in price bin i, z_i is the distance between price bin i and the cutoff \bar{h}_v , and q is the order of the polynomial (q = 5 in Figure 4). The second term in (11) includes fixed effects for prices that are multiples of the round numbers in the set \mathcal{R} , where $\mathcal{R} = \{500, 1000, 5000, 10000, 25000\}$, \mathbb{N} is the set of natural numbers, and $I\{\cdot\}$ is an indicator function. Finally, the third term in (11) excludes a region (h_v^-, h_v^+) around the notch that is distorted by bunching responses to the notch, and μ_i is a residual reflecting misspecification of the density equation. Our estimate of the counterfactual distribution is defined as the predicted bin counts \hat{c}_i from (11) omitting the contribution of the dummies in the excluded range, and excess bunching is estimated as the difference between the observed and counterfactual bin counts in the part of the excluded range that falls below the notch $\hat{B} = \sum_{i=\bar{h}_v^-}^{\bar{h}_v} (c_i - \hat{c}_i)$.

We may also define an estimate of missing mass (the hole) above the notch as $\hat{M} = \sum_{i>\bar{h}_v}^{\bar{h}_v^+} (\hat{c}_i - c_i)$, but this statistic is not used in the estimation of house price responses and house price elasticities (see section 2.1). Standard errors on all estimates are calculated based on a bootstrap procedure as in Chetty *et al.* (2011). As a robustness check we have tried values between 4 and 7 for the order of the polynomial and our results are not significantly altered.

²¹To simplify, our estimation of the counterfactual distribution ignores the marginal shift in the distribution above the hole due to intensive responses in the interior of the upper bracket. It is feasible to account for this shift in the distribution when estimating the counterfactual by using an initial estimate of the house price elasticity (based on ignoring the shift in the upper distribution) to obtain an initial estimate of the distribution shift, re-estimating the counterfactual and the house price elasticity to respect the initial estimate of the distribution shift, and continuing the

¹⁹Results for non-residential property are qualitatively similar, but noisier as we have far fewer observations.

²⁰Grouping transactions into price bins of £100, the regression used to estimate the counterfactual distribution around a notch at price \bar{h}_v is given by

this partial counterfactual distribution may be difficult to estimate precisely in the range above the notch, especially in situations where the hole is large and diffuse (Kleven & Waseem 2013; Kopczuk & Munroe 2014). The estimation of missing mass above the notch may be quite sensitive to parametric assumptions such as the order of the polynomial and the width of the exluded range. For this reason, none of our key results in this paper will rely on being able to estimate the counterfactual distribution and missing mass above the notch. The estimation of house price responses in this section as well as the estimation of extensive and timing responses in the next section make use only of our bunching estimates. And these bunching estimates are extremely robust due to the fact that observed bunching is very sharp and that the empirical distribution is very flat and smooth below the bunch. Our results are essentially unaffected by the degree of the polynomial, the excluded region, the binwidth, and whether we estimate bunching using only data below the notch. Hence, while we show estimates of the counterfactual distribution and missing mass in the following figures (as they may be of independent interest), it is important to keep in mind that these estimates are not strictly necessary for what we do.

In Figure 4, each panel shows estimates of excess bunching below the notch scaled by the counterfactual frequency at the notch (b), the size of the hole (missing mass) above the notch scaled by the counterfactual frequency at the notch (m), the difference between these two (m - b), the average house price response to the notch (Δh_v) , and the tax liability change at the notch (ΔTax) . Our main findings are the following. First, both notches create large and sharp bunching below the cutoff. Excess bunching is 1.85 and 1.64 times the height of the counterfactual distribution at £250,000 and £500,000, respectively, and is strongly significant in each case. Second, both notches are associated with a large hole in the distribution above the cutoff. The size of the hole is larger than the size of excess bunching, although the difference between the two is not statistically significant from zero. Third, the hole in the distribution spans a £25,000 range above each cutoff, implying that the most responsive agents reduce their transacted house value by five times as much as the jump in tax liability of £5,000. Fourth, the average house price response is £10,000 at both the £250,000 notch and the £500,000 notch, a response that is twice as large as the tax jump. Interpreting the third and fourth results in the context of our model with down-payment constraints in section 2, a response of £25,000 is consistent with the most responsive households being down-payment constrained and having a loan-to-value ratio of 80%. In the UK, many households have an LTV of 80% as this level is associated with a large notch in mortgage interest rates. Similarly the average price response suggests an average LTV ratio amongst bunchers of 50%, consistent with a mixture of down-payment constrained, highly leveraged buyers, and unconstrained buyers.

We now turn to the lower notch, the location of which has changed several times during the period under consideration. The cutoff was located at £60,000 until 16 March 2005, at £120,000 between 17 March 2005 and 22 March 2006, at £125,000 between 23 March 2006 and 2 September 2008, at £175,000 between 3 September 2008 and 31 December 2009, and again at £125,000

procedure until the estimation converges. However, given the size of the incentive (a marginal tax rate change of 1–2% above the notch) and the house price elasticities that we find, this shift will be extremely small and have no substantive effect on any of our conclusions.

from 1 January 2010 onwards. This section takes a static approach by considering bunching responses within each of these five periods separately, while the next section investigates dynamic adjustment paths around the reform episodes. Figure 5 shows results for the five periods in separate panels, each of which is constructed as in the Figure 4. The findings for the lower notch are qualitatively consistent with those for the other notches, with a clear and statistically significant bunching response to the tax notch in each period. The size of the bunch and the hole is smaller at the lower notch than at the upper notches, but so is the size of the notch. The effect of the notch on the average transacted house value is between £3,500 and £5,000, or about 4–5 times the size of the tax liability jump, implying that responses are proportionally larger at the bottom. Moreover, based on visual inspection of the hole (which is not as sharp here as for the upper notches), the most responsive households reduce their transacted house value by as much as 20 times the size of the notch. These extremely large responses are consistent with the most responsive households being down-payment constrained at an LTV ratio of 95%, not uncommon among first-time buyers in our sample period.²² These findings are consistent with the observation that a greater share of properties at the lower end of the price distribution are starter homes being bought by highly leveraged first-time buyers.

In 2011 and 2012, the government introduced two new notches affecting very high value properties, one at £1 million on 6 April 2011 and another one at £2 million on 22 March 2012. The stamp duty notch at £2 million is commonly referred to as the "mansion tax". Even though these are very recent notches, they have already created a clear house price distortion as shown in Appendix Figure A.1. This figure is constructed in the same way as the previous ones, except that the counterfactual distribution is obtained differently. We take advantage of the tax reform (notch introduction) by comparing the empirical house price distribution after the introduction of the notch to the empirical distribution in the year leading up to the introduction of the notch. The results are qualitatively very similar to the previous results, with an average house price response of £30,000 at the £1 million notch (3 times the tax liability jump of £10,000) and £100,000 at the £2 million notch (2.5 times the tax liability jump of £40,000).

4.2 House Price Responses to Moving Notches

This section investigates the dynamics of behavioral adjustment to the changes in the position of the lower notch that were mentioned above. When considering dynamic adjustments, it is important to keep in mind that there is always a lag between agreeing on a purchase price and completing the housing contract. In the UK housing market, this lag is under 90 days for most transactions and about 60 days on average (Besley *et al.* 2014). Since the official transaction date in our data refers to contract completion, the time it takes for the market to settle into a new equilibrium is bounded from below by about 3 months.

Figure 6 considers the movement of the lower notch from £120,000 to £125,000 on 23 March

²²For example, in 2006, 13% of mortgage originations were at LTV ratios above 90% (Financial Conduct Authority 2014).

2006. Each panel shows the empirical and counterfactual distributions in a given month between February 2006 and September 2006. The two vertical lines demarcate the £120,000 and £125,000 cutoffs and are either solid green (for the cutoff that is active in month in question) or dashed black (for the cutoff that is inactive). April 2006 is the first full month where the new cutoff is in place. The figure shows very clearly how the bunch moves over time in response to the changed location of the notch. Most of the adjustment has occurred after four months (in July 2006) and a new equilibrium has been reached after 6 months (in September 2006).²³ Hence, most of the lag in the adjustment to the new equilibrium can be explained by the administrative lag between contract exchange and contract completion.

The next three figures consider the movement of the lower notch from £125,000 to £175,000 on 3 September 2008 (the start of stamp duty holiday) and the subsequent movement back to £125,000 on 1 January 2010 (at the end of stamp duty holiday). When interpreting the findings, it is worth keeping in mind that the start of the holiday was unanticipated while the end of the holiday was anticipated (see section 3.1). Figure 7 shows monthly bunching graphs over a 12-month period around the beginning of the holiday. It is constructed like the preceding figure, except that we now add estimates of excess bunching b around the two cutoffs in each month.²⁴ The main findings are the following. First, it takes 3-4 months for bunching at the old £125,000 cutoff to disappear (bunching becomes statistically insignificant for the first time in December 2008), corresponding roughly to the lag between contract agreement and completion. Second, it takes about 3 months for bunching at the new £175,000 cutoff to build up and reach a steady state (bunching b is around 0.9 from November 2008 onwards). Third, although bunching at £175,000 in the winter months of 2008/09 is smaller in absolute terms than bunching at £125,000 in the summer months before the holiday, bunching in proportion to the counterfactual distribution (b)—the right measure of responsiveness—is in fact slightly larger at £175,000. The presence of smaller absolute bunching at £175,000 is a result of seasonality in the housing market with fewer house transactions in the winter than in the summer.²⁵ The presence of larger relative bunching b at £175,000 is consistent with the fact that this notch is larger than the previous one at £125,000 (tax liability jumps of £1,750 and £1,250 respectively).

Figure 8 turns to the 12-month period around the end of the holiday on 1 January 2010 and is constructed exactly as the preceding figure. It is interesting to see the difference in the speed of adjustment to a tax change that is fully anticipated. First, the bunching at £175,000 vanishes immediately in January of 2010 when this cutoff is no longer a notch point. This shows that buyers and sellers did indeed anticipate the end of the holiday and made sure to complete their housing contracts before the end of December 2009. We see such behavior in the graph for December 2009: there is a large upward shift in the December distribution between £125,000 and £175,000 (even

²³

Animated versions of all the figures from this section that show the dynamics more vividly can be found at http://personal.lse.ac.uk/kleven/Downloads/MyPapers/WorkingPapers/best-kleven_landnotches_april2013_videos.pdf

²⁴Animated versions of these figures are online at the address in footnote 23.

²⁵Seasonality in the housing market is a well-known phenomenon that has been studied in the macro literature (e.g. Ngai & Tenreyro 2014).

though this is normally a low-season month) and an increase in excess bunching at £175,000. The next section investigates such short-term timing behavior in greater detail. Second, it takes about 2 months for bunching at the new £125,000 cutoff to build up and reach a stable equilibrium (*b* is roughly constant from February 2010 onwards). While this is faster adjustment than at the start of the holiday, it is not as fast as the disappearance of bunching at the end of the holiday. The implication is that, while buyers and sellers were rushing to *complete* agreed housing contracts below the £175,000 notch just before the end of the holiday (immediate disappearance of old bunching), they did not to the same degree *agree* (but not complete) housing contracts below the £125,000 notch just before the end of the holiday (slower emergence of new bunching).

Figure 9 summarizes the evidence in the preceding figures by showing the monthly bunching estimate *b* from January 2007 to January 2011 at the £125,000 cutoff (blue dots) and the £175,000 cutoff (orange crosses) with 95% confidence intervals around each series. The solid vertical lines demarcate the beginning and end of the stamp duty holiday, while the dashed vertical line demarcates the de facto time at which the holiday took full effect given the lag between agreed and completed house purchases. The figure highlights just how sharply house prices react to tax notches and to changes in tax notches even at the monthly level. The level of bunching at the £125,000 cutoff is constant during the holiday. The steady state level of bunching at £175,000 ($b \approx 0.9$) is larger than at £125,000 ($b \approx 0.6$) as the former notch is larger. Once we account for the built-in sluggishness due to the time it takes to complete a housing contract, the market adjusts to a new stable equilibrium remarkably quickly. We also do not see any difference in price responsiveness during good times and bad times (compare early part of 2007 to the rest of the period).

Compared to recent bunching evidence from labor markets (e.g. Saez 2010; Chetty *et al.* 2011; Kleven & Waseem 2013), the remarkable sharpness of our evidence suggests that behavioral responses in the housing market are much less affected by optimization frictions such as inattention, inertia, etc. Our evidence suggests that agents in the housing market respond precisely and quickly to tax incentives.

5 Timing and Extensive Margin Responses: Tax Reforms and Stimulus

We saw in the previous section that house prices respond sharply and quickly to transaction taxes. In this section, we investigate the effect of the transaction tax on whether individuals transact, and if so, when they choose to transact. To do this, we analyze the impact of two policies changes discussed in section 3.1. First, the stamp duty holiday that temporarily eliminated the stamp duty on house purchases in the £125,000–£175,000 price range. Second, a reform that permanently eliminated the stamp duty on house purchases in the £60,000–£120,000 price range.

The stamp duty holiday was an unanticipated stimulus program with a fixed and fully anticipated end date. Conceptually, as discussed in section 2.2, we expect such a policy to have two effects on transaction volumes. First, there will be a *timing effect* as some agents who would have transacted a house in the future bring that transaction forward to the current period. Second, there will be *extensive margin effects* as some agents engage in additional house transactions over their lifetime, including house purchases by those who would otherwise never buy (renter/homeowner margin) and house purchases by those who continue to transact as often as they otherwise would have in other periods (more moving by existing homeowners).

To evaluate fiscal stimulus programs of this kind, it is crucial to obtain estimates not just of the total stimulus effect during the program (timing and extensive margin effects), but also of the degree to which it is driven by timing (all of which will be reversed after program withdrawal) and the length of the horizon over which there is re-timing (which determines the speed of reversal). This section provides compelling evidence on all three questions. We also combine our stimulus estimates with survey data on moving-related household spending in order to provide a lower bound on the effects on real economic activity (not including general equilibrium effects of increased homeowner mobility, Keynesian multiplier effects, etc.).

We begin the analysis by studying short-term timing responses around the anticipated end date of the stamp duty holiday, and then turn to the analysis of medium-term timing and extensive margin responses.

5.1 Short-Term Timing Responses to Anticipated Tax Changes

As described in section 3.1, the tax increase at the end of the stamp duty holiday was fully anticipated, creating a *time notch* on 1 January 2010 for houses between £125,000 and £175,000. This time notch creates a strong incentive for individuals to conclude their transactions before New Year, and bunching in the timing of transactions allows us to estimate this short-term timing response.

Before discussing the empirical results, we make two remarks. First, the housing market almost shuts down between Christmas and New Year, so the notch is effectively a notch just before Christmas. Hence, agents should respond to the notch by moving the date of purchase from the early weeks of 2010 to the third week of December 2009. Second, the existence of the Christmas holiday (with or without a tax notch) may in itself lead to a piling up of house transactions in the third week of December. This means that we cannot analyze the time notch using a "pure" bunching strategy as observed bunching in transactions before Christmas 2009 may overstate the response to the tax notch. We therefore pursue a difference-in-bunching strategy by comparing bunching in the treated group (transactions between £125,000–£175,000 in December 2009) to bunching in control groups (other years and/or other price ranges).

Figure 10 shows the weekly number of transactions around New Year in different price ranges and different years. Panel A compares the treated price range £125,000–£175,000 in the treated period 2009/10 to surrounding price ranges in the same period. The treated group features very strong bunching just before the notch and a large hole after the notch. The control groups also feature bunching and a hole (Christmas effect), but to a much smaller extent. Furthermore, the shutdown of activity between Christmas and New Year is less extreme in the treated group than in the control groups. To evaluate the timing response, we estimate excess bunching in each distribution during the last three weeks of the year using a bunching approach analogous to our approach for the price notches.²⁶ The timing response is then given by the difference between bunching in the treated range and average bunching in the surrounding control ranges (D-i-Bunching in the figure). We find that excess mass induced by the time notch is almost 3 times the height of the counterfactual and strongly significant, implying that the average timing response to the notch is 3 weeks.

Panel B is constructed in the same way, except that it compares the treated price range £125,000– £175,000 in the treated period 2009/10 to the same price range in other periods (one year earlier or two years earlier). The results are very similar, with estimated excess mass before the notch being somewhat larger and still strongly significant. The placebo tests in the bottom panels repeat the strategy in Panel A (comparing different price ranges), but one year or two years earlier. In each case, the timing effect is close to zero and statistically insignificant.

Overall, this provides very compelling evidence of short-term timing responses to anticipated tax changes, consistent with the sharpness of price responses discussed above. These findings contribute to the previous literature on the timing of the realisation of taxable income (Auerbach 1988; Burman & Randolph 1994; Goolsbee 2000) and medical expenditures (Einav *et al.* 2013).

5.2 Medium-Term Timing and Extensive Margin Responses to Stimulus

During the stamp duty holiday, the tax rate was temporarily cut from 1% to 0% in the price range £125,000 to £175,000 without changing the tax rate in neighbouring price ranges, presenting us with an ideal opportunity to pursue a difference-in-differences approach to estimate medium-term timing and extensive margin responses to the stimulus. A naïve first cut at this (that we refine shortly) is to compare the evolution over time in transaction volumes in the treated range £125,000–£175,000 to a nearby control range. This is done in Figure 11, which compares the log monthly number of transactions in the treated range £125,000–£175,000 (blue dots) to a control range defined as £175,000–£225,000 (orange crosses). We have normalized the log number of transactions in each month by subtracting the average log number of transactions in the pre-treatment period (the 2 years leading up to the holiday) in order to make visual comparison of the two series easier. The solid vertical lines mark the beginning (3 September 2008) and the end (31 December 2009) of the stamp duty holiday.²⁷

$$c_{w} = \sum_{j=0}^{7} \beta_{j} (z_{w})^{j} + \eta I \{ w \in \text{end of month} \} + \sum_{k=\bar{w}^{-}}^{\bar{w}^{+}} \gamma_{k} I \{ w = k \} + \mu_{w},$$
(12)

²⁶To quantify bunching, we estimate a counterfactual number of weekly transactions based on the following regression

where c_w is the number of transactions in week w and z_w is the distance of week w from the end of 2009. The second term is a fixed effect for weeks at the end of the month (which feature heavier trading in every month), while the third term excludes weeks in a range (\bar{w}^-, \bar{w}^+) , which we set to include the last 3 weeks of 2009 and the first 10 weeks of 2010.

²⁷As described in section 3.1, a stamp duty relief scheme was implemented for first-time buyers in the price range £125K–£250K between 25 March 2010 and 24 March 2012 (*after* the end of the stamp duty holiday). Since we are also interested in estimating reversal after the stamp duty holiday, it is important to make sure that the first-time buyers' relief scheme is not a confounding factor during the reversal period. This motivates using a control range (£175K–£25K)

The two series display completely parallel trends leading up to the holiday and then begin to diverge precisely when the holiday starts. The positive effect of housing stimulus in the treated range increases during the first months of the holiday and features a sharp spike in the last month as people rushed to take advantage of the stimulus before it expired. After the holiday, there is a sharp dip in the treated series during the first month, but only slight additional reversal thereafter as the treated group is marginally below the control group for about a year and then converges with the control group in the later part of the sample. Taken at face value, this graph implies that housing stimulus gave a large boost to housing market activity during the policy with very weak reversal after the policy (apart from the short-term timing effect shown by the spike and dip right around the stimulus end date analysed in section 5.1 above).²⁸ However, we argue that this both overstates the positive impact of the stimulus policy and understates the slump after the end of the policy.

The issue with the analysis in Figure 11 is that treatment assignment (whether a transaction takes place in the £125,000–£175,000 price range) is endogenous to movements across bracket cutoffs. The stamp duty holiday creates an incentive to move into the treated price bracket from both sides. At the upper end of the range, the holiday creates a new notch at £175,000 that induces agents to move from a region above the cutoff to a point just below the cutoff (bunching). We have shown in section 4 above that bunching responses at £175,000 do indeed occur, and this increases activity in the treated range compared to the control range. At the lower end, the holiday eliminates the notch at £125,000 and therefore induces bunchers at this cutoff to move back into the hole above the cutoff. We have shown that the disappearance of bunching at £125,000 also occurs, and this further increases activity in the treated range compared to the control range. Hence, the positive effect of housing stimulus in Figure 11 combines the true effect on overall activity levels with endogenous price responses resulting from the change in the location of the notch.

There are two ways of dealing with this endogeneity issue. The simplest way is to widen the treatment range on each side (below £125,000 and above £175,000) in order to ensure that any price manipulation around notches occurs *within* the treatment range and so does not affect measured activity levels in this range. By including transactions outside the tax holiday area in the treatment group, this strategy captures an intent-to-treat effect and therefore understates the impact on the actually treated. We consider this intent-to-treat strategy in Appendix Figure A.2, but here we focus instead on a more sophisticated way of dealing with endogeneity. This strategy exploits the fact that we have monthly bunching estimates of price responses to notches and can therefore directly control for it. That is, we may consider the number of transactions in different price brackets adjusted for the effect of bunching behavior in each month. To be precise, in every

just above the treatment range (£125K–£175K), ensuring that both groups fall within the range eligible for first-time buyers' relief and therefore face the same incentive from this scheme. There could still be a concern that the treatment and control range respond differently to the first-time buyer incentive, which would be a confounding factor in the reversal estimates. To alleviate this concern, we drop all transactions claiming first-time buyers' relief throughout the analysis in this section. Including those observations only strengthens our findings below of incomplete reversal after the end of the stamp duty holiday.

²⁸Note that the control group also features a (much smaller) spike and dip around the end of the stamp duty holiday driven by the Christmas/New Year effect as discussed in section 5.1 above.

month, the estimated bunching mass just below £125,000 is reallocated to the treatment range £125,000–£175,000 while the estimated bunching mass just below £175,000 is reallocated to the control range £175,000–£225,000. By using these bunching-adjusted counts in our difference-in-differences strategy, we avoid bias from selection into treatment.

Figure 12 shows the results from this bunching-adjusted strategy. Panel A shows the normalized logs of the monthly number of transactions in the treatment and control ranges exactly as in Figure 11. It is visually clear that this strategy results in effects of housing stimulus that are qualititatively similar, but considerably smaller, and that there is a stronger lull in activity after the end of the stamp duty holiday. Panel A also suggests that the lull in activity lasts for approximately 12 months, after which the two series are completely parallel again. Panel B shows the cumulative sums of the two series in panel A as well as the cumulative sum of the differences between the two series (in green diamonds) in order to emphasize the effects we are studying. Panel B confirms that the two series track each other before the stimulus, diverge gradually during the stimulus period, and then converge for around 12 months until they revert to their pre-stimulus, parallel trends.

In order to quantify the effects of the stimulus, we run the following regression on a panel of monthly activity levels in price bins of £5,000 (over the range £125,000–£225,000) between September 2006 and October 2012

$$n_{it} = \alpha_0 Pre_t + \alpha_H Hol_t + \alpha_R Rev_t + \alpha_P Post_t + \alpha_T Treated_i + \beta_H Hol_t \times Treated_i + \beta_R Rev_t \times Treated_i + \beta_P Post_t \times Treated_i + \nu_{it},$$
(13)

where n_{it} is the log number of transactions in price bin *i* and month *t*, Pre_t is a dummy for the pre-period September 2006–August 2008, Hol_t is a dummy for the stamp duty holiday period September 2008–December 2009, Rev_t is a dummy for the post-holiday reversal period January–December 2010, $Post_t$ is a dummy for the later months January 2011–October 2012, $Treated_i$ is a dummy for the treated price range £125,000–£175,000, and finally ν_{it} is an error term that we allow to be clustered at the monthly level.²⁹ The coefficients we are interested in are β_H (positive effect during stimulus) and β_R (negative effect after stimulus due to re-timing).

Panel A of Figure 12 shows our estimates of the coefficients β_H , β_R and β_P . The coefficient $\hat{\beta}_H = 0.20 \ (0.022)$ implies that average monthly activity was approximately 20% higher during the holiday than it would have been in the absence of stimulus. The coefficient $\hat{\beta}_R = -0.08 \ (0.032)$ implies that average monthly activity was about 8% lower in the first year after the stimulus than it otherwise would have been. Together, these estimates imply that 31% of the additional activity created by the stimulus program was a timing response by people bringing forward their pur-

 $^{^{29}}$ Since we run the difference-in-differences regression (13) using bunching-adjusted activity levels in £5K bins, we have to reallocate bunching mass below the two cutoffs to specific £5K bins above the cutoffs. We reallocate bunching mass below a cutoff to the five bins above the cutoff in proportion to the amount of missing mass (difference between the estimated counterfactual mass and the observed mass) in each bin. Furthermore, since activity levels are adjusted using *estimated* bunching at the thresholds, we are introducing measurement error to our dependent variable coming from misspecification of the counterfactual when calculating the amount of bunching at £125K and £175K. However, since this measurement error is effectively noise in the dependent variable, it does not cause bias in our estimates, but simply increases our standard errors.

chases in order to benefit from the tax cut, while the remaining 69% was a lasting extensive margin effect.³⁰ These large and lasting effects are consistent with our conceptual framework in section 2 in which even small changes in the transaction tax are leveraged up into large changes in the house that down-payment constrained households can afford, and hence can have large impacts on their decision to participate in the housing market.

Since the end date of the reversal period (December 2010) was chosen visually as the point at which the two series become parallel again, there might be a concern that our estimate of total reversal is sensitive to the choice of this end date. In order to address this, Panel C of Figure 12 shows how this result changes as a different end date is chosen. The green diamonds show estimates of total reversal as a share of total stimulus as the regression (13) is performed using different reversal period cutoffs, and the grey shaded area depicts the 95% confidence interval around these estimates.³¹ The reversal estimate is not sensitive to this choice, never rising above 40%, and we can always confidently reject the presence of full reversal.

When considering the simpler intent-to-treat strategy described above (see Figure A.2), the effects are qualitatively similar but quantitatively somewhat weaker as one would expect. The intent-to-treat strategy produces larger reversal as a share of stimulus (40–50%) than the bunching-adjusted strategy, but we can still reject full reversal in all specifications.

These reversal findings stand in sharp contrast to Mian & Sufi (2012), who find complete and swift reversal following a short (1 month) stimulus program offering car transaction subsidies in the US. Apart from the fact that the housing market may work differently than the car market (for example with respect to down-payments and leverage), the contrast between our findings and those of Mian & Sufi (2012) suggests that stimulus policies that are of extremely short duration, such as the one they study, do not give households sufficient time to respond along the extensive margin and therefore have only short-term timing effects. Hence, our findings highlight the importance of the length of the stimulus program. Of course, while the strength of reversal is important for evaluating stimulus, it does not by itself indict or validate such policies as their key rationale is to create more economic activity when the economy is slack (even if this comes at the expense of less economic activity when the economy is tight). The next section provides a rough estimation of the immediate increase in real economic activity created by the UK housing stimulus program.

It is important to note that our quasi-experimental micro approach to evaluating stimulus policy does not capture potential general equilibrium or multiplier effects. If the program had a salutary effect on the housing market and macroeconomy as a whole, this effect would be present in both treatment and control groups and therefore not show up in our difference-in-differences estimates. Besides general equilibrium and multiplier effects, a source of spillovers between treatments and controls may arise from *real estate chains*, i.e. linked house transactions whereby someone selling a house in the treatment range is simultaneously buying a house in the control range. Bias from chain effects can be reduced or eliminated by considering control ranges further away

³⁰The estimate of total reversal as a share of total stimulus is calculated as $-(12\hat{\beta}_R)/(16\hat{\beta}_H)$.

³¹The point estimates are calculated as $-(\sum_t Rev_t \times \hat{\beta}_R) / (16\hat{\beta}_H)$, where $\sum_t Rev_t$ denotes the length of the reversal period in the particular regression. Standard errors are computed by the delta method.

from the treatment range, but such strategies create other problems with comparability and parallel trends. The key thing to realize is that potential chain effects unambiguously work against us and create attenuation bias, and so the (large) stimulus estimates we obtain by comparing neighboring price ranges are, if anything, conservative.

5.3 GDP Effects of Stimulus

While we have established that the stamp duty holiday had a large effect on transaction volume in the housing market (and therefore on household mobility), a motivation for the policy was also to stimulate real economic activity through larger household spending driven by the complementarities between moving house and spending. Investigating the spending effect of the UK housing stimulus program also allows for a comparison between our findings and previous work on the consumer spending effect of fiscal stimulus such as income tax rebates (e.g. Shapiro & Slemrod 2003a,b; Johnson *et al.* 2006; Agarwal *et al.* 2007; Kreiner *et al.* 2012). A fully rigorous analysis of the effects of housing transactions on expenditure is beyond the scope of this paper, but we perform some back-of-the-envelope calculations to shed light on the likely magnitude of these effects.

Using data from the UK Living Costs and Food Survey, we estimate in Appendix table A.1 that households spend roughly an additional 1.6% of the value of their home on repairs, improvements, furnishings, appliances and other durable goods when they move. This is a conservative estimate compared to similar calculations for the US (Siniavskaia 2008; Zillow.com 2012). Estate agents' fees average 1.98% of the house value and other commissions come to 1.24%, giving an estimate of the total expenditure accompanying a house transaction of 4.8% of the house value. Denoting this estimate by ϕ , the immediate impact of the policy on GDP is $\Delta GDP = \phi h_v^m \Delta n$ where h_v^m is the average value of houses bought during the stimulus, and Δn is the number of additional transactions resulting from the policy. To arrive at an estimate of the effectiveness of the policy that is comparable to other stimulus policies, we scale it by the foregone tax revenue, $\Delta Tax = \tau_0 h_v^m n_0$, where $\tau_0 = 1\%$ is the pre-stimulus tax rate and n_0 is the counterfactual number of transactions in the price range affected by the stimulus. In the previous section, we estimated $\Delta n/n_0$ to be $\beta_H = 0.20$, and so we arrive at an estimate of the effect on economic activity per dollar of tax cut equal to $\Delta GDP / \Delta Tax = \phi \beta_H = 0.96$.³²

These calculations suggest that the stamp duty holiday was not only successful in stimulating housing market activity, but also provided a significant boost to real economic activity through the strong complementarities between moving house and consumer spending. These rough calculations exclude other indirect effects, for example labor market effects of increased mobility and Keynesian multiplier effects. As a benchmark, the previous work cited above on fiscal stimulus through income tax rebates found significantly smaller effects on consumer spending (0.2–0.7 dollars of spending per dollar of tax cut, as opposed to about 1 dollar of spending here). Overall, our findings suggest that transaction tax cuts (or subsidies) can be very effective at stimulating both

³²Appendix Table A.1 shows details of the calculations and their sensitivity to using the intent-to-treat estimate of β_H discussed in the previous subsection as well as an alternate estimate of households' additional expenditure.

housing market activity and real economic activity during downturns.

5.4 Extensive Margin Responses to a Permanent Tax Cut

On 16 March 2005, the bottom notch was permanently moved from £60,000 to £120,000. The reform took effect immediately after its announcement, and while a reform of this kind had been expected, the exact timing and details were not. Since this was a permanent reform, studying its impact over an extended period after its implementation will allow us to analyze the extensive margin effects of permanent reforms (since potential timing effects will only affect the months just after the reform). It is also worth noting that this reform was implemented during the height of the housing market boom, in sharp contrast to the stamp duty holiday implemented at the bottom of the recession.

The reform cut the tax from 1% to 0% over the price range £60,000 to £120,000 while leaving the tax unchanged in neighbouring price ranges, which again presents us with the opportunity to pursue a difference-in-differences strategy. The issue that treatment assignment is endogenous to movements across bracket cutoffs is present in exactly the same way as for the stamp duty holiday, and so we address it in the same way by using monthly bunching estimates to account for price responses. Figure 13 shows the results from our bunching-adjusted difference-in-differences strategy. Panel A shows the normalized log counts of monthly transaction volumes in the treatment range £60,000–£120,000 (blue circles) and the control range £120,000–£180,000 (orange crosses) together with the estimated treatment effect from a regression analogous to equation (13), while panel B shows the cumulative sums of the normalized log counts in the treatment and control ranges. As panel A shows, the treatment and control ranges were parallel in the months leading up to the reform, and then diverged sharply immediately following the reform. The estimated coefficient $\beta_P = 0.23$ (0.018) implies that this permanent reform increased monthly transaction volumes by approximately 23% on average. This extensive response is even larger than the extensive response to the stamp duty holiday, consistent with the idea that these effects are increasing in the length of the time horizon of the policy as discussed in section 5.2.

6 Conclusion

This paper has studied the impact of transaction taxes in the housing market, using administrative data on every property transaction in the UK from 2004–2012 and compelling quasi-experimental variation created by notches, tax reforms, and stimulus. We have presented evidence on the effects of transaction taxes on a range of margins—house prices, volume of house purchases, and intertemporal substitution—including an analysis of the dynamics of adjustment to both anticipated and unanticipated tax changes. The overall finding is that the housing market responds very strongly and quickly to transaction taxes, making such taxes very distortionary compared to standard recurrent taxes. We have intepreted our results in the context of a housing model with down-payment constraints (as in Stein 1995), arguing that the leverage channel amplifies behavioral responses to transaction taxes and provides a natural explanation for the enormous

behavioral responses we find. Of course, while we causally identify behavioral responses to transaction taxes, we do not causally identify the effect of leverage on those tax responses, which is an interesting topic for future research.

Our findings from the 2008–2009 stamp duty holiday contribute to a recent micro-macro literature on the effectiveness of fiscal stimulus and, in particular, present some of the first evidence on the effectiveness of using temporary tax changes to stimulate the housing market during economic downturns. The 16-month stamp duty holiday was enormously successful in stimulating housing market activity, increasing the volume of house transactions by as much as 20% in the short run (due to timing and extensive responses) followed by a smaller slump in activity after the policy is withdrawn (as the timing effect is cancelled out). Due to the complementarities between moving house and consumer spending, these stimulus effects translate into GDP effects that are considerably larger than what has been found for other forms of fiscal stimulus such as income tax rebates. More generally, these findings suggest that reducing the cost of housing transactions is an effective stimulus policy. Beyond the UK, this finding lends support to the homebuyer tax credit introduced by the 2009 Stimulus Bill in the US.

Finally, our study of transaction taxes in the property market contributes to the scant micro evidence on the effects of transaction taxes in asset markets more broadly, including the financial transaction taxes that have been discussed widely in recent years. An interesting question there turns on the ability of such taxes to affect the emergence of asset-price bubbles and the volatility of the economy more generally. Addressing this issue raises some daunting empirical challenges, ideally requiring exogenous variation in transaction taxes across economies, and so is left for future research.

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Date Range Price Range	1 Dec 2003 to 16 Mar 2005	17 Mar 2005 to 22 Mar 2006	23 Mar 2006 to 2 Sep 2008	3 Sep 2008 to 31 Dec 2009	1 Jan 2010 to 5 Apr 2011	6 Apr 2011 to 21 Mar 2012	22 Mar 2012 to April 2013
0 - £60K	0	0					
£60K - £120K		0	0	0	0	0	0
£120K - £125K	1	1					
£125K - £175K			1		1	1	1
£175K - £250K				1	I		
£250K - £500K	3	3	3	3	3	3	3
£500K - £1000K						4	4
£1000K - £2000K	4	4	4	4	4	5	5
£2000K - ∞						5	7

Table 1: Residential Property Tax Notches

Notes: The table shows how the stamp duty land tax schedule for residential property has varied over time. Each column represents a time period during which the tax schedule was constant. The rows represent price ranges, and the entry in each cell is the tax rate that applies to that price range in the time period.



Notes: Figure 1 illustrates the implications of a notched transaction tax schedule in budget set diagrams (Panels A–C) and a density distribution diagram (Panels D). The budget set diagrams depict preferences as in equation (2) and the budget set in (h_v, c) -space for three values of initial debt k to illustrate intensive responses for heterogeneous preferences A, but a specific demand elasticity ε . The notch creates bunching at the cutoff \overline{h}_v by all individuals in a preference range $(\overline{A}, \overline{A} + \Delta \overline{A})$, who would have bought houses on the segment $(\overline{h}_v, \overline{h}_v + \Delta \overline{h}_v)$ in the absence of the notch. The marginal bunching individual at $\overline{A} + \Delta \overline{A}$ is indifferent between \overline{h}_v and the best interior location \overline{h}_v^I . For low values of k (Panel A) neither \overline{h}_v^I nor the counterfactual choice $\overline{h}_v + \Delta \overline{h}_v$ are down-payment constrained. For intermediate values of k (Panel C), both $\overline{h}_v^I = \widehat{h}_v (k, t + \Delta t)$ and $\overline{h}_v + \Delta \overline{h}_V = \widehat{h}_v (k, t + \Delta t)$, but not $\overline{h}_v + \Delta \overline{h}_v$. For high values of k (Panel C), both $\overline{h}_v^I = \widehat{h}_v (k, t + \Delta t)$ and $\overline{h}_v + \Delta \overline{h}_V = \widehat{h}_v (k, t)$ are down-payment constrained. For each parameter vector $\{\varepsilon, k, \gamma\}$ the range $(\overline{h}_v, \overline{h}_v^I)$ will be empty, with the width of the range depending on the size of the demand elasticity ε (and converging to zero for $\varepsilon = 0$). In addition to intensive responses, the notch creates extensive responses above \overline{h}_v by individuals close to indifference between moving and not \widehat{B} ving $(k \approx \tilde{k}, \text{ where } \tilde{k}$ is defined in equation (7)). However, such extensive responses will be negligible *just* above the cutoff: if an individual prefers buying a house slightly above \overline{h}_v in the absence of the notch, then he will be better off buying a house at \overline{h}_v than not buying at all in the presence of the notch. Combining these effects, the observed density is predicted to be as illustrated in Panel D.



Figure 2: Tax Schedule in March 2013

Notes: Figure 2 shows the stamp duty land tax schedule for residential properties in place in March 2013 graphically as the solid blue line. The tax liability jumps discretely at the notches at £125,000, £250,000, £500,000, £1,000,000 and £2,000,000. Within the brackets defined by these notches, the tax rate is constant, and applied to the whole transaction price at the rates shown along the top of the figure.

Figure 3: Summary Statistics



Notes: Panel A shows the monthly average price of property transactions relative to the average price in April 2005 in London (blue circles) and the U.K. (orange crosses). The average price of property transactions in London during the period April 2005 - October 2012 was £345,360 and the average price in the U.K. during our data period was £199,479. Panel B shows the monthly total number of property transactions relative to the number that took place in April 2005 in London (blue circles) and the U.K. (orange crosses). The average monthly number of property transactions in London during the period April 2005 - October 2012 was 12,955 while the average monthly number of property transactions in this period in the U.K. was 103,561.



Figure 4: Bunching and Holes Around the Notches That Remain Constant

Notes: The figure shows the observed density of property transactions (blue dots) and our estimated counterfactual density (red line) around the notch at £250,000 where the tax liability jumps by £5,000 (from 1% to 3% of the transaction price) in panel A and around the notch at £500,000 where the tax liability jumps by £5,000 again (from 3% to 4% of the transaction price). The data used for these estimates excludes transactions that claim relief from the stamp duty land tax (except for those claiming first-time buyers' relief) as the regular tax schedule does not apply to these transactions. The counterfactual density is estimated as in equation (11), using bins £100 pounds wide and a polynomial of order 5. The vertical dashed lines denote the upper and lower bounds of the excluded region around the notch. The upper bound of the excluded region is chosen as the point where the observed density changes slope from positive to negative. The estimate of equation (11) controls for round number bunching at multiples of £500, £1,000, £5,000, £10,000, £25,000 and £50,000. Both the empirical and the counterfactual density are shown aggregated up to bins £5,000 wide. *b* is our estimate of the excess mass just below the notch scaled by the average counterfactual frequency in the excluded range, with its standard error shown in parentheses. *m* is our estimate of the missing mass above the notch scaled error in parentheses. The figures also show the average house value change created by the notch, and the tax liability change at the notch. All standard errors are obtained by bootstrapping the procedure 200 times.



Figure 5: Bunching and Holes Around the Lower Notch

Notes: The figure shows the observed density of property transactions (blue dots) and our estimated counterfactual density (red line) around the lower notch in the residential property tax schedule where the tax liability jumps from 0 to 1% of the transaction price. Panel A shows the period 1 November 2004 to 16 March 2005 when the notch was at £60,000. Panel B shows the period 17 March 2005 to 22 March 2006 when the notch was at £120,000. Panel C shows the period 23 March 2006 to 2 September 2008 when the notch was at £125,000. Panel D shows the period 3 September 2008 to 31 December 2009 when the notch was at £175,000. Panel D shows the period 3 September 2008 to 31 December 2009 when the notch was at £175,000. Panel E shows the period 1 January 2009 to 31 October 2012 when the notch was at £125,000. The data used for these estimates excludes transactions. The counterfactual density is estimated as in equation (11), using bins £100 pounds wide and a polynomial of order 5 in panels A, C, D and E and of order 4 in panel B. The vertical dashed lines denote the upper and lower bounds of the excluded region around the notch. The upper bound of the excluded region is chosen as the point where the observed density stops increasing and becomes decreasing (apart from spikes at round numbers). The estimate of equation (11) controls for round number bunching at multiples £500, £1,000, £5,000, £25,000 and £0,000. Both the empirical and the counterfactual density are shown aggregated up to bins £5,000 wide. *b* is our estimate of the excess mass just below the notch scaled by the counterfactual density at the notch, with its standard error shown in parentheses. m - b is our estimate of the difference between the missing mass above the notch scaled by the counterfactual density at the notch, with its standard error shown in parentheses. m - b is our estimate of the difference between the missing mass and the bunching mass, again with its standard error in parentheses. The figures also show the average house value chan



Figure 6: Dynamics of Bunching at Bottom Notch around March 2006

Notes: The figure shows the observed density of property transactions (blue dots) and our estimated counterfactual density (red line) in the region £75,000 – £225,000 separately for each month. On 23 March 2006, the bottom notch moved from £120,000 to £125,000. The estimation of the counterfactual is as described in section 4.1 and in the notes to figures 4 & 5. The estimation excludes data in the regions £115,000 – 140,000 and £170,000 – £190,000 and uses a polynomial of order 5. Animated versions of these figures that show the dynamics more vividly can be found at http://personal.lse.ac.uk/kleven/Downloads/MyPapers/WorkingPapers/best-kleven_landnotches_april2013_videos.pdf



Figure 7: Dynamics of Bunching Around the Beginning of Stamp Duty Holiday

Notes: The figure shows the observed density of property transactions (blue dots) and our estimated counterfactual density (red line) in the region £75,000 – £225,000 separately for each month. On 3 September 2008, the bottom notch was moved unexpectedly from £125,000 to £175,000. The estimation of the counterfactual is as described in section 4.1 and in the notes to figures 4 & 5. The estimation excludes data in the regions £115,000 – 140,000 and £170,000 – £190,000 and uses a polynomial of order 5. Animated versions of these figures that show the dynamics more vividly can be found at http://personal.lse.ac.uk/kleven/Downloads/MyPapers/WorkingPapers/best-kleven_landnotches_april2013_videos.pdf



Figure 8: Dynamics of Bunching Around the End of Stamp Duty Holiday

Notes: The figure shows the observed density of property transactions (blue dots) and our estimated counterfactual density (red line) in the region £75,000 – £225,000 separately for each month. On 1 January 2010, the bottom notch was moved back from £175,000 to £125,000 as announced previously. The estimation of the counterfactual is as described in section 4.1 and in the notes to figures 4 & 5. The estimation excludes data in the regions £115,000 – 140,000 and £170,000 – £190,000 and uses a polynomial of order 5. Animated versions of these figures that show the dynamics more vividly can be found at http://personal.lse.ac.uk/kleven/Downloads/MyPapers/WorkingPapers/best-kleven_landnotches_april2013_videos.pdf



Figure 9: Bunching Estimates Over Time

Notes: The figure shows our estimates of $b(\bar{h}_v)$, the bunching mass just below \bar{h}_v scaled by the counterfactual frequency at \bar{h}_v , by month from January 2007 to February 2011 and for two values of \bar{h}_v , £125,000 (blue circles) and £175,000 (orange crosses). The first vertical line is at September 2008 when the stamp duty holiday was unexpectedly announced, moving the notch from £125,000 to £175,000. The dashed vertical line is at December 2008 to represent the observation that house transactions take up to 90 days to conclude, and so some inertia in the bunching responses is to be expected. The second vertical line is at December 2009 when the stamp duty holiday came to an end as anticipated, and the notch was moved from £175,000 back down to £125,000.



Figure 10: Time Notch

Notes: The figures show the weekly number of transactions around the end of the stamp duty holiday on 31 December 2009. Panel A shows the number of transactions taking place between 2009w27 and 2010w26 in the treated price range $\pounds 125,000 - \pounds 175,000$ (blue circles) alongside the number of transactions in the price ranges $\pounds 75,000 - \pounds 125,000$ (orange crosses) and $\pounds 175,000 - \pounds 225,000$ (green diamonds). Panel B shows the number of transactions taking place in the treated price range ($\pounds 125,000 - \pounds 175,000$) around the end of the stamp duty holiday, 2009w27 to 2010w26 (blue circles) as well as 1 year earlier (orange crosses) and 2 years earlier (green diamonds). Panel C shows the same price ranges as in panel A, but using data from 1 year earlier. Similarly, panel D shows the same price ranges as in panel A, but using data from 2 years earlier. The solid vertical line is placed at the end of the year (which at the end of 2009 is the end of the stamp duty holiday) and the dashed vertical lines demarcate the last 3 weeks of the year and the first 10 weeks of the year, which are the excluded range for the counterfactual estimates. The counterfactual is estimated according to (12):

$$c_{w} = \sum_{j=0}^{7} \beta_{j} (z_{w})^{j} + \eta I \{ w \in \text{end of month} \} + \sum_{k=\bar{w}^{-}}^{\bar{w}^{+}} \gamma_{k} I \{ w = k \} + \mu_{w}$$

where c_w is the number of transactions in week w and z_w is the distance of week w from the end of 2009. The second term is a fixed effect for weeks at the end of the month (which feature heavier trading in every month), while the third term excludes weeks in the excluded range (\bar{w}^-, \bar{w}^+) . Each picture shows the difference-in-bunching estimate corresponding to the choice of treatment (blue circles) and control groups (orange crosses and green diamonds) depicted in the picture. The DiD estimate is the difference between the (normalized) bunching in the treatment group and the average bunching in the two control groups.



Figure 11: Effects of the Stamp Duty Holiday Stimulus: Naive Diff in Diff

Notes: The figure shows how the level of housing market activity changed over time in the price range affected by the stamp duty holiday (\pounds 125,000 - \pounds 175,000) and the neighbouring price range \pounds 175,000 - \pounds 225,000. The figure shows the normalized log monthly number of transactions defined as the log of the number of transactions in that month minus the average of the log of the number of transactions in the 24 months leading up to the start date of the Stamp Duty Holiday (September 2006 - August 2008).



Figure 12: Effects of the Stamp Duty Holiday Stimulus: Adjusting for Bunching

-(12β_R)/(16β_H) = 0.31 (0.124)

Notes: The figure shows the effect of the stamp duty holiday stimulus on housing market activity using the price range $\pounds 125,000 - \pounds 175,000$ as the treated price range and $\pounds 175,000 - \pounds 225,000$ as the control price range. However, all counts are adjusted for price manipulation using bunching estimates by moving excess transactions at $\pounds 125,000$ to prices between $\pounds 125,000$ and $\pounds 150,000$ and moving excess transactions at $\pounds 175,000$ to prices between $\pounds 125,000$ and $\pounds 200,000$. Panel A shows the normalized log monthly number of transactions defined as the log of the number of transactions in that month minus the average of the log of the number of transactions in the 24 months leading up to the start date of the Stamp Duty Holiday (September 2006 - August 2008). Superimposed on that are our estimates of β_H , β_R and β_P from

201¹m1

201¹m7 Month 2012m1

2012m7

2010m1

2010m7

the regression

$$n_{it} = \alpha_0 Pre_t + \alpha_H Hol_t + \alpha_R Rev_t + \alpha_P Post_t + \alpha_T Treated_i + \beta_H Hol_t \times Treated_i + \beta_R Rev_t \times Treated_i + \beta_P Post_t \times Treated_i + \nu_{it}$$

where n_{it} is the log of the monthly number of transactions Pre_t is a dummy for the pre-period September 2006–August 2008 inclusive, Hol_t is a dummy for the stamp duty holiday period September 2008–December 2009, Rev_t is a dummy for the post-holiday reversal period January–December 2010 inclusive, and $Post_t$ is a dummy for the later months January 2011–October 2012 inclusive. $Treated_i$ is a dummy for the treated price range and finally ε_{it} is an error term. Panel B shows the cumulative sum of the normalized log counts in panel A (blue dots and orange crosses) as well as the cumulative sum of the differences between the treatment and control groups (green diamonds). Panel C shows how the proportion of the total effect of the stamp duty holiday that is undone by reversal after the end of the holiday changes as we use different months as the first month after the effect is gone. Specifically, it shows ($\Sigma_t Rev_t \times \beta_R$) / (16 β_H) as the end date of the period used to define Rev_t changes. The vertical line is at our preferred choice for the first month of $Post_t$, January 2011, which gives an estimate of the proportion of the total effect undone by reversal of 0.31 (0.124).



Figure 13: Effects of the Permanent Reform: Adjusting for Bunching

A: Normalized Log Counts

Notes: The figure shows the effect of the permanent tax cut of March 2005 when the bottom notch was moved from $\pounds 60,000$ to $\pounds 120,000$ on housing market activity using the price range $\pounds 60,000 - \pounds 120,000$ as the treated price range and $\pounds 120,000 - \pounds 180,000$ as the control price range. However, all counts are adjusted for price manipulation using bunching estimates by moving excess transactions at $\pounds 60,000$ to prices between $\pounds 60,000$ and $\pounds 85,000$ and moving excess transactions at $\pounds 120,000$ and $\pounds 145,000$. Panel A shows the normalized log monthly number of transactions defined as the log of the number of transactions in that month minus the average of the log of the number of transactions in the 5 months we have data for leading up to the date of the reform (November 2004 - March 2005). Superimposed on that is our estimates of β_P from the regression

$n_{it} = \alpha_0 Pre_t + \alpha_P Post_t + \alpha_T Treated_i + \beta_P Post_t \times Treated_i + \nu_{it}$

where n_{it} is the log of the monthly number of transactions Pre_t is a dummy for the pre-period November 2004–March 2005 inclusive, $Post_t$ is a dummy for the months after the reform April 2005–March 2006 inclusive. $Treated_i$ is a dummy for the treated price range and finally ε_{it} is an error term. Panel B shows the cumulative sum of the normalized log counts in panel A (blue dots and orange crosses).

A Appendix

A.1 Bounding ε Using Bunching Estimates in the Presence of Down-payment Constraints

As shown by Kleven & Waseem (2013), the relationship between bunching and the demand elasticity can be characterized by considering the marginal bunching household who is indifferent between the notch point and its best interior location–what they call the "structural approach". In our framework, this relationship is complicated by the possibility that the marginal bunching household can be down-payment constrained. When initial debt levels are low, the marginal bunching household is not down-payment constrained either in the interior of the high tax bracket at \bar{h}_v^I or in the counterfactual world absent the notch at $\bar{h}_v + \Delta \bar{h}_v$, as depicted in panel A of figure 1. In this case, our analysis of the marginal bunching household is exactly analogous to that in Kleven & Waseem (2013). The indifference condition requires that

$$1 + p - (1 + t + \Delta t)\bar{h}_v^I + \frac{A\left(\frac{\bar{h}_v^I}{pA}\right)^{1 + 1/\varepsilon}}{1 + 1/\varepsilon} = 1 + p - (1 + t)\bar{h}_v + \frac{A\left(\frac{\bar{h}_v}{pA}\right)^{1 + 1/\varepsilon}}{1 + 1/\varepsilon}$$
(14)

Along with the first-order conditions for the best interior choice \bar{h}_v^I and the no-notch location $\bar{h}_v + \Delta \bar{h}_v$, (14) implies that the marginal bunching individual satisfies

$$\frac{1}{1+\Delta\bar{h}_v/\bar{h}_v} - \frac{1}{1+1/\varepsilon} \left[\frac{1}{1+\Delta\bar{h}_v/\bar{h}_v} \right]^{1+1/\varepsilon} - \frac{1}{1+\varepsilon} \left[1 + \frac{\Delta t}{1+t} \right]^{1+\varepsilon} = 0.$$
(15)

With our estimate of the width of the bunching segment $\Delta \overline{h}_v$ and the tax parameters \overline{h}_v and $\Delta t / (1 + t)$, this condition gives a unique demand elasticity ε . For this case to apply requires that

$$k < \min \left\{ p \left[1 - A p^{\varepsilon} \left(1 + t \right)^{\varepsilon} \left(\gamma + t \right) \right], p \left[1 - A p^{\varepsilon} \left(1 + t + \Delta t \right)^{\varepsilon} \left(\gamma + t + \Delta t \right) \right] \right\}$$
(16)

For expositional ease, in what follow we will assume that

$$p\left[1 - Ap^{\varepsilon}\left(1 + t + \Delta t\right)^{\varepsilon}\left(\gamma + t + \Delta t\right)\right] < p\left[1 - Ap^{\varepsilon}\left(1 + t\right)^{\varepsilon}\left(\gamma + t\right)\right]$$
(17)

which requires that $1 > \left(\frac{1+t}{1+t+\Delta t}\right)^{\varepsilon} \frac{\gamma+t}{\gamma+t+\Delta t}$. An unconstrained elasticity $\varepsilon > -1$ is sufficient for this to be true. However, analogous derivations to the ones below can be applied whenever (17) does not hold. For slightly higher values of k, (16) is violated, we enter the case depicted in panel B of figure 1 and the marginal bunching household is down-payment constrained at its best interior choice \bar{h}_v^I , but not at its no-notch location $\bar{h}_v + \Delta \bar{h}_v$. In this case, using $\bar{h}_v^I = \hat{h}_v (k, t + \Delta t)$ and the first-order condition for $\bar{h}_v + \Delta \bar{h}_v$ (14) becomes

$$-\frac{1+t+\Delta t}{\gamma+t+\Delta t}\left(p-k\right)+\frac{1+t}{\left(\bar{h}_v+\Delta\bar{h}_v\right)^{1/\varepsilon}}\frac{\left(\frac{p-k}{\gamma+t+\Delta t}\right)^{1+\frac{1}{\varepsilon}}}{1+1/\varepsilon}=-\left(1+t\right)\bar{h}_v+\frac{1+t}{\left(\bar{h}_v+\Delta\bar{h}_v\right)^{1/\varepsilon}}\frac{\bar{h}_v^{1+1/\varepsilon}}{1+1/\varepsilon}$$

This is an equation in three unknowns (ε, k, γ) so we cannot identify ε . However, we can use the conditions under which the marginal bunching household is down-payment constrained (4) and plausible values for γ to put bounds on ε . At one extreme, the down-payment constraints do not bind, and (15) provides an upper bound on ε . At the other extreme, the marginal bunching household is down-payment constrained at its no-notch location, $\bar{h}_v + \Delta \bar{h}_v = \hat{h}_v (k, t) = (p-k) / (\gamma + t)$ and so a lower bound on ε is given by

$$\frac{1}{1+\Delta h_v/\bar{h}_v} - \frac{1+\frac{\Delta t}{1+t}}{1+\frac{\Delta t}{\gamma+t}} + \frac{1}{1+1/\varepsilon} \left[\frac{1}{1+\frac{\Delta t}{\gamma+t}}\right]^{1+1/\varepsilon} - \frac{1}{1+1/\varepsilon} \left[\frac{1}{1+\Delta\bar{h}_v/\bar{h}_v}\right]^{1+1/\varepsilon} = 0$$
(18)

together with assumptions on the value of γ . Finally, when k is high (above $p [1 - Ap^{\varepsilon} (1 + t)^{\varepsilon} (\gamma + t)]$) the marginal bunching household is down-payment constrained both at its non-notch location $\bar{h}_v + \Delta \bar{h}_v$ and its best interior choice \bar{h}_v^I as depicted in panel C of figure 1. In this case, (18) provides an upper bound on ε and 0 the lower bound.

A.2 Additional Figures & Tables



Figure A.1: Bunching and Holes Around the Highest Notches

A: Notch at £1,000,000

Notes: The figure shows the observed density of property transactions (blue dots) and the density of property transactions in the year leading up to the introduction of the notch (red line) around the notches for very high value properties. The vertical dashed lines denote the upper and lower bounds of the excluded region around the notch. The upper bound of the excluded region is chosen as the point where the observed density changes slope from positive to negative. Panel A shows the notch at £1,000,000 introduced on 6 April 2011 where the tax liability jumps by £10,000 (from 4% to 5% of the transaction price) with both densities aggregated up to bins £25,000 wide. Panel B shows the notch at £2,000,000 introduced on 22 March 2012 where the tax liability jumps by £40,000 (from 5% to 7% of the transaction price) with both densities are aggregated up to bins £50,000 wide. *b* is our estimate of the excess mass just below the notch scaled by the average counterfactual frequency in the excluded range and *m* is our estimate of the missing mass above the notch scaled by the average counterfactual frequency in the excluded range. m - b is our estimate of the difference between the missing mass and the bunching mass. The figures also show the average house value change created by the notch, and the tax liability change at the notch.



Figure A.2: Effects of the Stamp Duty Holiday Stimulus: Diff in Diff With Wider Treatment Range

C: Sensitivity to End Date of Reversal Period



Notes: The figure shows the effect of the stamp duty holiday stimulus on housing market activity using the price range \pounds 115,000 - \pounds 195,000 as the treated price range and \pounds 195,000 - \pounds 235,000 as the control price range. Panel A shows the normalized log monthly number of transactions defined as the log of the number of transactions in that month minus the average of the log of the number of transactions in the 24 months leading up to the start date of the Stamp Duty Holiday (September 2006 - August 2008). Superimposed on that are our estimates of β_H , β_R and β_P from the regression

$$n_{it} = \alpha_0 Pre_t + \alpha_H Hol_t + \alpha_R Rev_t + \alpha_P Post_t + \alpha_T Treated_i + \beta_H Hol_t \times Treated_i + \beta_R Rev_t \times Treated_i + \beta_P Post_t \times Treated_i + \nu_{it}$$

where n_{it} is the log of the monthly number of transactions Pre_t is a dummy for the pre-period September 2006–August 2008 inclusive, Hol_t is a dummy for the stamp duty holiday period September 2008–December 2009, Rev_t is a dummy for the post-holiday reversal period January–December 2010 inclusive, and $Post_t$ is a dummy for the later months January 2011–October 2012 inclusive. $Treated_i$ is a dummy for the treated price range and finally ε_{it} is an error term. Panel B shows the cumulative sum of the normalized log counts in panel A (blue dots and orange crosses) as well as the cumulative sum of the differences between the treatment and control groups (green diamonds). Panel C shows how the proportion of the total effect of the stamp duty holiday that is undone by reversal after the end of the holiday changes as we use different months as the first month after the effect is gone. Specifically, it shows ($\beta_R \Sigma_t Rev_t$) / ($16\beta_H$) as the end date of the period used to define Rev_t changes. The vertical line is at our preferred choice for the first month of $Post_t$, January 2011, which gives an estimate of the proportion of the total effect undone by reversal of 0.42 (0.123).

	Time	Time Since Last Move					
	< 1 Year	\geq 1 Year	\geq 5 Years				
Panel A: Moving-Related Household Spending							
Repairs & Improvements	3,153	1,707	1,447				
Furnishings	2,912	817	751				
Appliances	153	87	100				
Other Durables	426	434	436				
Total Expenditure	6,644	3,043	2,734				
Difference Movers - Stayers		3,600	3,909				
Difference Movers - Stayers (% of house value)		1.57	1.70				
Estate Agent Commissions (% of house value)		1.98	1.98				
Other Commissions (% of house value)		1.24	1.24				
Impact of Purchase on Expenditure: ϕ (% of house value)		4.79	4.92				
Panel B: Immediate Impact of Policy on GDP							
Impact of Policy on GDP per £ of Tax Cut ($\beta_H \times$ using $\beta_H = 0.20$	ϕ)	0.96	0.98				
Impact of Policy on GDP per £ of Tax Cut ($\beta_H \times$ using $\beta_H = 0.17$	ϕ)	0.81	0.84				

Table A.1: Immediate Impact of Fiscal Stimulus on GDP

Notes: The table shows estimates of the immediate impact of the stamp duty holiday stimulus on GDP. Using the UK Living Costs and Food Survey from 2011, the first 7 rows of panel A present estimates of moving-related spending on repairs, renovations, furnishings, appliances and other durables. To obtain only the moving-related part of these spending categories, we compare homeowners who moved within the last year (movers) to homeowners who moved more than 1 year ago or more than 5 years ago (non-movers). Row 6 shows our estimates of total moving-related spending on these categories in absolute numbers (£3,600-£3,909 depending on comparison group), while row 7 scales the estimates by the average house price of houses transacted in 2011, £230,000. Rows 8 and 9 show spending on commisions to agents, lawyers, etc. A 2011 survey by *Which? Magazine* estimates that estate agents' fees average 1.8% of the house price before VAT, or 1.98% with VAT (see http://www.which.co.uk/news/2011/03/estate-agents-fees-exposed-248666/). ReallyMoving (2012) estimates that other commissions and fees total £1,880 on average, and do not vary much with house value, so we scale this by the average value of houses bought in the range affected by the policy (£152,000). Combining rows 1-9, we reach our rough estimate of the effect of a house purchase on household spending (in % of the house price), which we denote by ϕ . This number is just below 5% independent of comparison group. In panel B we calculate the immediate impact of the policy. The total GDP effect is $\Delta GDP = \phi h_v^m \Delta n$ where h_v^m is the mean price of houses in the price range affected by the policy, and Δn is the number of additional house transactions. Combining these expressions, the effect of the policy is $\Delta GDP / \Delta Tax = \phi \Delta n / (\tau_0 n_0)$, where $\Delta n / n_0$ is our difference-in-differences estimate β_H in equation (13). The first row of panel B uses $\beta_H = 0.20$ as estimated in Figure 12, while the second row uses $\beta_H = 0.17$ a setimated in Figure A.2.