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Minimum wage and tax kink effects in the formal and informal sectors in Zambia

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Abstract: This paper explores two policy interventions in Zambia, a minimum wage hike in 2018 and an upward revision in the first kink in the progressive income tax schedule in 2017, to examine and compare the impact of minimum wage and tax kink changes on wages and the earnings distribution in the formal and informal sectors. The analysis builds on two thus far separate strands of literature that investigate the effects of minimum wages and bunching around tax kinks in developing countries using Zambian personal income tax data and data from the ILO Labour Force Surveys over the period 2012–21. Applying the idea that minimum wage effects, despite being targeted at the formal sector, may spill over into the informal sector to tax kinks, this paper proposes a *new lighthouse effect*—that is, the fact that tax kinks may serve as a reference point for wage setting in the uncovered informal sector. Results show that the minimum wage hike pushes the lower end of the earnings distribution rightward and produces significant and economically meaningful increases in wages in both the formal and informal sectors in Zambia. Interestingly, a hike in the first tax kink produces similar distributional effects and also significantly raises the wages of formal workers in the local earnings distribution around the kink. Combined with evidence of bunching of informal wages at the first and third tax kink, these results suggest that the *new lighthouse effect* is relevant in the case of Zambia, and underscores the need for policy-makers to take a holistic approach when formulating minimum wage and income tax policies that takes into account spillover effects into the informal sector.

Key words: bunching, personal income tax, minimum wage, lighthouse effect, informality, Zambia

JEL classification: H24, J31, J42

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1 Introduction

The effect of minimum wages is not straightforward in developing countries that are typically characterized by large informal sectors, heterogeneous labour markets, and limited compliance with such labour regulations (Fang and Ha 2022). More than two-thirds of the employed population in emerging and developing countries are engaged in informal employment (ILO 2018). A relatively large empirical literature has been motivated by the hypothesis that labour market regulations that target the formal sector, such as minimum wages, also affect informal workers who are not effectively covered (Harrison and Leamer 1997). It has often been found that, contrary to predictions from standard economic theory, wages increase also in the informal sector after a minimum wage hike (Derenoncourt et al. 2021; Khamis 2013; Katzkowicz et al. 2021; Lemos 2009). This has typically been explained in terms of a *lighthouse effect*, whereby the minimum wage acts as a reference for wage bargaining, even in the unregulated informal sector (Maloney and Mendez 2004).

An issue that has so far been neglected is that the lighthouse effect may also be relevant for another key policy to reduce poverty and inequality that is targeted at the formal sector—income taxation. A small but growing literature applies the bunching methodology developed by Saez (2010) and Chetty et al. (2011) to tax administrative data from developing countries and documents strong effects of kinks in a progressive income tax schedule on the wage distribution (Bell 2020; Bergolo et al. 2021; Kleven and Waseem 2013). These studies find that such kinks, in particular the first kink where income initially becomes liable to tax, create an excess mass in the distribution just below them due to workers receiving lower wages to avoid the higher tax bracket (Kleven and Waseem 2013). This finding implies that the first kink in the tax schedule may constitute a kind of wage ceiling for a significant proportion of the wage distribution. Therefore, changing the location of this kink could produce effects comparable to minimum wage adjustments, in the sense that this may also affect both the level and distribution of formal wages. Moreover, the evidence that the first kink constitutes a clear reference point for wage setting in the formal sector suggests that there may be spillover effects on the wages and earnings distribution in the informal sector. In other words, there may be a *new lighthouse effect* whereby the first tax kink, despite being targeted at the formal sector, serves as a reference point for wage bargaining in the informal sector such that firm owners set informal workers' wages according to the after-tax wages in the formal sector.

To better understand the effects of minimum wage and tax kink adjustments in the context of developing countries, I look at the case of Zambia and connect the two separate bodies of literature analysing the lighthouse effect of minimum wages and bunching at tax kinks. More specifically, I compare the effects on the wages and the earnings distribution in both the formal and informal sectors of two policy shocks in Zambia over the period 2012–2021: an upward revision in the location of the first kink in the personal income tax schedule in 2017 by 10 per cent (from ZMW3,000 to ZMW3,300) and a minimum wage hike in 2018 by 50 per cent (from ZMW700 to ZMW1,050). My main research objectives can be summarized in the following questions:

1. What is the effect of a minimum wage hike in Zambia in 2018 on the wages and earnings distribution of formal workers? Is there a spillover effect into the informal sector (a lighthouse effect)?
2. What is the effect of an upward revision in the first kink of the personal income tax schedule in 2017 on the wages and earnings distribution of formal workers? Is there a spillover effect into the informal sector (a *new lighthouse effect*)?

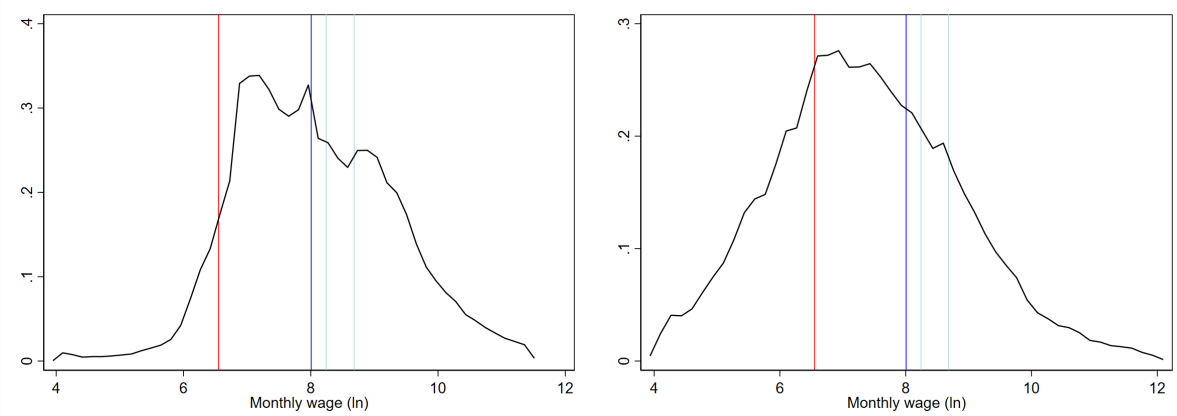
Figure 1 depicts the formal (left) and informal (right) workers' earnings distribution in Zambia in 2017 and gives an intuition for why one may expect such effects and why they may be comparable. It indicates that the minimum wage (vertical red line) affects the wages and wage distribution in both sectors—that is, there is a peak in the formal and informal earnings distribution above this wage floor (in line with the lighthouse effect (Maloney and Mendez 2004)). The first tax kink (vertical dark blue line) also

produces strong responses in the formal sector, with a peak just below it. Since the first kink potentially affects a sizeable proportion of informal workers earning wages around and above it, one may expect spillover effects from the formal sector responses. Figure 1 illustrates that the first kink should receive particular attention, rather than kinks further up the income distribution (light blue vertical lines) since it produces the largest responses among formal workers and affects the largest proportion of both formal and informal workers. The policy shocks of interest can be understood as a rightward shift of the vertical red and dark blue lines in Figure 1.

Figure 1: Wage and distributional effects of minimum wage versus tax kink adjustments in the formal and informal sectors in Zambia

(a) Formal workers' earnings distribution

(b) Informal workers' earnings distribution



Source: author's illustration based on PAYE 2014–21 and LFS 2012–21 data.

This analysis is of particular importance in Zambia, where poverty and inequality rates are among the highest in the world (IMF 2022). According to the latest poverty statistics from 2015, 54.4 per cent of the Zambian population falls below the national poverty line (61.4 per cent fall below the international poverty line of US$2.15 per day), and the country has the fifth highest Gini coefficient in the world (World Bank 2023). In this context, it is critical to have reliable evidence on the wage and distributional effects in the formal and informal sectors of two key policies that aim to address these challenges—minimum wages and income tax. Understanding the consequences in the informal sector is of particular importance since around 86 per cent of Zambia's employed population was in informal employment in 2021.[1]

The empirical analysis is divided into two parts. First, data from the ILO's Labour Force Surveys (LFS) over the period 2012–21 is used to examine the wage and distributional effects of the minimum wage in Zambia and whether it has spillover effects into the informal sector. Kernel density plots are used to investigate the distributional effects and a minimum wage adjustment in 2018 is analysed using a difference-in-difference set-up that exploits regional variation in the wage level, and therefore the bite of the minimum wage across districts in Zambia, to gauge the wage effects. In the second part, it is tested whether the first tax kink in the PAYE schedule produces similar distributional and wage effects in the formal sector to the minimum wage hike using individual personal income tax data for Zambia over the period 2014–21. Since the PAYE data does not contain information on informal workers, and the bunching effects found in the tax administrative data do not translate into the LFS data, the analysis cannot be directly applied to the informal sector. However, the LFS data is used to discuss the existence of the new lighthouse effect and to provide some preliminary evidence.

In terms of formal wages, I find that the upward revision in the location of the first tax kink in Zambia in 2017 produced a similar effect to the minimum wage hike in 2018. Both policy shocks increased

[1] Data extracted on 20 March 2023 from https://ilostat.ilo.org/topics/informality/.

the average real wages of formal workers and shifted the left tail of the formal earnings distribution rightward. While the tax kink and minimum wage change effects are similar, the former unfolds at a more local level in the surrounding area of the kink, whereas the latter affects a larger share of the overall earnings distribution. I document an 11.5 per cent and a 7–9 per cent increase in the wages of formal workers following the minimum wage and tax kink hike, respectively. In terms of informal wages, I find an even slightly larger effect of the minimum wage hike—an 11.9 per cent increase. This confirms the existence of the lighthouse effect that has been found in other studies (Katzkowicz et al. 2021; Khamis 2013; Maloney and Mendez 2004). Due to data limitations, I cannot make any conclusive statements about whether there exist spillover effects of the tax kink adjustment on informal wages, but preliminary evidence indicates some bunching of informal workers around the first and third kink in the PAYE schedule. This suggests that informal wages may be affected by the formal after-tax wage distribution and, hence, that the kinks are used as reference points for wage bargaining, in line with a new lighthouse effect.

This paper contributes to several different strands of literature. The proposed analysis extends the work on lighthouse effects, where previous studies have found that the introduction of a minimum wage (or a minimum wage hike) positively affects wages not only in the formal but also in the informal sector (Derenoncourt et al. 2021; Katzkowicz et al. 2021; Khamis 2013; Lemos 2009). In their seminal contribution to this literature, Maloney and Mendez (2004) study minimum wages in several Latin American countries and find evidence of higher wages as well as wage compression—that is, wages increase relatively more at the lower end of the wage distribution—in both the formal and informal sectors following the introduction of a minimum wage. While there is a large lighthouse effect literature, minimum wages have been less studied in Sub-Saharan Africa, and not with a focus on the lighthouse effect.[2] This paper also adds to the relatively small literature that analyses the behavioural responses of taxpayers by applying the bunching framework developed by Saez (2010) and Chetty et al. (2011) to tax administrative data from a developing country. In a key contribution to this literature, Kleven and Waseem (2013) apply the bunching approach to the personal income tax system in Pakistan and find large and sharp excess bunching below every notch in the tax schedule.[3] In the context of the South African income tax system, Bell (2020) identifies significant bunching for self-employed workers at the kinks in this schedule. Bergolo et al. (2021) find evidence of bunching at the first kink in the income tax schedule in Uruguay and an increase in the number of bunchers over time, suggesting a learning process. I build on this literature by proposing a theoretical framework and estimation strategy to analyse *changes* in the location of tax kinks.

The main contribution of this paper is to extend the minimum wage and bunching literature in developing countries, by applying the idea of the lighthouse effect from minimum wages to another formal policy—income tax, specifically the first kink in a progressive income tax schedule—while building on the bunching approach. While previous studies have used the bunching methodology to identify minimum wage effects (e.g. Jales 2018; Katzkowicz et al. 2021), to the author's knowledge, no studies have connected the two bodies of literature to investigate the effects of tax kink changes on wages and the earnings distribution in the formal and informal sectors.

The rest of the paper is organized as follows. Section 2 provides some context on the minimum wage and personal income tax legislation in Zambia. Section 3 provides theoretical considerations regarding a minimum wage hike, develops a theoretical framework for analysing changes in the first tax kink, and provides further intuition for the new lighthouse effect. Section 4 describes the data, discusses the definition of informality, and gives a detailed description of the estimation approach. Graphical

[2] See, for example, Andalón and Pagés (2008), Dinkelman and Ranchhod (2012), and Bhorat et al. (2020).

[3] Notches are similar to kinks, but the discontinuity occurs in the average rather than the marginal tax rate (Kleven and Waseem 2013).

evidence and regression results are presented in Section 5. Section 6 summarizes and compares the results, discusses the new lighthouse effect, and concludes.

2 The context of Zambia

During the period of analysis, 2012–21, the minimum wage was adjusted twice, in 2012 and in 2018. In 2012 it was revised upward from the previous level of ZMW419 per month (set in 2011) to ZMW700, and in 2018 again to ZMW1,050 per month. In US dollars in 2021 purchasing power parity (PPP) terms this is around US$65 before 2012, US$109 between 2012 and 2017, and US$164 since 2018. This is the general wage floor that applies to most workers in Zambia and is illustrated in Table 1.[4] To put this into perspective, the national poverty line lies at ZMW214 per month, or around US$1.2 per day in 2021 PPP terms (World Bank 2023). Table 1 shows that the real minimum wage fluctuated over the period 2012–21, during which Zambia saw an average inflation rate of 11.03 per cent.[5]

Table 1: Minimum wage and personal income tax bands regarding monthly income

	Minimum wage		Personal income tax							
	MW	Real MW	1st band	Rate	2nd band	Rate	3rd band	Rate	4th band	Rate
2012	700	570.50	0–1,500	0%	1,500–2,100	25%	2,100–4,750	30%	>4,750	35%
2013	700	533.28	0–2,200	0%	2,200–3,000	25%	3,000–5,900	30%	>5,900	35%
2014	700	494.64	0–3,000	0%	3,000–3,800	25%	3,800–5,900	30%	>5,900	35%
2015	700	449.25	0–3,000	0%	3,000–3,800	25%	3,800–5,900	30%	>5,900	35%
2016	700	381.14	0–3,000	0%	3,000–3,800	25%	3,800–5,900	30%	>5,900	35%
2017	700	357.61	0–3,300	0%	3,300–4,100	25%	4,100–6,200	30%	>6,200	37.5%
2018	1,050	499.02	0–3,300	0%	3,300–4,100	25%	4,100–6,200	30%	>6,200	37.5%
2019	1,050	457.19	0–3,300	0%	3,300–4,100	25%	4,100–6,200	30%	>6,200	37.5%
2020	1,050	395.04	0–3,300	0%	3,300–4,100	25%	4,100–6,200	30%	>6,200	37.5%
2021	1,050	323.75	0–4,000	0%	4,000–4,750	25%	4,750–6,900	30%	>6,900	37.5%

Note: Zambia's national poverty line is ZMW214 per adult per month (about US$1.2 per day in 2021 PPP terms). The monthly minimum wage rates of ZMW700 and ZMW1,050 are around US$109 and US$164 in 2021 PPP terms, respectively.

Source: information on tax bands and rates provided by the Zambia Revenue Authority (ZRA). Information on minimum wage changes taken from the ILO NATLEX database.

The minimum wage adjustment of interest that is analysed occurred in 2018—it became effective in September 2018 (Statutory Instrument No. 71, 2018). This policy change meant a 50 per cent increase in the legally binding wage floor in nominal terms and around 40 per cent increase in real terms, thus a large increase.

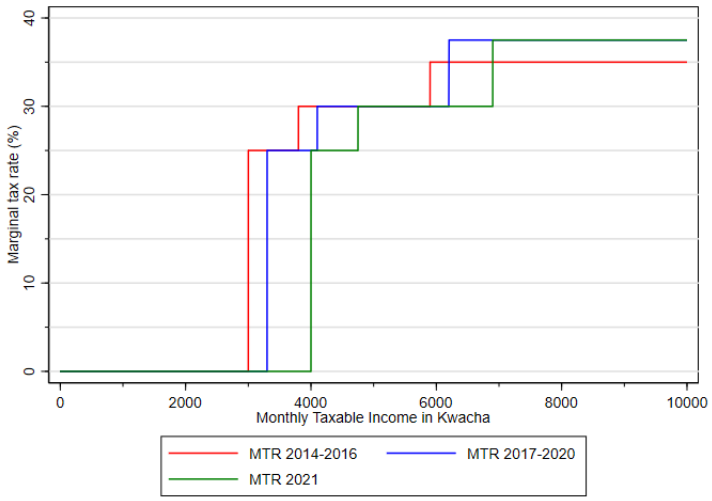
Table 1 also illustrates the tax bands and applicable rates set under the Zambian personal income tax schedule. Pay as you earn (PAYE) is a method of deducting tax from employees' emoluments in proportion to what they earn, and it applies to all offices and employments. Figure C1 in the Appendix shows that the personal income tax in Zambia currently raises revenue of 3.5 per cent of gross domestic product (GDP), or 18 per cent of total tax revenue (ZRA 2021b). In terms of proportion of total tax revenue, PAYE has been fairly stable since 2006, and after value-added tax (VAT), PAYE constitutes the most important source of tax revenue. In 2021 this changed, where tax revenue from company income tax and other taxes was slightly higher than PAYE returns. From 2013 to 2015, tax revenue accounted for 16 per cent of GDP. After a drop in 2016 it started increasing, to 19.7 per cent in 2021 (see also Figure C2 in the Appendix).

[4] The minimum wage in Zambia applies to all employees besides those working in government, local authority, occupations regulated by the Industrial and Labour Relations Act, domestic services (separate minimum wage law), management, or in a sector for which the minister, by statutory instrument, has prescribed the minimum wage.

[5] Data extracted on 10 May 2023 from the World Development Indicators database.

The PAYE tax is designed as a graduated schedule with a fixed marginal tax rate in each bracket, and therefore a kink at each bracket cut-off. As depicted in Figure 2, the tax rate increases over three kinks from 0 to 25 per cent to 30 per cent and finally to 35 per cent before 2016, and to 37.5 per cent since 2017. These kinks create strong incentives since the tax rate jumps are large and they arise at high income levels. The income level at which this first tax increase takes place—the value of the first kink point—has also increased steadily over the period 2014 to 2021. Figure 2 shows that during the period 2014–16, income above ZMW3,000 was liable to tax. This increased to ZMW3,300 for the period 2017–20 and finally ZMW4,000 in 2021. Given these kink locations, it becomes clear that the investigation of the first kink, in particular, is critical, as the jump from 0 tax to 25 per cent tax creates very strong incentives for bunching. Moreover, the first kink during the period 2014–16 and 2021 represents a round number, ZMW3,000 and ZMW4,000, respectively, and therefore a focal point that can create bunching for reasons other than the financial incentive (Kleven 2016).

Figure 2: Personal income tax (PAYE) schedule in Zambia



Note: ZMW3,000 is around US$979 in 2014 and US$469 in 2021 in 2021 PPP terms.
Source: author's illustration based on PAYE 2014–21 data.

This paper focuses on the change in the kink location in 2017, when it was revised upward from ZMW3,000 to ZMW3,300. This presents a 10 per cent increase in nominal or a 3.2 per cent increase in real terms in the tax kink. Hence, the effects arising from the tax kink adjustment should be a lot smaller than the ones due to the minimum wage adjustment.

3 Theoretical framework

This section outlines some theoretical considerations regarding the responses of formal and informal workers to changes in the minimum wage and the first tax kink and, thus, the expected wage and distributional effects.

3.1 Minimum wage adjustment

The expected impact of a minimum wage change depends on the view one takes of the labour market. Studies considering developing countries have typically examined the existence of dual labour markets, whereby there exists a formal sector with complete coverage and enforcement and an informal sector where enforcement is rare and incomplete (Gindling and Terrell 2007; Khamis 2013; Lemos 2009; Maloney and Mendez 2004). The standard Harris–Todaro labour market model predicts that after a minimum wage hike some formal workers become unemployed and seek employment in the informal

sector (Harris and Todaro 1970; Harrison and Leamer 1997). This increase in labour supply in the informal sector drives down the informal sector wage and increases informal employment, while the opposite occurs in the formal sector (Harris and Todaro 1970; Harrison and Leamer 1997).

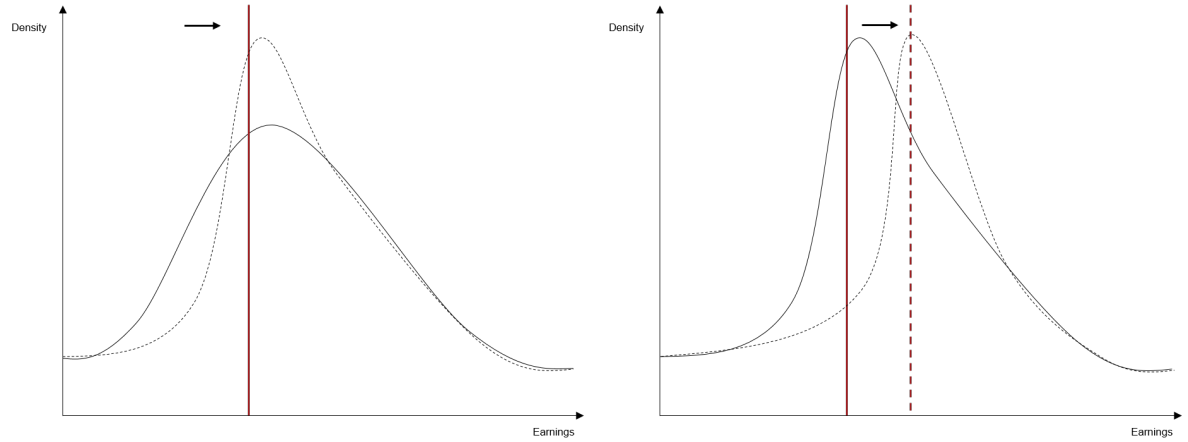
Other studies find that the minimum wage can have positive wage effects on the uncovered informal sector, and have explained this result with the theory of the *lighthouse effect*. The main explanation for this effect is that, despite operating in the formal sector, the minimum wage is used as a reference point for what is considered fair remuneration in the informal sector and increases the bargaining power of informal workers (Maloney and Mendez 2004). The formal sector wage increase may also induce composition effects, whereby capital is reallocated into the labour-intensive informal sector (Harrison and Leamer 1997) or low-skilled informal workers move into the formal sector, raising average productivity in the informal sector (Boeri et al. 2011; Fajnzylber 2001). The lighthouse effect theory does not provide clear predictions on the overall distributional effects, but it implies that both the formal and informal earnings distribution may shift to the right. The informal sector wage gap may initially increase, as informal workers already earning wages relatively close to the new minimum wage gain more in terms of wage bargaining power than those workers earning wages far below the new minimum wage. Over time this gap may decrease, as low-earning informal workers benefit from the increasing wage trend. The formal earnings distribution, as in the dual labour market model, should become narrower, assuming that the formal workers at the bottom of the distribution are most affected while high-earning formal workers are unaffected by the change in the wage floor. In addition to this initial effect, the wages of these lower-earning formal workers may continue to grow as competition with informal workers increases, driven by the lighthouse effect. Overall, these responses should lead to a declining wage gap in the formal sector and a declining wage gap overall, as both the generally lower-earning informal workers and the lower-earning formal workers experience wage growth.

Figure 3 illustrates the effect of the introduction of a minimum wage (Figure 3a) and a minimum wage hike (Figure 3b) in the formal sector in a developing country. In both models discussed above, the introduction of a minimum wage should push the left tail of the formal earnings distribution rightward, as employers react to the legally binding wage floor (besides some non-compliers). Because the higher end of the earnings distribution should not be effected (as much[6]), the overall distribution becomes narrower. A hike in the minimum wage further pushes the left tail of the distribution and the peak above the minimum wage rightward, thereby further increasing average wages and reducing the formal wage gap. Under the lighthouse effect theory, the effects on the informal wages and wage distribution should be similar but less pronounced due to the non-binding nature of the regulation. This would be in line with an exit view of informality, under which formal policies also have direct impacts on the informal sector and some informal employers choose to comply with these regulations while others do not (Maloney 2004). Under the dual labour market model, the informal distribution should not be affected or move leftward. This would be in line with an exclusion view of informality where the informal sector is disconnected from the formal sector and informal workers do not have access to formal sector benefits such as a minimum wage (Perry et al. 2007).

[6] Some studies (e.g. Maloney and Mendez 2004) document a numeraire effect whereby workers in the higher end of the distribution receive wages in multiples of the minimum wage.

Figure 3: Introduction of a minimum wage and minimum wage hike in the formal sector

(a) Introduction (b) Hike

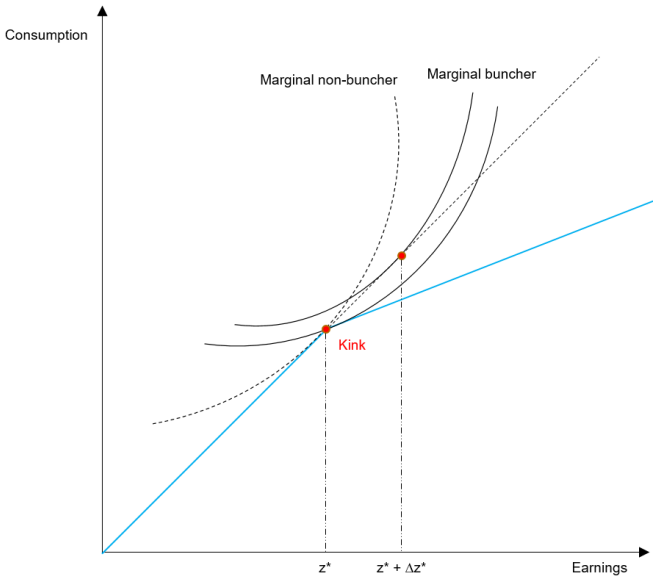


Source: author's illustration.

3.2 Bunching theory

A growing literature applies the bunching methodology developed by Saez (2010) and Chetty et al. (2011) to data from developing countries and documents behavioural responses by taxpayers (formal workers) to such kinks. The bunching methodology utilizes the predictions of a standard taxable income labour supply model. In this model, individuals' preferences are defined over after-tax income (consumption) and before-tax income (cost of effort). At the baseline, the tax system is smooth so that all individuals face the same marginal tax rate and individual optimization generates a smooth earnings distribution. Figure 4 illustrates the effect of the introduction of a kink—a discrete increase in the marginal tax rate—at an earnings threshold z^*. After the introduction of the kink, the individual initially located at $z^* + \Delta z^*$ is tangent to the kink point z^* and therefore moves down to the kink. This is the marginal bunching individual. All individuals initially located in the earnings interval $[z^*, z^* + \Delta z^*]$ move to the kink point z^*. Those individuals initially located above this interval reduce their earnings but stay in the interior of the upper bracket and do not move all the way to the kink point. All individuals earning below z^* remain subject to the baseline marginal tax rate and thus, absent any changes in incentives, the earnings distribution to the left of the kink is unaffected.

Figure 4: Bunching at a kink

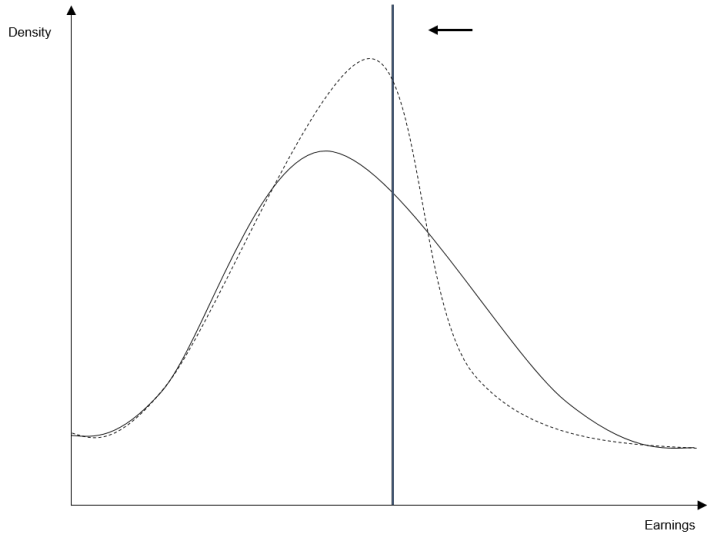


Source: author's illustration based on Kleven (2016).

Taken together, these responses produce excess bunching in the earnings distribution at the kink point. This does not produce a hole in the distribution above the kink, though, given the earnings response of the individuals who are initially located above the interval $[z^*, z^* + \Delta z^*]$ to the now higher marginal tax rate. These individuals reduce their earnings and thereby fill the hole left by the individuals that move to the kink point z^*.

Figure 5 shows the basic bunching result on the formal earnings distribution in the area surrounding the kink (local earnings distribution) after the introduction of a tax kink. The figure indicates that it is the mirror effect of the minimum wage and can be seen as a kind of wage ceiling. While workers earning below the kink should not be affected, those above have an incentive to reduce their wages to avoid the higher tax bracket so that the right tail of the local distribution moves leftward and an excess mass below the kink emerges (Kleven 2016; Saez 2010).

Figure 5: Introduction of a kink in the formal sector (local earnings distribution)



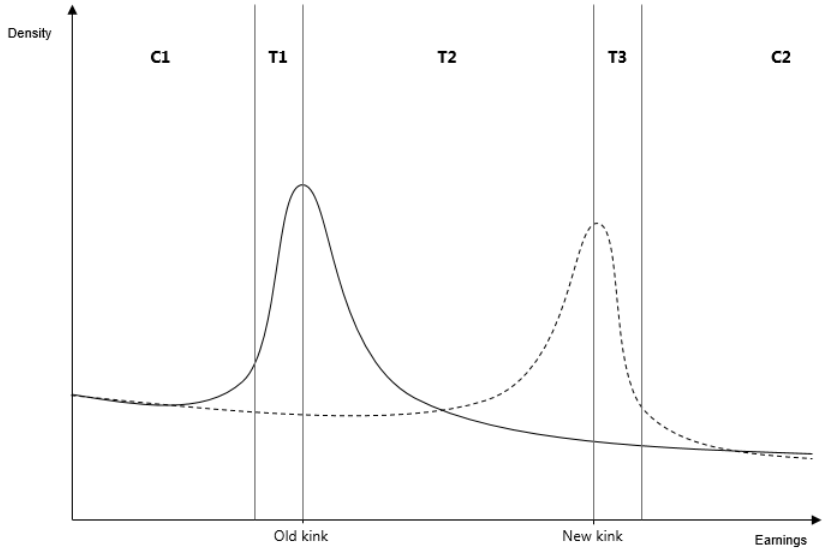
Source: author's illustration.

It is not well understood how the earnings distribution changes over time in response to the tax kinks and to alterations in the location of these tax kinks. Figures 6 and 7 depict a basic theory of change in regards to tax kinks. Figure 6 shows how the bunching changes, while Figure 7 shows the aggregate changes in the local earnings distribution.

In Figure 6 the solid black line shows the bunching result with the excess mass in the earnings distribution around the old kink that arises from the dynamics discussed before in Figure 4. The dashed black line shows the bunching result at the new kink. In the figure, the earnings distribution is divided into different sections that respond differently to the change in the location of the tax kink. Sections C1 and C2 consist of control individuals who are not affected by the policy change. Individuals in C1 are *non-taxpayers* and are below both the old and the new kink and are therefore not affected by the changes in the incentive structure. Similarly, individuals in C2 are *always-taxpayers* and are well above the kink, which means that they pay tax both before and after the kink change and that they are too far above the kink to be incentivized to bunch at the kink. T1 shows the *previous-bunchers*, the individuals that bunch at the old kink (corresponding to the earnings interval $[z^*, z^* + \Delta z^*]$). After the kink is revised upwards, these individuals no longer have an incentive to artificially reduce their wages to avoid paying taxes and they should see their wages grow relatively fast after the tax policy change. Individuals in T2, the *old non-bunchers*, previously experienced a drag on their wage growth given that they were eligible to pay tax but they do not bunch at the old kink (they correspond to the individuals located above $[z^*, z^* + \Delta z^*]$, some of whom reduce their income in response to the tax kink). After the policy change they are relieved from the higher tax rate so they should experience wage growth, albeit slower than

the *previous-bunchers*. After the introduction of the new kink, individuals up to a marginal bunching individual—the *new-bunchers*—will reduce their wages and bunch at the new kink. These are located in section T3.

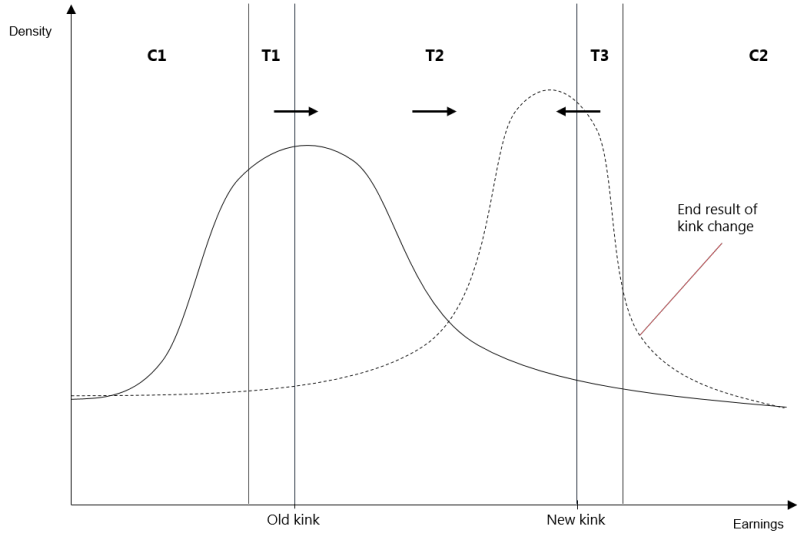
Figure 6: Theory of change: bunching



Source: author's illustration.

Figure 7 illustrates the expected aggregate changes arising from the change in the kink location. The local earnings distribution is expected to shift to the right and reduce the local wage gap, resulting in the dashed line, as individuals in T1 and T2 see their wages grow while individuals in T3 reduce their wages. The distribution therefore becomes thinner (less dispersed) and has a higher peak than previously (solid black line).

Figure 7: Theory of change: aggregate



Source: author's illustration.

It is important to keep in mind that tax kinks produce responses in local earnings distributions. Therefore, the overall effect on the earnings distribution should be a lot smaller than that of the minimum wage. While a minimum wage directly affects all workers with wages below the wage floor (at least in the covered sector), tax kinks typically only generate responses by individuals in the surrounding region and more indirectly via changing the incentives for wage reporting.

The effects of an introduction as well as a change in the first tax kink on the local formal wage distribution may also appear in the informal sector but to a weaker extent. The first kink in the tax schedule may also serve as a reference point for wage bargaining in the informal sector in the form of a *new lighthouse effect*, especially if there are clear responses in the formal sector. Specifically, after the introduction of a tax kink, employers may reduce the wages of their informal workers who earn above this kink in view of this new wage ceiling and due to the decreasing after-tax wages of formal workers (as depicted in Figure 5). An upward revision in the location of the first kink and, hence, a rise in the wage ceiling combined with increasing after-tax formal wages in the local distribution may also lead to increasing informal wages and a rightward shift in the local informal wage distribution.

3.3 Summary and comparison

I propose a theoretical framework whereby wage and distributional effects in the formal and informal sector of a minimum wage adjustment and a change in the location of the first tax kink are similar. However, the dynamics that produce these effects differ and the kink effects take place in the local earnings distribution around the kink, whereas the minimum wage effects arise in the aggregate distribution. The starting point is that the introduction of a kink mirrors the effect of the introduction of a minimum wage in the formal sector. While the latter pushes the right tail of the (local) distribution leftward and creates excess mass below the kink (thereby serving as a kind of wage ceiling), the former moves the left tail of the formal wage distribution rightward and creates excess mass above the minimum wage (wage floor). An increase (*change*) in the location of either should produce similar effects by shifting the left tail of the earnings distribution and the excess mass (peak of the distribution) rightward, as the legally binding wage floor and the non-binding but influential wage ceiling are raised. Thereby average wages are expected to increase and the wage gap to decrease. The dynamics that produce this overall effect differ—legal requirement versus changes in the incentive structure. Whether these effects spill over into the informal sector depends on the view one takes on the labour market. The lighthouse and the new lighthouse effect differ in the sense that the former may increase the bargaining power of informal workers by shifting perceptions of what is considered a fair wage (Maloney and Mendez 2004; Dinkelman and Ranchod 2012), while the latter may increase the bargaining power of employers vis-a-vis informal workers by introducing a wage ceiling and putting downward pressure on after-tax formal wages.

4 Methodology

4.1 Data

Labour Force Survey data

The Zambian LFS data is based on a household-based sample survey that is conducted quarterly and provides information on the labour market activities of individuals in selected households. It includes information on both the formal and informal economy, and therefore the LFS data is used to incorporate the informal sector into the analysis. I have access to the annual data for the years 2012, 2014, and 2017–21, which arises from combining the respective years' quarterly datasets.

For the analysis, the wages of formal and informal workers is the main outcome variable of interest and, therefore, some additional adjustments were made in regards to this variable. The LFS data provides information on the hours worked per week, days worked per week, and the frequency at which an individual receives their income. Individuals may report their hourly, daily, weekly, bi-weekly, monthly, or annual wage. To ensure the comparability of wages across individuals and over time, wages are recalibrated based on a 48-hour workweek (the legal maximum working hours per week in Zambia). Individuals who report a negative income as well as those who do not report work hours, days worked,

or the frequency of earnings were coded as not reporting a wage income. In the end, this leaves 42,598 individuals that are used for the analysis, or around 12–13 per cent of the raw data in each year.

PAYE income tax data

The PAYE data only covers the formal economy in Zambia, in the sense that it includes information on enterprises that are registered with the Zambia Revenue Authority (ZRA). The chargeable emoluments are the main variable of interest since these represent the formal workers' wages that are liable to income tax. Three aspects of the data had to be adjusted to create a panel of formal workers whose wages can be traced over time: (1) create a unique ID; (2) identify outliers; and (3) collapse the data to the annual level (a detailed explanation is provided in Appendix A).

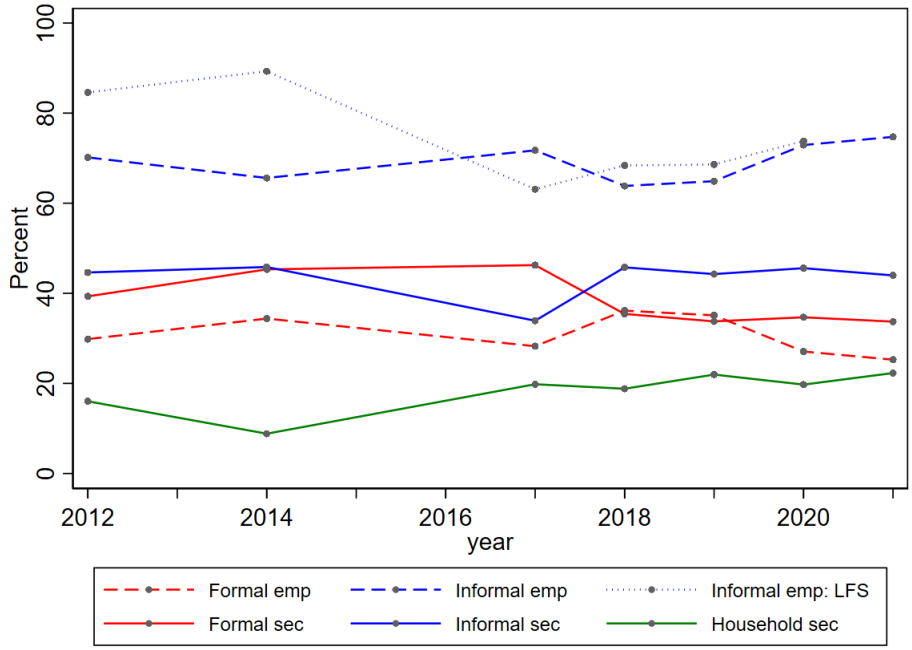
4.2 Definition of informality

The Zambia Statistics Agency (ZamStats), following the ILO guidelines, distinguishes between sector of employment—formal, informal, or household sector—and type of employment—formal or informal work. The sector of employment derives from whether the business is registered with the relevant authority and registered with the ZRA, while the type of employment derives from whether individuals benefit from at least one social security measure. The latter definition of informality directly concerns the employment arrangements of workers, and therefore this is used as the main definition of informality.[7] Workers are categorized into formal or informal employment depending on whether they have access to a social security scheme, paid leave, paid sick leave, or paid paternity/maternity leave. It is important to use a job-based (rather than an enterprise-based) definition of informality since informal sector employment does not capture all jobs characterized by informal employment arrangements. For example, workers in the formal sector (in a fully registered business) may still not have access to a minimum wage if they are informally employed (do not have access to social security benefits).

Figure 8 shows the evolution of the share of workers employed in the formal, informal, and household sectors, as well as formally and informally employed workers over the period 2012–21. It highlights the important distinction between an informality definition that takes the perspective of enterprise versus one that looks at workers. The figure shows that informal employment, around 70 per cent of employment, is a lot higher than informal sector employment, around 45 per cent of employment. This suggests that many workers who do not work in the informal sector are still informally employed—most of these are in the household sector but also in the formal sector.

[7] This is the standard definition of informality used in minimum wage studies—see, for example, Dinkelman and Ranchod (2012), Khamis (2013), and Katzkowicz et al. (2021).

Figure 8: Sector and type of employment: share of total employment 2012–21



Source: author's illustration based on LFS 2012–2021 data.

4.3 Identification strategy

Minimum wage adjustment

I employ a difference-in-difference estimation using the differential bite of the minimum wage across districts in Zambia and LFS data to identify the effect of the minimum wage hike on formal and informal workers' wages. This approach has become quite common in the minimum wage literature and consists of comparing more and less affected groups due to geographical location (e.g., Derenoncourt et al. 2021; Khamis 2013; Stewart 2002). The idea behind this method is that the 'bite' of the minimum wage change varies across geographical areas—Zambia's districts in this case—depending on the share of 'low-paid' formal workers who are paid below the new minimum wage before the policy change (Figure C3 in the Appendix provides a map of Zambia's districts).

The following difference-in-difference estimator then gives the impact on the high-impact group, the mean treatment effect on the treated:

$$\hat{\theta}_{DiD} = (\bar{W}_{dt} - \bar{W}_{dt-1})_T - (\bar{W}_{dt} - \bar{W}_{dt-1})_C \tag{1}$$

The first bracket is the before–after difference of the mean outcome variable $\bar{W}$ for the treatment group T at time t, after the minimum wage increase, and at time $t-1$, before the policy change. The second bracket represents the same difference but for the control group C. Districts are separated into a treatment group and a control group depending on the initial proportion of formal workers earning below the new minimum wage in 2018. Specifically, the average proportion of formal wage earners below the new minimum wage across districts is calculated, and the treatment and control groups are defined as above-or-below-average impacted districts—that is, whether a district has a higher or lower proportion than this overall average. The policy was implemented in September 2018, and an underlying assumption in this set-up is that its impact becomes visible in 2019 while 2018 wages are largely unaffected. This set-up allows testing for the existence of the lighthouse effect. By definition, treated districts have more low-earning formal workers, who are more affected by the policy change, and the signalling effect of the minimum wage change should be greater than in control districts as more informal workers are

aware of the policy change and use this information in their wage bargaining. Other potential lighthouse channels such as composition effects—capital reallocation via formal workers moving to the informal sector—should also be greater.

The simple estimator in Equation 1 can be interpreted as the causal effect of the 2018 minimum wage change under the assumption that in the absence of the policy change the change in average wages would not have been systematically different in low-impact and high-impact districts (parallel trends assumption). The data only allows observing two pre-treatment periods—2017 and 2018—and therefore it is not possible to fully compare the pre-trends of the control and treated districts. However, by specifying the following regression at the individual level, key factors that may affect the assignment into treatment and the outcome variable can be taken into account, thereby minimizing the bias arising from differing underlying trends:

$$W_{idt} = \beta_0 + \beta_1 Post_t + \beta_2 Post_t D_d + \beta_3 X_{idt} + \pi_d + \varepsilon_{idt} \tag{2}$$

In Equation 2, W_{idt} is the wage of an individual i in district d and year t. The regression is run separately for all workers, formal workers only, and informal workers only. $Post_t$ is a time dummy that takes the value 1 for the years from 2019 onward and 0 for the years before. The interaction term $Post_t D_d$ is a dummy variable that takes the value 1 if an observation is in the treatment group and in the period after the minimum wage has increased (i.e. 2019 or after). Thus, the coefficient β_2 captures the causal effect of the treatment on the outcome for this group—the effect of the minimum wage change on districts where the minimum wage bite is strong—which is the estimator of interest $\hat{\theta}_{DiD}$ from Equation 1. Other factors that change over time, may differ between the two groups, and could influence both the assignment into the treatment group and wages are captured in X_{idt}. District fixed effects, π_d, are used in all specifications to ensure that only individuals within the same districts are compared before and after the minimum wage change. Following Dinkelman and Ranchod (2012) and Khamis (2013), clustered standard errors are used at the level at which the treatment is defined. In this case, ε_{idt} is a clustered standard error at the district–year level to account for the potential issue that some unobserved factors not captured in Equation 2, which affect individuals in the same district and the same year, may be correlated. A final adjustment that is made is to exclude districts with fewer than ten observations when defining the assignment into treatment and control groups and running the regression in Equation 2. This is to ensure that the effect is not driven by outliers.

Tax kink adjustment

The analysis of the tax kinks mirrors the minimum wage analysis, as a difference-in-difference approach is used to examine the change in the location of the first kink in the PAYE schedule in the return year 2017 from ZMW3,000 to ZMW3,300. Section 3.2 laid out how different parts of the local earnings distribution are expected to be affected and how they can be categorized into control and treatment groups. The main outcome of interest is how the wages of the *previous-bunchers* (T1), the *non-bunchers* (T2), and the *new-bunchers* (T3) are affected. Therefore, they are defined as three treatment groups that are analysed separately. The *always-taxpayers* (C2) are used as the control group in all three regressions and are defined as those earning above or equal to ZMW6,500 and below or equal to ZMW10,000. This way they are well above the third kink at ZMW5,900 and ZMW6,200 in 2016 and 2017, respectively, and hence should not be affected by the tax kink changes. Treatment group T1 is defined as those earning between ZMW2,900 and ZMW3,000—the tax kink—in 2016. In terms of distance to the kink, this is around 3 per cent, which is the same as used in other bunching papers to define the bunching window (e.g. Bell 2020; Boonzaaier et al. 2019). Group T2 is defined as those earning just above the old kink and below the new kink in 2016—that is, the earnings interval $[3001, 3299]$. In line with the earnings interval for the *previous-bunchers*, the *new-bunchers* (T3) are defined as those earning above ZMW3,300 and below or equal to ZMW3,400.

$$w_{it} = \beta_0 + \beta_1 Post_t D_i + \alpha_i + \gamma_t + \varepsilon_{it} \tag{3}$$

In Equation 3, w_{it} is the wage of individual i in year t. The interaction term $Post_t D_i$ is a dummy variable that takes the value 1 if an observation is in the treatment group and the period after a tax kink has been revised upward (i.e. 2017 or after). In the PAYE data, potentially relevant worker characteristics that affect wages, such as gender, are not fully observed, but individual fixed effects α_i are included so that the same individuals are compared with each other over time, and these factors should cancel out via differencing. Year fixed effects γ_t are also included to account for any unobserved time trends, and ε_{it} is a statistical noise term clustered at the individual level (the level at which the treatment is defined).

5 Results

5.1 Minimum wage

Graphical evidence

Figure 9 depicts kernel density plots of the informal (solid black line) and formal (dashed black line) earnings distributions, as well as the applicable minimum wage red vertical line in the years 2012, 2014, and 2017–21. For the years 2012 and 2019, the previous year's minimum wage is also displayed as a dotted vertical red line. All information is presented in real terms, deflated using the annual consumer price index (CPI).[8]

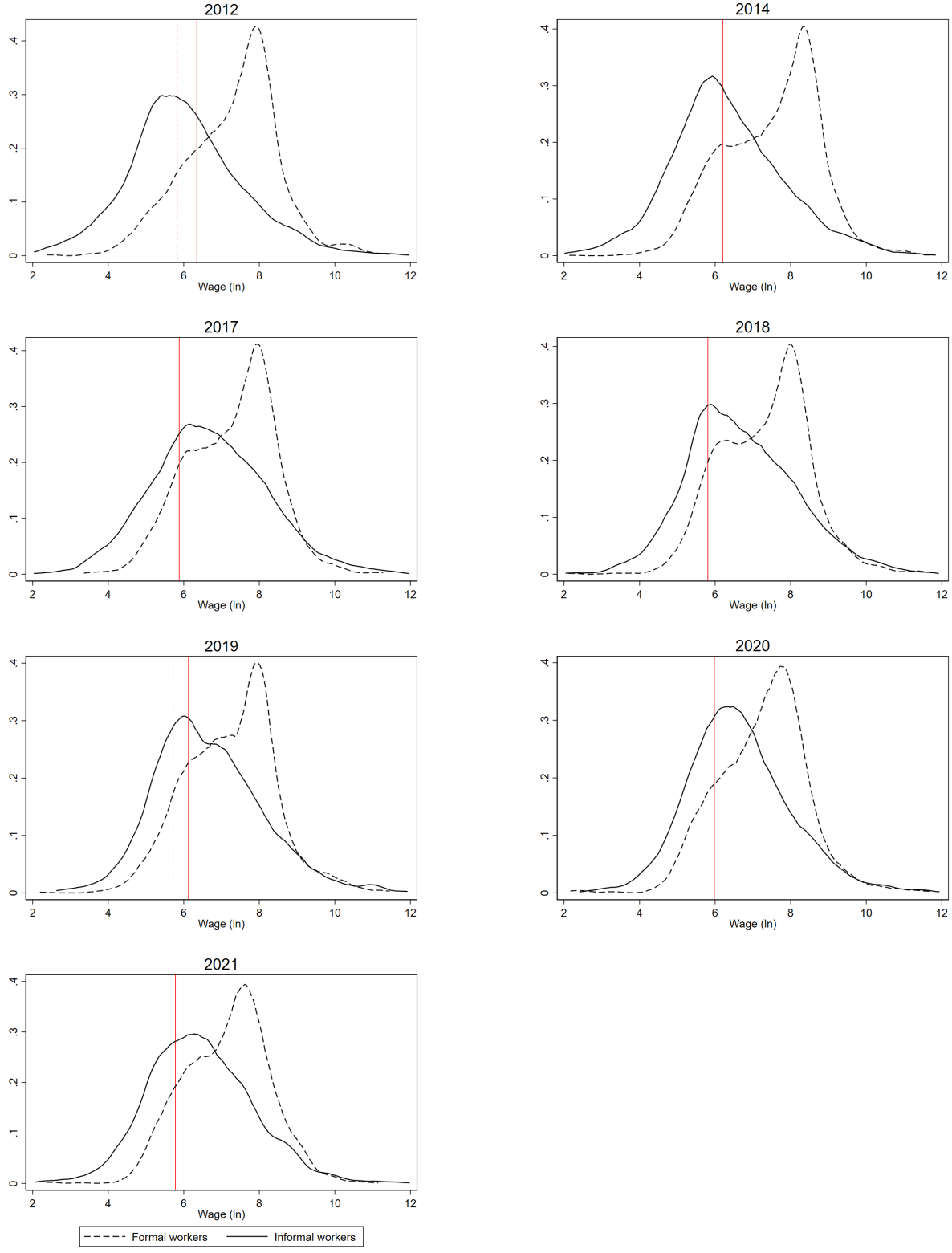
The informal earnings distribution lies to the left of the formal distribution in all years, as has been found for other countries in the lighthouse effect literature (Khamis 2013; Maloney and Mendez 2004). Both formal and informal workers appear to respond strongly to the minimum wage adjustments in 2012 and 2018 and these responses unfold over time. Formal workers who earn wages well above the minimum wage do not appear to be affected, which can be seen in the peak that remains roughly the same at a logarithmic wage of 8 or around ZMW3,000. The theory predicts that the left-hand tail of the formal earnings distribution should be impacted by minimum wage adjustments, and this is the case. The left tail shifts rightwards year-by-year, and in 2014, 2017, and 2018 there is visible bunching at or just above the minimum wage.

The peak of the informal earnings distribution also shifts rightwards over time, in line with the reference point theory by Maloney and Mendez (2004). While it lies below the minimum wage in 2012 and 2014, it moves closer to this wage floor and in 2017 and 2018 moves above it, indicating that the majority of informal workers now earn above the minimum wage. In 2018, there is clear bunching of informal wages at the minimum wage. The fact that the response takes place over time and is not immediate makes sense, because the potential channels explaining this lighthouse effect—composition effects or increasing bargaining power via reference points—do not unfold immediately—that is, labour mobility and wage setting are not completely flexible. It also appears that as informal wages increase, this puts upward pressure on formal wages. Formal wages do not remain at the minimum wage level, the legally binding wage floor, but continue to increase, narrowing the formal wage distribution—this is most visible in 2019 and 2020. The development in the non-compliance rate—the share of workers who receive wages below the minimum wage—over the period of analysis also speaks towards the validity of the lighthouse effect theory, as it suggests that the minimum wage is binding in both sectors. It declines among both formal and informal workers throughout the period 2012–21, except for a jump in 2019 after the minimum wage hike in 2018 and a small increase in the informal sector in 2021. Initially in 2012, 22.3 per cent of formal and 62 per cent of informal workers received wages below the minimum wage. This share declined steadily to 12.2 and 32.9 per cent, respectively, in 2021. The fact that there is a large overlap between the two distributions further indicates that a dual labour market view with segmented

[8] CPI data was extracted from ZamStats on 19 May 2023.

labour markets (Harris and Todaro 1970) does not seem to apply in the case of Zambia. Rather, there are many workers who earn formal sector wages—that is, they have access to a minimum wage but lack other formal employment benefits.

Figure 9: Minimum wage effects in the formal and informal sector 2012–21



Note: the solid black line is the informal and the dashed line is the formal workers' earnings distribution. The solid vertical red line is the effective minimum wage and the dotted vertical red line is the previous year's minimum wage.
Source: author's illustration based on LFS 2012–21 data.

Overall, the graphical evidence strongly indicates the validity of the lighthouse effect theory rather than the dual economy theory in the case of Zambia. The distributional changes in the formal and informal earnings distribution are mostly in line with the theoretical predictions in Section 3 (a rightward shift of the left tail of both distributions and an excess mass above the minimum wage; however, no clear narrowing of the distribution). The next section presents the results from the identification strategy developed in Section 4.3 to identify the formal and informal wage effects of the minimum wage hike in 2018.

Difference-in-difference results

A simple estimation of $\hat{\theta}_{DiD}$ from Equation 1 can be illustrated using a two-by-two table. Table 2 shows that an individual working in a district with above-average formal workers has on average a 0.16 higher wage (in logarithm) than an individual in the control group in 2019, and this difference is significant at the 1 per cent level. This overall effect is driven by both the increase in formal workers' wages (logarithm) in the treated groups following the minimum wage change, which are 0.17 higher, and the informal ones, which are 0.16 higher. Both differences are significant at the 5 per cent level.

Table 2: Changes in average real monthly income (ln) (overall, formal, and informal): before and after the minimum wage hike

	Log(real wage)			Log(real formal wage)			Log(real informal wage)		
	T	*C*	*Diff*	*T*	*C*	*Diff*	*T*	*C*	*Diff*
Observations	4,326	5,988	10,314	1,476	2,225	3,701	2,850	3,763	6,613
Pre: 2018	6.74	7.06	–0.32***	7.01	7.54	–0.52***	6.60	6.78	–0.19***
	(0.028)	(0.024)	(0.037)	(0.044)	(0.032)	(0.054)	(0.036)	(0.032)	(0.048)
Post: 2019	6.77	6.93	–0.16***	7.04	7.40	–0.36***	6.64	6.66	–0.02
	(0.033)	(0.028)	(0.043)	(0.051)	(0.039)	(0.064)	(0.042)	(0.036)	(0.055)
Diff	0.03	–0.13***	0.16***	0.03	–0.14***	0.17**	0.04	–0.12**	0.16**
	(0.044)	(0.037)	(0.057)	(0.068)	(0.051)	(0.084)	(0.055)	(0.049)	(0.074)

Note: significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: author's calculation based on LFS 2012–21 data.

This result is based on a simple pre- versus post-policy change comparison across the two groups without controlling for other potentially relevant factors. The regression in Equation 2 was run to better identify the effect of the minimum wage change and minimize any potential bias.

Table 3 presents the regression results for the minimum wage hike effect. Columns (1), (4), and (5) show the basic pre- versus post-policy change comparing wages across the control and treatment groups in 2018 and 2019, and mirror the results presented in Table 2. The remaining columns show results for the period 2017–20 (i.e. two pre- and two post-years). Columns (2), (5), and (8) display the results after controlling for a few basic worker characteristics such as age, gender, education, and experience. Columns (3), (6), and (9) show the results after including additional controls, the industry, workplace, and size of the firm that an individual works in, as well as year fixed effects. The effect of the minimum wage adjustment on overall, formal, and informal wages shrinks by roughly 5 per cent in columns (3), (6), and (9) compared to the basic result, and is less well determined but remains significant at the 10 per cent level. Individuals in districts where the policy change has a strong bite, on average, see a 10.4 per cent increase in their wages after the policy shock compared to individuals in low-bite districts. The effect is driven by both formal and informal workers. In column (5), the effect on formal wages becomes insignificant but turns significant again at the 10 per cent level when adding the additional controls—industry, workplace, and firm size fixed effects—and year fixed effects. In other words, only when comparing similar formal workers according to these categories do we see a smaller but significant effect. Formal workers in high-impact districts on average see their wages grow by 11.5 per cent, and this effect is significant at the 10 per cent level. For the informal workers, the effect also decreases by around 1 per cent after controlling for worker characteristics, but the effect remains significant at the 5

per cent level. After including the additional controls and year fixed effects, the effect shrinks to 11.9 per cent and is significant at the 10 per cent level.

Table 3: DiD results: formal 'bite'

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Wage (ln)	Wage (ln)	Wage (ln)	Formal wage (ln)	Formal wage (ln)	Formal wage (ln)	Informal wage (ln)	Informal wage (ln)	Informal wage (ln)
T	–0.320***			–0.525***			–0.185***		
	(0.037)			(0.055)			(0.048)		
D (Y>2018)	–0.128***	0.046		–0.140***	–0.007		–0.120**	–0.098*	
	(0.037)	(0.067)		(0.051)	(0.076)		(0.048)	(0.056)	
$T \times$ D	0.157***	0.144*	0.104*	0.169**	0.159	0.115*	0.163**	0.152**	0.119*
	(0.057)	(0.087)	(0.055)	(0.085)	(0.107)	(0.067)	(0.073)	(0.076)	(0.071)
Age		0.061***	0.046***		0.054***	0.041***		0.047***	0.044***
		(0.006)	(0.006)		(0.011)	(0.010)		(0.007)	(0.007)
Age^2		–0.001***	–0.000***		–0.001***	–0.000***		–0.000***	–0.000***
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Gender		0.063***	0.115***		–0.134***	–0.031		0.115***	0.171***
		(0.024)	(0.022)		(0.033)	(0.029)		(0.031)	(0.032)
Lower sec.		0.057	0.034		0.251***	0.155***		0.056	0.045
		(0.035)	(0.034)		(0.060)	(0.056)		(0.038)	(0.037)
Upper sec.		0.356***	0.236***		0.800***	0.525***		0.249***	0.218***
		(0.034)	(0.038)		(0.059)	(0.046)		(0.042)	(0.045)
Tertiary		0.644***	0.625***		1.389***	1.088***		0.165***	0.320***
		(0.049)	(0.064)		(0.066)	(0.065)		(0.051)	(0.083)
Experience		0.044***	0.033***		0.054***	0.031***		0.032***	0.028***
		(0.004)	(0.004)		(0.006)	(0.005)		(0.005)	(0.004)
Experience^2		–0.001***	–0.001***		–0.001***	–0.001***		–0.001***	–0.001***
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Local gov.			–0.277***			–0.190***			–0.090
			(0.069)			(0.063)			(0.219)
Parastatal/ SOE			–0.371***			–0.204***			–0.605**
			(0.085)			(0.070)			(0.253)
Embassy/ int. org.			–0.394**			–0.349*			–1.473***
			(0.170)			(0.201)			(0.181)
NGO			–0.211**			–0.071			–0.120
			(0.104)			(0.125)			(0.230)
Faith-based org.			–0.704***			–0.685***			–0.477***
			(0.086)			(0.100)			(0.179)
Business/ farms			–0.797***			–0.610***			–0.645***
			(0.059)			(0.060)			(0.164)
Producers' cooperative			–0.955***			–0.979***			–0.737***
			(0.110)			(0.243)			(0.202)
Household			–0.697***			–0.503***			–0.516***
			(0.070)			(0.114)			(0.167)
Constant	7.062***	4.944***	5.803***	7.536***	4.822***	5.388***	6.781***	5.354***	5.816***
	(0.024)	(0.121)	(0.142)	(0.032)	(0.207)	(0.214)	(0.032)	(0.139)	(0.221)
District FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Add. controls	No	No	Yes	No	No	Yes	No	Yes	Yes
Observations	10,314	20,896	20,896	3,701	6,701	6,701	6,613	14,192	14,192
R^2	0.01	0.09	0.18	0.03	0.27	0.40	0.00	0.06	0.09

Note: clustered standard errors at the district–year level in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Additional controls are industry and firm size. Columns (1), (4), and (7) are based on the pre- versus post-treatment comparison using a simple OLS regression and 2018/19 data and mirror the results shown in Table 2. The remaining columns are based on 2017–20 data.

Source: author's calculation based on LFS 2012–21 data.

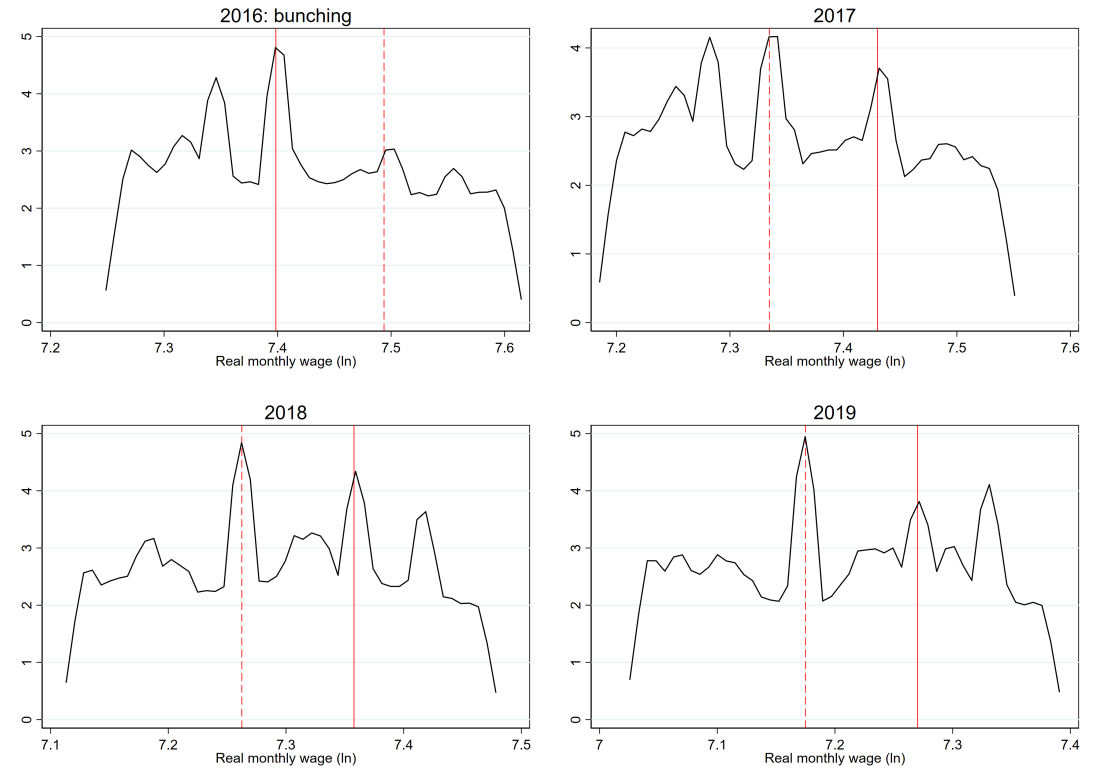
These results confirm the graphical evidence in the previous section that the lighthouse effect is at play in Zambia—that is, both formal and informal workers' wages increase following the minimum wage hike in 2018. The effect in the informal sector is slightly larger, which has also been found by Maloney and Mendez (2004) and Khamis (2013). The wage effects are significant and economically meaningful—a 40 per cent real increase in the minimum wage leads to a real increase in wages of 10 per cent overall and around 12 per cent in formal and informal workers' wages (in high- compared to low-impact districts). This means that employers passed on around one-quarter of the policy change to their employees. This lighthouse effect result only holds for one of the two robustness checks presented in Table B2 in the Appendix. Overall, the evidence still strongly suggests that this effect is at play. The even larger result on informal wages obtained from one of the robustness checks compared to the result in Table 3 supports the conclusion from the baseline result that not only formal but also informal wages increase following the minimum wage hike, as predicted by the lighthouse effect theory.

5.2 Tax kinks

Graphical evidence

Figure 10 depicts the formal earnings distribution based on the PAYE data for the years 2016–19. The vertical solid red line marks the effective tax kink, which is located at ZMW3,000 in 2016 and ZMW3,300 from 2017 onward, while the dashed line indicates the previously (or not yet) effective tax kink. Since tax kinks are expected to produce effects in the surrounding earnings distribution, Figure 10 shows kernel density plots for the earnings interval [2600;3700]. This interval includes both the old and new kink and a buffer of individuals earning up to ZMW400 below the old or above the new kink. The interval was chosen so that the second kink in the tax schedule, which is located at ZMW3,800 before and ZMW4,100 from 2017, does not influence the result.

Figure 10: Tax kink adjustment effects in the formal sector 2014–21



Note: the solid vertical red line is the effective tax kink and the dotted vertical red line is the previous year's tax kink. The graphs on the left illustrate the bunching effects and the graphs on the right show the aggregate effects.
Source: author's illustration based on PAYE 2014–21 data.

Figure 10 illustrates strong bunching at the old kink in 2016, in line with the predictions from the bunching theory—these are the previous-bunchers (group T1) in Section 3.2. There is no clear bunching at the new kink, which is not effective that year, but a small hump just above. These individuals are the new-bunchers (group T3), who are expected to reduce their wages and bunch at the new kink at ZMW3,300 once it becomes effective. The area between the solid and dashed red line consists of the non-bunchers (group T2), who are liable to pay tax. The second peak to the left of the kink may be due to 'round-number bunching', as round numbers serve as focal points for wage setting (Kleven 2016). From 2017 onward, when the new tax kink becomes effective, one can see a clear excess mass developing at the new kink in line with the theoretical prediction in Figure 6. Moreover, an additional hump in between the old and the new kink gradually emerges and moves rightward. This matches the prediction that the non-bunchers (T2), who are now relieved from tax liability, see faster wage growth. This also suggests that the left tail of the local earnings distribution shifts rightward, just like the left tail of the overall distribution following the minimum wage hike. In 2017, the excess mass at the old kink remains, but has a wider tail on the right side which indicates that some individuals—possibly the previous-bunchers (T1)—are not bound by the kink anymore. The fact that strong bunching remains at the old kink (dashed red line) suggests that a lot of the initial bunching response is due to incentives other than financial ones such as round-number bunching.

Figure C5 in the Appendix shows the same distribution with a larger kernel to get an idea of the aggregate effects. It shows that the overall peak lies to the left of the kink before the change in the first tax kink. Over time, this moves rightward, which matches the theoretical prediction of generally increasing wages in the local earnings distribution, and is similar to the movements in the peak of the formal and informal earnings distribution following the minimum adjustments that were discussed in Figure 9.

Table B1 in the Appendix shows that before the tax kink adjustment, average nominal wages in the local earnings distribution are falling, in line with the drag on the wage growth of individuals above the kink. From 2017 onward, however, they are increasing. Neither graphically, in Figure 10 and Appendix Figure C5, nor from the development in the interquartile range in Table B1 can a clear pattern in regards to the local earnings gap be discerned—that is, whether there is wage compression. Thus, as with the minimum wage hike, the theoretical prediction of a decreasing wage gap (reduced inequality) cannot be confirmed. Overall, the distributional dynamics match the predictions from the theory and are similar to the minimum wage dynamics.

Difference-in-difference results

As in the minimum wage analysis, one can begin with a simple estimation of the before 2017 versus after difference in real wages for the three treatment groups, T1, T2, and T3, compared to the control group. Table 4 shows that all three groups have higher wages, around 0.08 and 0.07 for the two latter groups, in logarithmic terms. This effect is significant at the 1 per cent level and is driven largely by a strong decline in real wages in the control group in 2017 compared to 2016. The previous-bunchers and non-bunchers see a well-determined increase of around 0.01, while the new-bunchers wages are unchanged in the simple pre- versus post-treatment comparison. While the result for the groups T1 and T2 mostly matches the theoretical prediction, the result for the new-bunchers is the opposite of what was expected. Since the new-bunchers are just above the new kink, a decline in their wages was predicted given that they can avoid paying the higher tax rate by slightly reducing their wages. The result in Table 4 suggests, however, that the new-bunchers do not bunch at the new kink at ZMW3,300. Moreover, a slightly larger wage increase for the previous-bunchers than for the non-bunchers was also expected since these are the workers that had higher wages in the first place but artificially reduced them to bunch at the old kink. This is not visible in Table 4.

Table 4: Changes in average real monthly income (ln) (T1, T2, and T3): before and after the tax kink change

	T1: Previous-bunchers			T2: Non-bunchers			T3: New-bunchers		
	T1	C	Diff	T2	C	Diff	T3	C	Diff
Observations	14,761	95,750	110,511	31,626	95,750	127,376	8,917	95,750	104,667
Pre: 2016	7.39	8.37	–0.99***	7.44	8.37	–0.93***	7.51	8.37	–0.87***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)
Post: 2017	7.40	8.31	–0.91***	7.45	8.31	–0.86***	7.51	8.31	–0.79***
	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)	(0.002)	(0.004)	(0.001)	(0.003)
Diff	0.01***	–0.07***	0.08***	0.01***	–0.07***	0.07***	0.00	–0.07***	0.07***
	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.003)

Note: significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: author's calculation based on PAYE 2014–21 data.

The regression results in columns (1), (4), and (7) of Table 5 match the results presented in Table 4. In columns (2), (5), and (8), a panel regression using 2016–18 data is run and individual and year fixed effects are included. The effect for T1 shrinks by 1.2 per cent, while it increases slightly for T2—both remain statistically significant at the 1 per cent level. This makes sense, since the previous-bunchers may mainly see an initial wage effect after the kink is revised upward, whereas the non-bunchers experience a maintained faster wage growth. The non-bunchers group likely consists of more productive workers who previously were impaired by a drag on their wage growth due to tax liability. The previous-bunchers, on the other hand, consist of many workers who presumably artificially receive reduced wages to avoid paying taxes. Therefore, right after the tax kink adjustment, these workers see a large increase in wages but over a longer period the non-bunchers see a larger increase in wages. This is in line with the emerging hump between the old and the new kink seen in Figure 10. The effect for the new-bunchers in column (8) remains positive and significant, which confirms that these formal workers do not bunch at the new kink. On the contrary, the upward revision of the kink appears to push these workers' wages upward. This means that the excess mass at the new kink in Figure 10 cannot be explained by the dynamics in group T3. In the remaining columns (3), (6), and (9), year–location–industry fixed effects are included to test whether the result holds when comparing only individuals in the same year, jurisdiction, and broad industry. The result becomes slightly larger for T1, quite a lot larger for T2, and stays more or less the same for T3 compared to the baseline result.

Table 5: DiD results: tax kink

Dep. var.:	T1: Previous-bunchers			T2: Non-bunchers			T3: New-bunchers		
Ln(wage)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	–0.988***			–0.931***			–0.866***		
	(0.001)			(0.001)			(0.001)		
D (Y>2016)	–0.067***			–0.067***			–0.067***		
	(0.001)			(0.001)			(0.001)		
$T \times D$	0.078***	0.066***	0.084***	0.073***	0.074***	0.089***	0.072***	0.063***	0.073***
	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)
Constant	8.374***	8.228***	8.228***	8.374***	8.129***	8.129***	8.374***	8.268***	8.268***
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Panel	16–17	15–18	15–18	16–17	15–18	15–18	16–17	15–18	15–18
Individual FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No	Yes	No
Year×industry×location FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	110,511	111,176	110,988	127,376	127,816	127,636	104,667	107,268	107,100
R^2	0.824	0.892	0.902	0.871	0.919	0.926	0.704	0.845	0.861

Note: standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1), (4), and (7) are based on the simple pre- versus post-treatment comparison using 2016 and 2017 data and mirror the results shown in Table 4. The remaining columns are based on 2015–18 data.

Source: author's calculation based on PAYE 2014–21 data.

The first tax kink revision positively affects formal workers' wages, as was the case for the minimum wage hike. This speaks in favour of the proposed theoretical model. However, these effects are quite large compared to the increase in the tax kink of around 3.2 per cent in real terms (10 per cent nominal). As a robustness check, an alternative control group was defined consisting of workers earning above ZMW3,400 and below ZMW3,700—that is, they earn more similar wages to the treatment individuals but should not be affected by the tax kink changes. Table B3 in the Appendix shows that in the simple pre- versus post-treatment comparison only a statistically significant effect for the previous-bunchers remains. This effect is positive and smaller: previous-bunchers, after the tax kink adjustment, have around 0.9 per cent higher wages compared to the control group. In terms of magnitude, this is more realistic given the small adjustment in the tax kink. The effect for the non-bunchers disappears. There is also no effect on the new-bunchers, and this better matches the theoretical prediction that these workers have no incentive to increase their wages given that they can avoid paying taxes by reducing their wages or leaving their wages unchanged. In the panel regression the effect surprisingly turns significantly negative, but this effect disappears when controlling for differing trends across industries and locations (jurisdictions). This shows that the tax kink effects found in Table 5 are not robust to the use of this alternative control group.

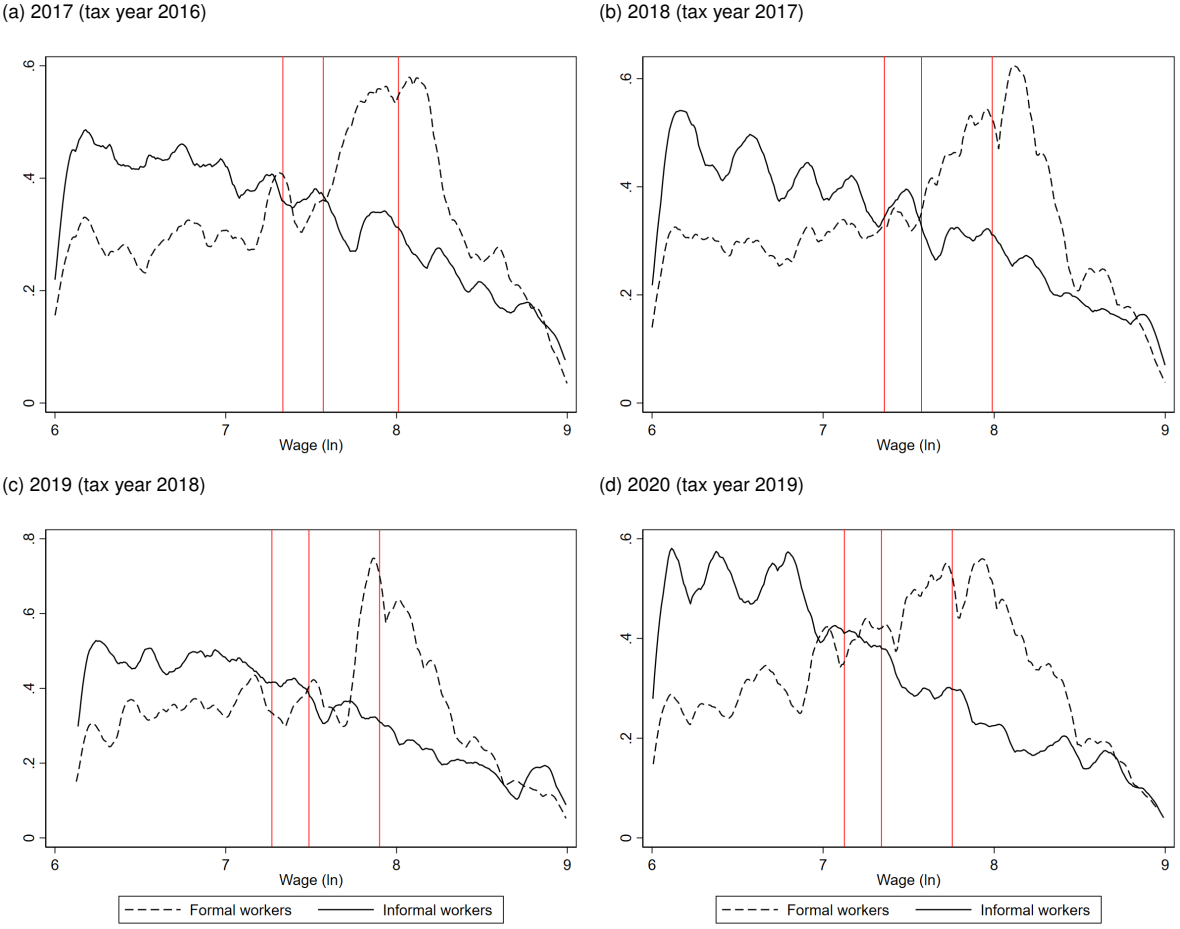
Overall, while not being fully robust, the results indicate the tax kink adjustments hold the potential to produce similar effects to minimum wage changes. The evidence suggests that all three categories of workers around the kinks that were defined in Section 3.2 see increases in their average wages and the local earnings distribution, as well as the peak (excess mass), shifts rightward. While the results for the previous-bunchers and the non-bunchers are as expected, increasing wages among the new-bunchers goes against what was predicted and it appears that these workers do not bunch at the new kink. When using an alternative control group that is closer to the treatment groups in the earnings distribution, only the positive wage effect for the previous-bunchers remains in the simple comparison, while the effects over the four-year period disappear. However, this result is based on a less safe control group than the high-earners as the risk of kink effects spilling over into this control group is higher, given that they are located in between kinks.

Informal worker responses: a new lighthouse effect?

It was previously shown that, despite being targeted at the formal sector, a minimum wage hike in 2018 in Zambia led to economically meaningful and significant increases not only in formal but also in informal workers' wages. Given the evidence that an increase in the first tax kink in 2017 produced similar wage and distributional effects to the minimum wage hike among formal workers, but at the local earnings distribution, the question arises whether these effects can also be found for uncovered informal workers.

The PAYE data that was used to conduct the tax analysis so far does not include information on informal workers. Therefore, the LFS data had to be used to shed some light on this question. However, as shown in Figure C6 in the Appendix, the clear bunching of formal workers at the first tax kink found in the PAYE set does not translate into the LFS data and the number of observations with wages in the relevant earnings interval is very low. It illustrates that even though there is some indication of bunching at the first kink by formal workers in 2017 and informal workers in 2018 and 2019, the low number of observations renders the estimation strategy applied to the PAYE data to detect the effect on formal wages infeasible when looking at informal wages in the LFS data. Instead, Figure 11 depicts kernel density plots of the formal and informal workers' earnings distributions in the LFS data in an earnings interval that spans all three tax kinks, to discuss whether the new lighthouse effect may be relevant in the case of Zambia.

Figure 11: Tax bunching among formal and informal workers in the LFS data



Note: the solid black line is the informal and the dashed black line is the formal earnings distribution. The three vertical red lines correspond to the three kinks in the PAYE tax schedule.
Source: author's illustration based on LFS 2012–21 data.

Figure 11 mirrors the lighthouse effect figures shown earlier for the minimum wage. Again, the solid black line depicts the informal and the dashed line the formal earnings distribution, while the vertical red lines correspond to the three tax kinks in the PAYE tax schedule—everything is shown in real terms. If the new lighthouse effect exists—that is, tax kinks serve as a reference point for wage setting in the informal sector—one should observe an excess mass of informal wages around the tax kinks and in particular the first kink. In Figure 11 there is some indication of bunching of both formal and informal workers around the first kink only in 2017. In all years there is some evidence for bunching around the third kink in the formal sector and a weak indication of bunching also in the informal sector (except in 2019).

This goes against the prediction that the first tax kink should produce the strongest responses in the formal sector and, hence, should most likely cause spillover effects into the local informal earnings distribution around this kink. However, it does provide some support for the main mechanism brought forward for the *new lighthouse effect*, namely that the informal wage distribution is affected by the after-tax formal wage distribution, and hence, tax kinks may serve as a reference point for wage setting also in the informal sector. In 2017, the year when the bunching evidence at the first kink in the formal sector is most convincing, there is also the clearest indication of bunching around the first kink in the informal sector. Similarly, bunching around the third kink is most clear in the formal earnings distribution, and around this kink there is also a hump in the informal distribution in all years besides 2019. Moreover, Figure C7 in the Appendix pools the LFS data from the years 2018–21 (where the tax kinks remained unchanged) to tackle the issue of low observations and plots the informal distribution in earnings bins of 50 (left column) and 200 (right column). This figure shows some excess mass around the first and

third kink, supporting the notion that the new lighthouse effect may be at play—that is, employers may use the wage ceiling(s) and dynamics in after-tax formal wages as leverage when bargaining wages with their informal workers. Again, due to the low number of observations this cannot be seen as conclusive evidence, but it suggests that such dynamics should not be ruled out.

6 Discussion and conclusion

This paper examined the wage and distributional effects of two policy shocks in Zambia: a minimum wage hike in 2018 and an upward revision in the location of the first kink in the progressive income tax schedule, in both the formal and informal sectors. It demonstrated that these effects are comparable for formal workers, in that they both led to increases in average wages and shifted the formal wage distribution rightward. While the minimum wage hike led to changes in the aggregate distribution, the first tax kink produced these effects in the local earnings distribution around the kink. I also document spillover effects into the informal sector following the minimum wage policy shock, thereby confirming the existence of the lighthouse effect (Maloney and Mendez 2004) in Zambia. Using preliminary evidence, I built a case for the existence of spillover effects of the first tax kink adjustment into the informal sector in Zambia—a *new lighthouse effect*. Similar to the minimum wage lighthouse effect, the first tax kink may provide a reference point for wage setting in the informal sector.

Overall, the results are largely in line with the theoretical predictions outlined in this paper and the results found in the literature. The results fit with the large literature documenting positive effects on both formal and informal workers' wages following a minimum wage hike (Derenoncourt et al. 2021; Gindling and Terrell 2007; Katzkowicz et al. 2021; Lemos 2009). In particular, Maloney and Mendez (2004) and Khamis (2013) also document spikes in both the formal and informal earnings distribution around the minimum wage and larger effects of a minimum wage introduction/hike on the informal sector. The finding of strong bunching of formal workers at the first tax kink (both the old and the new one) is in line with other studies in the bunching literature (Bergolo et al. 2021; Kleven and Waseem 2013).

This paper cannot clearly establish the existence of the new lighthouse effect. Still, two aspects, besides the preliminary bunching evidence of informal wages around the first tax kink, support the notion that the first tax kink may serve as a focal point for wage setting in the informal sector in Zambia. The first is the strong responses to the first tax kink and its characteristic as a kind of wage ceiling for many workers in the formal sector. This increases the likelihood that firm owners also adjust the wages of their informal workers in accordance with the formal after-tax wage distribution, given this wage ceiling. The second aspect is that the formal and informal sectors do not seem to operate in separate economic spheres in Zambia. Figure 1 showed that many informal workers are earning formal sector wages and wages above and around the first tax kink. Figure 9 illustrated that there is a large intersection of the formal and informal earnings distribution, making up around 40 per cent of the labour force. Section 4 discussed semi-formality (Berkel 2023) in Zambia and how employers may provide some formal benefits but not others. Finally, the difference-in-difference results show that a minimum wage change had an even larger effect on informal than formal workers' wages. Taken together, this relatedness between the two sectors implies that spillover effects from other adjustments of the first tax kink are probable.

This implies the need for policy-makers to think in a less dichotomous way about providing social protection and decent jobs for informal and formal workers and to instead choose a holistic approach. In Zambia this is of particular importance, given that most employment arrangements continue to be informal, and improving the economic conditions for informal workers, who make up the bottom of the wage distribution, is crucial for attaining national development goals and the SDGs. It is important to keep in mind that the results presented and conclusions drawn apply to the context of Zambia, but

other developing countries may be characterized by different dynamics between formal and informal workers. Moreover, other variables that may be affected by minimum wage and tax kink changes, such as employment, working hours, and the size of the formal/informal sector, were not considered. Thus, while the results show the potential of both minimum wage and first tax kink hikes to improve the economic conditions of a significant portion of the workforce, not only in the formal sector but also in the informal sector, the overall welfare effects are not fully understood and these positive effects may be offset by a reduction in work hours, employment, or formal employment (Broecke and Vandeweyer 2015; Jales 2018; Maloney and Mendez 2004).

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Appendix A: Additional information on the LFS and PAYE data cleaning

Table A1: ILO LFS cleaning steps and data overview

	2012	2014	2017	2018	2019	2020	2021
Population							
Raw observations	59,247	58,985	45,685	49,551	34,010	45,354	45,740
Drop	–	–	78	15	–	–	581
Remaining	100.00%	100.00%	99.83%	99.97%	100.00%	100.00%	98.73%
Working-age population	32,270	31,808	24,613	27,362	18,761	24,923	25,293
Share of total	54.47%	53.93%	53.88%	55.22%	55.16%	54.95%	55.30%
Working-age population							
Labour force	26,412	15,919	8,634	8,805	6,182	8,015	8,795
Share	81.85%	50.05%	35.08%	32.18%	32.95%	32.16%	34.77%
Outside labour force	5,858	15,889	15,979	18,557	12,579	16,908	16,498
Share	18.15%	49.95%	64.92%	67.82%	67.05%	67.84%	65.23%
Unemployed	894	1,938	1,115	1,046	850	1,110	1,148
Unemployment rate	3.38%	12.17%	12.91%	11.88%	13.75%	13.85%	13.05%
Employed	25,518	13,981	7,519	7,759	5,332	6,905	7,647
Employment rate	96.62%	87.83%	87.09%	88.12%	86.25%	86.15%	86.95%
Employed population							
Report an income	7,969	7,930	6,629	6,788	4,704	5,990	6,558
Share of employed	31.23%	56.72%	88.16%	87.49%	88.22%	86.75%	85.76%
Cleaned income (final data)	7,248	7,345	6,061	6,147	4,326	5,421	6,050
Share of employed	28.40%	52.54%	80.61%	79.22%	81.13%	78.51%	79.12%
Share of raw	12.23%	12.45%	13.27%	12.41%	12.72%	11.95%	13.23%

Source: ILO 2012–21 data.

The raw data contains some data inconsistencies, and after initial inspection and cleaning 42,598 individual observations are left for the analysis. Table A1 shows that the raw data sets contain 59,247, 58,985, 45,685, 49,551, 34,010, 45,354, and 45,740 individual observations in 2012, 2014, 2017, 2018, 2019, 2020, and 2021, respectively (338,572 observations in total). Overall, 674 observations are dropped due to data inconsistencies (e.g. double entries). A total of 185,030 observations remain after excluding individuals younger than 15 years of age (the legal working age in Zambia). Following the definitions by ZamStats,[9] just over 55 per cent of the working-age population are labelled as outside of the labour force, and 4.4 per cent as unemployed, leaving 40.4 per cent employed individuals. The labour force includes employed and unemployed individuals where employed individuals are those of working age who during the reference period (the previous week) were in paid employment or self-employed (whether at work or having a job but not at work). Unemployed are persons of working age who were not employed during the reference period, available for work, and seeking work. Out of the employed individuals, only 46,598 individuals report a wage.

The LFS surveys were conducted by the Central Statistical Office before 2017 and by the Zambia Statistics Agency (ZamStats) since 2017. As a result, there are some discrepancies in the definitions used pre- and post-2017. To ensure comparability over time in the data set used, this paper used ZamStats' definitions for the period after 2017. This explains why the size of the labour force and the number of employed individuals are estimated a lot larger in 2012 and 2014. This should not be a reason for concern, however, since only employed individuals who report an income are used for the analysis and this number is in line with the years starting in 2017. Moreover, data from the years 2012 and 2014 is only used for some descriptive results, while the main analysis involving the LFS data is conducted using the 2017–21 data to analyse the 2018 minimum wage change.

[9] The definitions are detailed in the LFS survey reports.

Table A2: PAYE cleaning steps

	Raw	Drop ID	% of raw	Drop <p1	Drop >p99	Clean	% of raw
2014	4,653,865	5,793	0.12	30	46,479	4,601,585	98.88
2015	4,770,504	5,761	0.12	8	47,647	4,717,096	98.88
2016	4,650,100	5,318	0.11	–	46,447	4,598,335	98.89
2017	5,301,171	2,618	0.05	–	52,985	5,245,568	98.95
2018	5,805,011	277	0.00	–	58,046	5,746,688	99.00
2019	5,933,279	31,158	0.53	–	59,021	5,843,100	98.48
2020	6,204,777	524,542	8.45	–	56,802	5,623,445	90.63
2021	6,482,701	629,247	9.71	–	58,534	5,794,920	89.39

Source: PAYE 2014–21 data.

In total, the data set includes 44,768,466 observations. Table A2 shows that from 2014 to 2016 there are around 4,700,000 observations in each year. Between 2017 and 2021 the number of observations increased from 5,300,000 to 6,500,000. This is also reflected in increasing monthly average observations over the period 2014–21, from around 400,000 in 2014–16 to around 450,000 in 2017, around 500,000 in 2018–19 and the highest average in 2021 with 560,000.

Create a unique ID: The dataset includes both employer and employee IDs. The former is mandatory to fill in the PAYE returns and is a unique 12-digit number that was allocated to the firm when it registered formally. The latter is not mandatory to fill in and is created by firms on behalf of their employees. It is typically an eight-digit ID, but some firms choose other numbers, including the National Registration Card (NRC) number or simply zeros. Moreover, since the firm chooses the number, the employee IDs are not unique. In this context, in a first step all the employee IDs that are different from an eight-digit number were dropped. As indicated in Table A2, in the years 2014 to 2019 this is a small proportion—less than 1 per cent. In 2020 and 2021, though, this amounts to dropping 8.45 and 9.71 per cent of observations, respectively. After this step, the employer and employee IDs were grouped to generate a new unique ID that allows tracking individuals over time.

Identify outliers: In terms of cleaning the main outcome variable, formal wages (chargeable emoluments), some outliers were dropped—that is, values below the 1st percentile and above the 99th percentile. As shown in Table A2, after this cleaning process more than 98 per cent, and closer to 99 per cent in most years, of the raw observations remain, except in 2020 and 2021 again, where around 90 per cent are kept.

Collapse the data to the annual level: Lastly, chargeable emoluments as well as other variables of interest, such as industry and jurisdiction, were collapsed to the annual level to obtain an average monthly wage. In the data set, all the variables are provided at the monthly level but the external shock (treatment) of interest, the change in the location of the first tax kink, was introduced for the tax return year 2017 so that the assignment into treatment and control group to identify any effects should also take place at the annual level. This assignment is based on whether individuals in the PAYE data fall into certain earnings intervals in the return year 2016, the year before the shock. If the assignment was conducted at the monthly level, this could lead to some potential issues, since workers may distribute their salary over the year so that the wage is not the same every month and the wage in a specific month may not be representative of that worker's average monthly earnings. As a result, an individual may be allocated to the wrong group. Moreover, the annual tax kink is just the sum of the monthly tax kinks (in 2016 it is ZMW3,000 multiplied by 12 = ZMW36,000), which means that the sum of workers' monthly earnings, if they are bunching at the kink and trying to avoid the higher tax bracket, should be below or at the annual kink number, and in return the average monthly earnings should also be below or at the monthly kink number.

Table A3: Semi-formality in Zambia

Variable	Description	2012	2014	2017	2018	2019	2020	2021
Registration	The establishment/business is registered with any of the following: Registrar of Societies, PACRA, local authority (council), registrar for NGOs, registrar for cooperatives	372 (5.1%)	1,581 (21.5%)	1,264 (20.9%)	1,463 (23.8%)	1,043 (24.1%)	1,082 (20.0%)	1,214 (20.1%)
Tax registration	The establishment/business is registered with the ZRA	–	–	1,213 (20.0%)	1,322 (21.5%)	844 (19.5%)	935 (17.2%)	985 (16.3%)
Social security	The establishment/business provides one or more of the following benefits to its employees: Social security scheme (e.g. NAPSA, workers compensation, PSPF, pension scheme), paid leave, paid sick leave, paid paternity/maternity leave	682 (9.4%)	2,526 (34.4%)	1,713 (28.3%)	1,939 (31.5%)	1,268 (29.3%)	1,468 (27.1%)	1,529 (25.3%)
Fully formalized	The establishment/business is registered with the relevant authority, registered with ZRA and provides at least one benefit to its employees	37 (0.5%)	906 (12.3%)	682 (11.3%)	783 (12.7%)	525 (12.1%)	529 (9.8%)	574 (9.5%)
Employed	All individuals who are of working age (15 years or older) and during the last seven days did any work for pay or usually do but were absent and report an income	7,248	7,345	6,061	6,147	4,326	5,421	6,050
Informal sector workers	Individuals who are employed in an informal enterprise	3,235 (44.6%)	3,367 (45.8%)	2,056 (33.9%)	2,813 (45.8%)	1,915 (44.3%)	2,471 (45.6%)	2,661 (44.0%)
Informal workers	This is the main definition of informality used in this paper and is based on whether the worker receives any social security benefits following the ILO definition of informal employment. In 2012 the data was incomplete, so instead this was based on: access to paid maternity leave, trade union membership, income tax deducted from salary	5,086 (70.2%)	4,819 (65.6%)	4,348 (71.7%)	3,924 (63.8%)	2,807 (64.9%)	3,953 (72.9%)	4,521 (74.7%)

Note: frequencies and share of the employed population that reports an income in parentheses.

Source: author's calculation based on LFS 2012–21 data.

Appendix B: Summary statistics and robustness checks

Table B1: Formal wages in the local earnings distribution around the first tax kink

	Mean	Median	IQR	(%)	<k1	<k2	<k3
2014	3,105.75	3,075.04	501.11	-	57.3	62.8	72.7
2015	3,100.75	3,064.44	510.14	1.8	56.2	61.9	71.8
2016	3,084.95	3,035.55	491.66	–3.6	54.8	60.7	70.6
2017	3,089.79	3,046.73	496.99	1.1	58.8	64	72.9
2018	3,111.99	3,102.94	494.26	–0.5	57.3	62.9	71.9
2019	3,115.88	3,109.22	530.53	7.3	55.6	62	71.3
2020	3,117.02	3,100.01	541.43	2.1	54.2	60.8	69.8
2021	3,123.61	3,086.32	533.85	–1.4	60.4	64.8	72.6

Note: local earnings distribution corresponds to the earnings interval [2600;3700]. k1, k2, and k3 refer to the three kinks in the tax schedule and the share of observations below each kink. As in the LFS data, real wages declined over the same period due to inflation, from ZMW2,195 in 2014 to ZMW963 in 2021.

Source: author's calculation based on PAYE 2014–21 data.

Table B2: MW DiD results: robustness check

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Wage (ln)	Wage (ln)	Wage (ln)	Formal wage (ln)	Formal wage (ln)	Formal wage (ln)	Informal wage (ln)	Informal wage (ln)	Informal wage (ln)
T2 × *D*	0.164***			0.044			0.225***		
	(0.059)			(0.075)			(0.078)		
T3 × *D*		0.158**			–0.097			0.241***	
		(0.062)			(0.092)			(0.076)	
T × *D*			0.104*			0.115			0.119
			(0.061)			(0.074)			(0.084)
Constant	5.788***	5.794***	5.803***	5.402***	5.430***	5.388***	5.766***	5.761***	5.816***
	(0.157)	(0.157)	(0.160)	(0.264)	(0.263)	(0.262)	(0.227)	(0.227)	(0.228)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,171	21,171	20,896	6,756	6,756	6,701	14,411	14,411	14,192
R-squared	0.18	0.18	0.18	0.40	0.40	0.40	0.09	0.09	0.09

Note: clustered standard errors at the district level in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls are the same as in the main specification columns (3), (6), and (9).

Source: author's calculation based on LFS 2012–21 data.

Table B2 presents the results from two robustness checks. The first robustness check is to run the same regression using two alternative definitions for the treatment and control groups. Instead of basing the definition on formal workers only, the first alternative definition categorizes into treatment and control groups based on the bite of the minimum wage across all workers, including the informal ones. Using this definition, the overall effect increases and is entirely driven by the increasing informal wages. An informal worker in a high-impact district, on average, now has a 22.5 per cent higher wage than an individual in a control district after the minimum wage hike. The effect on formal wages is now small and insignificant. The second alternative definition defines the treatment group as districts with an above-average share of informal workers in 2018. This produces a similar effect as the first alternative definition. Presumably, in more informal districts, informal workers also earn lower wages, thereby generating this similar effect. The second robustness check is to use a more conservative standard error, whereby potential clustering in individuals' errors within the same district and across years is taken into account—standard errors are clustered at the district as opposed to the district–year level. While the overall wage effect remains significant, the effect on formal and informal wages now turns insignificant.

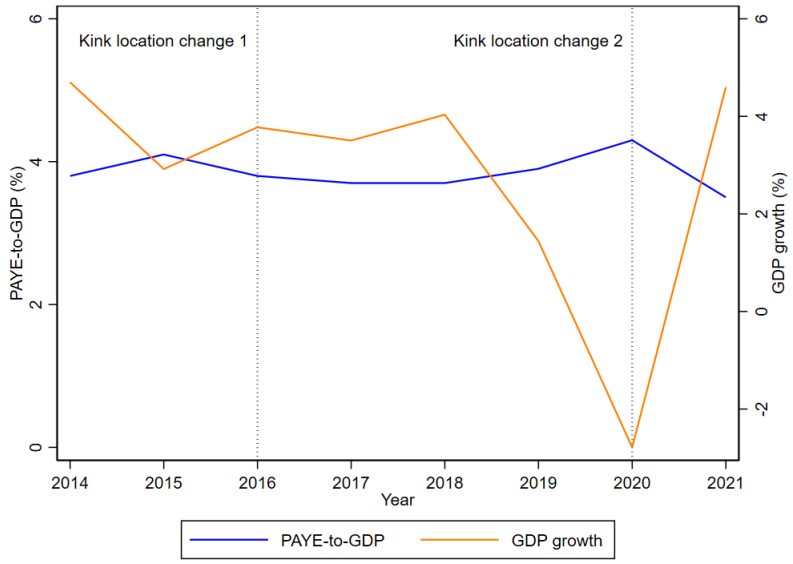
Table B3: Tax DiD results: robustness check

Dep. var.:	T1: Previous-bunchers			T2: Non-bunchers			T3: New-bunchers		
Ln(wage)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T	–0.179***			–0.122***			–0.057***		
	(0.002)			(0.000)			(0.000)		
D (Y>2016)	0.002			0.002			0.002		
	(0.002)			(0.002)			(0.002)		
$T \times D$	0.009**	–0.016***	–0.005	0.004	–0.008*	0.005	0.003	–0.019***	–0.005
	(0.004)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)
Constant	7.565***	7.535***	7.531***	7.565***	7.526***	7.523***	7.565***	7.582***	7.580***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Panel	16–17	15–18	15–18	16–17	15–18	15–18	16–17	15–18	15–18
Individual FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No	Yes	No
Year×industry×location FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	37,018	33,352	33,192	53,883	49,992	49,852	31,174	29,444	29,344
R^2	0.27	0.533	0.596	0.147	0.495	0.567	0.029	0.452	0.533

Note: standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1), (4), and (7) are based on the simple pre- versus post-treatment comparison using 2016 and 2017 data. The remaining columns are based on 2015–18 data. The control group here is defined as those earning above ZMW3,400 and below ZMW3,700 in 2016.
Source: author's calculation based on PAYE 2014–21 data.

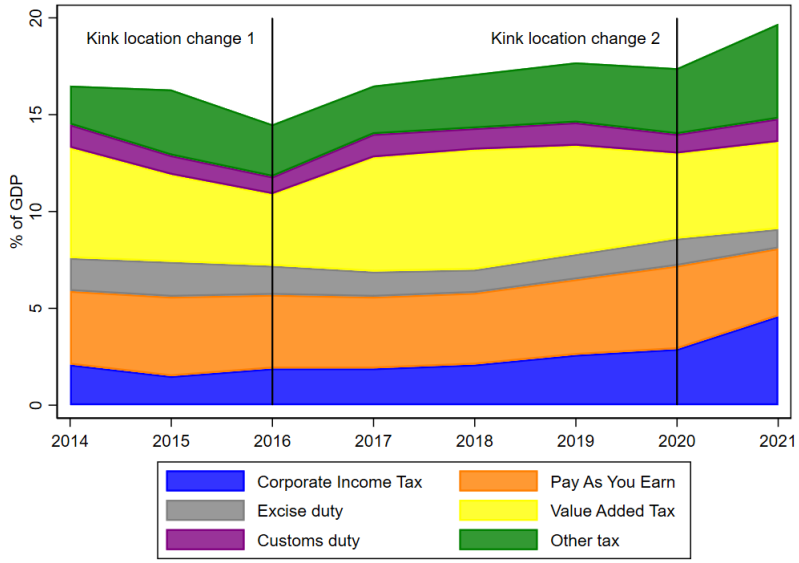
Appendix C: Additional figures

Figure C1: Pay as you earn (PAYE) tax as a percentage of GDP 2014–21



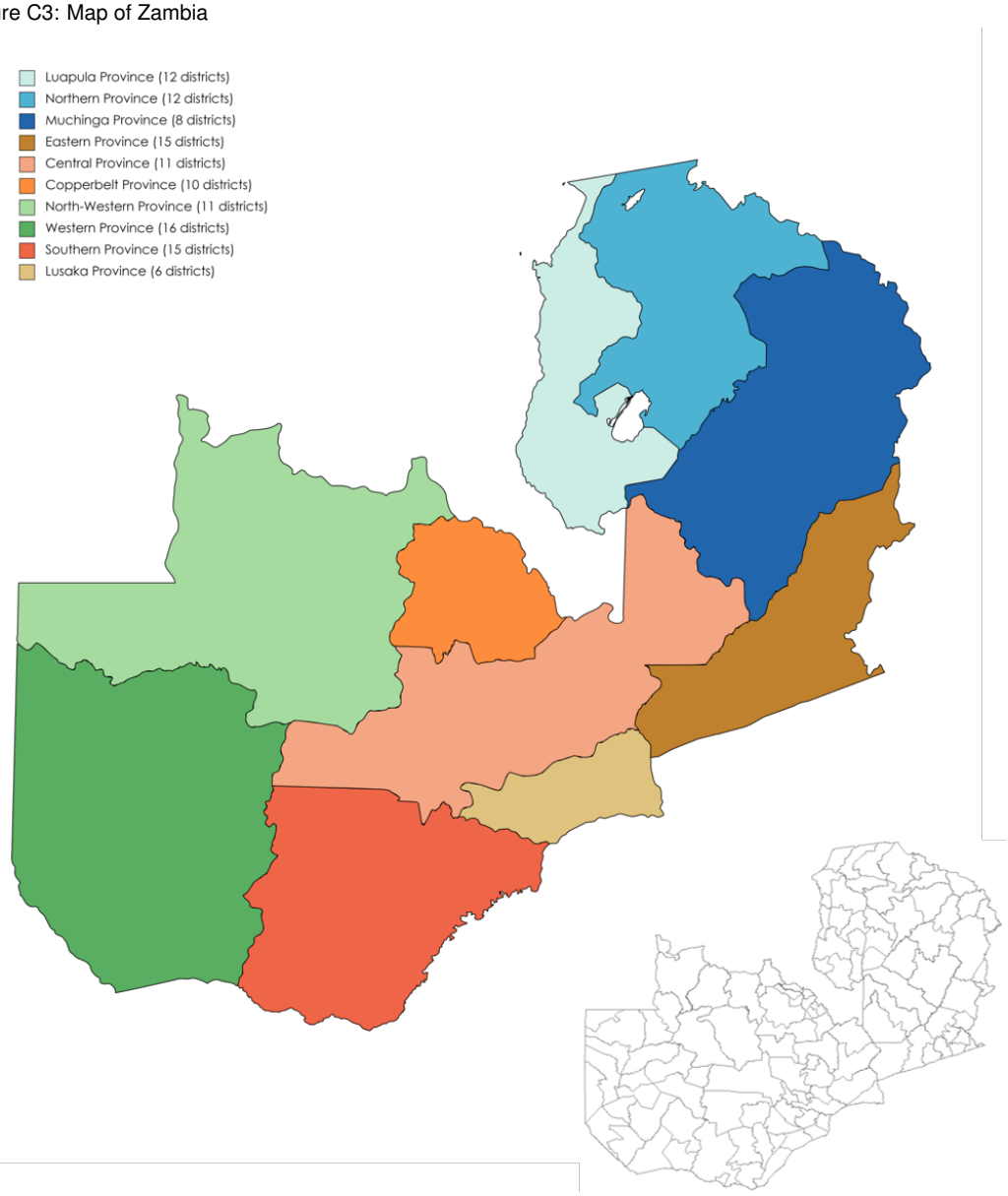
Note: Zambian GDP in 2014 was 27.14 billion (current US$) or 167.05 billion (current LCU) (source: World Development Indicators).
Source: author's illustration based on ZRA (2021b).

Figure C2: Tax type as a percentage of GDP 2014–21



Source: author's illustration based on ZRA (2021a).

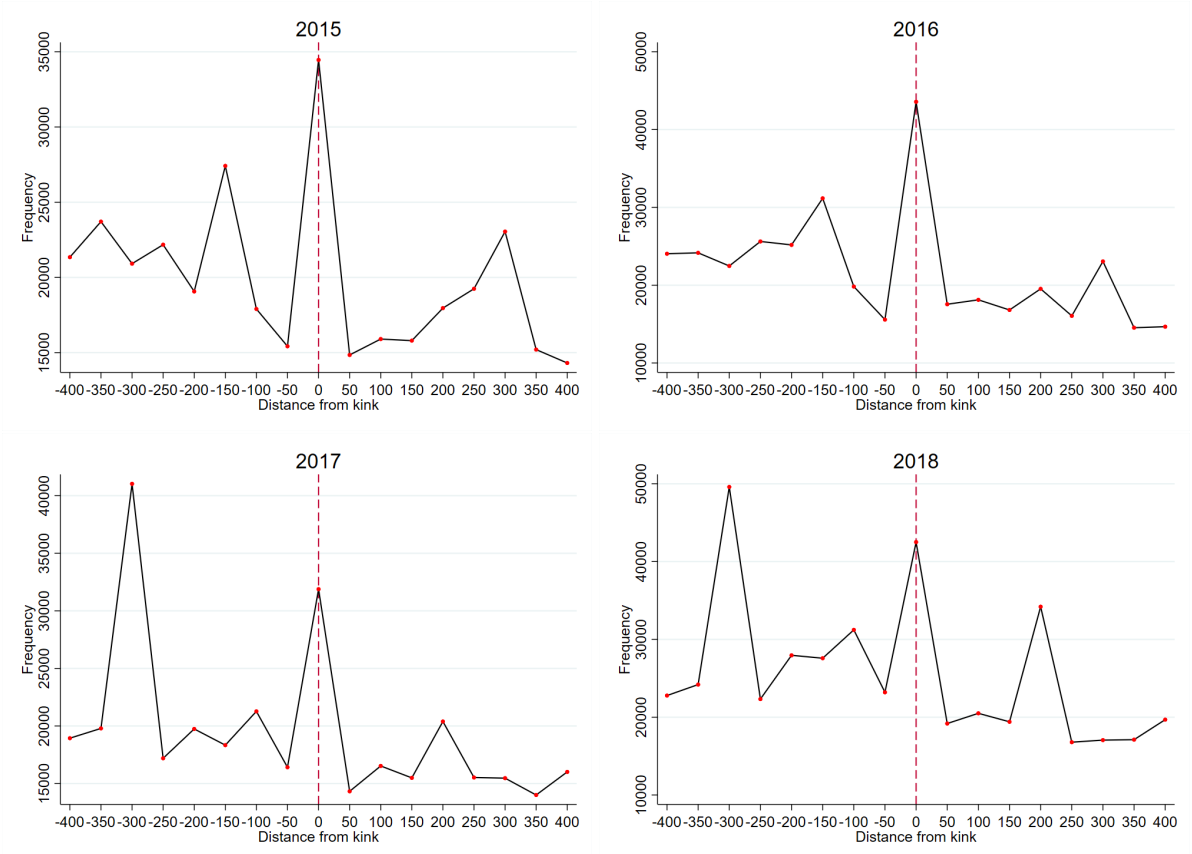
Figure C3: Map of Zambia



Note: Zambia has 116 districts, out of which 103 are covered in the LFS data.

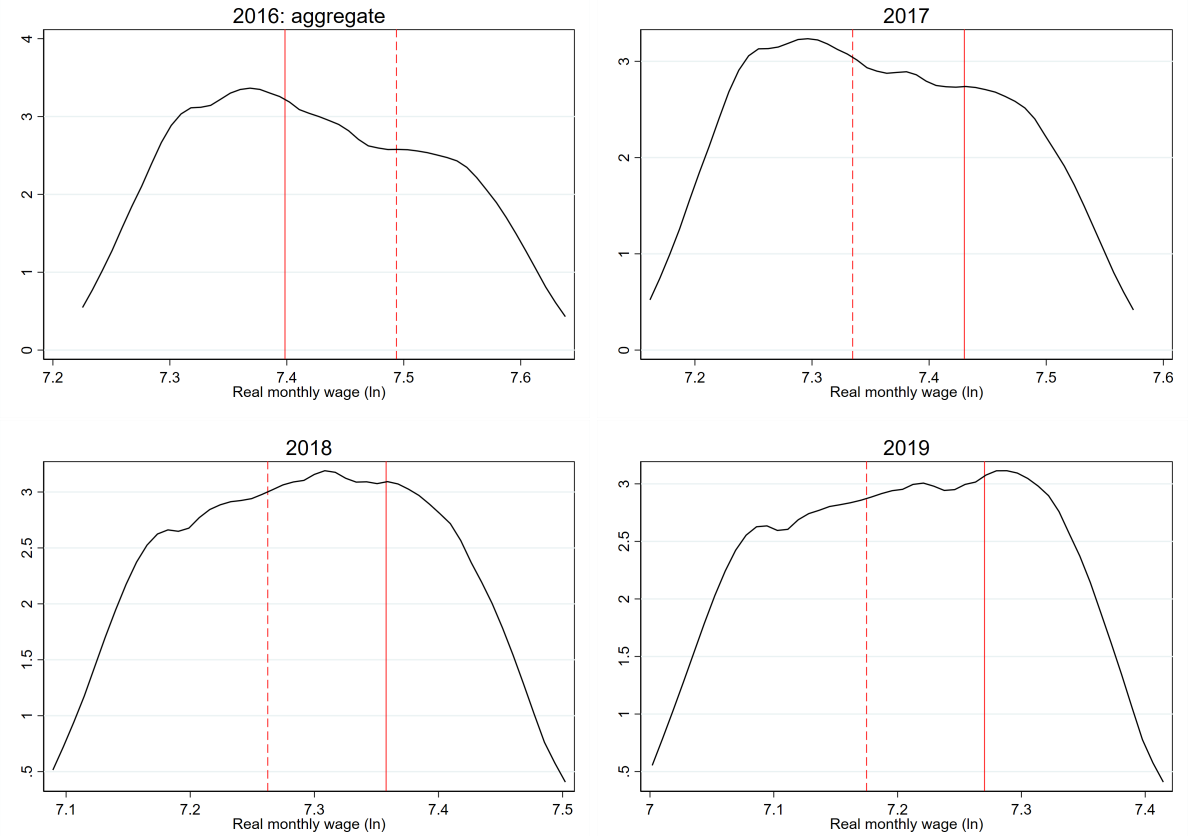
Source: the map was created using mapchart.net and information from the United Nations Office for the Coordination of Humanitarian Affairs (OCHA): https://data.humdata.org/dataset/cod-ab-zmb.

Figure C4: Tax bunching among formal workers 2015–18



Note: tax kink location is ZMW3,000 in 2015 and 2016, and ZMW3,300 in 2017 and 2018. This figure plots the same distribution as Figure 10, but gives a sense of the amount of formal workers that bunch at the old and the new kink.
Source: author's illustration based on PAYE 2014–21 data.

Figure C5: Aggregate tax kink adjustment effects in the formal sector 2016–19

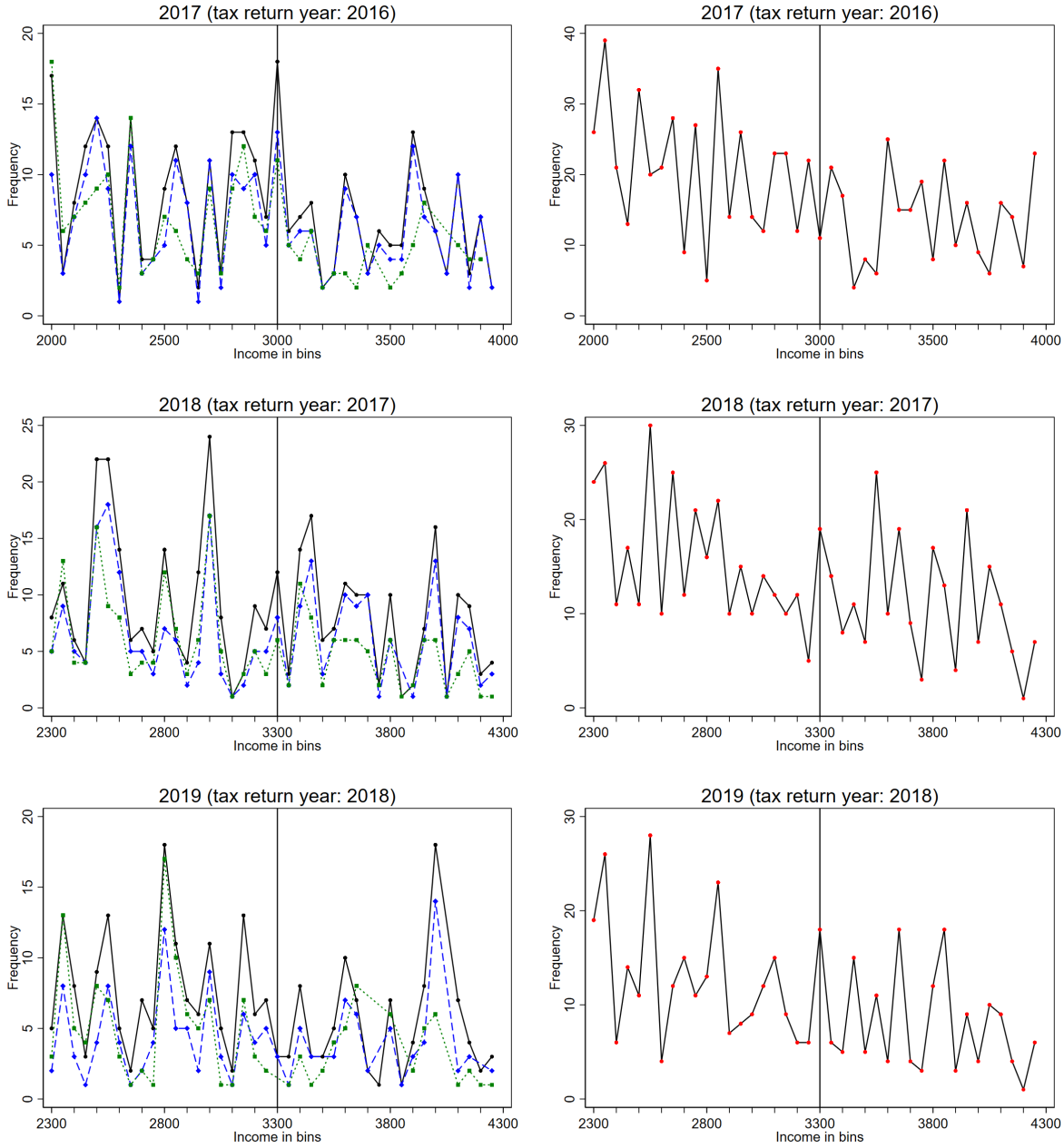


Note: solid vertical red line is the effective tax kink and dotted vertical red line is the previous year's tax kink. The graphs illustrate the aggregate effects in the local earnings distribution.
Source: author's illustration based on PAYE 2014–21 data.

Figure C6: Tax bunching among formal and informal workers in the LFS data

(a) Formal workers

(b) Informal workers

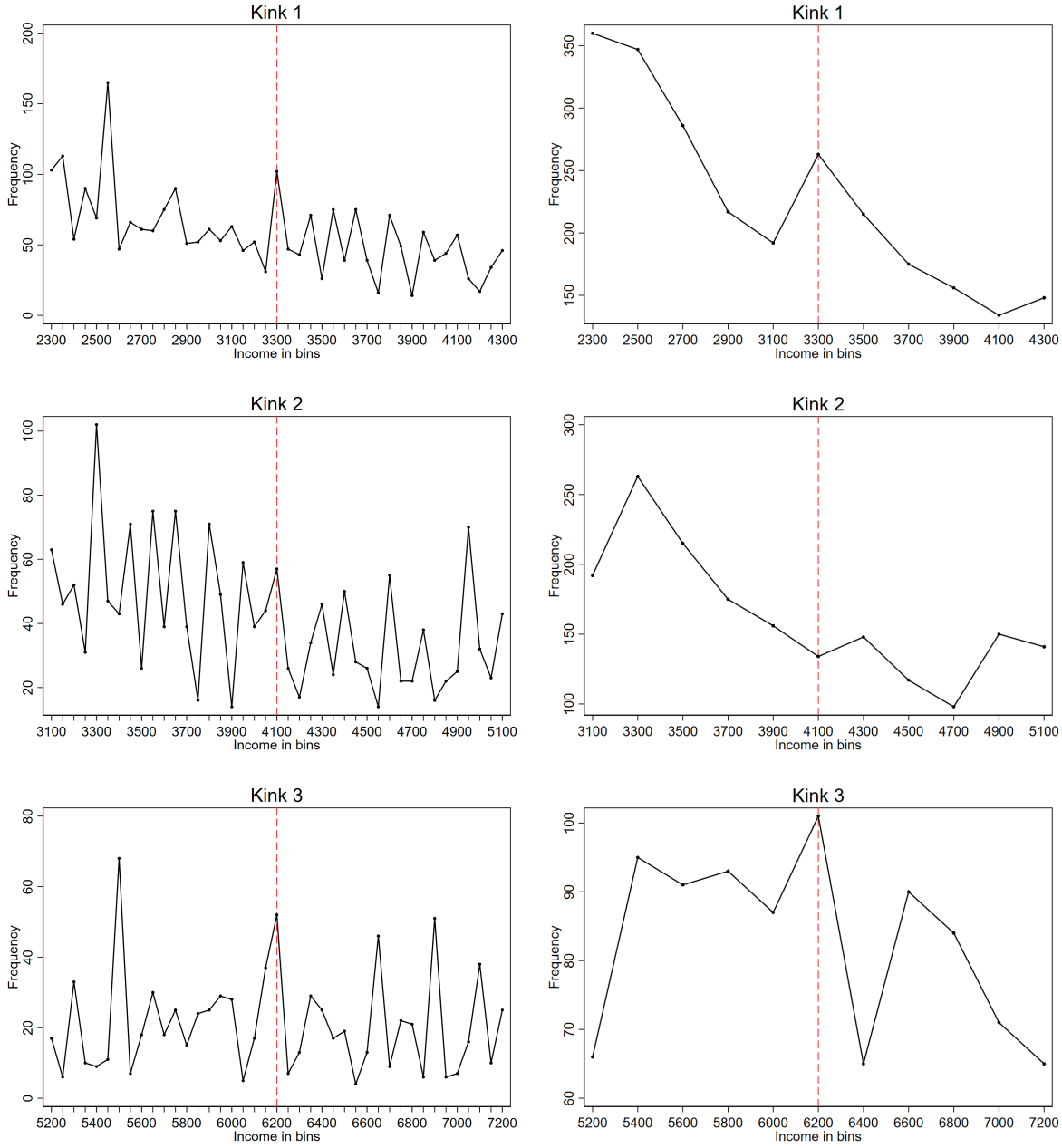


Note: in the graphs for formal workers, different definitions were used to test for evidence of bunching. The black line indicates all formal workers, the blue line those that are formal and pay tax, and the green line those that are formal and are registered with the ZRA.

Source: author's illustration based on LFS 2012–21 data.

Figure C7: Tax bunching among informal workers: pooled data for 2018–21

(a) Bin width: 50 (b) Binwidth: 200



Note: data was pooled for the years 2018–21 to increase the number of informal observations. The location of the tax kinks did not change during this period.

Source: author's illustration based on LFS 2012–21 data.