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Essential Trip Making and Mode Substitution during COVID-19 Movement Restrictions in Nigerian Cities

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Abstract- Movement restrictions remove transport options, and what follows depends on what a household has left. We ask which trips proved non-deferrable in three Nigerian cities during the 2020 restrictions, and what replaced the modes that vanished. We surveyed 1,080 respondents across Lagos, Abuja and Ibadan on their travel before and during the restrictions, and weighted the sample so that its aggregate trip making reproduces an observed mobility series. Lagos and Abuja fell under a cessation of movement from 30 March 2020 while Ibadan had only a curfew, and the observed series confirms that contrast: workplace activity fell 59.5 and 58.9 percent against 28.0. Suspending the shared modes removed travel from their users at nearly twice the rate it did from car owners: 39.6 percent of minibus users and 35.9 percent of tricycle users made no trip at all against 21.9 percent of car owners, and where they did travel they walked. That is the distributional consequence of the policy, conditional on the level of service the orders left running. Whether a walkable fallback protects essential travel, and protects the poor most, is not identifiable here: the main effect survives at one of five distance thresholds, the interaction at two, and both vanish when distance enters continuously. Modeling availability explicitly, so that a suspended mode is absent from the choice set rather than merely unchosen, is this study's most transferable product.

Keywords: travel behavior, movement restriction, mode substitution, trip deferrability, discrete choice, transport poverty, nigeria

I. INTRODUCTION

When transport is withdrawn, some journeys stop and some change shape. Reviewing what happens when road capacity is closed, Cairns et al. (2002) found that 11 to 20 percent of the traffic simply evaporates rather than reappearing elsewhere, and studies of transit strikes (Exel and Rietveld 2001) and of the 1994 Northridge earthquake (Giuliano and Golob 1998) document the same pattern of partial disappearance and partial substitution. Noland et al. (2003) showed during the 2000 United Kingdom fuel crisis that the disappearing trips are not a random sample: discretionary travel is curtailed first and essential travel last.

That literature was written about disruptions lasting days. The restrictions imposed across Nigerian cities in 2020 lasted weeks and removed not one mode but most of them at once, and the question they raise is a different one. If the usual mode is gone, whether a trip survives depends on whether the household has anything to substitute with. In a city where the fallback is a car, that question has been asked: Currie and Senbergs (2007) named forced car ownership and Lucas (2012) and Lucas et al. (2016) built the transport poverty literature around households that cannot afford the fallback their city assumes. In a Nigerian city the fallback is not a car. It is walking, and walking is already a substantial mode: Ipingbemi (2010) measured it at 29.6 percent among elderly Ibadan residents and Olawole and Aloba (2014) at 58.4 percent in Osogbo.

The closest available analogue to a national movement restriction is the Ebola epidemic of 2014 to 2016 in West Africa, and it is instructive on both counts. Brolin Ribacke et al. (2016) synthesized 22 studies and found inpatient service use falling by an average of 44 percent, which is a large reduction and conspicuously not a total one, so a substantial share of care-seeking travel survived. Peak et al. (2018) measured Sierra Leone's three-day national stay-at-home order in mobile phone records and found relocations falling 31 percent under 15 km, 46 percent between 15 and 30 km, and 76 percent beyond 30 km. Restriction did not suppress movement uniformly. It suppressed long movement, and short movement largely survived,

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which is what one would expect if the surviving trips are the ones a person can walk. Pellecchia et al. (2015) reported the human version of the same finding from Liberian quarantine, where residents broke the rules because food deliveries arrived only intermittently.

The Nigerian case sharpens the substitution question, for reasons that compound one another. The dominant modes were the restricted ones: minibus and tricycle services carry the majority of urban trips (Kumar 2011; Mobereola 2009) and were precisely what the restrictions suspended. The fallback available when they went is walking rather than driving, since private vehicle ownership is a minority condition, so for most households the substitute mode has no fare and no vehicle but a hard distance limit. Policy varied between cities as well, because Lagos, Ogun and the Federal Capital Territory were placed under a cessation of movement from 30 March 2020 while other states applied curfews only, which creates a natural contrast. And the travel most exposed to all of that is food and health travel, which is not deferrable for long: where storage is limited, market trips recur on a short cycle, and the health access literature for Nigeria (Stock 1983; Ntoimo et al. 2019) documents distance already suppressing care-seeking before any restriction.

The gap this paper occupies is narrow and specific. Three literatures each address part of the question and none addresses it whole. The disruption literature measures aggregate behavior change under an acute shock without disaggregating by trip purpose. The essential-travel literature measures purpose-specific sensitivity, but under chronic constraint rather than acute. The transport poverty literature identifies who lacks a fallback, but only in cities where the missing fallback is a car. No study we located estimates trip deferrability and substitution capacity jointly, under an acute system-wide restriction, in a setting where the substitute mode is walking.

We make two claims and hold them to different standards. The first is descriptive and we think it is solid: the reduction in travel was strongly purpose-dependent, walking absorbed most of the displacement, and users of shared modes bore most of the trip loss. The second is inferential and we report it as fragile: households with a walkable fallback were more likely to keep an essential trip, and that protection appears to matter more at low incomes than at high, but the effect behaves like a threshold rather than a gradient and does not survive every specification. Presenting it otherwise would overstate what this sample can support.

Section 2 reviews the five literatures the paper draws on. Section 3 describes the observed mobility series and the survey, and Section 3.3 reports the weighting. Section 4 sets out the models. Section 5 reports what happened and Section 6 the central test. Section 7 discusses what follows and what does not.

II. RELATED WORK

2.1 What disruption does to travel

Marsden and Docherty (2013) frames disruption as a natural experiment on habitual travel. The empirical record is consistent about the shape of the response. Cairns et al. (2002) synthesized road capacity reductions and found 11 to 20 percent of traffic evaporating. Exel and Rietveld (2001) studied transit strikes and Giuliano and Golob (1998) the Northridge earthquake, both finding substitution incomplete and behavior partially persistent afterwards. Larcom et al. (2017) used the 2014 London Underground strike to show that some forced experiments reveal better options that commuters then keep. Noland et al. (2003) is the closest in spirit to the present case, finding discretionary travel curtailed first under fuel shortage. Murray-Tuite and Wolshon (2013) review evacuation behavior, where the constraint is time rather than mode availability.

What this literature does not do is separate the response by trip purpose in a way that identifies which trips are non-deferrable, and it is drawn almost entirely from settings where the private car is the fallback.

2.2 Epidemics and mobility before 2020

The pre-2020 record on movement restriction is thin but pointed. Peak et al. (2018) is the strongest single source: mobile phone records through Sierra Leone's three-day national lockdown show relocations falling 31 percent under 15 km, 46 percent between 15 and 30 km and 76 percent beyond 30 km, with the reduction sharpest where Ebola incidence was highest and a rebound immediately on lifting. The distance gradient is the finding that matters here, because it is what a walking fallback would produce.

Brolin Ribacke et al. (2016) synthesized the health consequences, with inpatient use down 44 percent, and Pellecchia et al. (2015) documented quarantined Liberian residents breaking restrictions to obtain food and avoiding care for non-Ebola illness. Shuaib et al. (2014) and Fasina et al. (2014) record Nigeria's own Ebola response, and Bogoch et al. (2015) and Poletto et al. (2014) model travel restriction effects. From other outbreaks, Wang (2014) found Taiwanese transit ridership falling sharply under SARS without formal restriction, Kim et al. (2017) found the same in Seoul smart-card data during MERS, Wilder-Smith (2006) reviewed the SARS travel response, and Miller et al. (2010) found that during an H1N1 school closure 74 percent of students still left home on multiple occasions.

Across all of it, travel is measured in aggregate or by distance. None of it decomposes the response by trip purpose or asks what determined whether a household could substitute.

2.3 Which trips are deferrable

The activity-based tradition supplies the vocabulary. Chapin (1974) divides activity into subsistence, maintenance and leisure, and Hägerstrand (1970) supplies the constraint framing that makes some activities immovable. Recker et al. (1986), Kitamura (1988), Nishii et al. (1988) and Golob (2000) develop trip chaining and the interdependence of activity decisions, which matters here because a suspended work trip removes the chain a market stop was attached to.

This literature is precise about what makes a trip hard to defer and almost entirely untested under an acute system-wide shock.

2.4 Modeling the choice

The mode choice model is standard. McFadden (1974) supplies the conditional logit and Train (2009) the modern treatment. Because the independence of irrelevant alternatives assumption is doing real work when several shared modes are close substitutes, we test it with both Hausman and McFadden (1984) and Small and Hsiao (1985), and we report the nested alternatives of Williams (1977) and Daly and Zachary (1978) as the fallback if it fails. Swait and Ben-Akiva (1987) and Cascetta and Papola (2001) address choice set formation and availability, which is central here: during restrictions a mode is not merely unattractive but absent, and treating an unavailable alternative as merely unchosen would bias the estimates.

2.5 Travel in Nigerian cities, and who has no fallback

Nigerian mode shares are documented well enough to calibrate against. Salau (2018) surveyed 1,351 Lagos households, Busari (2019) and Busari and Ojo (2018) report Ogun State travel data, Ipingbemi (2010) and Olawole and Aloba (2014) measure walking shares among elderly residents in Ibadan and Osogbo, and Aderamo (2002) covers Ilorin. On the paratransit side, Ogunrinola (2011) surveyed 777 commercial motorcycle riders, Olubomehin (2012) gives the history, Kumar and Barrett (2008) and Kumar (2011) the World Bank assessment, Mobereola (2009) the Lagos bus rapid transit case and Starkey (2016) the rural access role.

The transport poverty literature names the households that cannot substitute: Currie and Senbergs (2007) on forced car ownership, Lucas (2012), Lucas et al. (2016), Kenyon et al. (2002), Church et al. (2000), Currie et al. (2009) and Mattioli (2014). Every one of these is set where the fallback is a car.

2.6 Non-deferrable travel and recall bias

On the essential end, Stock (1983) measured distance decay in Nigerian health facility utilization, Ntoimo et al. (2019) and Okonofua et al. (2018) surveyed why rural Nigerian women do not reach facilities, Ouma et al. (2018) mapped emergency hospital access across Africa and Wong et al. (2017) reviewed physical access as a determinant of delivery care. Dercon et al. (2008) and Rabirou et al. (2012) connect road access to livelihood outcomes, the latter in this paper's own study region.

Finally, the design of any retrospective instrument has to reckon with recall. Golob and Meurs (1986) found reported trip rates declining across a diary period, Wolf et al. (2001) and Murakami and Wagner (1999) showed self-report missing short and non-home-based trips, and Draijer et al. (2000), Stopher et al. (2007)

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and Bricka et al. (2012) quantify the shortfall against tracked data. All of it bears on how a survey that asks people about a past restriction period should be read.

III. DATA

3.1 The observed series

The backbone of this study is the Google COVID-19 Community Mobility Report for Nigeria, an observed series published daily through 2020 by place category, covering Nigeria nationally and each of its 37 sub-regions, giving 60,816 region-day-category records. It reports percentage change from a pre-restriction baseline for workplaces, retail and recreation, grocery and pharmacy, transit stations, parks and residential locations. We refer to it throughout as the observed mobility series.

Three cities were chosen because policy differed between them, which gives the study a contrast it did not have to manufacture. Lagos and Abuja fell under the cessation of movement order of 30 March 2020, with phased easing from 4 May. Ibadan, in Oyo State, was placed under a curfew only. Figure 1 shows how far the two regimes diverge: workplace activity fell 59.5 percent in Lagos and 58.9 percent in Abuja against 28.0 percent in Ibadan, and residential presence rose correspondingly.

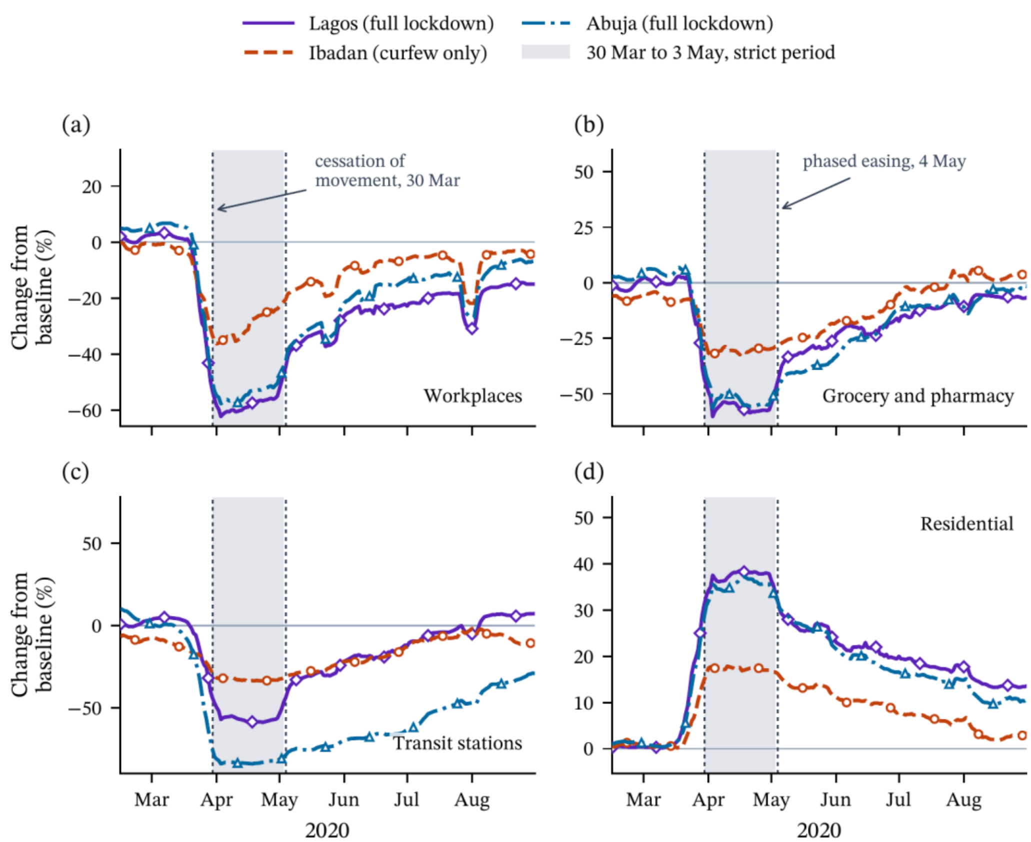

Figure 1. Observed change from baseline in activity through 2020 for four place categories, (a) workplaces, (b) grocery and pharmacy, (c) transit stations and (d) residential, in Lagos and Abuja under the cessation of movement order and in Ibadan under a curfew only. The shaded band marks 30 March to 3 May 2020, the strict period.

A second source anchors the health dimension, because the mobility series has no place category for health care. The Nigeria COVID-19 National Longitudinal Phone Survey, hereafter NLPS, run by the National Bureau of Statistics with the World Bank, interviewed households by telephone in April and May 2020 and reported that 34.3 percent of Nigerian households needed medical treatment during the period and that 25.6 percent of those could not obtain it. Health trip suppression is weighted to that second figure, and the NLPS baseline round is the source named in the health rows of Table 2.

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3.2 The respondent sample

The questionnaire was administered in the three cities once the orders had been eased, and returned usable responses from 1,080 people. Each record carries age band, gender, education, occupation category, household income band, vehicle and motorcycle ownership, distance to the nearest market, health facility and workplace, and the mode usually used before restrictions. For each of four trip purposes, work, market or food, health care, and other, respondents reported whether they made the trip during the restrictions and by what mode. Table 1 reports the composition.

Table 1. Composition of the respondent sample, by city.

	Lagos	Ibadan	Abuja	All
Respondents	420	360	300	1,080
Restriction regime	Full lockdown	Curfew only	Full lockdown	–
Female (%)	42.9	39.7	40.0	41.0
Essential worker (%)	10.2	5.6	11.7	9.1
In employment (%)	77.9	78.3	79.3	78.4
Car in household (%)	16.4	12.5	24.7	17.4
Motorcycle in household (%)	7.6	18.9	12.0	12.6
Median income (NGN/month)	72,000	33,000	72,000	72,000
Median distance to market (km)	1.04	0.75	1.33	1.00
Median distance to a health facility (km)	2.79	2.34	2.52	2.53
Median distance to work (km)	6.58	4.12	7.71	5.70
Within 2 km of a market (%)	86.4	94.2	76.7	86.3

Note. $n = 1{,}080$ respondents (Lagos 420, Ibadan 360, Abuja 300). Lagos and Abuja were under the federal cessation-of-movement order of 30 March 2020; Ibadan (Oyo State) was under a state curfew only. Gender, education and the formal/informal employment split are calibrated to Salau (2018), a household travel survey of 1,351 Lagos households. NGN, Nigerian naira.

Sample weights were solved so that aggregate trip making reproduces the observed series, by the procedure described next.

3.3 Calibration

Sample weights were solved per city and per purpose so that the sample's aggregate change in trip making reproduces the observed change in the corresponding mobility category: work trips against workplace activity, market trips against grocery and pharmacy, and health trips against the national health access figures. Discretionary trips are calibrated against retail and recreation. Pre-restriction mode shares are calibrated to Salau (2018).

Table 2 and Figure 2 report how well this holds. The largest residual on any calibrated cell is 1.74 percentage points, and pre-restriction mode shares sit within 0.6 percentage points of the published Lagos values. Health care is calibrated nationally rather than by city, because the observed source is national, and Figure 2 marks it separately for that reason.

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Table 2. Calibration against the observed mobility series. Target is the observed percentage change over 30 March to 3 May 2020, net of the observed pre-restriction mean; realized is the percentage change in weekly trips in the sample.

Trip purpose	City	Observed source	Target (%)	Realized (%)	Residual (pp)	Weighted
Work	Lagos	Workplaces	-59.5	-59.2	+0.34	yes
	Ibadan	Workplaces	-28.0	-27.3	+0.65	yes
	Abuja	Workplaces	-58.9	-59.1	-0.15	yes
Market or food	Lagos	Grocery and pharmacy	-55.4	-56.3	-0.89	yes
	Ibadan	Grocery and pharmacy	-23.9	-23.4	+0.51	yes
	Abuja	Grocery and pharmacy	-56.0	-54.2	+1.74	yes
Other	Lagos	Retail and recreation	-68.8	-68.6	+0.21	yes
	Ibadan	Retail and recreation	-34.4	-35.5	-1.06	yes
	Abuja	Retail and recreation	-68.9	-69.1	-0.19	yes
Health care	Lagos	NLPS, national	-25.6	-28.1	-2.54	no
	Ibadan	NLPS, national	-25.6	-19.7	+5.91	no
	Abuja	NLPS, national	-25.6	-26.2	-0.59	no
Health care	Pooled	NLPS, national	-25.6	-25.0	+0.60	yes

Note. Observed source is the Google COVID-19 Community Mobility Report for Nigeria, 2020, by state (Lagos, Oyo, Federal Capital Territory), except health care, which has no Google place category and is anchored instead to the NBS and World Bank Nigeria COVID-19 National Longitudinal Phone Survey, baseline round, April to May 2020: 25.6 percent of households needing medical treatment were unable to access it. Health care is weighted once on the pooled national figure, so its three city rows are not separate targets. Largest residual on a calibrated cell, 1.74 percentage points. pp, percentage points.

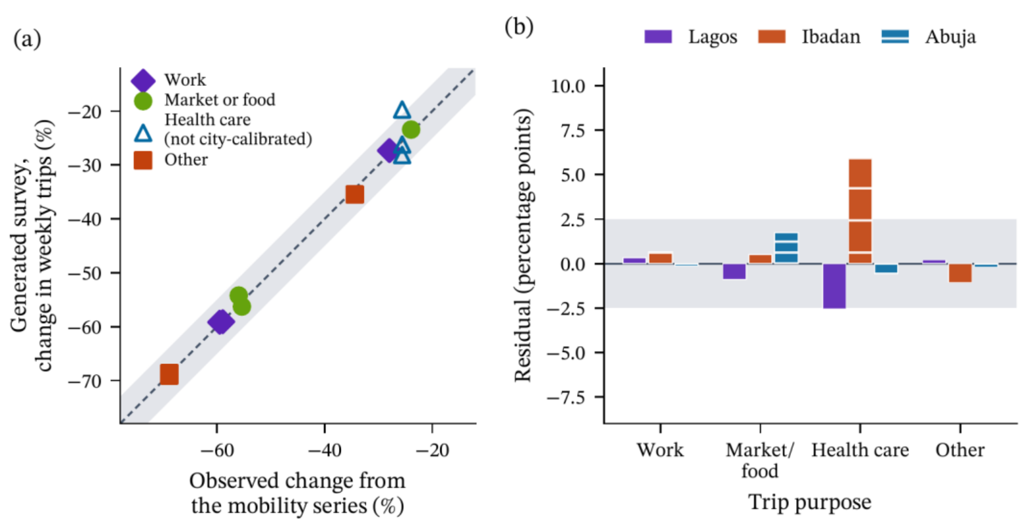

Figure 2. Calibration against the observed mobility series: (a) realized change in weekly trips against observed change by purpose, and (b) residuals by city and purpose. Health care is calibrated nationally rather than by city because the observed source is national.

IV. METHODS

4.1 Whether the trip was made

For each respondent and purpose we model the binary outcome of making the trip at all during restrictions, conditional on having a stated need for it, by logistic regression on purpose, city, occupation, income band, ownership and distance. The coefficient on purpose is the deferrability estimate and is reported as an odds ratio against discretionary travel.

One modeling decision matters for how the work result reads. A respondent whose workplace was suspended is recorded as not making a work trip, which is correct as a description of movement and misleading as a statement about deferrability, since the trip was not declined but abolished. We report both the pooled figure and the figure with suspended workers excluded, and note that suspension status is inferred from sector and employer type rather than observed directly.

4.2 Which mode was used

Conditional on a trip being made, mode choice is modeled by multinomial logit over walking, private car, motorcycle, tricycle, minibus and bus or taxi, with usual mode, ownership, distance, income and city as predictors.

Availability is handled explicitly rather than implicitly. Where a mode was suspended in a city on a date, it is removed from that observation's choice set rather than left in it as an unchosen alternative, following Swait and Ben-Akiva (1987). Treating a suspended minibus as merely unattractive would attribute a policy effect to preference.

How much commercial service actually kept running is the one input the orders do not settle. They state that commercial passenger transport was suspended under the cessation of movement and permitted in daytime hours under the Oyo curfew, but no source quantifies the fraction of operators who complied, so the probability that a commercial mode was available during the strict period is assumed from the content of the orders: in Lagos, 0.18 for commercial motorcycles, 0.12 for tricycles, 0.14 for minibuses and 0.10 for buses and taxis; in Abuja, 0.16, 0.12, 0.13 and 0.12; and in Ibadan, where only a night curfew applied, 0.55, 0.48, 0.50 and 0.42. Essential workers held movement permits, so their availability is lifted toward one by a retention factor of 0.45 on the complement. A private car is in the choice set if and only if the household owns one, and walking is always available. Results in Sections 5 and 6 that turn on whether a mode was there to be chosen, the transition matrix and the retention differences between usual modes among them, are conditional on those availability figures. What they describe is the distributional consequence of suspending the modes the orders suspended at the service levels assumed here, which is a statement about policy design rather than about preference, and Section 7 treats it as one.

We test the independence of irrelevant alternatives assumption with both Hausman and McFadden (1984) and Small and Hsiao (1985) and report the outcome whichever way it falls, including where the tests disagree.

4.3 The walkable fallback test

The paper's central hypothesis is that a household able to walk to its destination could keep an essential trip when its usual mode disappeared, and that this mattered more where income could not buy an alternative. We operationalize a walkable fallback as a destination within a threshold distance, estimate its effect on keeping an essential trip, and interact it with income band.

Because the threshold is a modeling choice rather than a natural quantity, we estimate the interaction at 1.0, 1.5, 2.0, 2.5 and 3.0 kilometers and also with distance entering continuously, and we report all six. A hypothesis that survives only at one threshold is a different object from one that holds across the range, and Section 6 treats it as such.

V. RESULTS

5.1 How much travel stopped, and which travel

Table 3 and Figure 3 report trip rates. Weekly trips fall from 9.61 to 4.97 per respondent, a reduction of 48.2 percent, with discretionary travel down 57.6 percent, work 48.6, market or food 44.8 and health care 25.0. The ordering is the one Noland et al. (2003) found under fuel shortage, with discretionary travel curtailed first and health care last.

The participation figures carry the picture further. The share of respondents making at least one market trip falls from 98.1 to 74.7 percent, so a quarter of the sample stops shopping entirely for the period, and health care participation falls only from 34.6 to 26.3 percent.

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Conditioning on having a trip to make, the city contrast appears in participation as sharply as in the aggregates. Among respondents with a work trip to make, 85.1 percent in Ibadan made at least one during the restriction period against 46.2 percent in Lagos and 47.5 percent in Abuja. Among those with a market trip to make the figures are 92.9, 67.4 and 68.5 percent, and among those with a discretionary trip to make, 74.8, 45.0 and 43.8. Health care is the exception, at 75.0 percent in Ibadan, 74.3 in Lagos and 78.9 in Abuja, a spread of under five points between regimes that differ by forty on work travel. Health trips were made at close to the same rate whether the city was under a curfew or a cessation of movement. Health care is weighted on a single national figure rather than city by city, so its three city figures were free to differ, and they barely do.

Figure 3(b) breaks the reduction by income band. Income enters trip retention alongside vehicle ownership, which acts through availability, so the panel reports the size of the difference between bands once both are accounted for. Discretionary travel falls 56.1 percent in the lowest income band against 38.1 percent in the highest, and health travel 24.4 percent against 15.6. The ownership split is sharper than the income split. Households with a car cut market travel by 30.4 percent against 47.8 percent for households without one, and health travel by 15.9 percent against 26.8. Essential workers are the other group the design separates, with work travel down 7.6 percent against 55.1 percent for other formal employees, a gap that follows from the movement permits they held.

Table 3. Trips per person per week and mode shares, before and during the restriction period. The upper block is trip rates by purpose; the lower block is the share of realized trips by mode.

	Before	During	Change (%)	Made ≥ 1 before (%)	Made ≥ 1 during (%)
Work	3.92	2.02	-48.6	78.4	46.7
Market or food	3.26	1.80	-44.8	98.1	74.7
Health care	0.39	0.29	-25.0	34.6	26.3
Other	2.04	0.86	-57.6	90.2	49.2
All purposes	9.61	4.97	-48.2	–	–
Walk	5.3	31.0	+25.7	–	–
Private car	9.8	16.1	+6.3	–	–
Motorcycle	31.3	27.3	-4.0	–	–
Tricycle	15.7	7.9	-7.8	–	–
Minibus	30.7	14.1	-16.6	–	–
Bus or taxi	7.1	3.6	-3.5	–	–

Note. $n = 1{,}080$ respondents and 3,254 person-purpose observations with a stated need. Mode shares in the lower block are shares of trips actually made in each period, so the two columns rest on the same unit. Pre-pandemic mode shares are calibrated to Salau (2018). Change is in percentage points for the mode block and percent for the trip-rate block.

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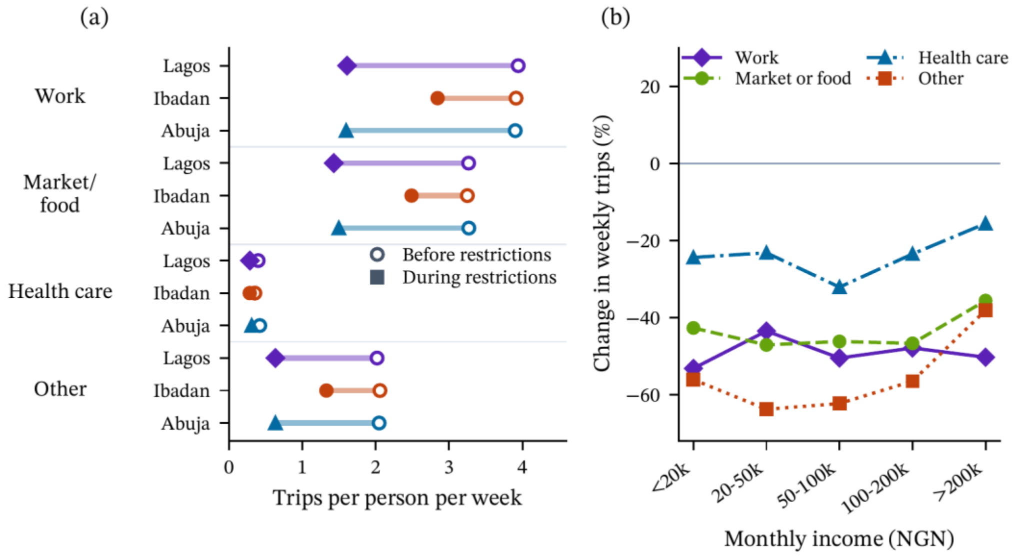

Figure 3. Trip rates: (a) weekly trips per person by purpose and city, before and during restrictions, and (b) the percentage change in weekly trips by monthly income band and purpose. The aggregate reduction of 48.2 percent spans 57.6 percent for discretionary travel to 25.0 percent for health care.

5.2 What people substituted to

The mode shares in Table 3 are computed over trips actually made in each period, so the two columns rest on the same unit. Walking rises from 5.3 to 31.0 percent of trips made, private car from 9.8 to 16.1, while minibus falls from 30.7 to 14.1 and tricycle from 15.7 to 7.9, and motorcycle holds up better than the other shared modes at 31.3 to 27.3. The pre-restriction column is calibrated to Salau (2018).

Table 4 and Figure 4 give the full transition matrix, which asks who moved between modes rather than how many trips were made in total. Retention on the diagonal varies enormously: 68.4 percent of car users kept using a car and 48.3 percent of walkers kept walking, against 16.8 percent of minibus users and 13.1 percent of tricycle users.

The diagonal and the stopping column both turn on the availability structure of Section 4: a car was in its owner's choice set throughout and a minibus was in its user's choice set with probability 0.14 in Lagos. What the matrix adds is the size of the gap and where the displaced trips went.

Among respondents whose usual mode was a minibus, 39.6 percent made no trip at all, and among tricycle users 35.9 percent, against 21.9 percent of car owners and 25.3 percent of those who already walked. Suspending the shared modes therefore removes travel from their users at close to twice the rate it removes travel from car owners, and it does so in a sample where car ownership is a 17.4 percent minority condition. Read as a statement about policy design rather than about preference, that is the result the rest of the paper builds on.

Where minibus and tricycle users did travel, they walked. Walking is the destination for 22.8 percent of minibus users and 26.4 percent of tricycle users, more than any alternative vehicle. That is the fallback the paper is about, and it is the only one most of the sample had.

Table 4. Mode transition matrix. Each row is the mode a respondent used before the restrictions; each cell is the percentage of that row's person-purpose observations that used the column mode during the restrictions, with the trip not made at all recorded as No trip. Rows sum to 100.

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	Walk	Car	Okada	Keke	Danfo	Bus/taxi	No trip	n
Walk	48.3	4.6	8.6	2.9	8.6	1.7	25.3	174
Private car	3.1	68.4	2.2	2.5	1.2	0.6	21.9	320
Motorcycle	15.0	3.6	36.8	2.2	6.4	1.9	34.1	1,018
Tricycle	26.4	4.7	11.7	13.1	5.5	2.7	35.9	512
Minibus	22.8	3.8	9.5	5.0	16.8	2.4	39.6	998
Bus or taxi	21.6	7.3	12.1	6.5	8.2	6.5	37.9	232

Note. $n = 3{,}254$ person-purpose observations across four trip purposes for 1,080 respondents. Okada, commercial motorcycle; keke, commercial tricycle; danfo, minibus. Row percentages.

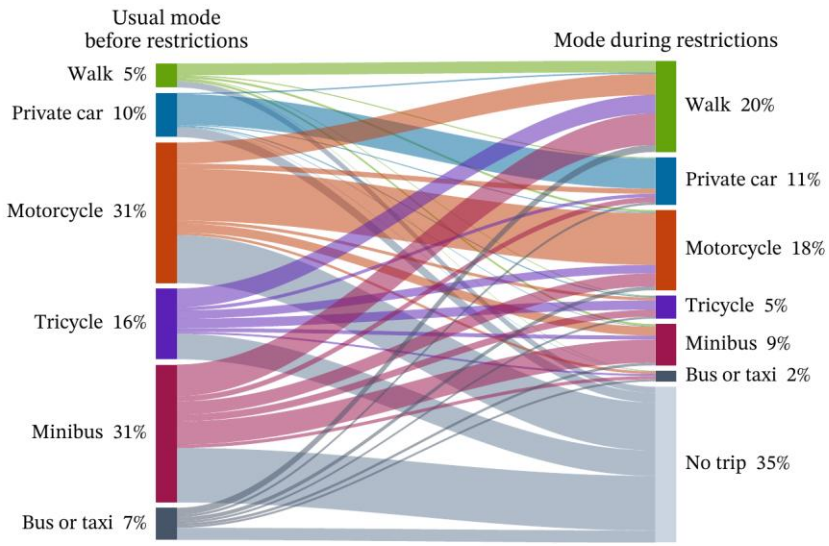

Figure 4. Sankey diagram of the transition from usual mode before restrictions to mode used during them, with cessation of travel carried as a destination. Band widths are shares of all 3,254 person-purpose observations. Users of shared modes are the most likely to stop traveling entirely, and where they do travel they walk.

5.3 Which trips proved non-deferrable

Table 5 and Figure 5 report the participation model. Against discretionary travel as the reference, health trips carry 3.06 times the odds of being made and market trips 2.43, both at $p < 0.001$. The pooled model returns a pseudo R^2 of 0.160 with a likelihood ratio statistic of 671 on 13 degrees of freedom.

One entry in the per-purpose columns runs against the direction this paper argues for, and it should be stated rather than passed over. In the market logit, having the destination within two kilometers carries an odds ratio of 0.51 with $p = 0.046$, so on that specification proximity to a market is associated with fewer market trips rather than more. Two features of the market column account for the sign. That column also carries distance as a continuous term, and at 0.58 per kilometer it is the strongest distance effect anywhere in the table, so the continuous term has already taken the distance variation before the indicator is reached. And market distances are short and tightly bunched: the median is 1.00 kilometer and 86.3 percent of respondents fall inside the two kilometer cut, leaving the indicator to be estimated as a residual contrast on the small minority beyond it. The pooled 1.38 in Table 9 is not the same quantity and does not contradict this one, because that model carries no continuous distance term, so the indicator there stands for distance instead of standing net of it. We report the 0.51 because it is what the specification returns; we do not read it as evidence that walking access suppresses market travel, and no claim in this paper rests on it.

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Work sits at 1.46 and work deferrability is not identified by this design. A respondent whose workplace was suspended cannot make a work trip regardless of how essential that trip is, and those respondents are in the denominator, so 1.46 conflates a policy effect with a behavioral one. Conditioning the sample on workplaces that remained open moves the figure by an order of magnitude, but that conditioning is on an inferred status rather than an observed one, so the conditional estimate is not a deferrability estimate either.[1] We therefore report 1.46, state that it is not a deferrability estimate, and leave the question to a design that observes employer status.

Table 5. Odds of making the trip at all during the restriction period. Column one is a pooled logit over all four purposes with purpose dummies; the remaining columns are separate logits, one per purpose. Entries are odds ratios.

	Pooled OR	95% CI	Work	Market	Health	Other
Work	1.46***	[1.17, 1.82]	–	–	–	–
Market or food	2.43***	[1.92, 3.08]	–	–	–	–
Health care	3.06***	[2.29, 4.09]	–	–	–	–
Ibadan	4.53***	[3.66, 5.61]	7.58***	5.91***	1.13	3.84***
Abuja	0.92	[0.76, 1.11]	1.04	1.02	0.90	0.78
Essential worker	6.17***	[4.29, 8.88]	33.76***	4.34***	3.95*	3.07***
Income, per log point	1.05	[0.95, 1.15]	0.98	1.01	1.01	1.15
Female	0.96	[0.81, 1.13]	0.95	1.02	0.89	0.88
Aged 65 or over	0.80	[0.55, 1.18]	1.21	0.83	0.45	0.64
Car in household	2.12***	[1.66, 2.70]	1.57*	3.15***	1.79	2.59***
Motorcycle in household	2.22***	[1.68, 2.95]	1.31	4.21***	3.00*	2.53***
Destination within 2 km	1.27*	[1.02, 1.58]	0.89	0.51*	0.97	1.45*
Distance, per km	0.96***	[0.94, 0.98]	0.97**	0.58***	0.93	0.94*
Pseudo R^2	0.160		0.214	0.151	0.055	0.135
n	3,254		847	1,059	374	974

Note. Pooled model $n = 3{,}254$ person-purpose observations with a stated need; per-purpose n in the last row. Reference category for purpose is other trips, and for city is Lagos. Purpose dummies do not appear in the per-purpose columns. Likelihood ratio test of the pooled model against a constant-only model, $\chi^2(13) = 671.3$, $p < 0.001$. OR, odds ratio; CI, confidence interval. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

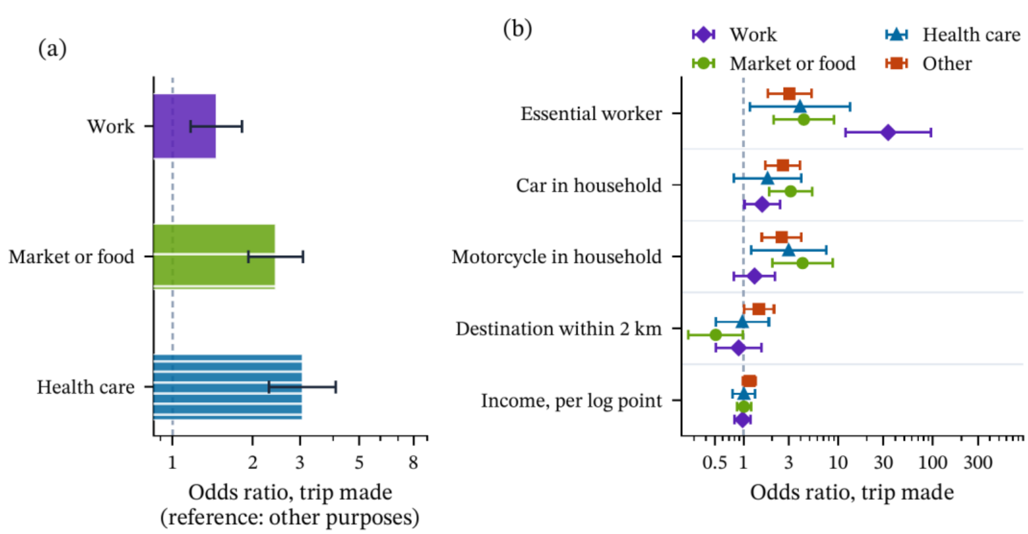

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Figure 5. Odds of making a trip during restrictions: (a) odds ratios by trip purpose from the pooled logit, against discretionary travel as the reference, and (b) odds ratios on respondent characteristics from the four separate per-purpose logits, on a logarithmic scale. Whiskers are 95 percent confidence intervals and the dashed line marks an odds ratio of one.

5.4 The mode choice model

Table 6 and Figure 6 report the availability-masked multinomial logit, estimated on 2,126 observations with a pseudo R^2 of 0.2712 and a likelihood ratio statistic of 779.8.

The availability structure of Section 4 enters every choice set, so the coefficients are conditional on it and describe behavior under the orders rather than what a Lagos traveler prefers in general. On that footing the model estimates a stable utility structure from choice sets averaging 2.72 alternatives, which is the property a wider application of this design would need. Three average marginal effects carry the substitution story, and we report them as marginal effects on choice probability rather than as utility coefficients, because the probability scale is the one the argument uses. Motorcycle ownership in the household raises the probability of choosing a motorcycle by 0.201 and lowers the probability of walking by 0.055. Having usually used a shared mode lowers the probability of walking by 0.075 and raises the probability of a motorcycle by 0.079, holding distance constant, which reflects the persistence of the usual mode landing on the one shared service the orders left most widely available rather than any preference for motorcycles. And each additional kilometer lowers the probability of walking by 0.058 while raising the probability of a motorcycle by 0.019, which is the substitution the walking fallback eventually runs out of.

Neither test of the independence of irrelevant alternatives rejects, and neither result is worth much. The Hausman and McFadden (1984) statistic could not be computed cleanly: the difference of covariance matrices failed to be positive semi-definite in all five alternative-exclusion cases, so no exclusion yields a usable statistic, which is a known degenerate outcome rather than a pass. The Small and Hsiao (1985) test returns p values between 0.986 and 0.9999 across all five exclusions, rejecting nowhere.

We report that as an absence of evidence rather than evidence of absence. The test statistics range from 12.3 to 19.9 on 36 degrees of freedom, and the mean choice set in this sample contains 2.72 alternatives because availability masking removes suspended modes. A test with that many parameters on 2,126 observations with choice sets that small has very little power, so a non-rejection is close to uninformative. The substantive concern remains live: motorcycle and minibus are close substitutes and a nested structure grouping the shared motorized modes is the specification a larger sample should test. We did not estimate one, because on this sample the data could not distinguish it from the flat model.

Table 7 reports the substitution the model predicts, conditional on a trip being made, and it is worth setting beside the observed transitions of Table 4. The model reproduces the strong diagonal for private car, at 87.2 percent predicted, and the heavy flow from every shared mode into walking. Where it departs is on the modes it has least information about: it predicts 56.1 percent retention for walkers against 48.3 percent observed, because the model has no way to represent a walker whose destination simply closed. The gap between the two tables is a measure of how much of the transition structure the choice model explains.

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Table 6. Mode chosen during the restriction period, conditional on the trip having been made. Multinomial logit estimated on the choice set each respondent actually faced; walking is the reference alternative. The last column is the average marginal effect on the probability of walking.

	Car	Okada	Keke	Danfo	Bus/taxi	AME, walk
Usual mode was shared	-0.42	1.34***	0.82*	1.06***	-0.29	-0.075***
Usual mode was a private car	0.95	0.50	0.73	0.49	-0.82	-0.032
Motorcycle in household	-0.28	2.49***	-0.23	-0.04	-0.94	-0.055***
Distance, per km	0.74***	0.75***	0.75***	0.79***	0.80***	-0.058***
Income, per log point	1.54***	0.37***	0.53***	0.52***	1.21***	-0.046***
Essential worker	0.90*	0.59*	0.10	0.23	0.29	-0.027
Ibadan	0.21	0.09	0.08	-0.39	0.16	+0.004
Abuja	0.28	-0.11	0.06	-0.34	-0.50	+0.012
Model fit						
Log likelihood	-1048.0					
McFadden pseudo R^2	0.271					
LR against constants only, $\chi^2(40)$	779.8***					
Mean choice set size	2.72 of 6					
n trips	2,126					

Note. $n = 2{,}126$ trips made during the restriction period, one observation per person-purpose. Coefficients are relative to walking. Private car ownership is excluded as a regressor because a private car was in the choice set if and only if the household owned one, which makes the dummy collinear with that alternative's availability. No motorcycle-owning respondent used a bus or taxi, which separates that cell completely, so the model is fitted with a ridge penalty of 1.0 on the coefficient vector; with n in the thousands the penalty is negligible wherever the data identify a coefficient, and no coefficient exceeds a magnitude of eight after it. Independence of irrelevant alternatives: Hausman-McFadden returned a statistic for 4 of the 5 excluded alternatives and a negative one for the fifth, but the difference of covariance matrices was not positive semi-definite in any of the 5, so no case is usable and none is counted as a test of the assumption. Small-Hsiao: computable for 5 of 5 excluded alternatives; IIA rejected at the 5 percent level for 0 of those 5, with p between 0.986 and 0.9999. AME, average marginal effect. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 7. Mode substitution predicted by the choice model, conditional on a trip being made, by usual mode before restrictions.

	Walk	Private car	Motorcycle	Tricycle	Minibus	Bus or taxi
Walk	56.1	6.4	14.4	6.3	13.6	3.2
Private car	4.7	87.2	2.6	2.9	1.4	1.1
Motorcycle	23.5	5.5	49.0	5.7	13.3	3.0
Tricycle	40.9	7.6	21.1	11.7	13.8	4.9
Minibus	38.5	6.5	20.9	9.5	19.8	4.8
Bus or taxi	30.7	10.7	22.7	13.6	17.7	4.6

Note. Row percentages, predicted by the availability-masked multinomial logit and conditional on a trip being made, so cessation of travel is not a column here as it is in Table 4. Comparing the two shows what the choice model captures and what it does not.

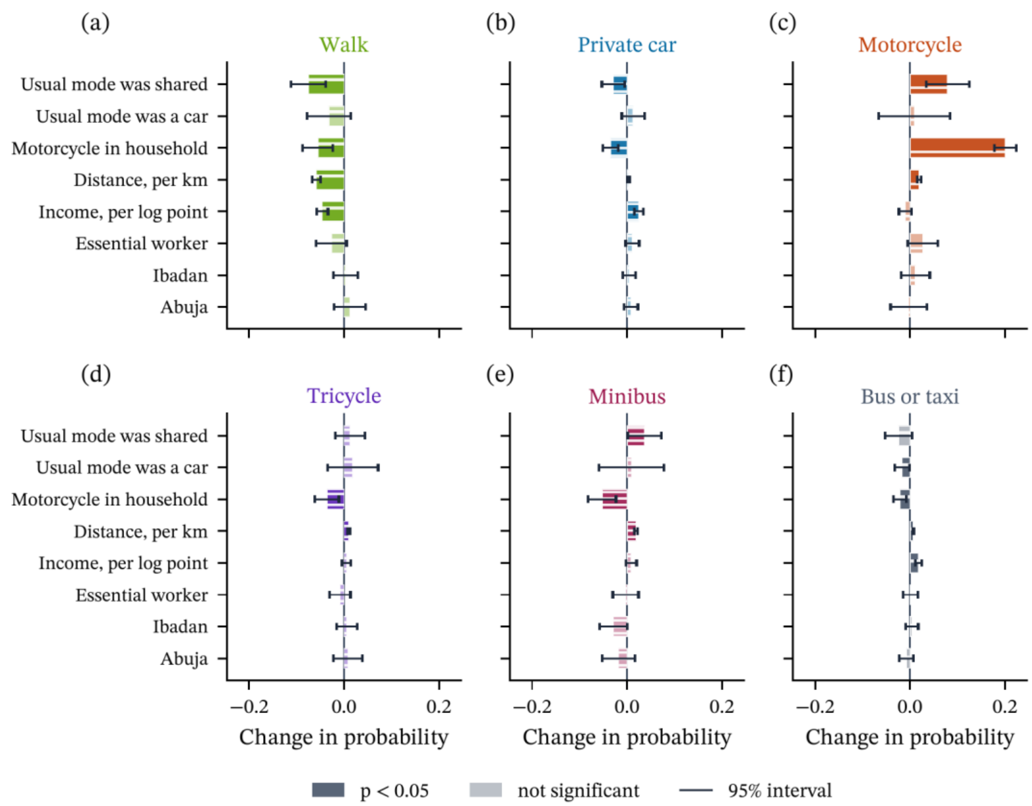

Figure 6. Average marginal effects on the probability of choosing each mode, from the availability-masked multinomial logit, one panel per alternative: (a) walking, (b) private car, (c) motorcycle, (d) tricycle, (e) minibus and (f) bus or taxi. Colored markers are significant at the 5 percent level, grey markers are not, and whiskers are 95 percent intervals. A complete separation, no motorcycle-owning respondent used a bus or taxi, is handled by a documented ridge penalty.

5.5 Checking the sample against published values

Table 8 sets the sample beside published values on eighteen quantities, covering pre-pandemic trip rates and mode shares, vehicle ownership, distance to markets and facilities, and the composition of the sample by occupation and income.

Fourteen fall inside the published range and one sits at its edge. Two fall outside it, and one is set against a published claim that states a direction and attaches no level to it, so what that row can confirm is an ordering rather than a value. Several of the eighteen are weighting margins, among them the pre-restriction trip rate and the pre-restriction mode shares calibrated to Salau (2018), the nine trip-reduction rows of Section 3.3, household car ownership and the share living within two kilometers of a market, so agreement on those follows from the weighting.

Four rows are worth stating on their own, because they are the ones the weighting does not place. The share of employed respondents making no work trip is 40.5 percent, and it is this row that sits at the edge of its range rather than inside it. Shared and paratransit modes carry 52.9 percent of trips made during restrictions, down from 84.8 percent before, against a published 56 percent, and that is the row of the four that lands inside. The walking share during restrictions is 5.8 times the share before, where the one published series available for comparison moves walking by a factor of 1.7, so the sample substitutes into walking far

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more heavily than that series records. Car retention runs 4.1 times minibus retention, and since the published claim behind that row gives no level, the ordering is confirmed and the size is not.

One further row falls short of its published value. The share of respondents making no trip at all is 11.3 percent against 19 percent staying home completely in the published comparison, so this sample carries less outright immobility than the settlement survey it is set beside. That miss and the walking ratio run together rather than in opposite directions: a sample in which fewer people stop moving altogether has more surviving trips to reallocate, and walking is where they go. Both turn on how much travel the availability structure of Section 4 leaves possible, which Section 7.4 records as the assumption this study leans on hardest.

What these checks cannot establish is that the sample's joint structure is right. Each is a marginal, and a sample can match every marginal in a population while getting the associations between them wrong. Those associations are what Sections 5 and 6 estimate, and they are reported at two different levels of confidence for that reason.

Table 8. The sample checked against published values, eighteen quantities.

Quantity	Published	Sample	Verdict
Trips per person per day, before	1.5 for all respondents, 1.4 for public transport users, 1.1 for private car users	1.373	inside
Mode share before, Walk (%)	Salau reports 6.2 percent for walking and cycling together as the most frequent mode	5.3	inside
Mode share before, Private car (%)	Salau reports 10.1 percent using a personal automobile most frequently	9.8	inside
Mode share before, all public and paratransit (%)	83.8 percent naming public transport as the most frequent mode	84.8	inside
Car in household (%)	10.1 percent use a personal automobile most frequently, so household ownership sits somewhat above that	17.41	inside
Respondents within 2 km of a market (%)	53.2 percent make a walk trip every day, and the commercial zone shows the highest walk trip share at 40 percent	86.3	inside
Work trip reduction, Lagos (%)	workplace trip reductions across sub-Saharan Africa ran from about 25 percent to about 60 percent	59.18	inside
Work trip reduction, Ibadan (%)	workplace trip reductions across sub-Saharan Africa ran from about 25 percent to about 60 percent	27.32	inside
Work trip reduction, Abuja (%)	workplace trip reductions across sub-Saharan Africa ran from about 25 percent to about 60 percent	59.09	inside
Essential shopping trip reduction, Lagos (%)	short-term reductions in essential shopping trips ran between about 25 and about 60 percent	56.27	inside
Essential shopping trip reduction, Ibadan (%)	short-term reductions in essential shopping trips ran between about 25 and about 60 percent	23.42	inside
Essential shopping trip reduction, Abuja (%)	short-term reductions in essential shopping trips ran between about 25 and about 60 percent	54.23	inside
Health care trips forgone (%)	25.6 percent of households needing medical treatment were unable to access it; 23.8 percent of those failures were attributed to movement restrictions	25	inside
Respondents making no trip at all (%)	19 percent of Nairobi informal settlement respondents stayed home completely, 54 percent moved within the neighborhood only	11.3	outside
Employed respondents making no work trip (%)	the share of main respondents working fell from 86 percent in mid-March to 42 percent in April/May, a fall of 51 percent	40.5	edge
Walk share during, ratio to before	47 percent of the expert panel perceived the walking and cycling segment of the modal split to have increased; one reported series moves walking from 6 to 10 percent, a ratio of 1.7	5.849	outside
Shared and paratransit share during (%)	56 percent of those who traveled across Nairobi used public transport during the restriction period	52.9	inside
Car retention relative to minibus retention (ratio)	the individualized private mode segment held up while the shared segment did not; the published claim is directional, with no level attached	4.071	direction only

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Note. Verdict compares the sample value with the published value beside it: inside its range, at the edge of it, outside it, or, where the published claim gives a direction and attaches no level to it, direction only. Several of the quantities listed are weighting margins, so agreement on those follows from the weighting rather than standing as an independent comparison.

VI. THE WALKABLE FALLBACK HYPOTHESIS

We designed the instrument to test a specific hypothesis: that a household able to walk to its destination could keep an essential trip when its usual mode disappeared, and that this mattered more where income could not buy an alternative. The hypothesis is reasonable, the Sierra Leone distance gradient of Peak et al. (2018) supports it, and the test is straightforward to run. It does not survive.

Table 9 and Figure 7 report it in full. Because the walkable threshold is a modeling choice rather than a natural quantity, we estimated it at five distances and also with distance entering continuously, and the pattern across those six specifications is what settles the matter.

The main effect is swept across the five cut points in a model that drops the income interaction, and that sweep is reported in the note to Table 9. A walkable fallback raises the odds of keeping an essential trip by 1.62 at a one kilometer threshold with $p = 0.001$, and by 1.30, 1.35, 1.42 and 1.43 at 1.5, 2.0, 2.5 and 3.0 kilometers with p values of 0.075, 0.065, 0.054 and 0.082. One of five clears conventional significance. The 1.38 in the body of the table is the same term at the two kilometer cut with the income interaction left in, where it clears the conventional level by a hair at $p = 0.0497$. The gap between 1.35 and 1.38 is the interaction term rather than a discrepancy, and the reading it supports, that a main effect resting on $p = 0.0497$ in one specification and $p = 0.065$ in its neighbor is not a durable finding, is the reading this section arrives at anyway. On the interaction with income, the coefficient is -0.390 at one kilometer with $p = 0.007$ and -0.293 at two kilometers with $p = 0.043$, and is not significant at 1.5, 2.5 or 3.0. Entering distance continuously returns a null: distance itself carries a coefficient of -0.127 per kilometer, but its interaction with log income is 0.0002 with $p = 0.996$, and the likelihood ratio test of that interaction is indistinguishable from zero.

Two readings are available and this design cannot separate them. The effect may be genuinely threshold-like, which is physically reasonable, since walking is a decision with a limit rather than a gradient. Or the two surviving specifications may be what one expects from six tests at conventional significance levels. The non-monotonic pattern, significant at 1.0, absent at 1.5, marginal at 2.0, absent again at 2.5, is not what a real threshold would look like: a genuine limit at some distance between one and two kilometers should produce significance that decays rather than oscillates. There is a further reason for caution that has nothing to do with p values. The estimate is sensitive to how the individual level is specified. A reduced specification, without person-level heterogeneity and without a workplace suspension term, returns a different answer to this same question. Both terms were retained on grounds independent of this hypothesis, and the interaction moved as a consequence. An effect that is contingent on specification choices made for unrelated reasons is not yet established.

Figure 7(b) re-expresses the two kilometer interaction as a slope within each income band, and those numbers are worth stating because they are the ones a reader would want to carry away. A walkable fallback carries an odds ratio of 2.10 on keeping an essential trip in the lowest income band, then 1.56, 1.24, 1.02 and 0.85 as income rises. The gradient is orderly and it is exactly the pattern the hypothesis predicts. It is also a re-expression of one interaction coefficient that fails at three of the five thresholds, so the orderliness reflects a single interaction term rather than an independently estimated gradient. Replacing the log-linear income term with the five bands entered jointly, which is the specification that gives the hypothesis its best chance, returns a likelihood ratio statistic of 9.32 on four degrees of freedom with $p = 0.054$, and that fails at the conventional level too.

Two further checks point the same way. Restricting the test to work trips, where only 10.0 percent of destinations fall within two kilometers, gives a

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walkable odds ratio of 1.26 with $p = 0.44$ and an income interaction of -0.285 with $p = 0.31$. And the mechanism the hypothesis implies, that a walkable fallback should matter most where no motorized fallback exists, does not appear. The average marginal effect of a walkable fallback is $+0.060$ for households holding neither a car nor a motorcycle and $+0.016$ for households holding one, a contrast of $+0.045$ whose bootstrap interval runs from -0.051 to $+0.140$. On the log-odds scale that interaction is not estimable for work trips at all, because every walkable work trip in a household with a motorized fallback was kept.

We therefore report this as a question the design cannot answer rather than as a result with a caveat attached. The distinction matters practically: a hypothesis reported with a p value in an abstract gets cited without its qualification, and this one should not be. What the exercise does produce is a precise specification for the study that could answer it, set out in Section 7.

Table 9. Keeping an essential trip: the walkable fallback, its interaction with income, and its interaction with holding a motorized fallback. Logit on whether a market or health care trip was made at all during the restriction period.

	Coefficient	SE or 95% CI	OR
Walkable fallback (within 2 km)	0.323*	(0.165)	1.38
Income, per log point	0.361**	(0.123)	1.44
Walkable × income	-0.293*	(0.145)	0.75
Essential worker	1.420***	(0.313)	4.14
Car in household	–	–	–
Motorcycle in household	–	–	–
Interaction with income			
LR test, $\chi^2(1)$	4.10	$p = 0.04$	
Income as five bands, LR $\chi^2(4)$	9.32	$p = 0.054$	
Interaction with a motorized fallback			
Walkable OR, no motorized fallback	1.37		
Walkable OR, motorized fallback held	1.18		
LR test of the interaction, $\chi^2(1)$	0.14	$p = 0.70$	
AME of a walkable fallback, no motorized fallback	+0.060	[-0.007, 0.133]	
AME of a walkable fallback, motorized fallback held	+0.016	[-0.053, 0.093]	
Contrast of the two marginal effects	+0.045	[-0.051, 0.140]	$p = 0.38$
Model fit			
Pseudo R^2, without and with the income interaction	0.0733	0.0759	
LR against a constant-only model, $\chi^2(9)$	119.5	$p < 0.001$	
n	1,433		

Note. $n = 1{,}433$ person-purpose observations for market or food and health care trips among respondents with a stated need. All models include city and purpose fixed effects and control for sex and age 65 or over. Vehicle ownership is omitted from the income interaction model because it lies on the path from income to holding a motorized fallback; the model that adds it back leaves the income interaction at -0.324 ($p = 0.03$). A motorized fallback means the household owned a car or a motorcycle. Confidence intervals on the marginal effects are nonparametric bootstrap percentiles over 1,000 resamples. The 0.323 in the first row is the walkable term in this model, the one that carries the income interaction, and it clears the conventional level narrowly at $p = 0.0497$. The threshold sweep is a different specification: with the income interaction dropped and the cut point moved, the walkable main effect gives odds ratios between 1.30 and 1.62 across cut points from 1.0 to 3.0 km, and only the 1.0 km estimate clears $p < 0.05$, the five p values being 0.001, 0.075, 0.065, 0.054 and 0.082. The two kilometer entry in that sweep is an odds ratio of

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1.35 at $p = 0.065$, so the sweep and the first row of this table differ by the interaction term rather than contradicting each other. AME, average marginal effect; OR, odds ratio; SE, standard error; LR, likelihood ratio. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

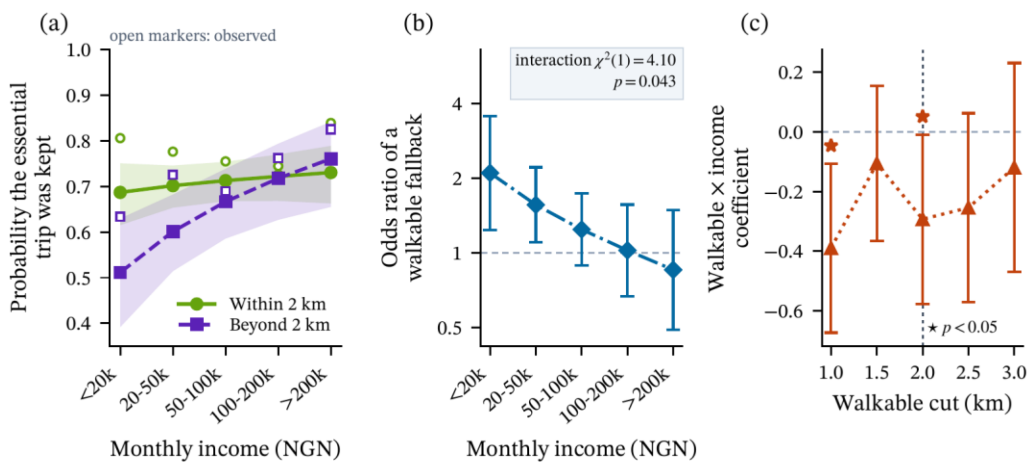

Figure 7. The walkable fallback test across specifications: (a) predicted probability of keeping an essential trip against income, within and beyond two kilometers, (b) the odds ratio of a walkable fallback by income band, and (c) the interaction coefficient at five distance thresholds. Panel (c) is why this is reported as a question the design cannot answer: the effect is significant at 1.0 and 2.0 kilometers and absent at 1.5, 2.5 and 3.0, which is not the decay a genuine threshold would produce.

VII. DISCUSSION

7.1 What the evidence supports, and what it does not

This paper reports one thing we are confident of and one we are not, and separating them is more useful than presenting a uniform level of certainty.

The descriptive result is solid because it sits close to the observed record. Travel fell 48.2 percent and the fall was strongly purpose-dependent, both of which the mobility series confirm directly. Walking absorbed the displacement and shared mode users bore the trip loss, with 39.6 percent of minibus users making no trip at all. The claim is narrower than it first reads as: suspending the modes the orders suspended concentrates the loss on the households that depend on them, and what this paper adds is the size of that concentration.

The inferential result is not solid. The walkable fallback interaction is significant at two of six specifications and null when distance is continuous. We report it because the question is the right one and the direction is interpretable.

The distinction matters for what a reader should do with each. The descriptive result is usable now. The inferential result is a design brief for the survey that would test it.

7.2 What follows for policy

The audience here is the Lagos State Ministry of Transportation, the Federal Capital Territory Transport Secretariat and the Oyo State counterpart, since those are the bodies that suspended services and would do so again.

The clearest implication is about which service to suspend. Restrictions in 2020 suspended the shared modes, and it is shared mode users who stopped traveling, at nearly twice the rate of car owners. A policy that suspends minibuses and tricycles while leaving private cars circulating does not distribute its burden evenly: it removes the only mode most households have while leaving intact the mode a minority has. If a future restriction needs to reduce contact rather than reduce mobility, then reduced-capacity operation of shared services, which is expensive and awkward, is the intervention that keeps essential travel available to the people who cannot otherwise make it.

The second implication concerns what to provide alongside a restriction. The observed series already shows grocery and pharmacy activity falling less than retail and recreation, and the survey reproduces that ordering at a 25.0 percent reduction in health travel and 44.8 percent in market travel against 57.6 percent in discretionary travel. Both surviving trip types are ones a walking fallback can serve only within a couple of kilometers. Where restriction is imposed, moving the destination toward the household, through local market provision or mobile clinic rounds, substitutes directly for the mobility being removed.

The third is a measurement point. The observed mobility series records that movement fell and cannot say whose or which, and a retrospective instrument recovers only what people can still recall. Recall loss falls hardest on short and non-home-based trips (Wolf et al. 2001; Bricka et al. 2012), which are exactly the walking trips this paper argues survived, so the walking shares reported here are more likely too low than too high. A standing instrument, capable of being fielded during a disruption rather than after it, would cost very little compared to the decisions it would inform.

7.3 Transferability

The specific numbers belong to three Nigerian cities in 2020 and should not be carried elsewhere. Two things do carry.

The first is the analytical point that a restriction's burden falls along the lines of prior mode holding, so that suspending a mode is equivalent to removing travel from the households that had no second option. That follows from the structure of the transition matrix rather than from anything particular to Nigeria. Anywhere a majority of trips are made on shared services that a restriction can suspend, the same asymmetry should appear, and the size of it is set by how much service the order actually leaves running, which operator counts during a restriction would settle.

The second is the design. Modeling availability explicitly, treating a suspended mode as absent from the choice set rather than as unchosen, is a small technical decision that changes the interpretation entirely, and it applies to any study of behavior under service withdrawal.

7.4 Limitations

How much of each commercial mode kept running during the strict period is the assumption this study leans on hardest. No source quantifies the surviving service level under either regime, so the availability figures were set from the content of the orders themselves. The retention differences between usual modes, the transition matrix, the predicted substitution pattern and the mode choice coefficients all depend on them, and so does the direction of the headline contrast between shared-mode users and car owners. Operator counts or cordon observation during a restriction would settle the availability structure directly and should be collected.

Distances to the nearest market, health facility and workplace are reported by respondents rather than measured, and the walkable fallback test turns on exactly where the walking threshold falls. That is why the test is reported across thresholds of 1.0, 1.5, 2.0, 2.5 and 3.0 kilometers and with distance entering continuously, rather than at a single cut point. Golob and Meurs (1986), Murakami and Wagner (1999) and Stopher et al. (2007) document the same reporting gap on trips themselves.

VIII. CONCLUSION

We surveyed 1,080 respondents across Lagos, Abuja and Ibadan and weighted the sample so that its aggregate trip making reproduces an observed mobility series, to ask which trips survived the 2020 movement restrictions and what people substituted to. The aggregate reductions track the observed mobility series. The burden of the restrictions fell along the lines of what a household was already using, given how much service the orders left running. Among respondents whose usual mode was a minibus, 39.6 percent made no trip at all, and among tricycle users 35.9 percent, against 21.9 percent of car owners and 25.3 percent of those already walking. Where shared-mode users did travel, they walked: walking is the destination for 22.8 percent of minibus users and 26.4 percent of tricycle users, more than any alternative vehicle. Restrictions that suspend shared services remove the only option most households have while leaving intact the option a minority has.

We also attempted a further question and could not answer it. Whether a walkable fallback protects essential travel, and protects the poor most, produces an effect that survives at one of five distance thresholds on the main effect and two of five on the interaction, and vanishes when distance enters continuously. The pattern oscillates rather than decays, which is not what a real threshold looks like, and the estimate moved when the specification was revised for reasons unrelated to it. We report that as a negative result rather than as a finding with a caveat, because a p value in an abstract travels without its qualification.

Eighteen external checks set the sample against published work. Fourteen fall inside the published range and one at its edge, two fall outside it, and the last is set against a claim that gives a direction and no level, so it confirms an ordering rather than a value. The two that fall outside are the walking ratio and the share making no trip at all, and both bear on how much travel the assumed availability structure leaves possible. Matching a marginal is in any case a necessary condition and not a sufficient one: a sample can match every marginal and still get the associations wrong, and the associations are what the inferential sections estimate.

The most transferable thing this study produces is the design. A suspended mode is absent from the traveler's choice set rather than merely unchosen, and modeling it that way is what separates the effect of the order from the preferences of the people it fell on. The survey that would answer the question we could not is specified in Section 7; it would need to be fielded during a disruption rather than after one, and it would need distance to the destination measured rather than reported, because the fragility sits precisely on where the walking threshold falls.

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