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Review Article

Impact of new rapid transit on physical activity: A meta-analysis

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ABSTRACT

New rapid transit investments have been motivated by environmental, economic, and health benefits. Given transit's potential to increase active travel, recent research leverages transit changes for natural experiment studies to examine physical activity outcomes. We aimed to quantify the association size, critically examine existing literature, and make recommendations for future studies to advance research and policies on active travel, transportation, and physical activity. Studies of physical activity impacts following transit interventions were systematically reviewed using seven health and transport databases (May–July 2017). Two investigators extracted data on sample size, intervention, pre- and post-intervention physical activity, and relevant measurement information. Inconsistency of results and estimated overall mean physical activity change post-intervention were assessed. Forest plots were created from physical activity change in each study using a general variance-based random effects model. Of 18 peer-reviewed articles examining health behaviors, 15 addressed physical activity and five were natural experiment studies with pre- and post- intervention measurements. Studies varied by intervention, duration, outcome measurement, sampling location, and spatial method. Q (201) and I^2 (98%) indicated high study heterogeneity. Among these five studies, after transit interventions, total physical activity decreased (combined mean - 80.4 min/week, 95% CI - 157.9, −2.9), but transport-related physical activity increased (mean 6.7 min/week, 95% CI - 10.1, 23.5). Following new transit infrastructure, total physical activity may decline but transport-related physical activity may increase. Positive transit benefits were location, sociodemographic, or activity-specific. Future studies should address context, ensure adequate follow-up, utilize controls, and consider non-residential environments or participants.

1. Introduction

Changing or adding to transit systems has been motivated by a multitude of potential benefits, including accommodating growing access needs for residents, reduction in environmental problems, increases in property values, and enhanced economic opportunities. Specifically, new systems with large passenger capacities that operate on a separated guideway, "Rapid Transit interventions," including Bus Rapid Transit (BRT), Light Rail Transit (LRT), and Rail Rapid Transit (RRT) are increasingly used in large cities to move growing populations more efficiently. These systems ensure that operations are not impeded by vehicle traffic or frequent stops using transit priority measures. Additional benefits of these extensions or new systems include reduced use of personal motor-vehicles, carbon emissions, air pollution, congestion, and collisions regionally (Bocarejo et al., 2012; Ding et al., 2016; Goel and Gupta, 2015; Saxe et al., 2017). For those living near rapid transit, but not necessarily regular passengers, the impacts include increased property values, higher density, and mixed land-uses (Bocarejo et al., 2013; Hurst and West, 2014; Rodriguez et al., 2016; Stokenberga, 2014; Zhu and Diao, 2016). Rapid transit can also allow regular transit users better access to economic opportunities, social and health facilities, and other desirable locations (Delmelle and Casas, 2012; Fan et al., 2012).

Beyond the many environmental, economic, and personal access benefits, rapid transit may also contribute to increased physical activity. Studies are increasingly finding associations between those who

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Abbreviations: BRT, Bus Rapid Transit; LRT, Light Rail Transit; MVPA, moderate to vigorous physical activity; RRT, Rail Rapid Transit

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use transit and higher physical activity (Besser and Dannenberg, 2005; Freeland et al., 2013; Lachapelle et al., 2011; Lachapelle and Frank, 2009). Transit use may be considered an instigator of active transportation, since it often requires walking or bicycling between transit stops and destinations (Bauman et al., 2012; Lachapelle et al., 2016; Voss et al., 2015). Therefore, it follows that people who use transit may be more likely to reach their recommended daily moderate to vigorous physical activity than those who use personal motor vehicles, car sharing, or carpooling (Besser and Dannenberg, 2005; Freeland et al., 2013). This increase in physical activity can contribute to a reduction in the odds of developing a chronic disease such as obesity (Brown et al., 2015; MacDonald et al., 2010). Given this evidence that active commuting increases protection against cardiovascular disease (Hamer and Chida, 2008), public health efforts increasingly target investments in new transit infrastructure that support active travel to increase overall physical activity.

Despite the multiple drivers of investments in new transit, including the physical activity benefits, little research has directly evaluated these new investments. With cities adding new transit lines and stations, some researchers have leveraged these changes to conduct natural experiment studies that aim to measure the population-level physical activity benefits of this new infrastructure. Although this field is still rapidly growing, quantifying early natural experiment studies' findings and examining existing literature can shape recommendations for future studies and inform future transit investment. To summarize findings for research and practice, we conducted a meta-analysis of natural experiment studies examining physical activity impacts of new rapid transit interventions (BRT, LRT, RRT).

2. Methods

2.1. Search procedures

Methods and inclusion criteria were specified in advance and documented in a protocol (Supplemental File 1), adhering to established recommendations for meta-analyses, including PRISMA guidelines (Liberati et al., 2009; Shamseer et al., 2015) (Supplemental File 2). Studies were identified from seven health and transport databases (Academic Search Complete, CINAHL, GEOBASE, Medline, PsycINFO, TRID, Web of Science) over May to June 2017. Search terms included, but were not limited to: rapid transit, public transit, light rail, health, physical activity, mobility, longitudinal, retrospective, prospective, intervention, and pedestrian.

2.2. Inclusion criteria

Studies were considered if they were in English, published recently (≤10 years), and included a rapid transit intervention. We define rapid transit interventions as new systems with large passenger capacities that operate on a separated guideway, such as BRT, LRT, and RRT. BRT systems operate on-road within a separated guideway and with transit priority signals, so that they are not impeded by vehicle traffic. Typically, BRT systems are in the center of the roadway with stations that include pedestrian walkways. LRT systems are very similar to BRTs, but are rail-based, rather than bus-based. RRT systems are also known as subway, metro-rail, and Mass Rapid Transit systems. They are any rail-based rapid transit system that operates completely on a separated guideway, without any potential interference of vehicle transit. They typically can carry more passengers and operate faster than LRT systems.

Preliminary searches and coding revealed 101 published studies. Our current review included only those measuring physical activity pre- and post-new transit infrastructure. We used prescriptive inclusion criteria; studies were excluded if they reported insufficient physical activity details (minutes/amount) for effect size calculation (i.e., mean pre- and post-, or mean change, and standard deviations [SD] or 95% Confidence Intervals [CI]) (Brown and Werner, 2007, 2008; MacDonald et al., 2010). Of note, two of these three excluded studies (Brown and Werner, 2007, 2008) were part of a series of papers otherwise reporting on the same populations for the same transit project (Brown and Werner, 2007, 2008; Brown et al., 2015; Miller et al., 2015); our analysis includes only a single report (Miller et al., 2015).

2.3. Data extraction

Two investigators (DD, JH) independently extracted sample size, intervention, pre- and post-intervention physical activity, and measurement information relevant for descriptive purposes. To harmonize data, we converted outcomes into total and transport-related physical activity (minutes/week) by collapsing subgroups (i.e. participant subsets or specific activities such as biking and walking) or scaling to identical units (daily to weekly).

2.4. Bias assessment

Two investigators (DD, JH) assessed bias risk using the Risk of Bias in Non-randomized Studies – of Interventions (ROBINS-I) assessment tool (Sterne et al., 2016).

2.5. Statistical analysis

We assessed statistical inconsistency using Cochran's Q and I^2 (Higgins et al., 2003). We estimated overall mean change post intervention from mean and standard deviations of physical activity change in each study using a general variance-based random effect model tool in Excel (Neyeloff et al., 2012). We chose random effects because study variation existed by location, population, and intervention.

3. Results

3.1. Descriptive summary of sample studies

Of 18 peer-reviewed articles examining health behaviors, 15 were on physical activity with only five of these incorporating natural experiment designs with sufficient pre- and post-intervention measurements (Chang et al., 2017; Hong et al., 2016; Huang et al., 2017; Miller et al., 2015; Panter et al., 2016) (Supplemental File 3). The meta-analysis and subsequent results focus only on the five papers with sufficient physical activity measurements reported.

One study used a repeated cross-sectional design (Chang et al., 2017), while the others were longitudinal within the same cohort (Table 1). Only one study included a control group in the original design (Hong et al., 2016). The rapid transit interventions included two BRTs in Mexico City, MX and Cambridge, UK as well as three LRTs in Los Angeles, Salt Lake City, and Seattle, US. All studied a complete new line, except for the Los Angeles study, which only studied six stations from the first phase of a new line addition (Hong et al., 2016). Additionally, three explicitly mentioned they included concurrent investments in bicycle- and pedestrian-related infrastructure that could have influenced active travel (Hong et al., 2016; Miller et al., 2015; Panter et al., 2016). Most sampled residents living geographically close (< 2 km) to the interventions, while one sampled workers close to the intervention and living within 30 km (Panter et al., 2016). Follow-up duration ranged from one to three years. All studies examined adults; three had > 60% females (Hong et al., 2016; Huang et al., 2017; Panter et al., 2016). Three studies used accelerometry (Hong et al., 2016; Huang et al., 2017; Miller et al., 2015); four specifically measured transport-related physical activity (Chang et al., 2017; Hong et al., 2016; Miller et al., 2015; Panter et al., 2016).

Each study found positive associations with increased physical activity only within specific study subgroups. Chang et al. (2017) found an increase in walking for transport in the surveyed population near

Table 1
Summary of characteristics of natural experiment studies examining physical activity after transit interventions (n = 5). Studies systematically reviewed (May–July 2017).

Author, year	Chang et al. (2017)	Hong et al. (2016)	Huang et al. (2017)	Miller et al. (2015)	Panter et al. (2016)
City, country	Mexico City, MX	Los Angeles, US	Seattle, US	Salt Lake City, US	Cambridge, UK
Transit intervention[a]	BRT - new line, 18 new stations	LRT - 6 new stations	LRT - new line, 13 new stations	LRT - new line, 5 new stations	BRT - new network
Parallel intervention(s)[b]	–	Landscaping & bicycle/pedestrian infrastructure	–	Complete Street & trail	Shared-use path
Study design	Repeated cross-sectional without control group	Longitudinal with control group	Longitudinal with control group determined retrospectively[c]	Longitudinal with control group determined retrospectively[d]	Longitudinal without control group
Scale	500 m	800 m	1.6 km	2 km	30 km[e]
Sampling	Household	Household	Household	Household	Workplace
Study initiation (first year)	2011	2011	2008	2012	2009
Study duration (years)	3	1	2	1	3
N (time 1)	1067	143[f]	276 [f]	939[f]	1143
N (time 2)	1420	73	198	536	469
Percent female (at baseline)	51% for post-test; 50% for pre-test	79% for intervention; 70% for controls	63%	51%	66.5%
Population	Adults 18–59	Adults 16 +	Adults 18 +	Adults 18 +	Adults 16 +
Outcome measurement	Survey[g]	Accelerometry	Accelerometry	Accelerometry	Survey[h]
Calculated mean difference in transportation physical activity (min/week) (SD)	27.4 (126.9)[i]	–	4.9 (86.4)[j]	0.3 (37.5)[j,k]	−10.5 (230.1)[k]
Calculated mean difference in total physical activity (min/week) (SD)	−114.2 (247.4)[i]	14.7 (397.3)[k,l]	−137.2 (632.3)[j]	5.1 (147.1)[j,k]	−166.0 (478.6)[k]

[a] Transit interventions were either Bus Rapid Transit (BRT) or Light Rail Transit (LRT). To be included they must be along fixed guideway (separated from road traffic).
[b] Parallel interventions are additional built environment changes that may influence physical activity, as mentioned in the study.
[c] During analysis this study created a "control" group retrospectively based on distance to transit.
[d] During analyses this study created a "control" group retrospectively based on transit use.
[e] Participants were selected based on workplace, but their residences had to be within 30 km of the city.
[f] Unclear how many of initial participants had outcome data, often reported only sample size for complete data for both time points.
[g] Measured using the International Physical Activity Questionnaire (IPAQ).
[h] Measure using the Recent Physical Activity Questionnaire (RPAQ).
[i] Walking and cycling added together.
[j] Scaled from daily to weekly.
[k] Summing groups.
[l] Computed from MVPA minutes.

transit and in subgroup analysis concluded that this was highest among females with low education. Both Hong et al. (2016) and Panter et al. (2016) found that there was an increase in transit-related physical activity only among those who were least active at baseline. Huang et al. (2017) found the greatest increase in station-area physical activity for residents closest to the station, illustrating a dose-response type relationship. Miller et al. (2015) found increases only among those switching to transit and on transit days.

3.2. Assessing bias risk in included studies

Most included studies had serious bias risk (Table 2). In repeated cross-sectional design (Chang et al., 2017) bias may exist if the latter sample differs in sociodemographic, health behavior, or secular trends. However, the authors did attempt to control for this by only sampling participants at time two who had lived in the neighborhood throughout the entire study period and using propensity score matching to create counterfactual groups (Chang et al., 2017). Since outcome measures were subjective, two studies with self-reported physical activity (Chang et al., 2017; Panter et al., 2016) had serious risk of measurement bias due to the potential for measurement error before and/or after the new transit project. There was moderate to critical bias risk in selection of reported results, as studies split results retrospectively by subgroup (Huang et al., 2017; Miller et al., 2015), outcome (Huang et al., 2017; Miller et al., 2015), or a subsample of a previous study (Hong et al., 2016; Miller et al., 2015). Given relatively uniform bias across studies, we did not weight studies according to bias.

3.3. Associations

Q and I^2 indicated high study heterogeneity (total physical activity Q = 201; I^2 = 98%). After transit interventions, total physical activity decreased (combined mean change - 80.4 min per week, 95% CI - 157.9, −2.9, Fig. 1), but transport-related physical activity increased (combined mean change 6.7 min/week 95% CI - 10.1, 23.5 transport-physical activity, Fig. 2).

4. Discussion

This work gives initial insight into methods and findings of natural experiment studies examining new rapid transit lines' impacts on physical activity. Despite substantial investment and transit's potential to impact mobility, very limited and inconsistent methods have been applied to assess impacts of new transit on physical activity. Studies varied by duration, physical activity measurement, sampling geography, and analysis method. All included studies compared pre- and post-data on physical activity which is an improvement over cross-sectional designs. However, there was potential for bias from selection into the study, uneven exposure to new transit, missing data from participant attrition, and substantial bias in selection of reported results.

Preliminary results indicated that following new transit, total physical activity declined but transport-related physical activity may increase. As previously stated, past research documents higher physical activity among transit users than non-users. Therefore, it seems intuitive that adding new transit may result in an increase in physical activity for non-users. However, these findings are consistent with some

Table 2

Assessment of risk of bias using the Risk Of Bias In Non-randomized Studies - of Interventions (ROBINS-I) assessment tool.

Assessment of risk of bias using the Risk Of Bias In Non-randomized Studies – of Interventions (ROBINS-I) assessment tool

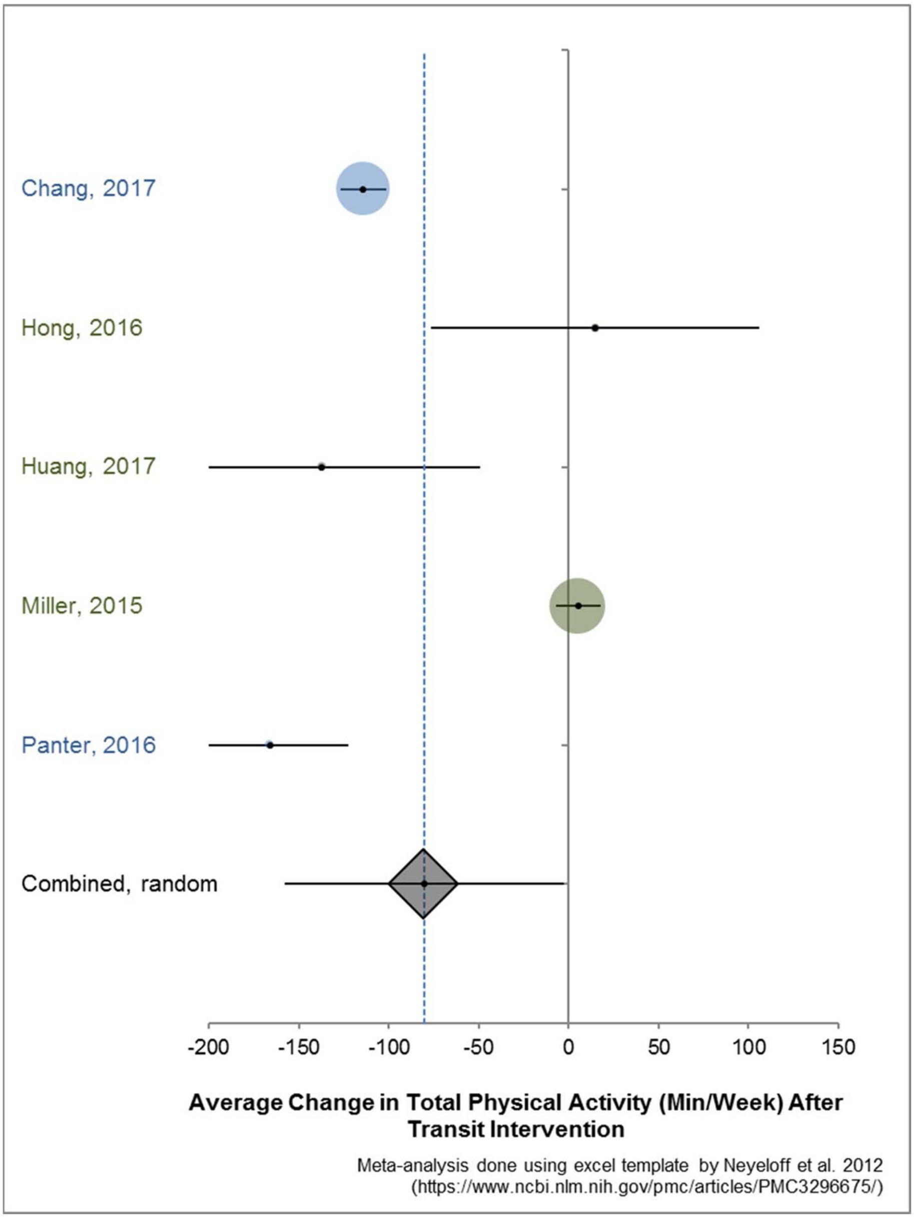
	Risk of bias pre-intervention and at-intervention domains			**Risk of bias post-intervention domains**				
Study	**Bias due to Confounding**	**Bias in selection of participants into the study**	**Bias in classification of interventions**	**Bias due to deviations from intended intervention**	**Bias due to missing data**	**Bias in measurement of outcomes**	**Bias in selection of the reported result**	**Overall Assessment of bias**
Chang (2017)	Moderate	Serious	Low	Moderate	No Information [a]	Serious	Moderate	Serious
Hong (2016)	Low [b]	Moderate	Low	Low	Low	Low	Serious	Serious
Huang (2017)	Low [b]	Moderate	Moderate	Moderate	Low	Low	Serious	Serious
Miller (2015)	Low [b]	Moderate	Serious	Serious [c]	Moderate	Low	Critical	Critical
Panter (2016)	Low [b]	Moderate	Low	Serious [c]	Moderate	Serious	Moderate	Serious

[a] Risk of bias due to missing data could not be estimated because this study was a repeated cross-sectional design (with different participants at the two times). This means individuals were not followed-up, and therefore could not be missing due to follow-up.

[b] These studies compare individuals to themselves, only examining within-person changes. Thus, confounders would need to be time-varying in order to create confounding. These studies adequately controlled for any potential time-varying confounders.

[c] These studies have multiple interventions occurring simultaneously that may influence physical activity (e.g. complete streets with new transit or new shared use path with new transit)

Key: Low risk of bias; Moderate risk of bias; Serious risk of bias; Critical risk of bias; No information



Meta-analysis done using excel template by Neyeloff et al. 2012 (https://www.ncbi.nlm.nih.gov/pmc/articles/PMC3296675/)

Fig. 1. Forest plot of natural experiment studies examining total physical activity (min/week) after transit interventions (n = 5). Studies systematically reviewed (May–July 2017).

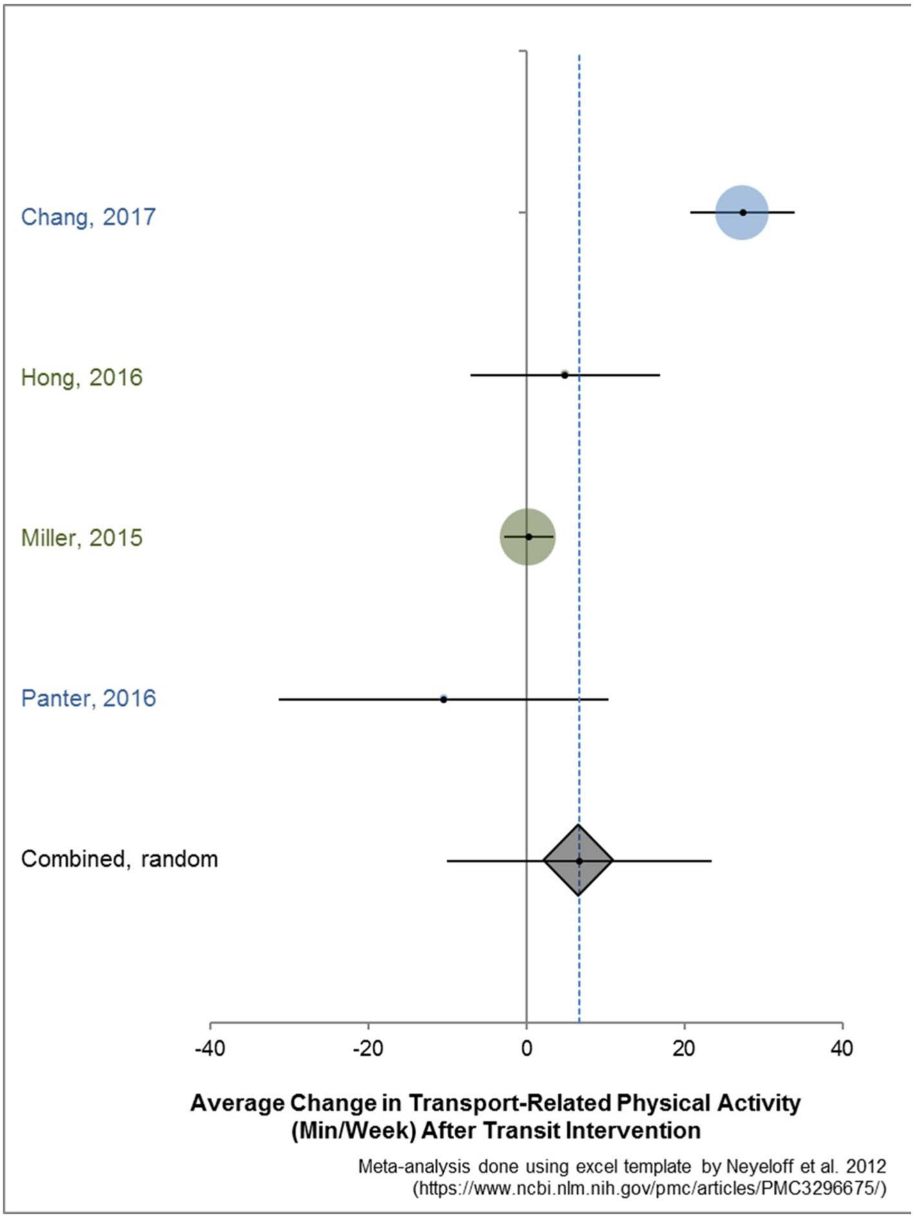


Fig. 2. Forest plot of natural experiment studies examining transportation physical activity (min/week) after transit interventions ($n = 4$). Studies systematically reviewed (May–July 2017).

previous work (Brown and Werner, 2007; Evenson et al., 2006; Evenson et al., 2010; Hong et al., 2016; MacDonald et al., 2010; Visser et al., 2002). One explanation is that transit construction disrupted activity patterns and contributed to decreased total physical activity or may have delayed new physical activity uptake beyond the end of the study period. It is also possible individuals shift their activity patterns to compensate for additional active commuting. This concept, which has been called the ActivityStat hypothesis (Gomersall et al., 2013), suggests that when physical activity shifts in one domain, there will be a compensatory change in another domain, in order to maintain an overall stable level of physical activity or energy expenditure over time. However, research to support this theory remains inconclusive (Gomersall et al., 2013). Nonetheless, previous studies suggest no physical activity substitution effect, since more active transportation was associated with both transit use and more leisure physical activity (Lachapelle et al., 2016). Miller et al. (2015) included a comparison of physical activity on transit days versus non-transit days, and found no substitution effect in their data. Given the self-report measurement techniques used, it is possible that individuals are substituting more, high-intensity active travel for lower, less-intense activities that contribute more time to overall physical activity. Since several studies had no controls, decreases in overall physical activity might be due to aging or secular factors (Visser et al., 2002). Finally, undetected changes in exogenous factors may also impact outcomes, since three of the five study areas included concurrent active travel infrastructure changes (Hong et al., 2016; Miller et al., 2015; Panter et al., 2016) and several of the new transit interventions were accompanied by other changes in the transit system (e.g. rerouting or reconfiguring an existing bus line). While active travel may decline if new transit decreased distance to transportation, preliminary evidence suggested transport-related physical activity might increase.

Context remains both an important consideration for the success of new rapid transit infrastructure projects and a complicating factor for generalizing the changes experienced in one city to other locations. Each of these transit interventions was designed for the specific economic, historic, and population needs of their cities (Mexico City, Los

Angeles, Seattle, Salt Lake City, and Cambridge). Thus, the interventions themselves differed in not only the number of new facilities but also the social and economic settings in which they were placed. Some interventions included a handful of new stations (Hong et al., 2016), while others included new lines with over a dozen new stations (Chang et al., 2017; Huang et al., 2017) or full new networks (Panter et al., 2016). This variability in scale could allow an examination of potential size and connectedness impacts of new rapid transit infrastructure if methods and populations were consistent across studies. Several of these studies also had other, simultaneous environmental changes (Hong et al., 2016; Miller et al., 2015; Panter et al., 2016). Additional investments, such as pedestrian crosswalks, trails, or other features, make teasing apart the impact of new rapid transit difficult, but represents positive prioritization by cities to enhance the active travel environment. Given the importance of local context, it is critical that the field continues to establish new studies to bolster this existing work and that these future studies include detailed descriptions of potential context factors that might be important for translation to other cities. Ideally, new research that performs several simultaneous natural experiment studies across numerous locales would greatly enhance this field. Yet, this type of large-scale study may be infeasible within financial and temporal constraints. Ultimately, while the tailoring of interventions may pose an obstacle for scientific evaluations, it is the nature and a necessity of changing the built environment.

An additional complication to generalizing across these studies is their differing methodologies, including types of physical activity measured, measurement tools, use of controls, and analysis techniques. Specifically, matching appropriate measurement tools to the types of active travel we would expect to see change may be important. For example, using accelerometry as an objective measure of physical activity may be informative for capturing physical activity by walking, but not for cycling (Slootmaker et al., 2009). Therefore, a new transit line may encourage more biking, which might not be captured by accelerometry. Several studies used self-reported physical activity (Chang et al., 2017; Hong et al., 2016; Panter et al., 2016), with potential recall or other measurement bias. The selection of control groups was also inconsistent across studies, with several doing post-hoc assignment of controls based on distance during analysis (Huang et al., 2017; Miller et al., 2015). To understand and eliminate the potential bias from secular trends, future work should identify and follow appropriate control area(s) or subjects from the onset. Finally, there was little consistency in analysis strategy across this work, particularly with regard to subgroup analyses.

Despite limited results found in the larger populations of these studies, many found statistically significant increases in physical activity for specific subgroups or domains. Several identified an association for those very near transit (Chang et al., 2017; Huang et al., 2017), or even just specific subpopulations within those near transit, such as low-education women (Chang et al., 2017) or those who were least active at baseline (Hong et al., 2016; Panter et al., 2016). This brings to light important discussions around relevant subgroups and potential targeted interventions. Often built environment infrastructure changes are thought of as a means to increase activity and health in the entire population living within the geographic area, to shift the entire population distribution. Alternatively, these studies seemed to find that the effects of new transit were more limited to particular groups or settings. Additional research to tease apart relevant groups or new campaigns in tandem with infrastructure to encourage more people to shift behavior may produce more physical activity outcomes.

4.1. Meta-analysis limitations

Limitations included restricting to studies in English, excluding dissertations and conference abstracts, and excluding three studies that provided insufficient data for the meta-analysis (Brown and Werner, 2007, 2008; MacDonald et al., 2010). However, two of these studies overlapped in population with an included study (Miller et al., 2015). We did not seek original, unreported data from authors as these would not reflect peer-reviewed findings. Significant heterogeneity suggested effect sizes may not represent a common population; potential heterogeneity sources should be sought. The limited number of eligible studies restricted further investigation of moderators, such as measurement tools (accelerometer versus survey), subpopulations, or study area/duration.

4.2. Gaps and recommendations

Interventions are difficult to assess comparatively, as transit was designed for the local context of each geography and its economic and population needs. Transportation benefits differed; some additions represented major regional accessibility gains while others went into areas realizing small gains over bus in access to destinations. We encourage studies to include a thorough description of context. While accelerometer-based physical activity measures reduced measurement bias, wear-time requirements limited sample size and overall generalizability. New studies should balance trade-offs between sample and measurement accuracy. Most studies had short durations (1–3 years) and examined one new transit element, which may be insufficient to detect physical activity changes. Future work should leverage existing cohorts where larger transit networks changed, to capture longer follow-up time with more extensive changes. Many studies lacked control groups (usually participants living elsewhere). We encourage future studies to include controls to assess trends independent of temporal, secular patterns. Only one study (Panter et al., 2016) captured non-residents; future research may consider new transit connections between residential and work locations or sample both residents and workers near new transit.

5. Conclusions

At present, inconsistent evidence suggests positive physical activity benefits of new transit were location-, subgroup-, or activity-specific. As cities invest in new or expanded systems, more researchers and practitioners should collaborate to evaluate physical activity impacts of these interventions.

Independent of the physical activity benefits of new transit infrastructure, there exists numerous potential health impacts from these new investments that remain to be assessed. Rapid transit remains a viable method of reducing automobile dependence and environmental issues, including air pollution. Similarly, rapid transit can alleviate congestion into major economic hubs or enhance the economic opportunities of those in new neighborhoods it connects. Furthermore, transit can enhance equity within a city by providing mobility options for those who are not able to drive, have ceased driving, or are unable to afford an automobile. Ultimately, future funding of infrastructure improvements should consider adding an evaluation component that measures the numerous and varied co-benefits of new rapid transit. This could help to not only determine actual impacts of changes, but also to inform other future interventions elsewhere within or across cities.

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Author contributions

LF, MW, and DD determined protocol for and DD performed the systematic review. DD and JH extracted data, assessed bias, and drafted the manuscript. JH ran the meta-analysis. LF, MB, and MW contributed to interpreting results and edited the manuscript. All authors read and approved the manuscript.

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