

Meaningfully reducing consumption of meat and animal products is an unsolved problem: A meta-analysis

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Abstract

Which interventions produce the largest and most enduring reductions in consumption of meat and animal products (MAP)? We address this question with a theoretical review and meta-analysis of randomized controlled trials that measured MAP consumption at least one day after intervention. We meta-analyze 35 papers comprising 41 studies, 112 interventions, and approximately 87,000 subjects. We find that these papers employ four major strategies to change behavior: choice architecture, persuasion, psychology, and a combination of persuasion and psychology. The pooled effect of all 112 interventions on MAP consumption is quite small (standardized mean difference (SMD) = 0.07 (95% CI: [0.02, 0.12])), indicating an unsolved problem. Interventions aiming to reduce only consumption of red and processed meat were more effective (SMD = 0.25; 95% CI: [0.11, 0.38]), but it remains unclear whether such interventions increase consumption of other forms of MAP. but it remains unclear whether such interventions also decrease consumption of other forms of MAP. We conclude that while existing approaches do not provide a proven remedy to MAP consumption, designs and measurement strategies have generally been improving over time, and many promising interventions await rigorous evaluation.

Keywords: meta-analysis, meat, plant-based, randomized controlled trial, climate change, sustainability

1. Introduction

Global consumption of meat and animal products (MAP) is increasing (Godfray et al., 2018) and is expected to continue doing so (Whitton et al., 2021). Abating this trend is vital to reducing chronic diseases caused by excessive MAP consumption and the risk of zoonotic pandemics (Willett et al., 2019; Landry et al., 2023; Hafez and Attia, 2020), mitigating environmental degradation and climate change (Poore and Nemecek, 2018; Koneswaran and Nierenberg, 2008; Greger and Koneswaran, 2010), and improving animal

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welfare (Kuruc and McFadden, 2023; Scherer et al., 2019). However, eating MAP is widely regarded as normal, ethical, and necessary (Piazza et al., 2022; Milford et al., 2019).

There is a vast and diverse literature investigating potential means to reduce MAP consumption. Example approaches include providing free access to meat substitutes (Katare et al., 2023), changing the price (Horgen and Brownell, 2002) or perceptions (Kunst and Hohle, 2016) of meat, and attempting to persuade people to change their diets (Bianchi et al., 2018a). Some interventions are associated with large impacts (Lentz, 2019; Boronowsky et al., 2022; Reinders et al., 2017), and prior reviews have concluded that some frequently studied approaches, such as using persuasive messaging that appeals to animal welfare (Mathur et al., 2021a), may be consistently effective. A particularly high-profile strand of this literature employs choice architecture, i.e. altering the contexts in which MAP is selected (Bianchi et al., 2018b), for instance by changing menu layouts (Bacon and Krpan, 2018; Gravert and Kurz, 2021), placing vegetarian items more prominently in dining halls (Ginn and Sparkman, 2024), or making plant-based options the default at catered meals (Hansen et al., 2021). Choice architecture could be a cheap, effective way of altering dietary behavior (Colgan, 2024), and governments, universities, and other institutions are increasingly implementing these approaches in such settings as dining halls (Pollicino et al., 2024) and hospital cafeterias (Morgenstern et al., 2024).

However, recurring design and measurement limitations compromise the literature on MAP reduction. Many interventions are either not randomized (Garnett et al., 2020) or underpowered (Delichatsios et al., 2001b). Measured outcomes are often imperfect proxies of MAP consumption, such as attitudes, intentions, and hypothetical choices (Raghoebar et al., 2020; Vermeer et al., 2010), yet behaviors often do not track with these psychological processes (Mathur et al., 2021b; Porat et al., 2024) and reported preferences (Hensher, 2010). Additionally, many studies with comparatively large effects specifically aim to reduce consumption of red and processed meat (RPM). However, because these studies exclusively measure changes in RPM, it is unknown whether they induce substitution to other forms of MAP, such as chicken or fish (Grummon et al., 2023). Thus, treating RPM consumption as a proxy of net MAP reduction, as prior reviews have done (Bianchi et al., 2018a; Chang et al., 2023; Kwasny et al., 2022), may cause bias. Finally, many studies measure only immediate rather than long-term effects (Hansen et al., 2021; Griesoph et al., 2021). This is of special concern if subjects who are encouraged to have a single vegetarian meal later compensate by consuming more MAP, which would make an immediate outcome measurement a biased estimate of overall effects. Such compensatory effects are common in dietary studies (Yeomans et al., 2001; Robinson et al., 2013; Lowe and Butryn, 2007).

In the past few years, a new wave of MAP reduction research has made commendable methodological advances in design, measurement validity, and statistical power. Historically, in some scientific fields, strong effects detected in early studies with methodological limitations were ultimately overturned by more rigorous follow-ups (Wykes et al., 2008; Paluck et al., 2019; Scheel et al., 2021). Does this phenomenon hold in the MAP reduction literature as well?

To address this question, we conducted a meta-analysis of randomized controlled trials (RCTs) that aim to reduce MAP consumption and that meet basic methodological standards (Andersson and Nelander, 2021; Kanchanachitra et al., 2020; Abrahamse et al., 2007; Acharya et al., 2004; Banerjee, 2019; Bianchi et al., 2022; Bochmann, 2017; Bschaden et al., 2020; Carfora and Catellani, 2023; Cooney, 2014, 2016; Feltz et al., 2022; Haile et al., 2021; Hatami et al., 2018; Hennessy, 2016; Jalil et al., 2023; Mathur et al., 2021b; Merrill and Aldana, 2009; Norris, 2014; Peacock and Sethu, 2017; Polanco et al., 2022; Sparkman et al., 2021; Weingarten et al., 2022; Piester et al., 2020; Aberman, 2018; Aldoh et al., 2024; Allen and Baines, 2002; Camp and Lawrence, 2019; Çoker et al., 2022; Sparkman et al., 2020; Berndsen and Van Der Pligt, 2005; Bertolaso, 2015; Fehrenbach, 2015; Mattson, 2020; Shreedhar and Galizzi, 2021). Specifically, we restricted eligibility to RCTs that measured consumption outcomes at least a single day after treatment was first administered and that had at least 25 subjects in both treatment and control (or, for cluster-assigned studies, at least ten clusters in total).

Studies in our meta-analysis pursued one of four theoretical approaches: choice architecture, psychological appeals (typically manipulations of perceived norms around eating meat), explicit persuasion (centered around animal welfare, the environment, and/or health), or a combination of psychological and persuasion messages. Interventions varied in delivery method, for example, documentary films (Mathur et al., 2021b), leaflets (Peacock and Sethu, 2017), university lectures (Jalil et al., 2023), op-eds (Haile et al., 2021), and changes to menus in cafeterias (Andersson and Nelander, 2021) and restaurants (Çoker et al., 2022; Sparkman et al., 2021). We estimated overall effect sizes as well as effect sizes associated with different theoretical approaches and delivery mechanisms. Although we find some heterogeneity across theories and mechanisms, we find consistently smaller effects on MAP consumption than previous reviews that placed fewer (if any) restrictions on studies' outcomes and methodological rigor (Bianchi et al., 2018b; Byerly et al., 2018; Chang et al., 2023; Harguess et al., 2020; Kwasny et al., 2022; Mathur et al., 2021a; Meier et al., 2022). When we included studies whose methodology fell short of our inclusion criteria (Alblas et al., 2023; Beresford et al., 2006; Food for Climate League, 2023; Celis-Morales et al., 2017; Dannenberg and Weingärtner, 2023; Delichatsios et al., 2001c; Epperson and Gerster, 2021; Frie et al., 2022; Garnett et al., 2020; Griesoph et al., 2021; Hansen et al., 2021; Johansen et al., 2010; Kaiser et al., 2020; Lentz, 2019; Loy et al., 2016; Matthews et al., 2019; Piazza et al., 2022; Reinders et al., 2017; Sparkman and Walton, 2017), this considerably increased the pooled estimate. In addition, studies that only aimed to reduce RPM consumption (Anderson et al., 2017; Carfora et al., 2017a,b, 2019b,a; Delichatsios et al., 2001a; Dijkstra and Rotelli, 2022; Emmons et al., 2005b,a; Jaacks et al., 2014; James et al., 2015; Lee et al., 2018; Lindström, 2015; Perino and Schwirples, 2022; Schatzkin et al., 2000; Sorensen et al., 2005; Wolstenholme et al., 2020), reported consistently stronger effects on behavior than studies aimed at reducing net MAP consumption. Overall, in contrast to previous reviews, we conclude that meaningfully reducing net MAP consumption is an unsolved problem, although many promising approaches still await rigorous evaluation.

2. Methods

2.1. Study selection

Our meta-analytic sample comprises RCT evaluations of interventions intended to reduce MAP consumption that had at least 25 subjects in treatment and control (or at least 10 clusters for studies that were cluster-assigned) and that measured MAP consumption at least a single day after treatment begins. We required that studies have a pure control group receiving no treatment. We further restricted our search to studies that were publicly circulated in English by December 2023. We also made three decisions regarding study inclusion after data collection began. First, we defined a separate analytic category for studies that only targeted RPM consumption. Second, we excluded studies that did not aim to reduce either all MAP or all RPM consumption and instead sought to induce substitution from one kind of MAP to another, e.g. that encouraged treated subjects to eat fish (Johansen et al., 2010). Third, we excluded studies whose interventions left no room for participants to voluntarily decide their MAP consumption, e.g.~interventions in institutions where subjects were simply served more vegetables on their plate.

Given our interdisciplinary research question and previous work indicating a large grey literature (Mathur et al., 2021a), we designed and carried out a customized search process. We: 1) reviewed 156 prior reviews, nine of which yielded included articles (Mathur et al., 2021a; Bianchi et al., 2018a,b; Ammann et al., 2023; Chang et al., 2023; Di Gennaro et al., 2024; Harguess et al., 2020; Ronto et al., 2022; Wynes et al., 2018); 2) conducted backwards and forward citation search; 3) reviewed published articles by authors with papers in the meta-analysis; 4) crowdsourced potentially missing papers from leading researchers in the field; 5) searched Google Scholar for terms that had come up in studies repeatedly; 6) used an AI search tool to search for gray literature (<https://undermind.ai/>); and 7) checked two databases emerging from ongoing nonprofit projects that both seek to identify all papers on meat-reducing interventions. All three authors contributed to the search. Inclusion/exclusion decisions were primarily made by the first author, with all authors contributing to discussions about borderline cases.

Figure 1 is a PRISMA diagram depicting the sources of included and excluded studies, which is detailed further in the Supplement.

[Figure 1 about here.]

2.2. Data extraction

The first author extracted all data. We extracted an effect size for one outcome per intervention: the measure of net MAP or RPM consumption that had the longest follow-up time after intervention. Additional variables coded included information about publication, details of the interventions, length of follow-ups, intervention theories, and additional details about interventions' methods, contexts, and open science practices (see accompanying code and data repository for full documentation: <https://doi.org/10.24433/CO.6020578.v4>).

When in doubt about calculating effect sizes, we consulted publicly available datasets and/or contacted authors. To assess risk of bias, we collected data on whether outcomes were self-reported or objectively measured, publication status, and presence of a pre-analysis plan and/or open data (Supplement).

All effect size conversions were conducted by the first author using methods and R code initially developed for previous papers (Paluck et al., 2019, 2021; Porat et al., 2024) using standard techniques (Cooper et al., 2019), with the exception of a difference in proportion estimator that treats discrete events as draws from a Bernoulli distribution (see appendix to (Paluck et al., 2021) for details). As our measure of standardized mean difference, we used Glass's Δ whenever possible, defined as $\Delta = \frac{\mu_T - \mu_C}{\sigma_C}$, where μ_T and μ_C respectively denote the treatment and control group means and σ_C denotes the pre-treatment control group standard deviation. If the control group SD was not available, we standardized on the pooled SD. When means and SDs were not available, we converted effect sizes from: regression coefficients, eta squared, or z-scores. When there was insufficient information to calculate a specific SMD, but the text reports the result as a null, we recorded the outcome as an “unspecified null” and set it to 0.01.

2.3. Statistical analysis

We used **Rmarkdown** (Xie et al., 2018) and a containerized online platform (Moreau et al., 2023; Clyburne-Sherin et al., 2019) to ensure computational reproducibility (Polanin et al., 2020). We conducted meta-analysis using robust variance estimation (RVE) methods (Hedges et al., 2010) as implemented by the **robumeta** package in R (Fisher and Tipton, 2015; Team, 2021). Many studies in our sample compared multiple treatment groups to a single control group. Therefore, we used the RVE method to allow for the resulting dependence between observations, as well as a standard small-sample correction.

Data analyses were largely conducted with custom functions building on **tidyverse** (Wickham et al., 2019). We assessed publication bias using selection model methods (Hedges, 1992; Vevea and Hedges, 1995), sensitivity analysis methods (Mathur, 2024), and the significance funnel plot (Mathur and VanderWeele, 2020b). These methods assume that the publication process favors “statistically significant” (i.e., $p < 0.05$) and positive results over “nonsignificant” or negative results. Our sensitivity check meta-analyzes only non-affirmative results, which creates an estimate under a hypothetical “worst-case” publication bias scenario where affirmative studies are almost infinitely more likely to be published than non-affirmative studies. We conducted these analyses using functions in **metafor** (Viechtbauer, 2010) and **PublicationBias** (Mathur and VanderWeele, 2020b; Mathur, 2024).

3. Results

3.1. Theoretical overview

Our meta-analysis included 35 papers comprising 41 studies and 112 separate point estimates. Each point estimate corresponded to a distinct intervention. The total sample size was approximately 87,000 subjects.

Because methodological quality is rapidly improving in this literature, the majority of eligible papers (18 of 35) were published from 2020 onwards, although the earliest was published in 2002 (Allen and Baines, 2002). Among studies where treatment was assigned to individuals rather than to clusters (e.g., school classes), the median analyzed sample size per study was 132 subjects (25th–75th percentiles: 109, 208).

We found that studies' theoretical approaches could be grouped into four categories.

Choice architecture studies (Andersson and Nelander, 2021; Kanchanachitra et al., 2020) (n = 2 studies with 3 estimates) manipulate aspects of physical environments to reduce MAP consumption, such as by placing the vegetarian option at eye level on a cafeteria's billboard menu (Andersson and Nelander, 2021).

Persuasion studies (Kanchanachitra et al., 2020; Aberman, 2018; Abrahamse et al., 2007; Acharya et al., 2004; Banerjee, 2019; Bianchi et al., 2022; Bochmann, 2017; Bschenen et al., 2020; Carfora and Catellani, 2023; Hennessy, 2016; Piester et al., 2020; Cooney, 2014, 2016; Feltz et al., 2022; Haile et al., 2021; Hatami et al., 2018; Jalil et al., 2023; Mathur et al., 2021b; Merrill and Aldana, 2009; Norris, 2014; Peacock and Sethu, 2017; Polanco et al., 2022; Sparkman et al., 2021; Weingarten et al., 2022) (n = 25 studies with 77 estimates) focus on health, environmental (usually climate change), and animal welfare reasons to reduce MAP consumption. Such messages are often delivered through printed materials, such as leaflets (Haile et al., 2021; Polanco et al., 2022), booklets (Bianchi et al., 2022) articles and op-eds (Sparkman et al., 2021; Feltz et al., 2022), and videos (Sparkman et al., 2021; Cooney, 2016; Mathur et al., 2021b). Less common delivery methods included in-person dietary consultations (Merrill and Aldana, 2009), emails (Banerjee, 2019), and text messages (Carfora and Catellani, 2023).

Psychology studies (Aldoh et al., 2024; Allen and Baines, 2002; Camp and Lawrence, 2019; Coker et al., 2022; Piester et al., 2020; Sparkman et al., 2020) (n = 9 studies with 12 estimates) manipulate the interpersonal, cognitive, or affective factors associated with eating MAP. The most common psychological intervention is centered on social norms seeking to alter the perceived popularity of non-MAP dishes (Sparkman et al., 2020, 2021). In one study, a restaurant put up signs stating that “[m]ore and more [retail store name] customers are choosing our veggie options” (Coker et al., 2022). In another, a university cafeteria put up signs stating that “[i]n a taste test we did at the [name of cafe], 95% of people said that the veggie burger tasted good or very good!” (Piester et al., 2020). One study told participants that people who ate meat are more likely to endorse social hierarchy and embrace human dominance over nature (Allen and Baines, 2002). Other psychological interventions include response inhibition training, where subjects are trained to avoid responding impulsively to stimuli such as unhealthy food (Camp and Lawrence, 2019), and implementation intentions, where participants list potential challenges and solutions to changing their own behavior (Aberman, 2018; Shreedhar and Galizzi, 2021).

Finally, some studies combine **persuasion** approaches with **psychological** appeals to reduce MAP consumption (Aberman, 2018; Berndsen and Van Der Pligt, 2005; Bertolaso, 2015; Carfora and Catellani, 2023; Fehrenbach, 2015; Hennessy, 2016; Mathur et al., 2021b; Mattson, 2020; Piester et al., 2020; Shreedhar

and Galizzi, 2021) ($n = 11$ studies with 20 estimates). These studies typically combine a persuasive message with a norms-based appeal (Piester et al., 2020; Mattson, 2020) or an opportunity to pledge to reduce one's MAP consumption (Mathur et al., 2021b; Shreedhar and Galizzi, 2021).

3.2. Main findings: overall small effects

In our dataset, the pooled effect of all interventions is standardized mean difference (SMD) = 0.07 (95% CI: [0.02, 0.12]), $p = .007$, with some heterogeneity (standard deviation of population effects $\tau = 0.082$). Given the pooled effect size and the estimated heterogeneity, we estimate that 26% of true effects are above SMD = 0.1, and just 8% are above SMD = 0.2 (Mathur and VanderWeele, 2019, 2020a).

[Table 1 about here.]

3.3. Subset and moderator analyses

Stratifying by theoretical approach, pooled estimates were similar across psychology, persuasion, and persuasion and psychology (SMDs from 0.07 to 0.11; Table 1). Estimates may have been somewhat larger among the choice architecture studies (SMD = 0.21), but the sample size was much smaller (3 estimates). Within studies with a persuasion component, pooled estimates are similar for environmental appeals (SMD = 0.09, 16 studies with 29 estimates), and health appeals (SMD = 0.08, 18 studies with 29 estimates), but are smaller for appeals to animal welfare (SMD = 0.03, 16 studies with 65 estimates). We did not conduct meta-regression for theoretical approach or type of persuasion because studies with multiple interventions could occupy multiple categories, and many persuasion interventions combined multiple types of message, e.g. presenting students with both environmental and health reasons to reduce MAP consumption (Jalil et al., 2023).

[Figure 2 about here.]

The 17 studies that only attempted to reduce consumption of RPM, comprising 25 point estimates, yielded a pooled effect of SMD = 0.25 (95% CI: [0.11, 0.38]), $p = .002$, $\tau = 0.201$. Among these studies, we estimate that 48% of true RPM effects are above SMD = 0.2. We observe consistently small effects across categories of population (all pooled SMDs < 0.1), but more heterogeneity by region: North America, where a majority of studies took place, had an average effect of SMD = 0.04 vs. 0.14 to 0.21 for other locations. Effect sizes have broadly been declining over time, from an average of SMD = 0.16 in the 2000s to SMD = 0.05 in the 2020s.

3.4. Publication bias and robustness checks

The meta-analytic mean corrected for publication bias that favors significant, positive results was 0.01 (95% CI: [-0.014, 0.033]), $p = 0.421$ (Hedges, 1992). Figure 3 displays a significance funnel plot (Mathur and

VanderWeele, 2020b). A conservative estimate that accounts for the possibility of worst-case publication bias yields an estimate of $SMD = 0.02$ (95% CI: [-0.01, 0.05]), $p = .177$ (Mathur and VanderWeele, 2020b; Mathur, 2024) (further sensitivity checks in Supplement).

[Figure 3 about here.]

Table 2 displays subset analyses and average differences in effect size by study population, region, era of publication, and delivery method. Average differences were estimated via meta-regression.

[Table 2 about here.]

As a robustness check, we also coded and meta-analyzed a supplementary dataset of 22 marginal studies, comprising 35 point estimates. Marginal studies were those whose methods fell short of our inclusion criteria, but typically met all but one, e.g. the control group received some aspect of treatment (Piazza et al., 2022), or treatment was alternated weekly but not randomly (Garnett et al., 2020) (Supplement). Expanding the meta-analytic dataset to include these marginal studies yields a pooled effect of $SMD = 0.2$ (95% CI: [0.09, 0.31]), $p < < 0.001$. Particularly large results were found in studies that measured outcomes immediately (Hansen et al., 2021) or that had smaller samples (Lentz, 2019).

4. Discussion

Our meta-analysis of RCTs estimated a small overall effect of $SMD = 0.07$, along with its upper confidence bound of $SMD = 0.12$. Effects were also consistently small across an array of locations, study designs, and intervention categories. Some individual studies found comparatively larger effects (e.g. five studies estimated $SMD > 0.5$: (Carfora and Catellani, 2023; Merrill and Aldana, 2009; Kanchanachitra et al., 2020; Bianchi et al., 2022; Piester et al., 2020)). We view these interventions as intriguing candidates for subsequent research and replication. However, these studies' heterogeneous theories, methods, and implementation details suggest that no singular approach, means of delivery, or message should be considered a well-validated method of reducing MAP consumption. Taken together, these findings suggest that reducing MAP consumption is an unsolved problem.

Perhaps surprisingly, our results diverged from the more positive findings of previous reviews (Mathur et al., 2021a; Meier et al., 2022; Mertens et al., 2022), which are summarized in the Supplement. Our much smaller estimate likely reflects our stricter methodological inclusion criteria. For instance, of the ten largest effect sizes in a previous meta-analysis (Mathur et al., 2021b), nine measured attitudes and/or intentions, and the tenth came from a non-randomized design. Prior research has found that intentions often do not predict behavior (Mathur et al., 2021b), and reviews in other fields have found systematic differences in impacts between randomized and non-randomized evaluations (Porat et al., 2024; Stevenson, 2023). Supporting this

interpretation, robustness checks in which we relaxed our methodological inclusion criteria produced results similar to those of previous reviews. This possibility will need further empirical evaluation.

Another potentially surprising result is that only two choice architecture papers met our methodological inclusion criteria. Most potentially eligible papers either measured hypothetical outcomes or measured outcomes immediately after the intervention. Moreover, prior reviews that found choice architecture approaches to be consistently effective at modifying diet typically focused on foods that may have weaker cultural and social attachments than MAP, such as sugary drinks and snacks (Venema et al., 2020; Adriaanse et al., 2009). We speculate that changes to how MAP is sold and consumed, by contrast, are more likely to be noticed and to engender political and cultural backlash (Popper, 2019).

Likewise, as our analyses show, studies aimed at reducing RPM consumption are associated with a considerably larger effect ($SMD = 0.25$) than those aimed at reducing all MAP consumption. Many prior reviews grouped MAP and RPM studies together, treating their outcomes as aimed at a single theoretical target (Slough and Tyson, 2023). However, if reductions in RPM lead to consumers' substituting to other forms of MAP, then analyses that synthesize the two categories of outcome may produce inflated estimates of net MAP reduction. We view such substitutions as likely: many health guidelines, such as the heart-healthy diet (Diab et al., 2023), encourage reducing RPM while also encourage moderate intake of poultry and fish, both of which come with severe externalities, such as risking zoonotic outbreaks from factory farms (Hafez and Attia, 2020) and causing land and water pollution (Gržinić et al., 2023). Additionally, raising chicken and fish may lead to substantially worse outcomes for animal welfare (Mathur, 2022). We speculate that cutting back on RPM by substituting to other forms of MAP may be easier and more socially normative than is cutting back on all MAP. This possibility might explain the observed difference in effect sizes.

Our analyses have limitations. Relatively few studies met our methodological inclusion criteria, limiting statistical precision. Additionally, as with all meta-regression analyses, ours should not be interpreted as causal estimates of study-level moderators. That is, estimated differences in effect sizes between groups of studies do not represent the causal effects of the study characteristics (e.g., theoretical approach) on their interventions' effects because studies' characteristics are not randomly assigned. Finally, although our methodological inclusion criteria were more stringent than those of previous reviews, the included studies still had limitations. For example, many outcome measures in our database were coarse, such as self-reported reduction vs. non-reduction in MAP consumption as a binary variable (Aberman, 2018) Other studies seek to associate eating MAP with a sense of threat (Fehrenbach, 2015) or with endorsing social hierarchy (Allen and Baines, 2002) and then collect self-reported outcomes. These designs raise the possibility of social desirability bias.

Overall, this literature shows encouraging trends in methodology. First, as noted, a majority of studies in our meta-analysis have been published since 2020, indicating the field's increasing attention to rigorous design and measurement. Second, we observe many fruitful collaborations between researchers and advocacy

organizations, as shown by the large number of nonprofit white papers in our sample. Third, many promising designs and interventions still await rigorous evaluation. For instance, no study that met our criteria evaluated extended contact with farm animals (Cerrato and Forestell, 2022), manipulations to the price of meat (Wilde et al., 2016), activating moral and/or physical disgust (Palomo-Vélez et al., 2018), watching popular media such as the *Simpsons* episode “Lisa the Vegetarian” (Byrd-Bredbenner et al., 2010) or the movie *Babe* (Novatná, 2019), and many categories of choice architecture intervention (Ólafsson, 2024). Moreover, emerging research designs help address longstanding measurement challenges, such as the possibility that interventions implemented at one time point (e.g., choice architecture at a lunch buffet) create later compensatory behavior (e.g., eating more MAP at dinner) (Voški et al., 2024). Ultimately, our findings suggest that meaningfully reducing MAP consumption is an unsolved problem, and point to the critical importance of the field’s increasing focus on methodological rigor.

5. Acknowledgments

Thanks to Alex Berke, Alix Winter, Anson Berns, Dan Waldinger, Hari Dandapani, Adin Richards, Martin Gould, Matt Lerner, and Rye Geselowitz for comments on an early draft. Thanks to Jacob Peacock, Andrew Jalil, Gregg Sparkman, Joshua Tasoff, Lucius Caviola, Natalia Lawrence, and Emma Garnett for help with assembling the database and providing guidance on their studies. Thanks to Sofia Vera Verduzco for research assistance. We gratefully acknowledge funding from the NIH (grant R01LM013866), Open Philanthropy, and the Food Systems Research Fund (Grant FSR 2023-11-07).

Declarations

The authors declare no conflicts of interest.

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6. Figures

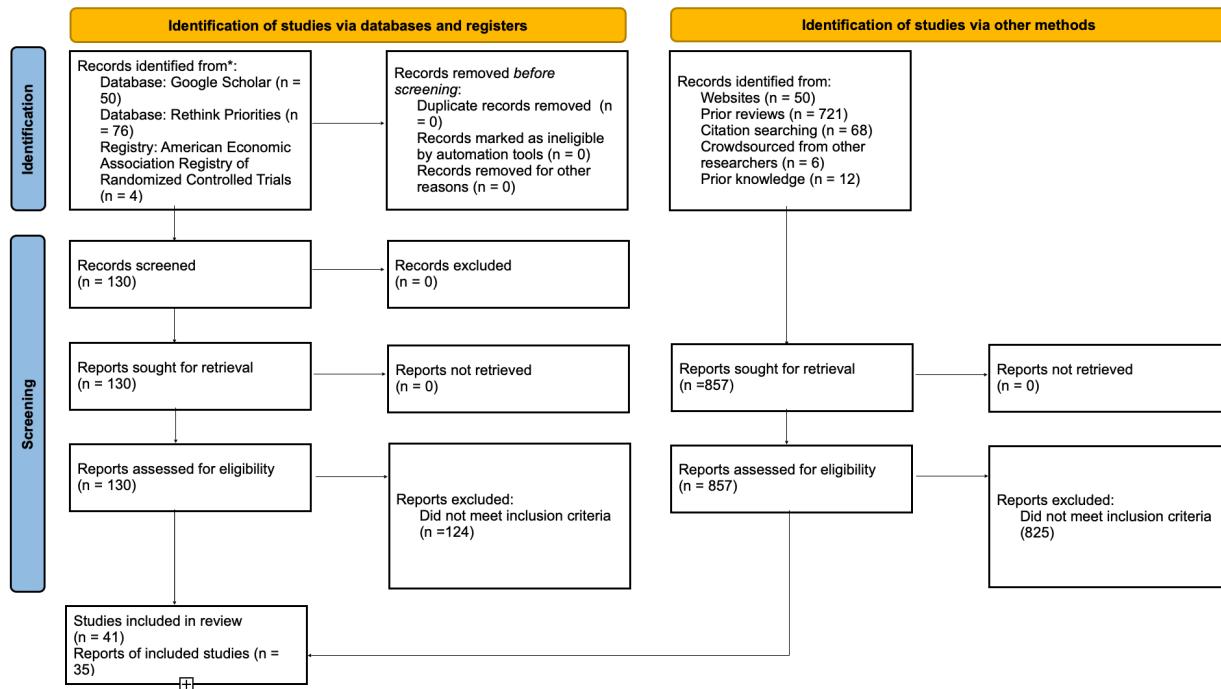


Figure 1: PRISMA diagram.

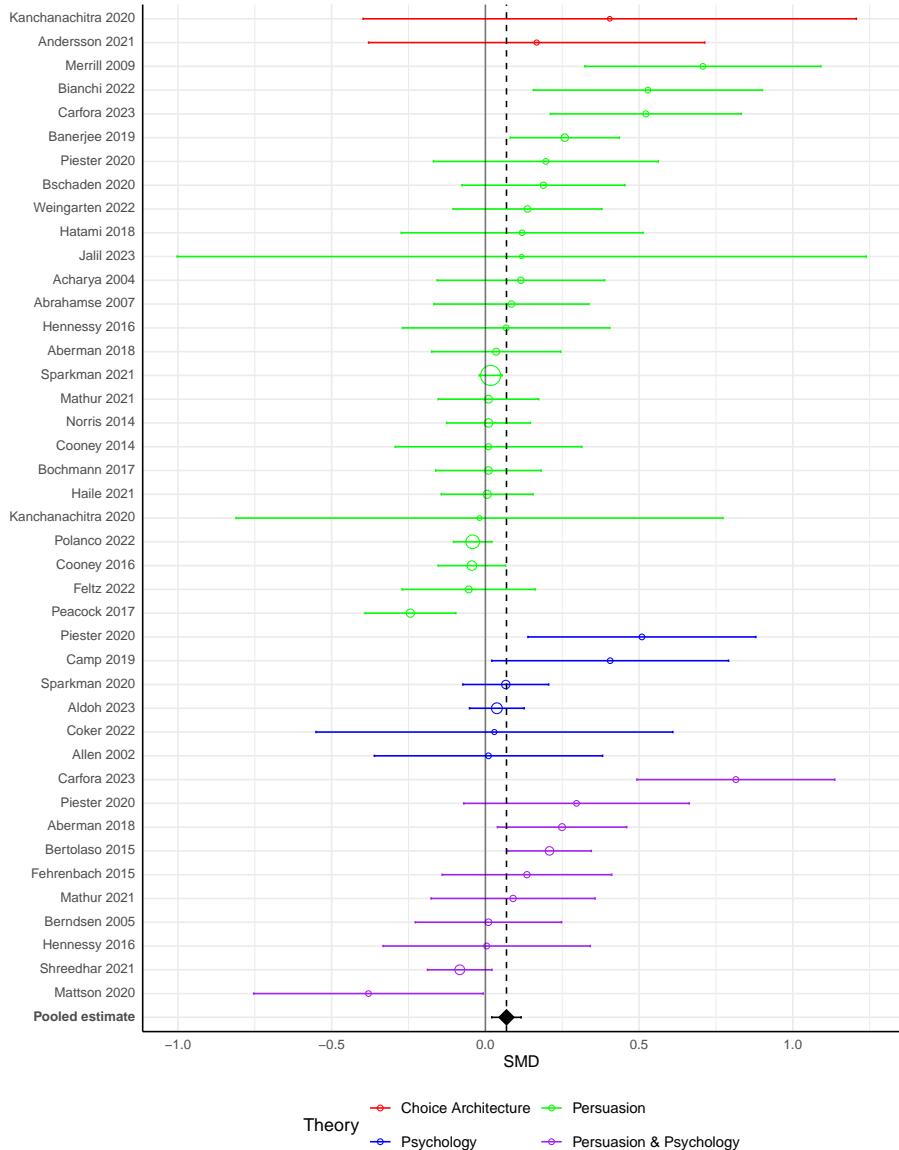


Figure 2: Forest plot of all meta-analyzed studies. For papers contributing multiple point estimates, the plotted point corresponds to a fixed effects meta-analysis for each paper for visual clarity. Papers employing multiple theoretical approaches are represented once per theory. Point size is inversely proportional to variance. Points are sorted within theory by estimate size. The vertical black line demarcates an effect size of zero, and the dotted line is the observed overall effect.

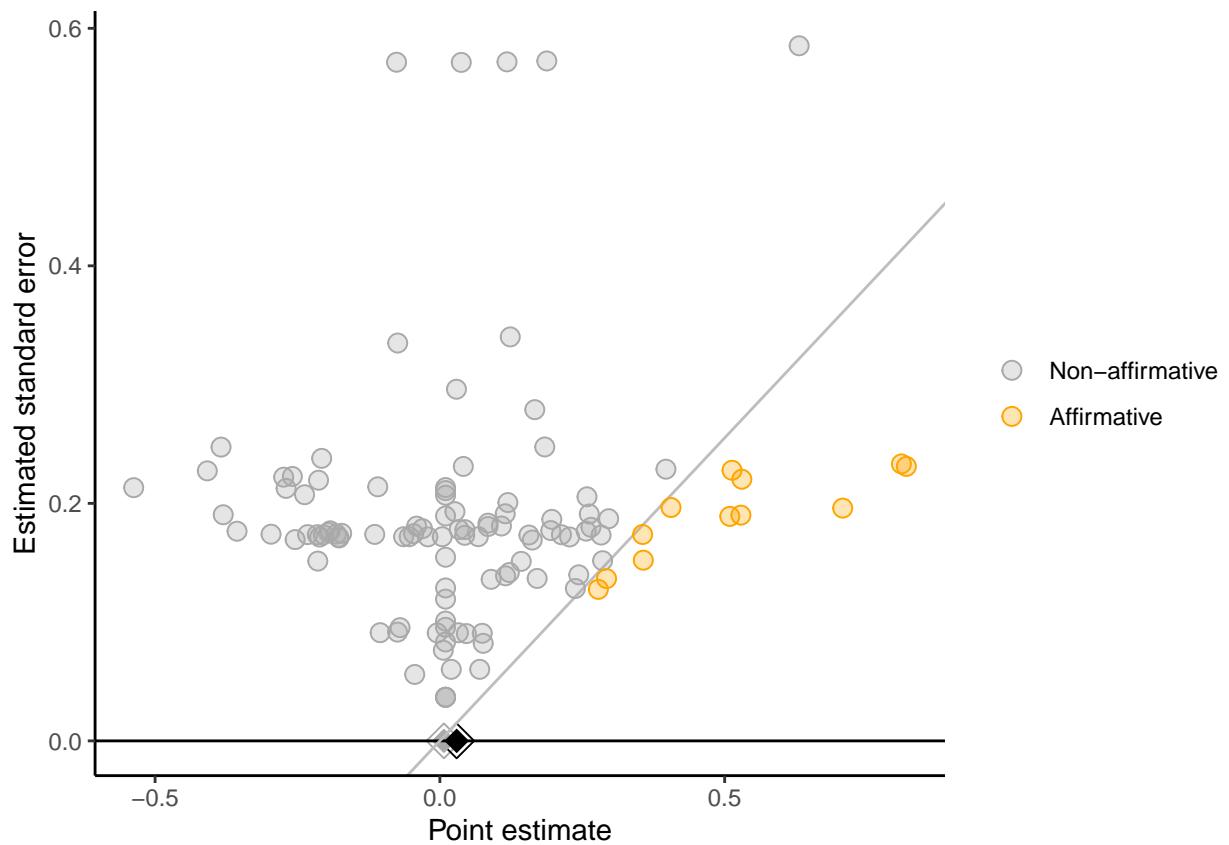


Figure 3: Significance funnel plot displaying studies point estimates versus their estimated standard errors. Orange points: affirmative studies ($p < 0.05$ and a positive point estimate). Grey points: nonaffirmative studies ($p \geq 0.05$ or a negative point estimate). Diagonal grey line: the standard threshold of statistical significance for positive point estimates; studies lying on the line have exactly $p = 0.05$. Black diamond: main-analysis point estimate within all studies; grey diamond: worst-case point estimate within only the nonaffirmative studies.

7. Tables

Table 1: Meta-analytic Results Overall and by Theoretical Approach

| Approach | N (Studies) | N (Estimates) | SMD | 95% CIs | p val |
|---------------------------|-------------|---------------|------|---------------|-------|
| Overall | 41 | 112 | 0.07 | [0.02, 0.12] | .007 |
| Theory | | | | | |
| Choice Architecture | 2 | 3 | 0.21 | [-0.99, 1.42] | .267 |
| Psychology | 19 | 32 | 0.10 | [0, 0.2] | .054 |
| Persuasion | 25 | 77 | 0.07 | [0.01, 0.13] | .023 |
| Persuasion & Psychology | 11 | 20 | 0.11 | [-0.06, 0.28] | .189 |
| Type of Persuasion | | | | | |
| Animal Welfare | 16 | 65 | 0.03 | [-0.02, 0.07] | .189 |
| Environment | 16 | 29 | 0.09 | [-0.02, 0.19] | .095 |
| Health | 18 | 29 | 0.08 | [-0.01, 0.17] | .064 |

Note that studies could occupy multiple categories for both theory and type of persuasion, that Ns for Types of Persuasion draw from both Persuasion and Persuasion and Psychology studies, and that some studies with multiple interventions are represented in multiple theoretical categories.

Table 2: Moderator Analysis Results

| Study Characteristic | N (Studies) | N (Estimates) | SMD | 95% CIs | Subset <i>p</i> value | Moderator <i>p</i> value |
|---------------------------|-------------|---------------|------|---------------|--------------------------|-----------------------------|
| Outcome | | | | | | |
| Meat and animal products | 41 | 112 | 0.07 | [0.02, 0.12] | .007 | ref |
| Red and processed meat | 17 | 25 | 0.25 | [0.11, 0.38] | .002 | .046 |
| Population | | | | | | |
| University students/staff | 18 | 38 | 0.07 | [-0.03, 0.16] | .139 | ref |
| All ages | 3 | 6 | 0.04 | [-0.16, 0.25] | .361 | .733 |
| Adults | 17 | 61 | 0.09 | [0.01, 0.18] | .034 | .714 |
| Adolescents | 3 | 6 | 0.02 | [-0.4, 0.44] | .806 | .686 |
| Region | | | | | | |
| North America | 23 | 74 | 0.04 | [-0.01, 0.08] | .142 | ref |
| Europe | 14 | 28 | 0.14 | [0.02, 0.27] | .029 | .156 |
| Multi-region | 1 | 4 | 0.21 | [0.21, 0.21] | N/A | N/A |
| Asia + Australia | 2 | 5 | 0.13 | [-0.17, 0.43] | .116 | .220 |
| Publication Decade | | | | | | |
| 2000s | 6 | 8 | 0.16 | [-0.12, 0.43] | .199 | ref |
| 2010s | 12 | 31 | 0.07 | [-0.03, 0.17] | .13 | .464 |
| 2020s | 23 | 73 | 0.05 | [-0.01, 0.11] | .074 | .369 |
| Method of Delivery | | | | | | |
| Educational materials | 15 | 48 | 0.01 | [-0.04, 0.07] | .591 | ref |
| Online | 8 | 22 | 0.16 | [-0.02, 0.34] | .067 | .173 |
| Dietary consultation | 2 | 2 | 0.40 | [-3.36, 4.15] | .409 | .441 |
| In-cafeteria | 8 | 13 | 0.10 | [-0.04, 0.25] | .101 | .126 |
| Video | 11 | 27 | 0.01 | [-0.04, 0.07] | .487 | .553 |

Moderation analyses by differences in outcomes, population, region, decade of publication, and delivery method. The first *p* value column tests the hypothesis that the subset of studies with a given characteristic is significantly different than an SMD of zero. The second compares effects within a given category to the reference category for that moderator. N/A *p* values were not calculated due to an insufficient number of qualifying studies.