

# E X C H A N G E

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## A CALL TO MAXIMIZE THE SOCIAL IMPACT OF OUR RESEARCH: AN EFFECTIVE ALTRUISM APPROACH

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**Connecting management research to policy is a focal route through which management research can contribute to meaningful and sustainable social change. Yet, management researchers often fail to make this connection. The present article discusses management research’s potential to inform effective policy and social change, and provides a roadmap for how to realize that potential. Our perspective is rooted in the effective altruism philosophy, which argues that people should use evidence and careful reasoning to figure out how to use their scarce resources (i.e., time and money) to do the most good. We hope that this article can help spark discussion and meaningful change within management research so that we, as individual scholars and as a field, can make a bigger and better impact.**

The creation of meaningful and sustainable social change often occurs through policy change. Thanks, in large part, to policy reforms—such as outlawing child and slave labor, giving women and people of color the right to vote, outlawing some farming practices to improve animal welfare, among many others—we have seen massive improvements in the fairness and justice of societies around the world. Policy reforms—both in the government and in the workplace—can directly improve lives in the short term and create massively beneficial outcomes for generations to come. Given the obvious importance of policy change (Davis, 2015; Hitt, 2005), there have been many calls to increase the societal and policy relevance of management research, including, most recently, the call from Aguinis, Jensen, and Kraus (2022) in *Academy of Management Perspectives*.

Aguinis et al. (2022) explored the policy relevance of all organizational behavior and human resource management (OBHRM) articles (i.e., articles that have examined individual- or team-level research, such as organizational behavior) published from a subset of the top management journals (i.e., *Academy of Management Journal* [AMJ], *Academy of*

*Management Review* [AMR], *Academy of Management Annals* [AMA], *Academy of Management Learning and Education* [AMLE], *Academy of Management Discoveries* [AMD], *Academy of Management Perspectives* [AMP], *Journal of Applied Psychology* [JAP], *Personnel Psychology* [PPsych], *Organizational Behavior and Human Decision Processes* [OBHDP], and *Journal of Management* [JOM]) between January 2010–December 2019. They defined policy as: “governance principles that guide courses of action and behavior in organizations and societies” (Aguinis et al., 2022: 4). Their analysis revealed that only 61 of these OBHRM articles explicitly discussed direct or indirect policy implications.<sup>1</sup> Aguinis et al. (2022) offered a word of

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<sup>1</sup> To find this number, Aguinis et al.’s (2022: 6) team conducted “a critical literature review of journals that publish OBHRM research to identify implications for policy-making.” Specifically, they used the Web of Science Core Collection to identify 4,026 articles published in the 10 identified journals from January 2010 through December 2019. Among these articles, they first identified which articles simply mentioned policy recommendations by

caution, suggesting that due to OBHRM's lack of policy focus, the field risks becoming societally irrelevant. One limitation of the work is that the 10 journals included in Aguinis et al.'s (2022) analysis may not represent the depth and diversity of management research across the globe. For example, it is certainly possible that other journals—especially journals that are more disciplinary or less U.S.-centric—do a better job at incentivizing socially impactful scholarship. However, these top management journals play an important role in setting the tone for the broader field of management, so we believe that Aguinis et al.'s (2022) findings highlight a pressing problem within the field.

Why is the field of management falling short? One reason could be that the field of management is inherently managerialist (Davis, 2015). Since its birth, the field has been focused primarily on improving management to boost productivity and profit. Hence, broader societal impact has historically not been at the forefront of the field's aims. It is also possible that this focus has obscured researchers' awareness of the negative societal externalities of business success, such as inequality and environmental devastation.

A second reason could be that many scholars want their research to impact society and policy, but lack a clear roadmap for thinking about *how* to increase the policy relevance of their work. In addition to striving to produce the highest-quality theory and empirical research, we—and, we assume, many other management scholars—also want to make a positive impact on the world. Fortunately, our work has immense potential to do just that by helping to change the way business is done in corporations, nonprofits, and government institutions. For example, work on high-reliability organizations (e.g., Perrow, 1999; Roberts & Bea, 2001; Sagan, 1993;

Vaughan, 1996; Weick, 1988; Weick & Sutcliffe, 2001) not only contributed to organizational theory but also meaningfully improved society by improving the way business is done among emergency-response and crisis-management teams. Weick and Sutcliffe's (2001) research led to the improvement of education and training for the wildland fire community, which, as a result, improved the effectiveness of emergency responses to “all-hazard events” such as huge mega-fires, hurricanes, floods, and earthquakes. Management research has the potential to literally save lives. Consistent with this aim, the field has recently broadened the very purpose of business to incorporate social values and a broader set of stakeholders (e.g., the environment, employees, customers; Wang, Tong, Takeuchi, & George, 2016). Yet, there is little guidance for management scholars on how to produce such socially impactful science—both independently and as a coordinating field. Although certain scholars are phenomenal exemplars of how to produce socially impactful work (e.g., Battilana & Casciaro, 2021; Battilana, Obloj, Pache, & Sengul, 2022; Van Wijk, Stam, Elfring, Zietsma, & den Hond, 2013), such scholars stand out, in part, because they seem like exceptions rather than the norm.

In the present article, we adopt an “effective altruism” (EA) perspective to develop a roadmap for producing socially impactful and policy relevant research. Although we are specifically focused on doing good effectively with our research, we recognize that many management scholars may have other goals and that a diversity of priorities within science is necessary to produce the highest-quality science. Furthermore, we recognize that EA is just one lens that scholars can use to produce socially impactful work. Nonetheless, we hope that the present article can serve as a jumping-off point for those who share the goal of producing socially impactful research, so that we can work together to accomplish it.

## PRODUCING SOCIAL IMPACT, EFFECTIVELY

EA is a young social movement inspired by moral philosophers (Ord, 2013; MacAskill, 2015; Singer, 1972, 2016), and challenges us to “do good better” (Galef, 2021; MacAskill, 2015, 2022; Singer, 1972, 2009, 2016; Todd, 2016). The EA philosophy calls for people to use evidence and reason to figure out how to benefit others as much as possible with their limited time and resources. Effective do-gooding has been defined as “(i) the use of evidence and careful reasoning to work out how to maximize the good

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searching for the following keywords: guidelines, guides, normative, norms, policies, policy, regulations, rules. Of the 4,026 articles 742 (18.4%) mentioned these keywords. Next, the authors selected only the articles that touched on OBHRM topics (research at the individual and team levels of analysis), which resulted in 369 articles (49.7% of the papers that mentioned policy keywords). Finally, the team went through the full text of these 369 articles and identified 297 empirical articles, removing the 72 purely conceptual articles. Finally, the team identified which of the 297 empirical OBHRM articles had direct or indirect policy implications, and identified 61 articles (20.5% of empirical OBHRM articles that mentioned policy keywords and 1.5% of all management articles published in the past 10 years).

with a given unit of resources ... and (ii) the use of the findings from (i) to try to improve the world” (MacAskill, 2019: 14). Furthermore, MacAskill (2019) defines “good” in terms of both the degree of improvement per person and the number of people helped by a decision or behavior, from an impartial perspective (i.e., all human lives, regardless of where or when they exist, are of equal worth).

EA is one of many potential frameworks for measuring the impact of one’s behavior (whether in one’s personal life or one’s professional endeavors, such as producing research), and, like any normative framework, is imperfect. In particular, there is internal disagreement within the EA movement about how to define and measure “doing the most good.” One of the biggest and most polarizing debates in the EA community today grapples with how much weight should be given to actions that have a small chance of being highly impactful in the very long run—such as reducing suffering<sup>2</sup> and existential<sup>3</sup> risks (consistent with “longtermism”; e.g., Beckstead, 2013; Bostrom, 2014; Greaves & MacAskill, 2021; MacAskill, 2022; Parfit, 1984; Ord, 2020) versus actions that are more certain to meaningfully improve the present and near-term generations. While some of our author team advocates for a moderate long-termist EA perspective, individuals who apply a steep discount rate to the well-being of future people, or hold a person-affecting view (i.e., the belief that only the well-being of currently existing humans matters; Beckstead, 2013; Caviola, Althaus, Mogensen, & Goodwin, 2022; Greaves, 2017; Holtug, 2004; Parfit, 1984), for example, may come to a different conclusion. We do not attempt to settle this debate, but merely wish to illustrate the complexity of the project of defining what the “most good” looks like in practice, and the heterogeneity of approaches to “doing good” within the EA community.

Additionally, the recent fraud scandal and collapse of cryptocurrency company FTX (see Yaffe-Bellany, 2022) ought to act as a warning against applying radicalized versions of long-termist EA. When people take dogmatic approaches to any ethical framework, this can lead to cruelty and misdeeds (Fiske & Rai, 2014). For example, if a person took a radicalized long-termist perspective on EA, they

could come to believe that nearly any sacrifice today—even harming and lying to thousands of people—is worth making if it meaningfully reduces existential risks. Nearly all EAs reject this trade-off, and believe that a strategic approach to ethics should *not* contradict practical ethics and widely held virtues (Schubert & Caviola, 2021), nor act as a justification for radical means-to-ends reasoning (MacAskill, 2022: 240–242). Nevertheless, it is important to consider how EA-inspired moral arguments could lead ethical actors—and entire fields of scholarship—astray.

Despite these limitations, we believe the project of EA is useful because it pushes scholars to use careful logic and reason to think through how we, as a collective field, can most effectively improve well-being and decrease suffering for all living beings, both now and in the future. Even though many open questions remain, we believe an EA perspective lays a solid foundation upon which others can improve.

Building on the definition of EA, we introduce the construct of effective-management research (EMR). We define EMR as (a) using evidence and careful reasoning to figure out how to maximize the social impact—in terms of degree of the improvement per person, the length of time each person remains better off, and the number of people helped—of management research; and b) taking action on that basis with one’s own research and career.

We believe EMR is a reasonable and tractable goal. First, incentives to work on socially impactful research are becoming more plentiful. Multiple granting institutions have come out of the EA movement over the last few years, such as the Forethought Foundation, Open Philanthropy, and EA Funds. Some universities are even adding a social impact criterion for tenure promotion. Further, management researchers study topics that are incredibly socially important. The Global Priorities Institute (GPI) at Oxford University has begun to identify some of the most important questions to research, for those who want to produce the most effective societal impact with their academic careers. Although GPI’s research agenda is currently tailored toward philosophers and economists, many of the priority topics identified by GPI are highly relevant to management researchers. For example, GPI has called for research on political and economic institutions, social movements, intergenerational fairness and altruism, impacts of artificial intelligence (AI) on society, collective action problems, judgment and decision-making under risk and uncertainty, cross-cultural morality and prosociality, happiness and well-being,

<sup>2</sup> For example, threats to liberty, totalitarian dictatorships, and moral backsliding (for a discussion, see Hilton, 2022).

<sup>3</sup> For example, nuclear war, extreme climate catastrophes, bioengineered global pandemics, and misaligned AI (for a discussion, see Todd, 2022).

**TABLE 1**  
**Examples of EMR Questions**

Effective Altruism's Social Impact Goals	Examples of Relevant Topics in Management Research	Example Research Questions for Management Researchers
Developing effective policies to improve health, well-being, life expectancy, and income for those struggling with poverty around the globe.	Entrepreneurship	How can NGOs and government organizations support the entrepreneurial ventures of people living in poverty?
	Wage disparities and inequality	How can the reallocation of global wealth be increased—for example, by changing incentives for corporate taxation?
	Supply chain management	How can the distribution and delivery of scarce goods to those struggling with poverty globally be improved (e.g., via aid, vaccines, technology)?
Helping to mitigate existential risks to humanity, such as climate disasters, pandemics, nuclear war, and AI.	Intertemporal and intergenerational choice	How can individuals be encouraged to engage in prosocial acts that benefit future generations (vs. just the current generation)?
	Negotiations and conflict resolution	How can communication between world leaders be improved to decrease the chance of conflict?
	Sustainability	How can individuals and organizations be incentivized to adopt sustainable practices?
Decrease extreme suffering risks, including extreme inequality and political threats to liberty (e.g., malevolent totalitarianism).	Diversity, equality, and inclusion	How can (educational and organizational) opportunities for disadvantaged populations be increased?
	Leadership and power	Why do dictators come into power, and how can we prevent the election of malevolent actors?
	Identity and ideology	How can individuals and organizations help to reduce ideological sectarianism and extremism?
Helping to shift societal norms toward prioritizing impartial altruism and effective-mindedness.	Social movements	How can leaders and individuals inspire others to join new social movements?
	Business ethics and morality	How can we shift individuals' and organizations' priorities from avoiding bad to doing good?
	Career selection	How can we incentivize individuals to pursue effective-altruistic careers?
Effectively improve the well-being of nonhuman animals—for example, by eliminating cruel practices in industrial animal systems.	Social issues in management	What are organizations' social obligations toward preventing harm to nonhuman animals in their organization?
	Judgment and decision-making	How can organizations nudge employees to make ethical food choices (e.g., through the choice architecture of their cafeterias)?
	Organization development and change	What types of formal or informal policies and mandates are most effective at meaningfully changing organizations—for example, improving the treatment of nonhuman animals in industrial animal systems?

*Notes:* These research questions are just a starting point, and we hope that management researchers continue this work by examining how their own research topics and questions fit into effective altruism's social impact goals. Of course, some societal problems are outside of the scope of management research, just as some problems are outside of the scope of chemistry, economics, philosophy, physics, and every other scientific discipline. The social problems discussed in this table are some examples that we believe are within the scope of management research. Lastly, just because societal problems and research questions are mentioned here, that does not mean that there is no management scholarship to date on these questions. In fact, research has explored many of these topics. We included some non-neglected societal problems and research questions because we believe that working on any one of these topics is a route to producing effective-management scholarship (i.e., the questions rank high on importance, tractability, and *absorbability*).

(super)forecasting, and much more (Global Priorities Institute, n.d.). Each of these topics fall squarely into the field of management research, especially when trying to predict, explain, and intervene on individual and organizational behaviors.

To illustrate this, we created a sample list of management research questions that relate directly to GPI's list of priority research topics and EA's social impact goals (see Table 1). These are not necessarily new topics of research—many of them already have a wealth of research devoted to them. For example, we recommend researching social movements even though there already exists a large body of literature on social movements and collective action in our field (e.g., Bail et al., 2018; Briscoe, Chin, & Hambrick, 2014; DeCelles, Sonenshein, & King, 2019; Kim, Shin, Oh, & Jeong, 2007; King, 2008; Weber, Heinze, & DeSoucey, 2008). We simply list topics that we believe would be effective to research, based on the importance, tractability, and neglectedness or absorbability of the topic. These topics, for example, are not necessarily neglected, but could effectively absorb additional attention. Of course, this list just serves as a starting point; we are optimistic that scholars can draw many more connections between EA's social impact goals and management research questions.

### A ROADMAP TO *EFFECTIVE-*MANAGEMENT RESEARCH

So, how can we achieve the goal of EMR? We take a problem-driven research approach to develop our roadmap (Davis, 2015; Wickert, Post, Doh, Prescott, & Prencipe, 2021). Thus, the aims of this paper are twofold. First, we begin by asking which social problem(s) effective-management researchers ought to consider addressing with their research. We use a long-termist EA lens to generate a list of high-impact research topics. Importantly, however, our second aim is to outline a path for EMR within any research stream, even if it does not align with these research topics. We organize our roadmap to EMR (see Figure 1 for a visualization) around these two aims, split across three central questions, in the following order: (a) What social problem should I work on (aim 1)? (b) How should I go about studying the problem (aim 2)? (c) What should I do with my findings (aim 2)?

#### EMR Roadmap Step 1: What Social Problem Should I Work On?

To help scholars figure out which societal problems to focus on, we use an adapted version<sup>4</sup> of the

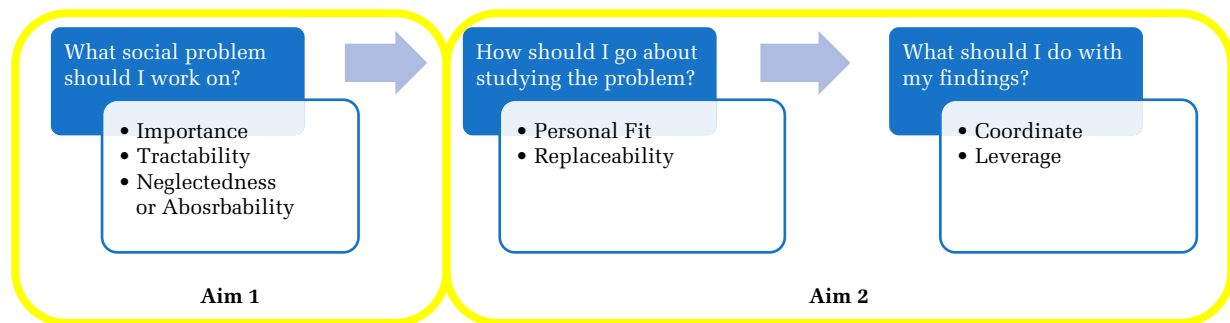
importance, tractability, and neglectedness (ITN) framework, which is widely used by EA meta-charities and organizations (Gainsburg, Pauer, Abboub, Aloyo, Mourrat, & Cristia, 2022; MacAskill, 2015). This framework suggests that management researchers should focus on problems that are (a) highly important, (b) tractable, and (c) neglected or absorbable. In Table 2, using our own research as inspiration, we walk through how to apply this framework to a specific research question.

**Importance.** A pressing societal problem is one that currently impacts a large number of lives, to a great degree, or is likely to impact a large number of lives, to a great degree, in the future if the current problem remains unsolved. Since we have a limited amount of time and resources, if we want to produce the greatest societal impact with our research, we ought to focus on the problems that are the most important. This is not to say that other, less-pressing social problems are socially *irrelevant*, nor that scholarship on less-pressing problems cannot produce a degree of social impact. Though, based on the tenet of effectiveness, if scholars are faced with a choice between spending their energy and resources on a more- versus less-pressing social issue, they can have more of an impact working on the former. As an example of highly pressing societal problems, questions pertaining to the long-term future of humanity (e.g., mitigating existential risks like nuclear warfare, bioengineered pandemics, climate disasters, and malicious AI, as well as suffering risks like extreme inequality, moral backsliding, and threats to liberty) have the potential to impact an extremely large number of lives, when factoring in the trillions of humans who could cease to be born, or fail to have rich and meaningful lives, if these issues are not resolved (Beckstead, 2013; Greaves & MacAskill, 2021; MacAskill, 2020, 2022; Ord, 2020). Management researchers could meaningfully contribute to mitigating such existential and suffering risks by continuing to explore topics such as conflict resolution and negotiations, institutional reform, collective action issues, intertemporal choice and intergenerational altruism, and much more (see Table 1 for examples).

**Tractability.** Effective-management researchers should also prioritize studying tractable problems—

<sup>4</sup>Our ITN framework is very similar to previous versions used by EA organizations and scholars. However, we chose to add absorbability to account for cases in which a social problem or research topic is not neglected, but could nonetheless effectively absorb significant talent and resources.

**FIGURE 1**  
**Problem-Driven Roadmap to EMR**



that is, those for which progress is highly probable, and would take fewer resources (both time and money) to accomplish. For example, faced with working on two problems that are equally important—but one of the problems is much easier to help understand, explain, or solve than the other—an effective-management researcher would prioritize the more tractable problem. This may sound unintuitive to management researchers, as we may be drawn to use our great minds to help untangle the most complicated social phenomena. However, since societal impact is a priority for effective-management researchers, working on more tractable problems—importance, neglectedness, and absorbability being equal—is more effective.

Moreover, management researchers are particularly well-positioned to uncover and intervene on organizational causes to pressing social problems. Unfortunately, organizations and organizational leaders have a large hand in perpetuating many of the pressing social problems we have identified thus far (e.g., inequality, environmental degradation, poverty traps, animal suffering, negative societal impacts of advancing AI). We management researchers are especially well-situated to consider, for example, how institutional and organizational leaders could reduce the negative externalities of profit-seeking and improve corporate social responsibility. In other words, if a key bottleneck to effectively mitigating a pressing social problem sits squarely in the world of organizations, management scholars are especially well-positioned to study and address these bottlenecks. Thus, the tractability of a given societal problem is informed both by how objectively solvable the problem is *and* how easily management scholars (in particular) could tackle a major antecedent or bottleneck to the social issue at hand.

**Neglectedness and absorbability.** With these problems in mind, it is important for effective-management researchers to then focus on the degree to which they can meaningfully impact the solution to a given problem. That impact is a function of neglectedness and absorbability. A scholar may not be able to move the needle on problems that are already receiving a great deal of attention, given diminishing marginal returns of investment (Gainsburg, Pauer, Abboub, Aloyo, Mourrat, & Cristia 2022), and therefore should seek out problems that are relatively neglected. However, there are some problems that are so important that they have already attracted a great deal of scholarship but are so large that the research on this problem could effectively absorb new academic talent. For example, the topics of fairness, justice, and equality (e.g., Chugh, 2018, 2022; Kray & Thompson, 2005; Sidanius, Levin, van Laar, & Sears, 2008; Sidanius & Pratto, 1999; Stephens, Fryberg, Markus, Johnson, & Covarrubias, 2012), as well as the societal impacts of advancing technologies (Gratch & Fast, 2022; Greene, 2016; Hunkenschroer & Luetge, 2022; Kosinski, Stillwell, & Graepel, 2013), are not neglected areas of management research. However, these research topics address such large social issues that solving them will require a great degree of research and, hence, likely could effectively absorb additional talent.

**Bringing together importance, tractability, and neglectedness or absorbability.** Factoring in the ITN framework may be a difficult task for many management researchers, especially when trying to balance each of the factors to optimize on impact. We believe that, as a field, we should start a conversation about what social problems are most important for management scholars to research (often referred to as “global priorities research”). To this

TABLE 2

**Applying the EMR Roadmap to the Following Research Question: “What Makes Nonprofit and Government Organizations Effective (vs. Ineffective) in Helping Recipients Escape Their Hardship?”**

Roadmap	Application Example
<p><b>Importance:</b> Does the social problem currently impact a large number of lives, to a great degree, or is it likely to impact a large number of lives, to a great degree, in the future if the current problem remains unsolved?</p>	<p><b>What is the social impact goal, and is it important?</b> The goal is to understand what makes take-up and (continued) utilization of help more likely, and to use these insights to design programs and interventions that can more effectively alleviate social problems. Improving help so that it is both economically and psychologically rewarding (e.g., motivating, hopeful, agentic, dignifying) to individuals experiencing hardships (e.g., illness, poverty, inequality, etc.) could increase the long-term value (i.e. indirect impact, spillover effects) of all forms of help.</p>
<p><b>Tractability:</b> Is it highly probable that this research will result in meaningful progress on the social problem at hand? If yes, would this progress take fewer resources (both time and money) to accomplish compared to other avenues of research?</p>	<p><b>Is this research likely to result in (cost-effective) progress?</b> Almost all helpers (whether they consist of a charity, individual giver, or government agency) want their aid and assistance to actually be used by recipients. Improving the likelihood of that goal (i.e., aid take-up and effectiveness) with low-cost, easy-to-implement interventions (e.g., changes in the way aid is described or advertised) would likely receive little pushback. By researching the recipient’s perspective, we can develop more successful interventions. Moreover, most nonprofits already collect recipient satisfaction data, but many do not do anything substantial with these data. It may be easy and relatively low-cost to make such data-collection opportunities more insightful for improving aid.</p>
<p><b>Neglectedness:</b> Are few to no other scholars working on research questions similar to this one?</p>	<p><b>Is this research neglected?</b> Most of the literature aiming to improve the effectiveness of help has focused on how to increase resources allocated toward such endeavors (e.g., increase charitable donations). The recipient’s perspective (i.e., understanding how receiving help impacts recipient psychological well-being and the take-up of aid) has been comparatively understudied, and almost completely neglected by management research.</p>
<p><b>Absorbability:</b> Is there still a significant amount left to learn to fully understand the social problem, why it occurs, and how it can effectively be solved?</p>	<p><b>Can this research topic absorb more scholarship?</b> Millions of people have fallen into poverty and food insecurity since COVID-19, and almost every person in the world has had to face uncertain and difficult health and economic decisions because of COVID-19. Unfortunately, there are no clear paths toward helping people through these hardships. These are all incredibly difficult and complicated social phenomena, so much research could be absorbed to help better understand how to most effectively help individuals cope with and escape such hardships.</p>
<p><b>Personal fit:</b> Do your passions, training, and connections make you uniquely situated to study this research topic?</p>	<p><b>Do you have personal fit?</b> If questions about morality, prosociality, suffering, and hardship keep you up at night; if you have training in these areas; and if you have access to or connections with aid or governmental organizations, this is a good fit for you.</p>
<p><b>Replaceability:</b> What difference could you make if you studied this research topic in this way?</p>	<p><b>What difference could you make?</b> Having a knack for building novel theory around morality, suffering, and the experience of hardship, as well as having a talent for running both laboratory and field experiments, may make someone working in this space particularly “irreplaceable.”</p>
<p><b>Coordination:</b> What additional research needs to be done before you can confidently make policy recommendations?</p>	<p><b>Are you coordinating with other researchers?</b> To answer this question effectively, it is important to coordinate with development economists, social psychologists, cultural psychologists, marketing scholars, management researchers, and moral philosophers, as well as researchers at nonprofit and government organizations. The ultimate goal of coordination is to develop basic psychological insights about what aid recipients need and want across contexts and cultures, and to develop interventions (e.g., by changing the way organizations approach aid recipients or market their services) that align with recipients’ needs and preferences.</p>
<p><b>Leverage:</b> Are you communicating with the public and change-makers to draw interest and resources to both the social problem you study and your research topic?</p>	<p><b>Are you speaking to the public and change-makers?</b> As insights are developed and robustly tested, it will be important to get this work in front of change-makers (e.g., nonprofit managers and policy-makers) to encourage prioritization of the recipient’s perspective in relevant organizations (e.g., nonprofit organizations, hospitals, and government agencies).</p>

end, we can collectively create a list of highly impactful management research topics and questions that have a high chance of leading to meaningfully social change (we begin such an endeavor in Table 1), many of which management scholars have already begun to address. The list in Table 1 is merely a starting point for coming up with some of the most important social problems for management researchers to pursue.

### EMR Roadmap Step 2: How Should I Go about Studying the Problem?

Shifting now to our second goal, we aim to outline a path for EMR within any research stream, even if it does not align with the research topics outlined in Table 1. Once scholars have identified an important social problem to research, they must next decide how to go about studying it. For example, what theoretical lens should they apply? What specific research topic should they pursue? Which methods should they use? Should they focus on more basic or applied research? Rather than simply conducting a literature review and trying to incrementally fill in gaps in the literature or come up with a sexy, new, and publishable effect, effective-management researchers reflect carefully on two factors before starting a research project: personal fit and replaceability.

**Personal fit.** Personal fit guides scholars toward specific research topics within a given social problem. For example, if the pressing problem one identifies is to help mitigate existential risks to humanity, what research topic should they pursue within that broader social issue? Do they study intertemporal and intergenerational choice (to understand why individuals and organizations do not invest more to help future generations)? Negotiations and conflict resolution (to decrease the chance of nuclear warfare)? Effective-management researchers carefully reflect on how their personal passions and training influence the impact they can make (MacAskill, 2015). As one's career evolves, personal fit is also based on one's area of expertise and the connections one has formed within and outside of academia. Personal fit can improve the chances of producing effective scholarship and meaningful social impact because fit helps scholars sustain motivation—through rejections and setbacks—to continue their research throughout the entirety of their (hopefully very long) academic career.

**Replaceability.** Effective-management researchers also consider their replaceability—that is, the marginal impact they (compared to other management researchers) can make on the social problem

and research topic they are studying. Replaceability is a way to help identify the true impact of one's research, as opposed to its direct impact. MacAskill (2015: 70) cleverly outlined the concept of replaceability with the following scenario:

Suppose, for example, that I see a woman collapse on the ground. She's had a heart attack. There's no one else around, so I run up to her and start performing CPR. Suppose I've never performed CPR before, but I manage to restart the woman's heart. She recovers but, as a result of the poor-quality CPR, is left with a disability. Even so, it's clear that I have done a great thing. Now suppose there had been a paramedic around when the woman collapsed. This paramedic would have surely restarted her heart without causing injury, but, while I was running towards the woman, I pushed the paramedic out of the way and started performing CPR myself. In this case, I still saved a life, but if I hadn't, the paramedic would have been able to do the same thing without causing damage. In this case, how should I feel about my actions? Am I a hero? After all, I "saved a life!" Of course I'm not a hero. The good I do is not a matter of the direct benefits I *cause*. Rather, it is the *difference* I make.

Thus, effective-management researchers ask themselves: If I do not work on this research project, would someone else, and would they do it better? To find research projects that no one could do better (i.e., where the scholar is "irreplaceable"), effective-management researchers capitalize on their unique methodological and theoretical talents. Every scholar has particular gifts, whether that is a talent for (a) embedding oneself in a situation or culture to qualitatively build theory, (b) abstractly thinking through complicated social phenomena to build theory and meta-theory, (c) coming up with clean and clever laboratory experiments to test hypotheses and explore causality or replicate prior work, (d) running field experiments across contexts or cultures to test for generalizability and ecological validity, or (e) understanding and using advanced statistical methods to extract meaning from big data or running large-scale meta-analyses. Effective-management researchers are aware of their unique talents and hone their skills so that they can become the best scholars within their particular method of study.

**Bringing together fit and replaceability.** Overall, questioning one's personal fit and replaceability allows effective-management researchers to look at their field more holistically, and ask where they are needed most. Specifically, after identifying a pressing social issue to study, effective-management researchers first identify which specific research



topic(s) they feel the most personal fit with: What questions keep them up at night? What literatures and topics of study have they developed expertise in? Where do they have connections? After identifying fit, effective-management researchers evaluate what is needed to better understand that research topic, and whether their talents align with that need. For example, imagine you have a particularly strong fit with studying conflict resolution. Now that you have selected your research topic, it is time to evaluate how to make a *difference*. If (a) there was no robust theory that could help to explain why intergroup conflict occurs and (b) you had a talent for theory-building, as an effective-management researcher you would work on the basic science behind intergroup conflict. If there was robust theory explaining what gives rise to intergroup conflict, but there was (a) little empirical work testing the replicability of conflicting theories, boundary conditions, or underlying mechanisms; and (b) you had a talent for experiments and statistical testing, you as an effective-management researcher would work on laboratory or replication experiments on conflict resolution. Finally, if there was both robust theory and laboratory experimentation, but there was (a) little applied research testing different real-world interventions to mitigate intergroup conflict in the field and (b) you had particularly strong fit with applied research methods, you as an effective-management researcher would work on applied conflict-resolution research.

### EMR Roadmap Step 3: What Should I Do with My Findings?

You have successfully uncovered novel and potentially impactful insights or interventions for a pressing societal problem. You have published a paper on it and even discussed policy implications. Now what? Although many scholars stop at publication, effective-management researchers know that answering one research question is only the first step in producing societal impact with their research. Effective-management researchers prioritize coordination with other scholars and change-makers to generate awareness and, eventually, societal impact.

**Coordination.** Effective-management researchers understand that coordination is key for good science (Davis, 2015; IJzerman et al., 2020). Many management researchers might be hesitant to make large, sweeping claims from their research, especially if their research entails untested theory or if their insights are based on exclusively laboratory studies that do not measure behavior, cross-cultural

differences, or long-term consequences. We believe that this hesitation is correct, as overclaiming impact from underdeveloped scholarship can actually produce more harm than good (MacAskill, 2015: 9–14, 70–74). Although a single paper or scholar may rarely effectively inform policy-makers on how to improve society, a large group of coordinating scholars with a robust body of replicable work *can*. Scholars ought to consider how their research contributes to our understanding of, or ability to solve, the social problem being studied, and meaningfully reflect on what additional research would need to be done before they could confidently make policy recommendations. Once they have this in mind, it is important to make clear to other researchers—inside and outside of academia—what they think still needs to be done before confident policy recommendations can be formed.

**Leverage.** Although coordination within management research and academia is necessary, so too is communication with change-makers (e.g., policy-makers, nonprofit leaders, funding organizations) and the public. One way to leverage your position is to work toward becoming a public intellectual. For example, rather than solely writing academic publications, teaching to just those in your classroom, and speaking at academic conferences, consider also writing popular press articles and books; sharing your work with, and teaching it to, a broader audience via social media platforms; speaking on podcasts; giving public-facing talks (e.g., TED talks); and presenting at or organizing conferences with a mix of practitioners and academics. The goal is to honestly and measuredly share your ideas with experts in different disciplines, change-makers, and the public on how to help solve some of the most pressing societal problems. Leverage helps effective-management researchers draw more interest and resources to the problem they are studying and the insights they are generating on how to help solve the problem. We must speak to society if we have any hope of improving it.

**Bringing together coordination and leverage.** Aguinis et al. (2022) argued that failing to bring policy implications into our publications creates a risk of management research becoming societally irrelevant. We would like to take this argument one step further: stating the policy implications of our work explicitly in our publications should be the *bare minimum*, if we want our work to lead to meaningful societal impact. Unfortunately, according to Aguinis et al. (2022), the vast majority of top OBHRM research is failing to reach this minimum. Therefore, we believe that the societal impact produced from

management research currently is only scratching the surface of the field's potential.

We recommend that effective-management researchers work to shift the norms around the “practical impact” section of their introductions and conclusions to foster increased coordination *within academia*. Rather than simply stating policy implications, consider outlining (a) the broader societal problem you are addressing, (b) where you think management research currently is in effectively solving the problem you are researching, (c) what is “next” before management research can confidently make policy recommendations and roll out societal interventions (e.g., Do we need more basic science to understand the problem? Do we need to test our theories in the field and cross-culturally?), and (d) what this cumulative body of research could mean for society.

It is important to then leverage your power and status as an academic researcher to foster coordination with those *outside of academia*. Disseminate your insights and the potential of this body of scholarship to both the public and change-makers so to build momentum and resources around solving the social problem you study. Although a single management researcher cannot do everything, a coordinating group of effective-management researchers working on the same problem—by examining a variety of research topics that feed into a broader understanding of the social problem at hand—who use their leverage as public intellectuals to inform both the public and policy-makers can do a great deal.

At this point, some readers may be wondering whether producing tangible societal impact from management research is an idealistic and unrealistic pipedream. We agree that the journey will likely be grueling, *but that should not stop us from dreaming up what a better, more impactful field could look like, and working to realize that dream*. Everyday management scholars dream up unexplored research questions and chip away at projects they know will be difficult to accomplish, and accomplish well. However, the difficulty of the research question does not stop them from studying it—the payoff of discovering something new is simply too enticing. Similarly, we believe that the project of EMR will be difficult to accomplish—and accomplish well—but that should not stop us from trying to achieve it.

## CONCLUSION

We are living in an age of global catastrophes. In the United States alone, we face the continuously

unfair treatment of Black Americans and political sectarianism (Finkel et al., 2020) leading to intergroup conflict and political stalemates. Globally, we face the COVID-19 pandemic, the #MeToo movement illuminating the unethical treatment toward women in the workplace, persistent extreme poverty, horrendous treatment of factory-farmed animals, a climate crisis threatening the lives of our future generations, ongoing risk of nuclear war, fast-developing AI with slow-developing AI ethics (Bostrom, 2014), and much more. We could be doing better, and more, than we are doing today. We do believe that each of these societal problems are at the top of many management researchers' minds. In fact, this past year's theme at the Academy of Management's annual conference—the biggest conference in the field of management and management research—was *Creating a Better World Together*. However, it took the tragedies of 2020 to start taking societal impact—rather than solely theoretical and corporate impact—seriously. We should not wait until the next global catastrophe to make effective social change a priority in our research agendas and in our field.

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