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Published in:
Age and work

DOI:
10.4324/9781003089674-14

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CONDUCTING EXPERIMENTS AND INTERVENTION STUDIES TO UNDERSTAND AGE AND WORK

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Most researchers probably agree that experiments and interventions are the methods of choice to test causal relationships and to provide theory-based solutions to practical challenges. It is also well recognized that the study of age and work is a relevant topic of our time. Still, researchers rarely consider these statements in concert: Compared to correlational designs, experimental and intervention research is relatively scarce in the context of age and work (Truxillo et al., 2015). This scarcity also holds true for lifespan developmental psychology in general (Freund & Isaacowitz, 2013).

The major reason for the scarcity of experiments on work and aging probably lies in the fact that researchers cannot randomly assign individuals to different chronological age groups—it is logically impossible to meet this key requirement of experimental designs (Freund & Isaacowitz, 2013). Similar to research on other naturally occurring groups, much of the research on age (and work) therefore remains correlational and quasi-experimental (Truxillo et al., 2015). A further reason for the scarcity of interventions or training studies probably lies in the relatively large amount of time and effort that need to be invested to develop and conduct well-designed interventions, along with the risk of low return on investment (Fernandez et al., 2019; Michie et al., 2011). However, there are still manifold possibilities for conducting true experiments and effective interventions to understand and address age-and-work issues and consequently render age-and-work research more valuable and relevant to nonacademic communities. This chapter outlines how such research can be realized in the future; we also draw on exemplary existing studies (e.g., Burmeister et al., 2020; Gärtner & Hertel, 2017; Hommelhoff et al., 2018; Müller et al., 2016) to illustrate different research approaches.

DOI: 10.4324/9781003089674-14

In the first part of our chapter, we discuss similarities and differences between experiments and interventions. Although both research designs involve the manipulation of certain variables or stimuli, they differ in their primary objective: Experimental research seeks to *understand and explain* causal relationships, while interventions aim to *change* work behavior and outcomes (e.g., workplace well-being and performance) for the better (e.g., Gerrig, 2013).

In the second part of our chapter, we highlight two basic strategies of how researchers can realize experiments on age and work. First, we focus on the experimental *manipulation of participants' internal context*. That is, participants' imagination or perceptions can be experimentally manipulated (e.g., their occupational future time perspective; Hommelhoff et al., 2018). Second, we describe the experimental *manipulation of participants' external context*. That is, outer context conditions can be manipulated in different ways to explain age-related differences in work behavior and outcomes (e.g., certain contexts allow expertise to compensate for declines in perceptual-motor efficiency; Bosman, 1993, 1994; Salthouse, 1984).

In the third part, we present a four-step approach of how interventions on age and work can be designed to maximize their potential effectiveness. This approach involves (1) a systematic problem description, (2) a theory-driven causal analysis to derive a logic model of the problem, (3) the development of the intervention, taking into account the specific implementation context, and (4) the monitoring and evaluation of the intervention (Buunk & Van Vugt, 2013; Fernandez et al., 2019). We will illustrate this systematic approach with examples of work and aging interventions (Burmeister et al., 2020; Müller et al., 2016).

In summary (see also Table 11.1 for an overview), our chapter aims to stimulate more experiments and interventions in the field of age and work. The fourth and final part of this chapter therefore derives practical recommendations for researchers who plan experiments and interventions on work and aging.

TABLE 11.1 Experiments and Interventions to Understand Age and Work

	Experiments	*Interventions*
Commonality	Controlled experimental variation of certain stimuli/conditions	
Differences	• Primary goal is to *understand/explain* causal relationships • Conducted in different contexts (e.g., lab, field, online) • Assesses short-term differences or changes in outcomes	• Primary goal is to *change* something for the better • Mainly conducted in applied contexts (field), thus randomization and control group not always possible • Strives for long-term changes in outcomes

(*Continued*)

TABLE 11.1 (Continued)

	Experiments	*Interventions*
Approach	*Two basic strategies* ***1) Experimental variation of participants' internal context*** *Explanation:* Participants' imagination or perceptions are experimentally manipulated in the context of age and work *Ideas for future research:* a) Further "*Please imagine that you are . . .* "-experiments*:* Participants are asked to imagine different occupational time perspectives, levels of muscular strength, life stages, etc. b) Further "*How do you think about . . .* "-experiments*:* Participants are asked to evaluate different kinds of (fictitious) CVs, employees, jobs, articles etc. with age-relevant content *Goals:* (1) Understanding of age-related mechanisms, age stereotypes, images of aging, and age-related expectations at work; (2) demonstration of how employees envision their own future and aging at work ***2) Experimental variation of participants' external context*** *Explanation:* Participants' external environment is experimentally manipulated in the context of age and work *Ideas for future research:* Vary external context conditions that should offset the hypothesized processes underlying observable age differences *Goals:* (1) Understanding the interplay of age, skill, prior knowledge, tenure, experience, subjective age, and motivation in different work tasks and contexts. (2) Understanding how older employees maintain functioning and compensate for losses (e.g., in fluid intelligence or sensory acuity) at work	*An approach in four steps* ***1) Systematic problem definition*** Investigate what the problem is that should be targeted by the intervention, for whom it is a problem, and in which contexts it is a problem. Derive measurable indicators that need to change in the target group. ***2) Logic model of the problem*** Specify the multilevel factors (e.g., at individual, group, or organizational level) that give rise to the problem. Resort to existing studies and theories and double-check with stakeholders/ practitioners. ***3) Intervention design*** Based on the causal analysis in step 2, develop possible and suitable intervention strategies and policies. Among all suitable interventions, select a feasible intervention and formulate a *theory of change* (which maps intervention activities to desired outcomes). ***4) Monitoring and evaluation*** Specify how the intervention will be monitored (in terms of participants and intervention activities) and evaluated (in terms of immediate, mid-term and long-term outcomes). Develop a research design and select appropriate measures.

Research Objectives of Experiments and Interventions

Psychological research strives to describe, to understand, and to predict and change human behavior (Gerrig, 2013). Research questions that focus on *describing* human experience and behavior can best be addressed through correlational and descriptive research designs, for example via cross-sectional or longitudinal surveys (i.e., data collections *without* manipulation or design-based control of variables; MacDonald & Stawski, 2016). Even though longitudinal surveys offer some advantages over cross-sectional ones in terms of approaching the question of causality, the experiment remains the ideal and often the only method to conclusively answer causal questions—that is, psychological experiments allow *understanding and explaining* human behavior (Tabachnick & Fidell, 2007). Experiments have at least three specific characteristics, namely random assignment of participants to different levels of the independent variable, manipulation of those levels, and control of potential confounding variables (Tabachnick & Fidell, 2007). Interventions and training studies are special types of experiments (Freund & Isaacowitz, 2013); they are closely related to experiments because they also involve the controlled manipulation of certain variables or stimuli. However, the main objective of an intervention is to *change* human behavior or increase positive outcomes (Robertson et al., 1993; Zabel & Baltes, 2015).

For example, existing experimental research in the context of age and work has looked into *why* employees prefer new versus familiar work teams (Gärtner & Hertel, 2017) or *why* they favor instrumental versus emotional social partners for a lunch break from work (Hommelhoff et al., 2018). Based on socioemotional selectivity theory (Carstensen, 2006) that predicts changes in social motivation when endings come closer, these experiments have explained employees' social preferences by their occupational future time perspective (Zacher & Frese, 2009). When the occupational future appears restricted or limited (e.g., when respondents are asked to imagine being close to retirement), instrumental social partners are preferred less for breaks (Hommelhoff et al., 2018), and familiar teams are preferred over new teams (Gärtner & Hertel, 2017). While these experiments focus on explaining human behavior, existing intervention studies in the context of age and work were designed to change or increase, for example, retirement adjustment (Seiferling & Michel, 2017), life satisfaction and workplace retention (Stevens-Roseman, 2009), or appreciation of team diversity (Jungmann et al., 2020).

More gradual differences between experiments and interventions follow from the difference in their main objective—explaining versus changing human behavior. Experiments are often conducted in the lab or online, while interventions are typically carried out in the field under real-life conditions. Thus, whereas (lab) experiments are commonly internally valid, less applied, more a snapshot in time, and more focused on making a theoretical contribution, interventions tend to be more externally valid, more applied, more long-term focused, and more focused

on making a practical contribution (e.g., Schram, 2005). However, some studies illustrate the gradual nature of these differences by combining both approaches; they involve mini-interventions in laboratory experiments. For example, Malinen and Johnston (2013) have shown that a mental imagery intervention changed explicit (but not implicit) attitudes toward older workers in a positive way.

Although this chapter encourages the use of experiments and interventions, we emphasize that the use of these methods is not an end in itself but should depend on the research interest. Most likely it is the *combination* of different methods that allows us to understand aging in the context of work. While interventions can help to improve outcomes for employees and organizations (Zabel & Baltes, 2015) and while (quasi-)experimental studies help us to explain and understand age-related differences in the world of work, longitudinal designs (without manipulation of variables) help us to describe aging-related within-person changes over longer time intervals (MacDonald & Stawski, 2016). Along these lines, it is noteworthy that not all experiments and interventions in the context of age and work fully achieve their research goal of explaining or changing human behavior. Some experimental research in the field of organizational behavior has been criticized as superficial and non-generalizable (Highhouse, 2009); and even well-crafted interventions can fail to produce the desired change or can even have unintended negative effects (e.g., when expectations are disappointed; Aust et al., 2010).

Two Basic Strategies for Conducting Experimental Research on Age and Work

One general strategy to experimentally examine age and work issues is the experimental manipulation of participants' *internal context*. Thus, the experimental stimuli are focused on the participants' inner world, their imagination and perceptions. A second strategy involves the controlled manipulation of participants' *external context* and thus the experimental situation. Usually, only one of the two strategies is applied in a given experiment (e.g., see Gärtner & Hertel, 2017 for the first principle and Bosman, 1993 for the second), although it is theoretically possible to combine them. In the following, we will illustrate these two strategies in more detail by describing typical procedures and main research goals, as well as avenues for future research (for an overview, see Table 11.1).

Experimental Manipulation of Participants' Internal Context

In this experimental framework, participants are typically asked to *imagine* different situations (e.g., being far from or close to retirement; Gärtner & Hertel, 2017; Hommelhoff et al., 2018), or they are asked to *evaluate* or otherwise react to different scenarios or descriptions at hand (e.g., different employee or

job descriptions; Bertolino et al., 2013; Gaillard & Desmette, 2010; Hanscom & Cleveland, 2018; Truxillo et al., 2012; Zacher et al., 2017). This experimental approach thus allows random assignment of participants to different stimuli that make age-related processes more or less salient in people's minds. Because participants can be randomized to these different imagination or evaluation tasks irrespective of their age, this experimental principle finds a way around the problem that it is impossible to manipulate chronological age while still investigating age-related phenomena (Freund & Isaacowitz, 2013). Depending on the research question, it is of course possible to include calendar age in the analysis as an additional independent, moderating variable; however, this means that (this part of) the experiment turns into a quasi-experiment (e.g., Bertolino et al., 2013; Truxillo et al., 2012; Zacher et al., 2017).

Besides conducting such experiments online (e.g., Hommelhoff et al., 2018; Rahn et al., 2019; Truxillo et al., 2012), lab and field experiments are also possible. In field experiments, researchers have constructed CVs of fictitious applicants that were identical except for the age of the applicants. These CVs were then randomly assigned to genuine job vacancies in the context of age discrimination research (e.g., Baert et al., 2016). Thus, the use of *fictitious* CVs or persons allows random assignment of different employee ages to different employers who then respond or react to the application. In lab experiments (e.g., Kulik et al., 2000; Malinen & Johnston, 2013), researchers have for example conducted mini-interventions to positively change attitudes or evaluations regarding age-related topics. While some small interventions show positive effects (in explicit attitudes; Malinen & Johnston, 2013), others reveal unintended effects: Kulik and colleagues (2000) demonstrate that those participants randomly assigned to view an age-diversity video asking them to suppress age-related thoughts later evaluated an older applicant less favorably than other raters did.

The *main goals* of studies following this first strategy of experimentation involve the understanding of age-related mechanisms (e.g., Gärtner & Hertel, 2017; Hommelhoff et al., 2018). After all, chronological age is never a causal explanation in itself; rather, it is linked to age-related cognitions, motivations, and capacities that influence behavior (Settersten & Mayer, 1997). This type of experimentation can further be used to understand the social context of aging workers, such as age stereotypes, images of aging, and age-related expectations and norms in the work context (e.g., Bertolino et al., 2013; Hanscom & Cleveland, 2018; Malinen & Johnston, 2013; Truxillo et al., 2012). These studies thus help understand how employees envision their own future and aging at work or how others react to them as a function of their age.

In *future studies*, researchers could ask participants either to imagine further work-related scenarios ("*Please imagine that you are . . .* " experiments) or to evaluate further employees, CVs, articles, jobs, teams, or work situations ("*How do you think about . . .* " experiments, see also Table 11.1). Depending on the research question and theoretical framework, participants' own age can be made more

or less salient via age questions with long scroll-down bars (e.g., Rahn et al., 2021), or positive versus negative aspects of aging can be highlighted through a quiz (e.g., Weiss & Lang, 2012). In general, researchers should consider not only calendar age but also other meanings of age (see also Chapter 3) such as subjective age, tenure, or lifespan age (e.g., measured by life stage via instructions like "please imagine that you have a full-time position and two toddlers vs. teenagers vs. an empty nest at home"). As to subjective age, researchers could also think of experiments that involve individuals' subjective or felt age as *dependent* variables (for a non-work-related study with this approach, see Stephan et al., 2013).

Experimental Manipulation of Participants' External Context

Within this approach, the experimenter systematically varies participants' external context and thus the outer experimental situation in ways that are hypothesized to either help or hinder older versus younger workers. If age-related differences in the outcome change in magnitude or even direction across conditions, conclusions can be made about how and why age affects work behavior. Typist studies (Bosman, 1993, 1994; Salthouse, 1984) are a classic and successfully replicated example for this approach from the work domain (other examples come from the field of air traffic control; e.g., Morrow et al., 1994). In these typing experiments, the preview of the text to be typed was manipulated (Bosman, 1993; Salthouse, 1984). This experimental variation allowed for an understanding of how older typists manage to maintain their performance despite declines in perceptual-motor efficiency: Older typists begin keystroke preparation earlier (i.e., they notice characters in advance sooner) than younger typists. In general, in this approach, the experimenter varies conditions in ways that should cancel out hypothesized processes underlying age differences (Freund & Isaacowitz, 2013). However, note that a completely random assignment of participants to different external conditions is not always possible: When calendar age is a prominent independent variable and thus when groups of younger and older workers are compared, the experiment becomes a quasi-experiment (for work-related examples from economics and ergonomics, see Charness & Villeval, 2009; Norheim et al., 2020).

Further examples of this second experimental strategy can be found in the general lifespan psychology literature and can serve as inspiration for age-and-work researchers. For example, Li and colleagues (2001) manipulated task difficulty in memory and walking tasks (via faster presentation rate of words in memory tasks or via higher wooden obstacles in walking tasks) to understand dual task costs in younger and older adults. Lindenberger and colleagues (2001) experimentally decreased participants' visual and/or auditory acuity to understand the relationships between sensory and cognitive functioning.

A special case of the experimental principle of changing external contexts is a natural experiment. For example, pension reforms and changes in statutory

retirement ages can be construed as experimental manipulations (although, of course, random assignment is not given). In the Netherlands, for instance, researchers have found that exogenous shocks to pension rights affected the expected retirement age and training participation (Montizaan et al., 2010).

The *main goals* of experiments following from this second strategy involve understanding the interplay of age, skill, prior knowledge, and motivation in different work tasks and environments. In particular, they strive to understand if and how older employees maintain functioning and compensate for losses and reductions (e.g., in fluid intelligence or muscular strength) at work.

In line with the previous experiments and Freund and Isaacowitz (2013, p. 361), *future experiments* could vary further "conditions that should compensate for the hypothesized developmental process underlying observed age differences." For example, information to be learned could be shown to different age, skill, and prior-knowledge groups for longer or shorter periods and in different formats. The lengths of breaks from learning different materials could be varied, as well as public accountability of task performance (Hess et al., 2001). Such experiments could thus address age differences in fluid intelligence, recovery times, and motivation. Future studies should again consider meanings of age beyond chronological age (see Chapter 3). For example, younger workers with longer tenure and older workers with shorter tenure could be in the sample. Even if some future experiments following this principle will turn into quasi-experiments, we think that the strategy of experimentally introducing external changes is promising in the field of age and work.

Four Steps of Designing Interventions to Address Age and Work Challenges

Developing effective interventions requires integrating theoretical and practical knowledge on intervention needs, possibilities, and the specific population and context in which the intervention will be embedded. To master this integration, systematic approaches have been developed in public health and applied social psychology, such as *intervention mapping* (Fernandez et al., 2019), the *behavior change wheel* (Michie et al., 2011), and the *from-problems-to-solutions approach* (Buunk & Van Vugt, 2013). A common feature of these approaches is that they define a number of steps that researchers should follow. We outline four common steps and illustrate these by relating them to two existing intervention studies from the work and aging field (Burmeister et al., 2020; Müller et al., 2016). Table 11.1 shows an overview of these steps.

Step 1: Systematic Problem Definition

Often, the impetus to develop an intervention arises from conversations with practitioners or from noticing unsolved problems in the workplace. For example,

an employee survey in a healthcare organization may show that older personnel lack the physical vitality to perform heavy physical tasks (Müller et al., 2016), or organizational leaders may observe that age-diverse teams fail to benefit from their various expertise (Burmeister et al., 2020). The first step of intervention research therefore is to develop a clear problem definition by precisely describing what the problem is, why and for whom it is a problem, and what the key aspects and possible causes are (Buunk & Van Vugt, 2013). Attention should be paid to identify the target group for a possible intervention, that is, the group whose cooperation is essential to solve the problem (Buunk & Van Vugt, 2013). In the examples, these could be older nurses with chronic health conditions or age-diverse coworkers who may give a new impetus to team processes.

The ultimate goal of this step is to derive measurable indicator(s) of the target behaviors or outcomes that would need to change in the target group for an intervention to succeed. The outcome variables thus specify the desirable end situation; they should be relevant to the problem, specific and concrete, and continuous (i.e., can be described as less or more; Buunk & Van Vugt, 2013). In our examples, measurable indicators may be the level of perceived work ability of healthcare employees and the degree of knowledge exchange in age-diverse teams, respectively (Burmeister et al., 2020; Müller et al., 2016).

Step 2: Logic Model of the Problem

Once the problem, target group, and outcome indicators are clearly defined, a systematic causal analysis of the problem is needed to gain leads for the intervention (Buunk & Van Vugt, 2013; Fernandez et al., 2019). Here, it is important to consider potential causes at all organizational levels (Michie et al., 2011). Researchers may consider factors at the level of individuals, the group, leadership, the organization, or the overarching social context (IGLOO; Nielsen et al., 2018). A further framework that can inform the logic model of the problem is the A-M-O model of organizational behavior (Blumberg & Pringle, 1982, see also Michie et al., 2011). Thus, researchers should consider abilities (e.g., skills, knowledge), motivation (e.g., intrinsic or extrinsic), and opportunities (e.g., physical space, social norms) in their search for the most plausible mechanism(s) to address in the intervention.

In the first example, researchers may conclude that the most plausible explanation for a threat to nurses' work ability may lie in their self-regulatory strategies captured by the model of selection, optimization, and compensation (SOC; Müller et al., 2016). In the second example, the most plausible explanation for the scant knowledge exchange between age-diverse coworkers may lie in a lacking awareness of each other's knowledge utility (Burmeister et al., 2020).

Step 3: Intervention Design

Once the causal factors of the target behavior or outcome have been exposed, an intervention can be developed, starting with choosing a general approach and

then working out specific intervention modules and activities (Fernandez et al., 2019). For example, the *behavior change wheel* (Michie et al., 2011) maps the causes identified in Step 2 to intervention strategies (at the individual level) and policies (at the organizational level; Michie et al., 2011). Strategies might involve training, education, persuasion, or incentives, while policies can comprise changes of guidelines, regulations, or services. When designing the intervention step by step, including details such as specific modules, materials, communication, timeline, and many more substeps (Fernandez et al., 2019), consideration of the specific context and target group are essential. Among all the possible interventions, only few will be suitable and feasible for the target group in the specific environmental context (Astbury & Leeuw, 2010; Walton & Yeager, 2020).

An important part of this step is an evidence-based *theory of change* underlying the intervention (Astbury & Leeuw, 2010; Fernandez et al., 2019). This theory is best illustrated by a graphical model that links specific intervention activities with immediate, midterm, and longer-term expected outcomes. This theory of change builds on the logic model of the problem and forms the basis for transparency in intervention goals and for monitoring and evaluation. In the first example (Müller et al., 2016), the research group designed a training with six sessions over a period of nine months, mainly grounded in the SOC model (P. B. Baltes & M. M. Baltes, 1990). The theory of change entailed that training nurses in the use of SOC behaviors leads to a more efficient use of their personal resources, which in turn enhances their sense that job demands and resources are in balance, resulting in higher levels of work ability (Müller et al., 2016). In the second example (Burmeister et al., 2020), the researchers designed a half-day training and follow-up call for age-diverse dyads of coworkers grounded in the information/decision-making perspective (Williams & O'Reilly, 1998). The theory of change involved that awareness of knowledge types and exposure of knowledge similarity and differences with older/younger coworkers would increase transactive memory, which in turn enhances knowledge transfer (Burmeister et al., 2020).

Step 4: Monitoring and Evaluation

Monitoring and evaluating an intervention are important to ensure and measure its effectiveness—or to understand why an intervention failed or produced unintended effects (Fernandez et al., 2019). Theoretically, an intervention can fail for two reasons. First, despite the best intentions, an intervention may not have been implemented as planned (indicating *implementation failure*; e.g., when materials were phrased too difficultly for participants). Second, the intervention may have been based on an inaccurate theory of change (indicating *theory failure*; e.g., when assumptions made were invalid, at least in the given context conditions; Walton & Yeager, 2020). For example, training SOC behaviors may only produce enhanced work ability if nurses have sufficient job autonomy to exercise these behaviors (Riedel et al., 2015). Furthermore, increasing knowledge exchange between

age-diverse coworkers may produce higher team performance only if teams are facing complex, interdependent tasks (Wegge et al., 2008).

Monitoring and evaluating an intervention is therefore crucial. *Monitoring* entails tracking attendance and immediate reactions to intervention materials and activities (D. L. Kirkpatrick & J. Kirkpatrick, 2006). It allows detecting implementation failure, as well as conducting evaluation analyses with only the subgroup who completed all intervention activities as planned (per-protocol analyses; e.g., Müller et al., 2016). *Evaluation* entails measuring all components of the theory of change in the prespecified temporal order and allows detecting theory failure. The optimal design for intervention evaluation is a longitudinal design with random assignment to intervention and control group (Lipsey & Cordray, 2000). The control group could receive the intervention at a later time point in a waitlist-control design (as in Müller et al., 2016) or receive an alternative intervention (as in Burmeister et al., 2020). The measures and their temporal spacing should be aligned with the theory of change. Typically, a baseline survey would include all outcomes and possible moderators; a post-training survey would include measures of immediate reactions and short-term outcomes, and one or more delayed surveys would include measure of mid- and long-term outcomes (D. L. Kirkpatrick & J. Kirkpatrick, 2006).

Practical Recommendations for Implementation and Data Analysis

This last section of our chapter provides further brief recommendations for planning, conducting, and analyzing data from experiments and interventions in the context of age and work. Some of these recommendations are specific for this context, while others are more generally applicable.

In the *phase of generating ideas and planning a study*, we consider it important to think about both the workplace and the larger social-cultural environment in which workers live (Truxillo et al., 2015). As pointed out before, we also recommend thinking about age beyond calendar age and to bear in mind other meanings such as subjective age, functional age, tenure, or lifespan age (e.g., Chapter 3). We further advise developing hypotheses that are grounded in theory (e.g., P. B. Baltes & M. M. Baltes, 1990; Bakker & Demerouti, 2007; Carstensen, 2006; see also Chapter 7) and suggest not only preregistration of hypotheses (e.g., via OSF.io or aspredicted.org) but also power analyses (e.g., Faul et al., 2007) to determine an appropriate sample size for experimental/intervention and control groups. As for sample composition, it is advisable to avoid extreme-group comparisons (e.g., career starters vs. near-retirees) because of potential overestimation of effects (Freund & Isaacowitz, 2013). Since inferences about causal effects are tentative until the research is successfully replicated (Tabachnick & Fidell, 2007), experiments and interventions that strive to both replicate and extend prior work seem particularly important.

When *designing their study*, researchers could also think of assessments that go beyond self- and other-reports (Gerpott et al., 2020; see also Chapter 8). Furthermore, many scholars have highlighted that experimental stimuli should be as realistic and as externally valid as possible (Freund & Isaacowitz, 2013; Highhouse, 2009). Overly long or tedious tasks (e.g., in within-person designs with many different scenarios that differ only slightly) should be avoided as well; if such tasks are necessary to answer a specific research question, they should at least be tested and adapted in a pilot study.

When designing an invention, we suggest thinking in advance of potential (additional) effects that are not apparent or desirable. Researchers should bear in mind that interventions can have unintended or negative effects despite positive intentions (Aust et al., 2010; Kulik et al., 2000). Although interventions are often time-consuming and resource-intensive, we want to note that there are also small, scalable, and psychologically rich interventions that can have relatively far-reaching benefits (Truxillo et al., 2015; Walton & Yeager, 2020).

As to *data analysis*, researchers have traditionally used GLM approaches (e.g., ANOVA) to analyze data from experiments or interventions (Breitsohl, 2019). When it comes to within-subjects designs (e.g., when ratings of scenarios are nested within participants), current studies have increasingly relied on multilevel and thus GLMM approaches instead of applying ANOVA with repeated measures (for examples, see Gärtner & Hertel, 2017; Zacher et al., 2017). For using structural equation modeling (SEM) to analyze data from experimental designs, a helpful guide is offered by Breitsohl (2019), who points out several advantages of the SEM versus the ANOVA approach, such as control of measurement error, accounting for unequal variances across groups, or the calculation of model fit indices.

In conclusion, we want to emphasize that the field will benefit most from "the right mix" of different research designs and methods to understand aging in the context of work (see also other chapters from Section III in this book). Correlational and descriptive work should be complemented with more (quasi-) experiments and interventions to properly test causal pathways or boundary conditions for age-related effects on work behaviors and outcomes. Whenever we find that age differences disappear after experimental/interventional manipulation (e.g., when the external work context is changed in certain ways or when imagined time perspectives are equated; Carstensen, 2006), researchers have probably tapped into mechanisms that influence age-related differences and changes. Moreover, by engaging in intervention research, despite the high effort and risky return on investment, work and age scholarship will boost its relevance for and impact on nonacademic communities.

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