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Surveying the forest: A meta-analysis, moderator investigation, and future-oriented discussion of the antecedents of voluntary employee turnover

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Abstract

Recent narrative reviews (e.g., Hom, Mitchell, Lee, and Griffeth, 2012; Hom, Lee, Shaw, and Hausknecht, 2017) advise that it is timely to assess the progress made in research on voluntary employee turnover so as to guide future work. To provide this assessment, we employed a three-step approach. First, we conducted a comprehensive meta-analysis of turnover predictors, updating existing effect sizes and examining multiple new antecedents. Second, guided by theory, we developed and tested a set of substantive moderators, considering factors that might exacerbate or mitigate zero-order meta-analytic effects. Third, we examined the holistic pattern of results in order to highlight the most pressing needs for future turnover research. The results of Step 1 reveal multiple newer predictors and updated effect sizes of more traditional predictors, which have received substantially greater study. The results of Step 2 provide insight into the context-dependent nature of many antecedent–turnover relationships. In Step 3, our discussion takes a bird's-eye view of the turnover "forest" and considers the theoretical and practical implications of the results. We offer several research recommendations that break from the traditional turnover paradigm, as a means of guiding future study.

The question of why employees voluntarily leave their jobs has captivated researchers for 100 years (Hom, Lee, Shaw, & Hausknecht, 2017). Given its impact on organizations' functioning and survival, it is no surprise that research aimed at understanding employee turnover remains an important topic for academics and practitioners (Holtom, Mitchell, Lee, & Eberly, 2008; Hom, Mitchell, Lee, & Griffeth, 2012). Turnover is costly: Recent data show that organizations incur costs often upwards of 200% of an employee's annual pay to recruit, select, and train successors (Allen, Bryant, & Vardaman, 2010). Less tangible costs are also noteworthy, including loss of tacit knowledge and social capital (Dess & Shaw, 2001), reduced customer satisfaction (McElroy, Morrow, & Rude, 2001), or turnover contagion (Felps et al., 2009).

Nearly two decades have passed since Griffeth, Hom, and Gaertner (2000) conducted the broadest meta-analysis of the turnover literature. Since then, narrower meta-analyses have sharpened our empirical understanding of turnover processes via more isolated snapshots of select precursors or outcomes (e.g., Berry, Lelchook, & Clark, 2012; Hancock, Allen, Bosco, McDaniel, & Pierce, 2013; Jiang, Liu, McKay, Lee, & Mitchell, 2012), thereby offering some clarity in predicting what has proved an elusive behavior. Since the turn of the millennium, however, a sizable number of primary studies have been published, also seeking to understand this phenomenon. Some effect sizes are out of date, whereas other antecedent–turnover relationships have yet to be meta-analyzed. Moreover, given the large amount of heterogeneity existing around many effect size estimates, it is also worthwhile to scrutinize moderators that would account for this variability.

The purpose of this paper is to present an updated and holistic picture of how oft-studied constructs operate within the turnover literature. Our focus is on individual voluntary turnover, defined as "voluntary cessation of membership in an organization, by an individual who receives monetary compensation for participation in that organization" (Hom & Griffeth, 1995, p. 5).[1] With the abundance of theoretical and empirical work advanced in recent years, it is prudent to assess the progress that has been made in describing, explaining, and predicting turnover.

In a recent narrative review of the literature, Holtom et al. (2008) tracked the nomological network development of turnover research since March and Simon (1958) introduced the first formal turnover theory, focusing mainly on the desire to leave (i.e., job satisfaction) and the ease of leaving (i.e., job alternatives) as the primary reasons why people quit. Almost 60 years have passed since this seminal theoretical work—and a century since the first empirical (mostly atheoretical) turnover articles surfaced (Hom et al., 2017)—such that the number of turnover predictors has dramatically increased in this time. Based on an extensive review of the literature, Holtom et al. (2008) acknowledged a total of 50 broad antecedents as having scientific value, of which some (like personality and role states) subsume even more variables. This sheer number of antecedents highlights the myriad perspectives by which researchers have studied turnover but also raises questions regarding which constructs have made the greatest impact. As such, it is timely, if not necessary, to assess what progress has been made since the first empirical tests of March and Simon emerged in the mid-1970s.

Our holistic effort begins with an updated meta-analytic empirical assessment of turnover research to assess main effect relationships (Step 1). Since Griffeth et al. (2000), a bevy of new constructs have entered into the academic vernacular, whereas other constructs have been studied in considerably more depth, perhaps warranting a revision of earlier estimates. As a point of illustration, whereas the Griffeth et al. analysis examined 45 predictors and 843 effect sizes, we include 57 predictors across 1,800 effect sizes (a 27% increase in constructs and a 114% increase in effects). We provide insight into new and influential predictors such as engagement, justice, and job characteristics, as well as examining potential changes in effect sizes compared to previous work.

Along with this initial meta-analysis, we developed and tested a set of substantive, a priori moderation hypotheses, grounded in theory, to account for variability in predictor–turnover effects (Step 2). Due to the context-driven nature of the turnover process, this section highlights those higher level factors (at the sample or economy level) that exacerbate or mitigate antecedent–turnover effects. Whereas some antecedents might not be strong predictors of turnover in a zero-order sense, it is possible that they become more or less impactful after accounting for the context in which they are embedded.

Third, because so many predictors have been tested in this literature, it is timely to highlight the major developments visible in our results and to draw conclusions for future work. We integrate the results of our first two steps to consider trends such as what variables appear to be predictive across a variety of contexts, what variables are not predictive across contexts, what variables appear highly context driven, and what variables still require more research. Humphrey (2011) asserted that every meta-analysis should answer the question, "Where are we going?" by providing a substantive analysis of one's results and what they mean. We concur, and conclude with a constructive discussion with eyes looking toward the most promising constructs and methods, along with specific recommendations to guide future turnover study and practice. In short, we seek parsimony and improved focus for this very large literature.

1 | STEP 1: INITIAL META-ANALYSIS

To begin our systematic exploration, we first conducted a zero-order meta-analysis to determine correlational relationships of multiple antecedents with turnover. For reasons of completeness, we coded all turnover antecedents examined in previous studies.

1.1 | Method

1.1.1 | Literature search and inclusion criteria

We first identified published empirical articles that examined any turnover antecedents. The articles were identified through an online search of the PsycINFO, EBSCO, JSTOR, and Google Scholar databases, as well as searching conference proceedings from the Academy of Management and Society for Industrial and Organizational Psychology annual meetings. We restricted our search to the time frame of 1975 through July 2016, because it was in the mid-1970s when the first theory-based, empirical studies on turnover emerged (Hom et al., 2017). We reviewed the abstracts from this initial search and eliminated studies of a theoretical nature (e.g., literature reviews), studies that only focused on employee involuntary turnover, studies that treated employee voluntary turnover as an independent variable, previous meta-analyses, and studies that treated "turnover" as withdrawal cognitions or attitudes (i.e., turnover intentions as the criterion). To be included in the meta-analysis, the study had to report sufficient data to calculate an effect size, had to use a design such that it measured predictors before turnover took place, and had to measure individual turnover behavior. In cases where there were not sufficient results reported to calculate an effect size, we reached out to one of the study authors to request information. These criteria led to a final primary study population of 316 articles.

1.1.2 | Coding procedures

In a first step, the first two authors extracted all information according to the coding scheme from the articles. Each author coded approximately half of the total set of manuscripts. In a second step, we grouped the various independent variables according to their conceptual overlap. In conducting this meta-analysis, we attempted to be as comprehensive as possible in capturing all relevant and empirically investigated antecedents. At the same time, we recognized the need to reduce the abundance of data to a manageable and interpretable level. For example, although there is undoubtedly much to be learned by examining the relationships among dimensions of commitment (i.e., affective, normative, and continuance) or fit (i.e., person-organization, person-job, person-vocation), we opted for parsimony over specificity and combined dimensions to represent overarching constructs. For instance, leadership was aggregated using its most common positive indicators: measures of leader–member exchange, consideration, and transformational leader style. Further, we do not report effects for any variables for which there were $k \leq 3$ studies (this included 14 variables, such as shocks, emotional labor strategies, and subjective norms). Throughout the coding process, we engaged in an iterative process and continued discussion to identify and resolve other trade-offs between parsimony and specificity. This resulted in a final set of 57 predictors. In order to verify agreement in coding, the first two authors independently coded a random sample of 10% of the others' articles. We found minimal differences, resolved by checking the original manuscripts and then collaboratively discussing disagreements.

1.1.3 | Meta-analytic procedures

Following Hunter and Schmidt (2004), we used psychometric procedures for a random effects meta-analytic model, in which we corrected for attenuation due to unreliability as well as for sampling error by weighting each study's effect size by its sample size. Random effects models are preferred over fixed effect models when the relationships tested in primary studies are presumed to be heterogeneous across studies, which is typically the case in turnover studies (Borenstein, Hedges, Higgins, & Rothstein, 2010). We computed composite correlations for those studies that included multiple measures of the same construct. Studies that included multiple independent samples were coded separately. We used Cronbach's α to correct for measurement error in the predictors. These values were provided in the majority

of the studies. However, when studies did not report this reliability coefficient, we used means across other studies. Reliabilities for objective variables, such as for employee age, pay, and turnover, were set to 1.00. With additional corrections for artifactual variance due to sampling error, we then performed random-effects meta-analyses based on the average sample-size weighted correlations and corrected correlations. We also report Cochran's Q statistic and the I^2 index. The former statistic is an absolute measure of the absence or presence of significant heterogeneity, whereas the latter statistic considers the percentage of variance attributable to heterogeneity in a predictor–turnover relationship, which, as a percentage, is advantageous for comparing across meta-analyses of different study sizes (Higgins, Thompson, Deeks, & Altman, 2003). Values of I^2 between 0–40% represent minor heterogeneity, 40–70% medium heterogeneity, and 70–100% substantial heterogeneity (Borenstein, Hedges, Higgins, & Rothstein, 2009).

Following previous research, relationships involving turnover behavior were corrected based on the nature of a point-biserial correlation including a dichotomous criterion. Such correlations may be downwardly biased if they violate assumptions of bivariate normality due to an uneven split of stayers and leavers (Kemery, Dunlap, & Griffeth, 1988). Hunter and Schmidt (2004) provide a correction formula, which requires coding for the mean turnover base rate across samples in the meta-analysis. For this procedure, where turnover rate data were unavailable (∼4% of studies), we used the average rate across studies in the most closely related industry (using standard industrial classification codes) from which the sample was obtained.

Across the primary studies coded, the mean sample size across studies was 1,053, mean base rate of turnover was 23.5% ($SD = 14.6\%$), mean time lag between predictor and turnover measurement was roughly 16 months (mode = 12 months), mean response rate was 67.9% ($SD = 23.6\%$), grand mean employee age was 35.3 ($SD = 6.7$), grand mean sex distribution was 53.3% male ($SD = 24.0\%$), and grand mean organizational tenure was 6.5 years employed ($SD = 4.0$). Among countries represented, a large majority (78.9%) came from the United States. Among occupations, 25.9% were involved with some sort of hospital work, nursing, or healthcare services; 12.6% were involved with banking or other financial services; 12.3% performed what would be considered "blue collar" work (e.g., manufacturing, food processing); 9.1% were military; the remaining studies represented smaller occupational percentages.

1.2 | Results and discussion

To facilitate interpretation of the results, we organized the 57 predictors by labels based on Holtom et al. (2008). Definitions and sample studies for each predictor are provided in Table 1, whereas Table 2 shows the results for each predictor. Although readers can examine the complete results in Table 2, there are two aspects of the results on which we focus here: (a) what effects have significantly changed compared to what Griffeth et al. (2000) reported and (b) what predictors are new and noteworthy, given increased empirical study in recent years.

To compare our effect sizes with those reported by Griffeth et al. (2000) and determine which antecedent–turnover relationships have significantly changed, we could not perform independent samples comparisons because the correlations we obtained were dependent with theirs (i.e., all antecedent–turnover samples we coded were partially redundant with theirs). Their study also did not report confidence intervals, so we were also unable to directly compare nonoverlapping estimates. If samples are independent and confidence intervals are available, these approaches would be recommended to test for significant effect differences (Steiger, 1980; Zou, 2007). To circumvent this issue, we used three alternative approaches, which we list in order of greater to lesser objectivity. Ultimately, no approach is perfect, so we suggest a combination be used based on data availability rather than relying on any single method alone: First, we reanalyzed all correlations in our data using only estimates from 2000 to 2016 (i.e., after the Griffeth et al. article) and examined whether those values—which are independent—were significantly different. To compare estimates, we followed the modified asymptotic method of correlational differences, outlined in Zou (2007, equation 15). This approach invokes Fisher's r-to-z transformation to compute a confidence interval for the difference in correlations. To test for significance, we used the mean study sample size, excluding outliers ($N = 1,053$). Second, we compared whether the Griffeth et al. estimates were different than those of this study by comparing 95% confidence intervals of their effects to our own. This is an approach widely used in meta-analyses to compare effect size differences between or among groups/subgroups. However, since Griffeth et al. did not report standard errors, we input those obtained for

TABLE 1 Definitions of turnover antecedents

Variable name	Definition/Clarification	Sample studies
(1) Individual attributes		
Abilities and skills	Proficiency in doing something, whether learned or innate. In this context, it refers to abilities or skills that are relevant to the job (e.g., cognitive ability, managerial skills).	Kraimer, Seibert, Wayne, Liden, and Bravo (2011); Trevor 2001
Age	Chronological number of years an individual has lived.	Stumpf and Dawley (1981)
Agreeableness	Personality trait capturing the extent to which individuals are courteous, trusting, cooperative, and empathic. Part of the five-factor model of personality (the Big Five, McCrae & Costa, 1987).	Barrick and Mount (1996); Timmerman (2006)
Children	Number of children an individual reports having, primarily, the number of children living at home (dependents).	Blegen, Mueller, and Price (1988); Lee and Maurer 1999
Conscientiousness	Personality trait capturing the extent to which individuals are dependable, organized, persistent, and achievement-oriented. Part of the five-factor model of personality (the Big Five, McCrae & Costa, 1987).	Barrick and Zimmerman (2009); Timmerman (2006)
Education	An individual's maximum level of education attained.	Bretz, Boudreau, and Judge (1994)
Emotional stability	Personality trait capturing the extent to which individuals are calm, secure, and not moody; part of the five-factor model of personality (the Big Five, McCrae & Costa, 1987).	Barrick and Zimmerman (2009); Cavanaugh, Boswell, Roehling, and Boudreau (2000)
Ethnicity/race	An individual's identification as being 0 = White or 1 = non-White. Often, studies do not disclose how this information is assessed. It is possible our dataset includes studies that inquired about biological distinctions (race), whereas others inquired about cultural distinctions (ethnicity).	Elvira and Cohen (2001); Zatzick, Elvira, and Cohen (2003)
Extraversion	Personality trait capturing the extent to which individuals are sociable, talkative, active, and ambitious (Barrick & Mount, 1996); part of the five-factor model of personality (the Big Five, McCrae & Costa, 1987).	Cavanaugh, Boswell, Roehling, and Boudreau (2000); Liao and Joshi (2008)
Internal motivation	Motivation based on internal factors such as enjoyment of the task, a sense of confidence, or a normative belief in the importance of work in general.	Iverson (1999); Mirvis and Lawler (1977)
Locus of control (higher = external)	Personality trait capturing the extent to which an individual attributes the cause of personal events—whether successes or failures—either to the self (internal locus of control) or to the environment (external locus of control).	Allen, Weeks, and Moffitt 2005; Blau (1987)
Marital status	Whether an individual is currently married (0 = nonmarried, 1 = married).	Hom and Hulin (1981)

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TABLE 1 (Continued)

Variable name	Definition/Clarification	Sample studies
Openness to experience	Personality trait capturing the extent to which individuals are imaginative, cultured, broad-minded, and flexible (Barrick & Mount, 1996); part of the five-factor model of personality (the *Big Five*, McCrae & Costa, 1987).	Barrick and Mount (1996); Timmerman (2006)
Sex	Biologically based categories of 0 = female and 1 = male.	Lyness and Judiesch (2001)
Tenure	Time employed with one's current organization (measured in years).	Benson, Finegold, and Mohrman (2004)
(2) Aspects of the job		
Instrumental communication	Degree to which organizations formally transmit information to employees about their job.	Blegen, Mueller, and Price (1988); Price and Mueller 1981
Job characteristics (VISAF)	Composite including five core features of Hackman and Oldham's (1976) job characteristics model: variety (degree to which the job offers different proficiencies), task identity (degree to which the job provides tasks with visible outcomes), task significance (degree to which the job is important for other people), autonomy (degree to which the job provides discretion in how to do the work), and feedback (degree to which the job provides specific and actionable information about one's performance/results).	Colarelli, Dean, and Konstans (1987); Schmidt and Daume (1993)
Job security	Degree to which one is confident about having stable present or future employment in their current job.	Arnold and Feldman (1982); Iverson and Pullman (2000)
Participation	Degree to which an individual can exercise power, has opportunity for input, and can make decisions on the job (Price & Mueller, 1981).	Allen, Shore, and Griffeth (2003); Price and Mueller 1981
Pay	Amount of money an individual receives for the job. We include all basic monetary compensation such as base salary and commission.	Motowidlo (1983); Tekleab, Bartol, and Lui (2005)
Role ambiguity	Degree to which role expectations are unclear.	Spector (1991)
Role conflict	Degree to which employee role expectations are conflicting or incompatible.	Iverson (1999); Spector (1991)
Routinization	Degree to which the job is repetitive and is subject to rules and regulations (Price & Mueller, 1981).	Martin (1980); Parasuraman and Alutto (1984)
Task complexity	Degree to which the job is intricate and has many cognitive requirements.	Parasuraman and Alutto (1984)

(Continues)

TABLE 1 (Continued)

Variable name	Definition/Clarification	Sample studies
Workload	Amount of work required by the individual; includes mostly measures of hours worked or how hard and fast an individual works.	Huffman, Adler, Dolan, and Castro (2005); Spector (1991)
(3) Traditional job attitudes		
Job involvement	Degree to which an individual identifies with his or her job.	Hollenbeck and Williams (1986)
Job satisfaction	Degree to which an individual likes his or her job, measured globally or via the job's different facets (e.g., pay, the work itself, peers, etc.).	Boswell, Boudreau, and Tichy (2005); Judge (1993)
Organizational commitment	Degree to which an individual experiences loyalty to the organization and desires to stay. Includes all three facets of organizational commitment: affective (a desire to stay), normative (an obligation to stay), and continuance (a need to stay) commitment (Meyer & Allen, 1991)	Lee, Ashford, Walsh, and Mowday 1992; Vandenberghe and Bentein (2009)
Other commitment	Degree to which an individual experiences loyalty to targets other than the organization, such as his or her occupation or career.	Blau (1989); Cohen (2000)
Other satisfaction	Degree to which an individual likes other aspects relevant to employment, such as his or her career or life in general.	Boswell, Boudreau, and Dunford 2004
(4) Newer personal conditions		
Coping	An individual's abilities to manage internal and external demands that are perceived as exceeding available resources.	Riolli and Savicki (2006); Wright and Bonett (1993)
Engagement	Degree to which an individual invests their entire self into their work role; their dedication, vigor, and devotion toward work.	Owens, Johnson, and Mitchell (2010); Spell, Eby, and Vandenberg (2014)
Stress/exhaustion	Various well-being-related variables such as strain (an aversive psychological reaction to stressful work), burnout (a state where emotional resources are depleted and can no longer be recovered), tension (being bothered by stressful work incidents), and emotional exhaustion (the experience of being emotionally depleted).	de Croon, Sluiter, Blonk, Broersen, and Frings-Dresen (2004); Riolli and Savicki (2006); Sheridan and Vredenburgh (1978)
(5) Organizational context		
Centralization	Degree to which power is concentrated and held by few individuals in an organization at higher hierarchical levels.	Bluedorn (1982); Martin (1980)

(Continues)

TABLE 1 (Continued)

Variable name	Definition/Clarification	Sample studies
Climate	The shared experiences, perceptions, and behavioral tendencies among a group of employees. A higher score denotes a more positive climate.	Spreitzer and Mishra (2002); Suszko and Breaugh (1986)
Organization prestige	Degree to which an organization is well regarded or is perceived as being high-status (Mael & Ashforth, 1992).	Joseph, Ang, and Slaughter (2015); Ramesh and Gelfand (2010)
Organization size	Number of employees of a focal organization.	Elvira and Cohen (2001)
Organization support	Degree to which an individual believes that the organization values their contributions and cares about their well-being (Eisenberger, Huntington, Hutchison, & Sowa, 1986).	Allen, Shore, and Griffeth (2003); Rhoades, Eisenberger, and Armeli (2001)
Reward contingency	Degree to which compensation is based on an individual's performance.	Allen and Griffeth (2001); Williams (1999)
Rewards offered	Rewards provided to employees beyond pay. Includes benefits, career/growth opportunities, and training time.	Kraimer, Seibert, Wayne, Liden, and Bravo (2011)
(6) Person–context interface		
Fit	"Compatibility between an individual and a work environment that occurs when their characteristics are well matched" (Kristof-Brown, Zimmerman, & Johnson, 2005, p. 281). Includes various facets, but most prominently coded as person-organization fit and person-job fit.	Cable and DeRue (2002); Ramesh and Gelfand (2010)
Influence	The effect an individual can have on another person, for example, by persuasion or informal leadership.	Buchko (1992); Clausen and Borg (2010)
Job embeddedness	A broad constellation of influences on why an individual stays within their job. An individual's "stuckness" within a larger social system as a function of external forces within the organization (on-the-job) and the community (off-the-job). Encompasses links (connections to other people and activities), fit (environmental compatible with the individual's values and needs), and sacrifice (what an individual would give up by quitting).	Harris, Wheeler, and Kacmar (2011); Mitchell, Holtom, Lee, Sablynski, and Erez 2001
Justice	Experience of fairness within one's work. Includes all facets of justice: distributive (fairness of outcomes), procedural (fairness of processes used to arrive at outcomes), informational (fairness of information provided), and interpersonal (fairness of interpersonal treatment received) justice (Colquitt, Conlon, Wesson, Porter, & Ng, 2001).	Erdogan and Bauer (2010); Spreitzer and Mishra (2002)

(Continues)

TABLE 1 (Continued)

Variable name	Definition/Clarification	Sample studies
Leadership	"A process whereby intentional influence is exerted by one person over other people to guide, structure, facilitate activities and relationships in a group or organization" (Yukl, 2002). Primarily measured with leadership styles (e.g., transformational leadership) and quality of leader–follower relationship (e.g., leader–member exchange). A higher score denotes more positive leadership.	Graen, Liden, and Hoel (1982); Tse, Huang, and Lam (2013)
Met expectations	Degree to which what an individual encounters at work is in line with the expectations they had for such experiences (Porter & Steers, 1973).	Hom, Griffeth, Palich, and Bracker (1998)
Peer/group relations	Degree to which an individual reports positive interpersonal experiences with coworkers or group members. Most often measured by coworker support but also with feelings of cohesion or social integration.	Price and Mueller 1981; Sheridan (1985)
Psychological contract breach	An individual's cognition that the employer has failed to meet one or more obligations of the employment relationship (Morrison & Robinson, 1997).	Bunderson (2001); Karagonlar, Eisenberger, and Aselage (2016)
Work–life conflict	Degree to which one's work role interferes with nonwork roles, and vice versa.	Hom and Kinicki (2001); Huffman, Casper, and Payne (2013)
(7) External job market		
Alternatives	Availability of employment opportunities. Includes subjective (e.g., perceived alternatives) and objective (e.g., unemployment rate) measures.	Boswell, Boudreau, and Dunford 2004; Trevor 2001

(Continues)

TABLE 1 (Continued)

Variable name	Definition/Clarification	Sample studies
(8) Attitudinal withdrawal		
Withdrawal cognitions	Thoughts about leaving or related withdrawal attitudes. Encompasses turnover intentions, thoughts of quitting, search intentions, and expected utility of withdrawal (Hom, Caranikas-Walker, Prussia, & Griffeth, 1992).	Allen, Weeks, and Moffitt 2005; Griffeth, Steel, Allen, and Bryan (2005)
(9) Employee behaviors		
Selection process performance	How an individual has performed on a given task during the selection process (i.e., on prehire performance tasks or an interview).	Hausknecht, Trevor, and Farr (2002); Kinicki, Lockwood, Hom, and Griffeth (1990)
Job search	Behavioral methods to general alternative employment opportunities.	Bretz, Boudreau, and Judge (1994); Linnehan and Blau (2003)
Absenteeism	Missing from work entirely when one is expected to be in attendance.	McElroy, Morrow, and Fenton (1995); Rosse 1988
Lateness	Arriving to work at a time beyond expected by the employer.	Blau (1994); Clegg (1983)
Employee performance	Activities that contribute to the technical core of the organization, often prescribed by an employee's job description.	Jackofsky, Ferris, and Breckenridge (1986)
Organizational citizenship behaviors (OCBS)	Individual discretionary actions that are not explicitly recognized by an organization's reward system, but that promote organizational effectiveness in the broader psychological and social environment.	Chen, Hui, and Sego (1998); Lee, Mitchell, Sablynski, Burton, and Holtom (2004)

TABLE 2 Meta-analytic predictors of voluntary turnover

Variable name	k	N	r̄ Point-biserial	$\hat{\rho}$	$SD_{\hat{\rho}}$	90% Credibility interval (CV) [LB, UB]	$SE_{\hat{\rho}}$	95% Confidence interval (CI) [LB, UB]	Q-Statistic	I^2 Index (%)
(1) Individual attributes										
Abilities and skills	15	17,651	−.06	−.06	.11	[−.20, .08]	.03	[−.12, .00]	217.38**	93.56
Age	121	209,588	−.21	−.21	.13	[−.39, −.04]	.01	[−.24, −.19]	4,247.00**	97.17
Agreeableness	6	2,449	−.07	−.08	.08	[−.18, .02]	.04	[−.15, .00]	14.78	66.17
Children	25	40,201	−.19	−.20	.11	[−.34, −.05]	.02	[−.24, −.15]	458.33**	94.76
Conscientiousness	8	3,409	−.15	−.16	.14	[−.34, −.03]	.05	[−.26, −.05]	78.83**	91.12
Education	51	59,574	.04	.04	.17	[−.18, .26]	.02	[−.01, .08]	1,105.78**	95.66
Emotional stability	16	7,593	−.17	−.19	.21	[−.45, .08]	.05	[−.30, −.08]	490.23**	96.94
Ethnicity/race (0 = White, 1 = non-White)	29	457,562	.02	.02	.03	[−.01, .06]	.01	[.01, .03]	494.06**	94.33
Ethnicity/race not including Hom et al. 2008 outlier	28	53,510	.02	.02	.09	[−.10, .14]	.02	[−.02, .05]	492.57**	94.52
Extraversion	13	6,795	.02	.02	.07	[−.07, .11]	.02	[−.03, .06]	38.01**	68.43
Internal motivation	17	5,960	−.13	−.16	.16	[−.36, .05]	.04	[−.24, −.07]	150.28**	89.35
Locus of control (higher=external)	13	3,187	.08	.10	.13	[−.07, .27]	.04	[.02, .18]	56.57**	78.79
Marital status (0 = nonmarried, 1 = married)	27	134,505	−.10	−.10	.09	[−.21, .02]	.02	[−.13, −.06]	1,099.32**	97.63
Openness to experience	5	1,009	.13	.14	.12	[−.01, .29]	.06	[.02, .26]	16.08**	75.12
Sex (0 = female, 1 = male)	89	602,869	.00	.00	.12	[−.15, .16]	.01	[−.02, .03]	2,078.58**	95.77
Sex not including Hom et al. 2008 outlier	88	198,817	−.01	−.01	.07	[−.09, .08]	.01	[−.02, .01]	1,066.54**	91.84
Tenure	118	669,753	−.20	−.20	.10	[−.33, −.07]	.01	[−.21, −.18]	7,652.76**	98.45
Tenure not including Hom et al. 2008 outlier	117	194,295	−.27	−.27	.17	[−.48, −.05]	.02	[−.30, −.23]	6,659.12**	98.23

(Continues)

TABLE 2 (Continued)

Variable name	k	N	$\bar{r}$	$\hat{\rho}$	$SD_{\hat{\rho}}$	90% CV	$SE_{\hat{\rho}}$	95% CI	Q-Statistic	I^2 Index (%)
(2) Aspects of the job										
Instrumental communication	8	5,185	−.14	−.14	.03	[−.25, −.04]	.02	[−.17, −.10]	33.46**	36.78
Job characteristics (VISAF)	16	12,869	−.16	−.18	.12	[−.33, −.02]	.03	[−.23, −.11]	74.94**	79.98
Job security	5	2,155	−.21	−.23	.06	[−.32, −.14]	.04	[−.30, −.16]	12.80**	68.75
Participation	5	1,895	−.11	−.13	.01	[−.14, −.11]	.03	[−.17, −.08]	5.27*	24.10
Pay	55	177,634	−.17	−.17	.07	[−.26, −.08]	.01	[−.19, −.15]	1,043.22**	94.82
Role ambiguity	8	5,765	.14	.15	.11	[.02, .29]	.04	[.07, .23]	59.38**	88.21
Role conflict	10	10,903	.13	.15	.09	[.04, .27]	.03	[.10, .21]	77.19**	88.34
Routinization	6	4,106	−.10	−.12	.09	[−.23, .00]	.04	[−.20, −.04]	30.25**	83.47
Task complexity	10	3,117	−.01	−.01	.10	[−.14, .12]	.04	[−.08, .06]	35.53**	74.67
Workload	21	82,204	−.10	−.10	.07	[−.18, −.01]	.02	[−.13, −.07]	330.19**	93.94
(3) Traditional job attitudes										
Job involvement	19	5,158	−.17	−.19	.15	[−.38, −.01]	.04	[−.26, −.12]	84.62**	78.73
Job satisfaction	174	107,625	−.25	−.28	.18	[−.52, −.05]	.01	[−.31, −.26]	2,046.12**	91.54
Organizational commitment	129	71,862	−.26	−.29	.14	[−.47, −.11]	.01	[−.31, −.26]	1,365.33**	90.62
Other commitment	12	3,601	−.30	−.34	.17	[−.55, −.12]	.05	[−.44, −.24]	95.38**	88.47
Other satisfaction	16	14,811	−.38	−.43	.22	[−.71, −.15]	.05	[−.54, −.32]	777.93***	98.07
(4) Newer personal conditions										
Coping	7	880	−.32	−.39	.24	[−.69, −.08]	.09	[−.57, −.20]	48.82**	87.71
Engagement	4	1,408	−.19	−.20	.04	[−.24, −.15]	.03	[−.26, −.14]	5.60*	46.43
Stress/exhaustion	32	18,740	.20	.21	.15	[.03, .40]	.03	[.17, .26]	384.85**	91.94

(Continues)

TABLE 2 (Continued)

Variable name	k	N	$\bar{r}$	$\hat{\rho}$	$SD_{\hat{\rho}}$	90% CV	$SE_{\hat{\rho}}$	95% CI	Q-Statistic	I^2 Index (%)
(5) Organizational context										
Centralization	6	4,128	−.06	−.06	.15	[−.26,.14]	.07	[−.19,.07]	94.41**	94.70
Climate	8	2,711	−.21	−.24	.18	[−.47,−.01]	.06	[−.33,−.09]	78.73**	91.11
Organization prestige	5	2,433	−.05	−.06	.13	[−.23,.11]	.07	[−.18,.06]	41.13**	90.27
Organization size	15	30,422	.03	.03	.13	[−.14,.20]	.03	[−.04,.10]	531.27**	97.36
Organization support	16	8,256	−.19	−.19	.18	[−.43,.04]	.04	[−.28,−.10]	291.81**	94.86
Reward contingency	4	678	−.17	−.20	.21	[−.40,.06]	.11	[−.42,.03]	26.93**	88.86
Rewards offered	25	30,743	−.28	−.28	.15	[−.48,−.09]	.03	[−.35,−.22]	754.02**	96.89
(6) Person−context interface										
Fit	17	4,146	−.25	−.29	.25	[−.60,.03]	.06	[−.41,−.17]	229.91**	93.04
Influence	7	24,331	−.08	−.09	.05	[−.15,−.02]	.02	[−.13,−.04]	71.88**	91.65
Job embeddedness	29	31,158	−.25	−.26	.09	[−.39,−.14]	.02	[−.30,−.22]	309.89**	90.96
Justice	30	17,556	−.15	−.17	.10	[−.30,−.05]	.02	[−.21,−.13]	151.30**	80.83
Leadership	42	28,637	−.23	−.24	.16	[−.45,−.03]	.03	[−.29,−.19]	748.09**	94.52
Met expectations	11	3,236	−.10	−.12	.23	[−.42,.17]	.07	[−.26,.02]	118.59**	91.57
Peer/group relations	24	11,104	−.13	−.14	.13	[−.30,.03]	.03	[−.19,−.08]	126.36**	81.80
Psychological contract breach	7	8,083	.17	.18	.06	[.11,.25]	.02	[.13,.22]	29.30*	79.52

(Continues)

TABLE 2 (Continued)

Variable name	k	N	$\bar{r}$	$\hat{\rho}$	$SD_{\hat{\rho}}$	90% CV	$SE_{\hat{\rho}}$	95% CI	Q-Statistic	I^2 Index (%)
Work–life conflict	7	12,107	.16	.19	.06	[.11, .27]	.03	[.14, .24]	45.73**	86.88
(7) External job market										
Alternatives	79	58,512	.20	.23	.19	[−.01, .48]	.02	[.19, .27]	1210.91**	93.56
(8) Attitudinal withdrawal[a]										
Withdrawal cognitions	211	73,405	.50	.56	.27	[.20, .90]	.02	[.52, .59]	8,004.92**	97.38
(9) Employee behaviors										
Absenteeism	36	44,405	.23	.23	.19	[−.02, .48]	.03	[.16, .29]	766.22**	95.45
Employee performance	86	473,624	−.07	−.08	.11	[−.23, .06]	.01	[−.11, −.06]	4,746.24**	98.21
Employee performance not including Hom et al. 2008 outlier	85	111,562	−.17	−.21	.19	[−.45, .03]	.02	[−.25, −.17]	3,200.19**	97.38
Job search	27	18,685	.38	.40	.14	[.22, .58]	.03	[.35, .46]	410.92**	93.67
Lateness	5	1,431	.14	.14	.06	[.06, .22]	.04	[.07, .22]	10.87**	63.20
OCBs	9	6,047	−.09	−.10	.05	[−.16, −.02]	.02	[−.12, −.07]	19.79**	72.91
Selection process performance	4	3,016	−.10	−.11	.03	[−.14, −.06]	.02	[−.16, −.06]	6.20	51.61

Note. * p < .05, ** p < .01.
[a]Withdrawal cognitions are a weighted aggregate of search intentions, expected utility of withdrawal, withdrawal cognitions, and withdrawal intentions.

each construct in our study to create a 95% confidence interval of their effects to thereby allow for comparison. Third, we statistically compared the effect size differences between Griffeth et al. and our own as if they *were* independent, again using Zou's (2007) formula. This third approach effectively allows one to compare updated correlations to older estimates as a means of assessing effect size stability based on increased study, including all available data. Invoking all comparisons together, we deemed that if at least two of three approaches yielded significant differences, then the meta-analytic correlations were considered reliably different.

1.2.1 | Individual attribute predictors

Among individual attributes, *tenure* ($\rho = -.27$, outlier excluded) *age* ($\rho = -.21$), *children* ($\rho = -.20$), *emotional stability* ($\rho = -.19$), and *conscientiousness* ($\rho = -.16$) demonstrate the strongest effects. Perhaps more important; however, age validities significantly differed compared to the Griffeth et al. (2000) analysis (hereafter, GHG: $-.11$, here: $-.21$), as did the effect size for abilities/skills (GHG: .02, here: $-.06$). Implications of this larger age effect in particular (i.e., also more negative for post-2000 compared to pre-2000 studies), merit comment. If older workers are less likely to quit, younger workers are, equally, more likely to quit. Some scholars (e.g., Bal & Jansen, 2016) find support for the idea that younger workers hold higher—perhaps even unrealistic— expectations than do older workers regarding what they want from their employers. Looking forward, researchers might monitor this trend, and if/how the broader definitions of careers and work relationships change, and what that means for theory and practice.

Within this category there are also newly meta-analyzed constructs. Specifically, we find that those individuals with a more internal *locus of control* and those more *internally motivated*, are less likely to quit ($\rho = -.10$ and $\rho = -.16$, respectively). In face of stressful job demands or performance setbacks that inevitably occur during work life, such individuals appear more likely to persevere and try to overcome such obstacles rather than quit. These variables are some of the stronger individual difference predictors, and could be promising selection tools.

1.2.2 | Predictors reflecting aspects of the job

Within this category are many newly estimated relationships, such as *job characteristics* ($\rho = -.18$), *job security* ($\rho = -.23$), *task complexity* ($\rho = -.01$), and *workload* ($\rho = -.10$). Many of these effects generate interesting thoughts for future research. For instance, the moderate negative relationship for job characteristics may indicate that managers can make active efforts to reduce an individual's turnover likelihood rather than assuming such decisions are made purely on the basis of general dissatisfaction or dispositional factors (Humphrey, Nahrgang, & Morgeson, 2007). The variability around these relationships also points to future research opportunities. For instance, a primary study might look at how workload interacts with family demands, role integration, or internal motivation to predict turnover. It may be that a high workload is only problematic for those who must also devote significant portions of their time to other roles (Brief, van Sell, & Aldag, 1979; Ilies, Wilson, & Wagner, 2009). Equally, complex work might only be detrimental to the degree that work is overly burdensome and stressful.

Our results show that *pay* (GHG: $\rho = -.11$, here: $\rho = -.17$), *role ambiguity* (GHG: $\rho = .24$, here: $\rho = .15$), and *role conflict* (GHG: $\rho = .22$, here: $\rho = .15$) all have significantly different effects than found by Griffeth et al. (2000). Although the face validity of the consistent negative effect for pay seems intuitive, challenges remain to explain why this effect is not stronger (compared to other predictors) or what it is about higher pay that reduces quitting. One explanation might be found in job embeddedness theory (Mitchell et al., 2001): This would suggest that pay affects off-the-job embeddedness as it provides support for one's lifestyle in a particular neighborhood or social caste; or, alternatively, pay might more strongly increase on-the-job embeddedness by providing an objective signal of the "worth" of one's employment, as an aspect of their job that he or she would be reluctant to sacrifice by departing.

1.2.3 | Traditional job attitudes predictors

For this category, we find some of the stronger attitudinal predictors. Broadly speaking, these variables constitute what March and Simon (1958) construed as "desirability of leaving" antecedents. Whereas the relationship for *organizational commitment* remains stable, we see stronger effects for *job involvement* (GHG: $\rho = -.12$, here: $\rho = -.19$) and *job*

satisfaction (GHG: $\rho = -.22$, here: $\rho = -.28$). This latter effect is interesting, because with the addition of over four times as many employees sampled, we now see nearly identical relationships between satisfaction and commitment, and turnover ($\rho = -.28$ and $-.29$, respectively). Such convergent validity is promising theoretically, as it offers greater justification to treat these variables as a single latent job attitude (Chang, Rosen, & Levy, 2009; Harrison, Newman, & Roth, 2006).

Equally noteworthy are the sizable effects for the newly meta-analyzed antecedents *other commitment* ($\rho = -.34$) and *other satisfaction* ($\rho = -.43$). Per our definitions (see Table 1), these consist of factors like career commitment and life satisfaction and, though based on fewer studies, demonstrate some of the strongest negative relationships with turnover. These findings may open new avenues for research contributions. For example, whereas theorizing often construes turnover as a response to proximal job conditions, the reported correlations suggest that quitting can also be a function of a more distal and general life dissatisfaction. Given that people's identities are often reflected by their choice of occupation, life dissatisfaction could signal individuals seeking a "fresh start" and a need for control over one's life via a career change.

1.2.4 | Newer personal conditions predictors

We find the relationship for *stress* is significantly larger than in earlier years (GHG: $\rho = .16$, here: $\rho = .21$, post-2000: $\rho = .23$). Indeed, the correlation between study year and stress effect sizes was also positive and significant ($r = .16$, $p < .05$). This finding perhaps alludes to generational effects, such as with regard to stress reactivity or work–life balance concerns. Perhaps individuals who have recently entered the workforce are less prepared to manage stress? Or, do such employees perceive stress differently than earlier generations? With increasingly blurred lines between work and nonwork roles, and demands to be "on 24/7," it is possible that what were previously considered positive challenge stressors (e.g., workload, time pressures) are, for some, slowly evolving into negative hindrance stressors (Podsakoff, LePine, & LePine, 2007). Supporting this logic, surveys find that younger generations report the most stress—particularly caused by work—and the least relief (American Psychological Association, 2012).

We also provide new meta-analyses for employee *coping* ($\rho = -.39$) and *engagement* ($\rho = -.20$) and turnover. These effect sizes are encouraging—particularly for engagement—which has recently gained increased study and practitioner interest (Bakker & Leiter, 2010; Wefald & Downey, 2009, see also our Section 5.3). These articles discuss how there is limited research on engagement consequences, so our results provide some preliminary testament to its predictive validity. Continued study of engagement will be quite useful, particularly as technological advances modify how physical work is performed and how the meaning of "engagement" changes (e.g., automation, virtual work, e-business, increased connectivity). It may be the case that engagement explains unique turnover variance beyond traditional predictors.

1.2.5 | Organizational context predictors

The organizational context has generally been ignored in turnover research until recently (Hom et al., 2017). As such, most meta-analyses within this category are new. We see notable effects for *climate* perceptions ($\rho = -.24$), *organizational support* ($\rho = -.19$), and *rewards offered* ($\rho = -.28$). Such results provide compelling evidence that the broader context does matter for turnover (Johns, 2006). We expand on this idea later, with moderators examining how context affects antecedent–turnover relationships. Further, because contextual factors like those above tend to be less affective in nature, in a multivariate sense, we might expect them to account for unique turnover variance beyond traditional attitudes (Carr, Schmidt, Ford, & DeShon, 2003).

Comparatively weaker effects were found for variables like *organization size* ($\rho = .03$) and *prestige* ($\rho = -.06$). Although we only compiled five studies for prestige, the results suggest that individual turnover decisions are less a function of what the organization is like in an absolute sense but rather are more strongly determined by how a given employee is immersed within that organization. However, especially for prestige, there was considerable variability around this effect ($SE = .07$), suggesting possible moderators. For instance, an organization's prestige might only affect turnover for those who desire it (i.e., as it indicates high status).

1.2.6 | Person–context interface predictors

Across all categories, we find some of the most promising results for person-context interface predictors. Many effects have changed, or are now conceptually different from previous work. For instance, with three times as many employees surveyed, we find significantly stronger *fit* effects compared to the Kristof-Brown, Zimmerman, and Johnson (2005) meta-analysis (theirs: $\rho = -.08$, here: $\rho = -.29$), significantly weaker effects for *met expectations* (GHG: $\rho = -.18$, here: $\rho = -.12$), as well as slightly stronger (although not significantly different) effects for overall *job embeddedness* compared to Jiang et al. (2012) (theirs [average on- and off-the-job embeddedness]: $\rho = -.16$, here: $-.26$). These sizable effects offer some indirect support for person–environment fit and attraction–selection–attrition theories, both of which emphasize how employees seek work environments that align with their demography, personality, and values. We elaborate more on this personal fit idea in our Step 2 moderator analysis.

This is also the first large-scale meta-analysis linking all aspects of *justice* to turnover behavior ($\rho = -.17$), as previous studies only examined its relationship to intentions or only looked at distributive justice. Our updated meta-analysis with nearly three times as many studies as Griffeth et al. (2000) reveals a significantly stronger effect (GHG: $\rho = -.11$). This effect is also relatively strong among the many attitudinal predictors examined, emphasizing how much individuals value fair and equitable treatment/outcomes from their employers. Indeed, many organizations have gone so far as to ban discussing pay differences or other equity issues in the workplace, which makes sense given the consequences shown here, should injustice occur.

1.2.7 | External job market predictors

For *alternatives*, our results with almost four times as many samples show a significantly increased effect size (GHG: $\rho = .15$, here: $\rho = .23$). Given that many turnover process models (e.g., Hom, Griffeth, & Sellaro, 1984; March & Simon, 1958; Mobley, 1977; Price & Mueller, 1981) include a stage in which employees compare alternatives to one's present job, the full explanatory power of such models depends on significant effects at each mediated link. Although a validity of .15 is modest, our updated estimate of .23 lends support to job alternatives as one of the more important antecedents of quit decisions. However, other theoretical approaches, such as the unfolding model (Lee & Mitchell, 1994), propose that some employees quit without an alternative job in hand. Although some studies position alternatives as a boundary condition to relationships such as job search–turnover (Swider, Boswell, & Zimmerman, 2011), there still remain many unanswered questions in this literature. In Step 2, we seek to add clarity to this literature by examining the moderating role of job market alternatives.

1.2.8 | Attitudinal withdrawal predictors

Not surprisingly, *withdrawal cognitions* such as intent to leave have the strongest correlation with turnover, and we find this effect to also be stronger compared to Griffeth et al. (2000), now having compiled three times as many studies (GHG: $\rho = .45$, here: $\rho = .56$). One noticeable implication of this result is that proximal withdrawal attitudes and other on-the-job judgments tend to better predict turnover than do more distal person or organizational characteristics. A second implication is that, although the turnover cognitions-behavior effect is quite strong, it is still not an identity. We would therefore caution researchers against treating turnover cognitions or intentions as a simple proxy for behavior, because there is slippage between intent and action. We elaborate on this issue in our Step 3 recommendations.

1.2.9 | Employee behaviors predictors

Lastly, regarding behaviors, we generally find similar results to other meta-analyses for predictors such as *performance* ($\rho = -.21$; Hom, Roberson, & Ellis, 2008 outlier excluded), *citizenship behavior* (OCBs, $\rho = -.10$), *lateness* ($\rho = .14$), and *absenteeism* ($\rho = .23$; Berry et al., 2012; Griffeth et al., 2000; Podsakoff, Whiting, Podsakoff, & Blume, 2009). Still, there remain few studies on lateness and turnover, and theory development is still needed, particularly with respect to isolating reasons for lateness itself: Is it an overt and conscious act of insubordination, does it often coincide with job search, is it largely dispositional (i.e., low conscientiousness), or is it a random behavior (i.e., difficult-to-time commutes)? Progression theories of withdrawal support lateness as a catalyst to absenteeism and subsequent quitting (Rosse, 1988),

yet work remains as to understanding what drives lateness in the first place or how employees perceive lateness to reinforce possible job dissatisfaction (Koslowsky, Sagie, Krausz, & Singer, 1997).

For *job search*, however, we do find a significantly stronger effect size (GHG: $\rho = .31$, here: $\rho = .40$), and even stronger among post-2000 studies ($k = 16$; $\rho = .43$). Although employees may have various reasons for searching (Boswell, Boudreau, & Dunford, 2004), the overall trend we observe is that those who search for alternatives are more likely to quit. Yet, this effect also has a relatively wide confidence interval, hinting at possible moderators (e.g., Swider et al., 2011).

1.3 | Progress assessment

In surveying the considerable growth of research that has accumulated in the 21st century, we have obtained a much clearer picture of the turnover forest for many predictors and their relationships with turnover behavior. Many effects increased, others decreased, whereas some have remained stable. Interestingly, many new predictors emerged that show promise. Although it is possible some effect size increases could be due to studies in contexts yielding larger effects (e.g., from occupations with high turnover), the increase in studies augurs well for the trustworthiness of the results. We know that traditional attitudes such as commitment and satisfaction are strong predictors of turnover, and withdrawal behaviors such as absenteeism and job search also signal impending exit. However, rather than hewing to only these predictors, we advise researchers and practitioners to also consider other factors that we find to also influence exit decisions, for there are many future opportunities for contribution and understanding. The increased relevance of personality, engagement, job embeddedness, and the organizational context suggests at the very least continued attention to these constructs will be worthwhile. For instance, core self-evaluations (CSEs) are a recently introduced meta-construct comprising of emotional stability, locus of control, generalized self-efficacy (i.e., an internal motivation construct), and self-esteem (Judge & Bono, 2001). Although we found no primary studies examining if, how, or why CSEs as a whole predict turnover, specific CSE dimensions do independently contribute to exit decisions (see also Zimmerman, Swider, Woo, & Allen, 2016 for an integrative review regarding how distal individual differences manifest into work withdrawal). Theory and testing about combinations of these attributes may be quite fruitful. Further, there are opportunities to build off of our content-related results (i.e., *what* predicts exit), with primary studies modeling processes and competitive theory testing (i.e., *how* and *why* antecedents predict exit).

Beyond these zero-order results, though, many constructs had a significant amount of heterogeneity around effect sizes. To better account for this, in Step 2, we developed theoretical arguments for multiple substantive moderators of turnover relationships. Due to the context-bound nature of turnover, such an analysis might yield greater understanding and more precise predictions as to who quits, when they quit, and why (Holtom et al., 2008).

2 | STEP 2: META-ANALYTIC MODERATOR ANALYSES

Given the variability observed around our estimates, we considered potential moderators of antecedent–turnover effects. Although meta-analytic methods generally preclude the testing of individual-level moderators (e.g., examining if the effect of job satisfaction on turnover depends on employees' personality traits), they do provide opportunities to test contextual or sample-level moderators, which are typically more challenging to capture in primary studies (Park & Shaw, 2013). Specifically, our moderation analyses broadly reflect how the organizational and economic context can influence the magnitude of the relationships between individual antecedents and turnover behavior. Johns (2006) noted that turnover studies rarely attend to context, despite contextual influences likely factoring into employee's turnover decisions and accounting for variance beyond individual antecedents alone (see also Cappelli & Sherer, 1991; Holtom et al., 2008; Steel, 2002). Following Johns (2006), in Step 2, we tested a set of contextual moderators in terms of cross-level effects, where the mean levels of a variable in the sample or organization (e.g., mean age across employees, mean base rates of turnover in an organization) or in the economy (e.g., unemployment rates when data were collected) are expected to render relationships between individual-level antecedents and turnover stronger or weaker.

Our meta-analytic dataset provides us information to examine how organizational and economic factors (Level-2) influence individual-level antecedent–turnover effect sizes (Level-1; see also Park & Shaw, 2013 for a similar examination of sample-level moderation regressions).

Context can be conceptualized and operationalized in different ways. In this paper, we view context from four different perspectives: personal fit, attitudinal climate, the job market, and turnover contagion. Context is important because it provides unique meaning as to how similar or different one is relative to others on a given variable (personal fit perspective): For instance, an employee who is unable to sell much of a product to customers may be likely to quit (out of frustration at their ability), but this relationship might be weakened if the employee were to find out that the average sales volume across all employees in the organization was not much different than their own. Context also matters because it provides a salient and social benchmark against which to gauge one's own attitudes and behaviors. In other words, the context or organizational climate helps an employee make sense of their own attitudes and behaviors, as it allows for self-comparison to others in the organization. In turn, this social comparison might buffer or amplify the likelihood of turnover. For example, an employee with high job satisfaction may be even more likely to stay if he or she is surrounded by many others who share a high level of satisfaction (climate perspective). Finally, context matters because it can elicit situational opportunities or constraints on behavior: The efficacy of an employee's search for a new job will likely be tempered by overall job availability in the external market (job market perspective), just as one's job dissatisfaction may create more motivation to leave if turnover rates in the organization are high (turnover contagion perspective; Johns, 2006).

Following these four perspectives (i.e., fit, climate, job market and turnover contagion) in the next sections, we developed a set of moderation hypotheses. Guided by theory, stipulating a sufficient number of effect sizes to test for moderation (≥ 15), examining moderators only where the zero-order effects results indicated significant heterogeneity, and excluding those studies where contextual information was not available,[2] we arrived at 10 moderators to test the personal fit perspective (sample mean levels of an antecedent moderating its respective individual-level antecedent–turnover relationship), two moderators to test the climate perspective (mean levels of job satisfaction and organizational commitment), two moderators to test the external job market perspective (mean levels of perceived alternatives and annual unemployment rate), and three moderators to test the turnover contagion perspective (turnover base rate, mean levels of withdrawal cognitions, and job search). We test these moderators for the following antecedent–turnover relationships: absenteeism, age, alternatives, education, employee performance, job embeddedness, job satisfaction, job search, organizational justice, organizational commitment, organizational tenure, pay, sex, stress/exhaustion, and withdrawal cognitions. However, not every moderator is tested for every antecedent–turnover relationship, partly due to data unavailability issues and partly due to lacking theoretical rationale for a given test.

2.1 │ Personal fit

Past theory and research suggests that people are more likely to seek out other individuals and remain in environments that are similar to themselves, in terms of biographical and personality factors, attitudes/beliefs/values, behavioral habits, and other sociodemographics. As a result, social networks tend to be relatively homogenous on these organizing characteristics, termed the *homophily principle* (McPherson, Smith-Lovin, & Cook, 2001). On the other hand, should individuals find themselves to be dissimilar to others on these factors, they will often remove themselves from such environments. Person–environment fit theory (Kristof, 1996) and, particularly germane to turnover, attraction–selection–attrition theory (Schneider, 1987), highlight this idea, in which homophily is reinforced as employees are attracted to organizations similar to themselves and are more likely to quit organizations—or more specifically, the people in those organizations—if they deem themselves dissimilar, or a misfit. In this way, Schneider theorized turnover as a means of correcting an error in homophily judgment, if the reality of the organization is or becomes divergent on one or more of these characteristics.

Although in our initial meta-analysis we did examine the "fit" construct, we expand the notion of fit here to represent the extent to which an employee is unique on demographic, attitudinal, or behavioral factors compared to other

employees. For demographics, for example, we expect to find that the relationship between sex (coded 0 = female, 1 = male) and turnover will be more negative when the sex makeup of an organization is predominately male (i.e., men will be less likely to quit if most other employees are also male). In terms of attitudes, we expect the fit effect to operate such that turnover effect sizes will be more positive when employees aversely trend away from the mean-level attitudes of employees in a sample. For example, the negative individual-level job satisfaction-turnover effect may become more negative (further away from zero) when sample mean-level satisfaction is higher (i.e., highly satisfied employees may be even less likely to quit if everyone else is also satisfied). Or, the positive relationship between individual stress and turnover is expected to be more positive when average stress levels across employees are lower (i.e., highly stressed employees may be even more likely to quit if others are not very stressed). More formally, we hypothesize that the relationship between a given antecedent and turnover at the individual level will be moderated by the respective antecedent's sample mean level. Based on the inclusion criteria noted above, we examined the following moderators: age, education, employee performance, job embeddedness, job satisfaction, justice, organizational commitment, organizational tenure, sex, and stress.

> **Hypothesis 1:** The individual-level relationships between employee age, education, performance, job embeddedness, job satisfaction, justice, organizational commitment, organizational tenure, sex and stress/exhaustion and turnover behavior will be moderated by the antecedent's respective sample mean-level, such that relationships will become more positive (or less negative) when employees are more dissimilar (i.e., a misfit) to others on that antecedent.

2.2 | Attitudinal climate

Turnover relationships may also vary as a function of the organizational climate, defined here as the shared experiences, perceptions, and behavioral tendencies among a group of employees (Schneider, Ehrhart, & Macey, 2013). Ostroff (1993) offered a taxonomy of climate perceptions, representing three broad facets: affective, cognitive, and instrumental (see also Carr et al., 2003). Drawing from this theoretical template, we hypothesized that some antecedent–turnover relationships might vary as a function of whether attitudinal climates are more or less favorable in valence. From our initial meta-analysis, we find employees are less likely to quit when working in more positive climates. We extend this notion to moderators, suggesting that a positive attitudinal climate might buffer certain antecedents from translating into turnover. From a social interactionist perspective, research has shown that climate can serve such a moderating role, because individuals are attuned to their environments, seek to cohere with them, and use them to derive important signals about their attachment to the organization (Eisenbeiss, van Knippenberg, & Boerner, 2008; Li, Liang, & Crant, 2010; Smith-Crowe, Burke, & Landis, 2003).

Specifically, we posit that turnover will be less likely (i.e., effects more negative) when sample mean job satisfaction and organizational commitment are more favorable. For example, as we find from Step 1 that withdrawal cognitions positively predicts turnover, we expect such a relationship to become less positive (i.e., employees who think about quitting will be not as likely to quit) when mean job satisfaction and organizational commitment are higher (e.g., Liu, Mitchell, Lee, Holtom, & Hinkin, 2012). A generally more satisfied/committed workforce might deflect an individual's thoughts of leaving in an effort to maintain the positive environment or because a positive climate might become embedding insofar as it would be a sacrifice to give up should one leave—and job embeddedness has been shown to have a buffering effect on turnover (Allen, Peltokorpi, & Rubenstein, 2016; Holtom & Inderrieden, 2006; Swider et al., 2011).

With this hypothesis, we must note two caveats: First, like our fit hypothesis, we measured "climate" in our initial meta-analysis. However, the key difference in our treatment of climate as a moderator is by using *sample mean perceptions* of satisfaction and commitment. Most primary studies of climate tend to instead use a referent shift approach (Chan, 1998), where employees rate the attributes of their unit or organization rather than gauging their own attitudes. However, we would argue that mean ratings across employees are perhaps a better indicator of climate than is a referent shift, for mean levels assess how employees themselves, on average, actually feel about the favorability of the

organization rather how they think others feel. Second, although one could argue that mean stress, performance, withdrawal cognitions, or even demographics (e.g., an older workforce) can be construed as other "climates," we did not examine them in the climate category, in part because some moderators were already subsumed in other categories (i.e., under the personal fit perspective), or due to insufficient data. Based on data availability, we tested the climate perspective for the following antecedents: age, alternatives, commitment, tenure, pay, sex, and withdrawal cognitions.

> **Hypothesis 2:** The individual-level relationships between employee age, alternatives, commitment, tenure, pay, sex and withdrawal cognitions, and turnover behavior will be moderated by sample mean-level job satisfaction and organizational commitment, such that the relationships will become less positive (or more negative) when the attitudinal climate is more favorable (i.e., when mean levels are higher).

2.3 | Job market

Third, we considered job market factors that might constrain turnover. Such moderators align with March and Simon's (1958) notion of "ease of leaving," suggesting that turnover will be less likely if there is general scarcity of alternative employment available. Although alternatives positively predict turnover, we also expect alternatives to moderate other antecedent–turnover relationships, in that more/less available jobs (actual or perceived) might expand/limit the extent to which antecedents are acted upon. For example, research has found that job search (Swider et al., 2011) and job satisfaction (Trevor, 2001) are less likely to result in quitting if one perceives few alternatives and that the majority of quitters do leave with replacement jobs in hand (Lee & Mitchell, 1994; Lee, Mitchell, Holtom, McDaniel, & Hill, 1999).

We tested two job market moderators: Unemployment rates at the time data were collected (for U.S.-based samples only) and sample mean job alternatives. When unemployment rates are higher, and average job alternatives perceptions are lower, we take this to mean that there are fewer jobs available in the external market and as such, positive (negative) turnover relationships should weaken (strengthen). The moderating effect of mean alternatives could only be tested for the following antecedents: alternatives, job satisfaction, and withdrawal cognitions. Yet, for unemployment rates as a moderator, all antecedents were examined (a total of 15).

> **Hypothesis 3:** The individual-level relationships between employee absenteeism, age, alternatives, education, performance, embeddedness, satisfaction, search, justice, commitment, tenure, pay, sex, stress/exhaustion and withdrawal cognitions, and turnover behavior will be moderated by sample mean-level job alternatives and U.S. yearly unemployment rates, such that relationships will become less positive (or more negative) when the unemployment rates were higher when data were collected and when sample mean-level job alternatives are lower.

2.4 | Turnover contagion

Turnover contagion theory (Felps et al., 2009) suggests that an employee's propensity to leave a job can partly be influenced by whether other employees have also left their jobs or are intending to do so. This line of theorizing describes a process whereby employees compare themselves and their attitudes to others in order to determine if they should quit. That is, one's own turnover propensity can be affected by the salience of others' attitudes about quitting and others' actual quitting behavior. This moderator was considered in Griffeth et al. (2000); however, they only examined one relationship, pay-turnover, and dichotomized turnover base rates as more or less than 15%. In our analyses, we examined base rates as a continuous moderator with many more samples (thereby improving the statistical power of such tests), along with considering other potential moderated antecedent–turnover relationships.

To test the original turnover contagion hypotheses, Felps et al. (2009) operationalized one's *coworkers'* (low) job embeddedness and job search behaviors as representing a high contagion environment. In the present study, we offer an even stronger test of turnover contagion theory by considering *sample-level* contagion indicators, thereby allowing comparisons across organizations. A clear assertion of turnover contagion would be reflected by actual turnover rates in a sample, mean withdrawal cognitions across employees or mean job search rates (we considered job

embeddedness, but it was ultimately omitted due to insufficient primary study data). Such moderation would involve testing whether, for example, employees are more likely, on average, to translate higher alternatives, job search, or withdrawal cognitions into quitting when a sizable proportion of employees have already quit, are thinking about quitting, or are searching for new work. Such questions cannot be answered as easily through primary studies but are readily accessible as cross-level meta-analytic moderators.

Although both job search × mean-level job search and withdrawal cognitions × mean-level withdrawal cognitions could be interpreted under the personal fit perspective, we treat them here as turnover contagion, insofar as we expect to see *positive* relationships. That is, if an employee is thinking about quitting or is searching for alternative employment, if everyone else is also thinking about quitting or is searching for new work, we expect the relationships between withdrawal cognitions and job search to turnover to be even stronger. Specifically, we tested the moderating role of turnover base rates for all 15 antecedents, sample mean-level withdrawal cognitions for 13 antecedents (excluding education and stress), and sample mean-level job search only for job search (technically, this also provides a test of the fit perspective).

Hypothesis 4: The individual-level relationships between employee absenteeism, age, alternatives, education, performance, embeddedness, satisfaction, search, justice, commitment, tenure, pay, sex, stress/exhaustion and withdrawal cognitions, and turnover behavior will be moderated by sample mean-level turnover base rates, withdrawal cognitions, and job search, such that relationships with turnover will become more positive (or less negative) when turnover base rates, withdrawal cognitions, and job search are higher.

2.5 | Method

2.5.1 | Coding and procedure

For each primary study, we coded for 14 sample-level moderators. They were sample means for age, tenure, sex makeup (percent male), education, job satisfaction, organizational commitment, stress/exhaustion, job embeddedness, alternatives, withdrawal cognitions, job search, employee performance, and the mean sample turnover base rate. Using only studies from the United States, unemployment rate was gathered from the year when the data were collected; if not noted, we used the year prior to the study being published (to allow for publishing time lag). We obtained yearly unemployment data from the Bureau of Labor Statistics website.

To obtain moderator sample means, we reviewed the "Methods" sections and correlation matrices of each primary study (i.e., mean and *SD* reports). As noted earlier, we excluded studies collapsing multiple organizations into a single sample to isolate moderation effects to only the sample for which mean values were applicable. For mean age, tenure, sex, turnover base rates, and unemployment rates, we used reported values (or website data, for unemployment), although for tenure we transformed all codings into tenure in years (some studies reported in months or weeks). For the other moderators, rescaling was necessary so as to interpret means equally across studies using different scale ranges. To do this, we divided reported values by the maximum of the given scale used in the study (Aguinis, Gottfredson, & Culpepper, 2013). For instance, if job satisfaction were rated on a seven-point Likert scale, a study reporting mean satisfaction levels of 3.50 would yield a standardized value of 0.50. We were conservative in this regard, in that we only coded what was reported in study text. Although a mean and *SD* might indirectly imply scale maximums (e.g., a mean of 3.50 with a *SD* of 0.77 might suggest a five-point scale), we did not extrapolate beyond what was reported.

We tested hypotheses using SPSS version 23, using a weighted least squares (WLS) regression approach. WLS is advantageous compared to testing for significant bivariate correlations between effect sizes and moderator levels, comparing hierarchical subgroups (two methods used in Griffeth et al., 2000), or ordinary least squares regression, because WLS accounts for correlations among moderators while proportionally weighing primary studies based on the inverse of the sampling error variance. As such, WLS regression is altogether a more statistically powerful method to test for moderation (Steel & Kammeyer-Mueller, 2002).

2.6 | Results and discussion

Descriptive statistics for standardized moderators (i.e., a proportion out of 1.00) were as follows: For education (many studies used scales rather than absolute years educated), the grand mean was 0.53, which on a five-point scale indicates roughly between some college and a bachelor's degree. The grand mean for job satisfaction was 0.70 ($SD = 0.11$); for organizational commitment, 0.67 ($SD = 0.08$); for stress/exhaustion, 0.53 ($SD = 0.09$); for job embeddedness, 0.54 ($SD = 0.15$); for alternatives, 0.68 ($SD = 0.16$); for withdrawal cognitions, 0.49 ($SD = 0.15$); for job search, 0.42 ($SD = 0.13$); and for employee performance, 0.66 ($SD = 0.18$).

Moderation results are shown in Table 3. The results for the personal fit, attitudinal climate, job market and turnover contagion perspectives are shown in the second through fifth columns, respectively. Each row lists antecedents for each hypothesis test. Cells filled in by a "–" had insufficient data or theoretical rationale for inclusion, and cells with a "#" denote an effect examined in a different moderator category (the result is shown in a different column). WLS regressions were simultaneously performed for all moderators across each antecedent row.

To aid readers in interpreting the results, we note that a negative moderator effect would make an antecedent–turnover relationship more negative (or less positive), whereas a positive moderator effect would make a relationship more positive (or less negative). This is because we interpret a zero-order relationship in terms of how higher levels of an antecedent translates to higher (positive effect) or lower (negative effect) turnover likelihood. For example, we find that the negative age–turnover relationship ($\rho = -.21$, older employees are less likely to quit, on average) is negatively moderated by respective mean sample age (i.e., personal fit perspective), suggesting that in organizations where the mean age is higher, individual-level age–turnover correlations are even more negative. Mathematically, Table 3 shows that for each year increase in the mean age of across employees in a sample, the correlation between individual age and turnover becomes negative by a further .02, or for every SD increase in mean employee age (roughly 6.7 years), the age–turnover relationship becomes further negative by .81 SDs (about .14 correlation points). Conversely, the alternatives-turnover relationship ($\rho = .23$) is positively moderated by mean withdrawal cognitions across employees in a sample ($\beta = .33, p < .01$), meaning that in organizations where employees as a whole are thinking more about quitting, the alternatives–turnover relationship becomes further positive (i.e., turnover contagion perspective).

Hypothesis 1 proposed a set of personal fit moderators, suggesting that positive (negative) individual-level antecedents–turnover relationships would become stronger (weaker) when employees are more dissimilar from sample mean-level of that respective antecedent. Column 2 in Table 3 summarizes these results. We found numerous significant moderated effects. Specifically, significant effects were seen for employee age ($\beta = -.81, p < .01$), education ($\beta = -.38, p < .05$), sex ($\beta = -1.13, p < .05$), job satisfaction ($\beta = -.39, p < .05$), organizational commitment ($\beta = -.21, p < .01$), tenure ($\beta = -.61, p < .01$), and stress ($\beta = -.57, p < .01$).

As noted above, we find that the negative relationship between individual age and turnover becomes increasingly negative in samples with higher mean-level age, whereas the nonsignificant relationship between education and turnover becomes significantly more negative in samples where the mean education level is higher. Interestingly, we also find that the null zero-order relationship between sex and turnover becomes significantly more negative when the sex makeup of the sample is increasingly male. A similar personal fit result is also seen for tenure, where the negative relationship between tenure and turnover becomes more negative in organizations mostly made up of senior-level employees. Pfeffer (1983) documented such an organizational demography phenomenon for age and tenure in the United States railroad industry, where younger, newcomer employees were deterred from remaining in the industry because most workers were older and of senior level. Subsequently, newcomers perceived greater misfit to others in the trade, along with limited opportunities for promotion/advancement. Here, we generalize this fit effect to many more organizations. One other finding is also noteworthy: Satisfaction and commitment as fit moderators are both significant and negative, suggesting that, in organizations where employees are more satisfied and committed, on average individual satisfaction/commitment–turnover relationships are even stronger (i.e., unhappy employees are even more likely to leave when surrounded by mostly happy peers). That is, not only does dissimilarity to others on surface-level factors affect turnover, but also employees are cognizant of attitudinal dissimilarity between themselves and others and that this dissimilarity can amplify negative perceptions. In a related vein, the moderation effect for

TABLE 3 Weighted least squares regression analysis of individual-level antecedent–turnover correlations as a function of contextual sample-level moderators

Moderator group	Perspective 1: Personal fit			Perspective 2: Attitudinal climate						Perspective 3: Job market					
Specific mean-level moderator	Mean respective antecedent			Mean job satisfaction			Mean organizational commitment			Mean alternatives			Year unemployment rate		
Independent variable	B	SE	β	B	SE	β	B	SE	β	B	SE	β	B	SE	β
Absenteeism	–	–	–	–	–	–	–	–	–	–	–	–	.06	.04	.47
Age	–.02	.01	–.81**	–.18	.11	–.29	–.24	.22	–.30	–	–	–	.02	.01	.26
Alternatives	#	#	#	–.03	.46	02	–.50	.73	–.23	–.05	.25	–.06	.01	.02	.18
Education	–.18	.10	–.38*	–	–	–	–	–	–	–	–	–	.05	.01	.70*
Employee performance	.38	.85	.20	–	–	–	–	–	–	–	–	–	.03	.04	.20
Job embeddedness	.28	.68	.14	–	–	–	–	–	–	–	–	–	–.04	.06	–.33
Job satisfaction	–.58	.30	–.39*	–	–	–	–	–	–	–.97	.41	–.70*	.04	.03	.34
Job search	#	#	#	#	#	#	#	#	#	–	–	–	.00	.03	.01
Justice	–.53	.43	–.43	–	–	–	–	–	–	–	–	–	.02	.02	.23
Organizational commitment	–.35	.09	–.21**	–.14	.23	–.12	#	#	#	–	–	–	–.01	.02	–.10
Organizational tenure	–.002	.001	–.61**	–.46	.74	–.12	–	–	–	–	–	–	.01	.03	.04
Pay	–	–	–	.64	.50	.39	–	–	–	–	–	–	–.01	.02	–.12
Sex (0 = female, 1 = male)	–.61	.18	–1.13**	–.20	.20	–.30	–	–	–	–	–	–	.08	.03	.76*
Stress/exhaustion	–.73	.18	–.57**	–	–	–	–	–	–	–	–	–	–.02	.01	–.29*
Withdrawal cognitions	#	#	#	1.03	.47	.54*	.88	.46	.36	.97	.42	.45*	–.06	.04	–.32

(Continues)

TABLE 3 (Continued)

| Moderator category | Perspective 4: Turnover contagion | | | | | | | | |
| Specific moderator | Mean turnover base rate | | | Mean withdrawal cognitions | | | Mean job search behaviors | | |
Independent variable	B	SE	β	B	SE	β	B	SE	β
Absenteeism	.64	.38	.50	–.20	.53	–.11	–	–	–
Age	–.70	.21	–.46**	–.30	.18	–.27	–	–	–
Alternatives	–.20	.59	–.12	.43	.11	.33**	–	–	–
Education	.08	.16	.09	–	–	–	–	–	–
Employee performance	–.45	.53	–.29	1.54	.76	.78*	–	–	–
Job embeddedness	1.29	1.92	.30	–1.54	1.27	–.45	–	–	–
Job satisfaction	–.85	.38	–.48*	–.17	.25	–.14	–	–	–
Job search	–.48	.50	–.40	–.64	.96	–.22	.41	.20	.67*
Justice	.40	.32	.35	–.53	.28	–.54*	–	–	–
Organizational commitment	–.04	.16	–.04	–.47	.19	–.51**	–	–	–
Organizational tenure	–.32	.23	–.26	1.11	.51	.46*	–	–	–
Pay	–.06	.47	–.05	.78	.34	.71*	–	–	–
Sex (0 = female, 1 = male)	.50	.34	.38	–.42	.40	–.32	–	–	–
Stress/exhaustion	–.41	.27	–.20	–	–	–	–	–	–
Withdrawal cognitions	–.57	.57	.26	.82	.36	.55*	–	–	–

Note. Perspective 1 "mean respective antecedent" represents each row's antecedent moderated by its own sample mean-level (e.g., individual age–turnover effect moderated by sample mean-level employee age; stress–turnover moderated by sample mean-level stress). Sex moderator coded as percent male. Each study was weighted by the inverse of the sampling error variance. Number of studies and blank cells vary due to specified hypotheses and/or insufficient data. Each row is a single WLS regression of all moderators regressed on a given antecedent–turnover effect size. "–" denotes insufficient available data to test moderator. "#" denotes a moderator examined in a different theoretical perspective. A negative moderation effect means an antecedent–turnover relationship becomes more negative, or less positive, as the moderator value increases in absolute magnitude. A positive moderation effect means that an antecedent–turnover relationship becomes more positive, or less negative, as the moderator value increases in magnitude.

$^*p < .05$ $^{**}p < .01$.

stress-turnover × sample mean-level stress was also significant, such that the positive zero stress-turnover effect size became more negative (i.e., less positive) in organizations where employees as a whole reported higher stress levels. Although nonsignificant moderation effects were found for performance, job embeddedness, and justice, most findings are significant, so we conclude that Hypothesis 1 is generally supported.

Hypothesis 2 considered climate moderators of mean-level job satisfaction and organizational commitment. These results are presented in Table 3, column 3. Unfortunately, few studies reported on these variables' sample mean values along with antecedents, so we were limited in the scope of our moderator tests. The only significant effect found was for satisfaction moderating withdrawal cognitions-turnover ($\beta = .54$, $p < .05$), but surprisingly, this effect is in the opposite direction as hypothesized. Thus, we generally find no support for Hypothesis 2.

Hypothesis 3 posited that job market conditions would moderate antecedent–turnover relationships such that relationships would be more positive (less negative) when more jobs are available. Again unfortunately, few studies measured both alternatives and other antecedents to fully examine this moderator, but we were able to test all moderating effects for unemployment rates (for U.S. samples only). As shown in Table 3, column 4, some moderation effects for alternatives or unemployment rate are not significant, but the following are: withdrawal cognitions × mean alternatives ($\beta = .45$, $p < .05$), stress × unemployment ($\beta = -.29$, $p < .05$), job satisfaction × mean alternatives ($\beta = -.70$, $p < .05$), education × unemployment ($\beta = .70$, $p < .05$), and percent male × unemployment ($\beta = -.76$, $p < .05$). These results suggest, for instance, that the positive relationship between thinking about quitting and actually quitting is stronger when others perceive there to be many available alternatives in the job market and that men are more likely to quit (or, women are less likely to quit) when the job market is tighter and fewer jobs are available. Taken together, we conclude that Hypothesis 3 is partially supported.

Hypothesis 4 concerned the turnover contagion perspective, shown in Table 3, column 5. We hypothesized that when sample mean-level turnover base rates, withdrawal cognitions, and job search behaviors are higher, antecedent–turnover relationships would become more positive or less negative. As shown in the table, numerous moderators were significant. Specifically, when an organization's turnover base rate is higher, age ($\beta = -.46$, $p < .01$) and job satisfaction ($\beta = -.48$, $p < .05$) are even more strongly negatively related to turnover. When mean withdrawal cognitions are higher, the positive relationships to turnover of alternatives ($\beta = .33$, $p < .01$) and withdrawal cognitions ($\beta = .55$, $p < .05$) become even more positive. Similarly, the negative relationships of justice ($\beta = -.54$, $p < .05$) and organizational commitment ($\beta = -.51$, $p < .01$) to turnover become more negative. The negative relationships of performance ($\beta = .78$, $p < .05$), tenure ($\beta = .46$, $p < .05$), and pay ($\beta = .71$, $p < .05$) become less negative as others think more about leaving. Said differently, although higher performance, longer tenure and higher pay typically keep employees in their jobs, higher performers, longer tenured employees, and higher paid employees may actually be more likely to quit when others around them think about leaving. The moderation effect for job search–turnover × sample mean-level job search was also significant, being more positive in samples with higher average search behavior ($\beta = .67$, $p < .05$). When many people are searching for new employment, we find that employees are more likely to translate their own job search behavior into quitting.

Surprisingly, turnover base rates did not moderate relationships for predictors such as commitment, embeddedness, or withdrawal cognitions. This may partly be a function of limited relative studies available, such as embeddedness (i.e., a possible Type II error), but it also points to an interesting consideration: Possibly, *who* leaves (i.e., a "bad apple" versus a supportive colleague) is more important than the raw metric of how many employees leave. In job embeddedness theory, considering the quality of links—beyond mere quantity—could be insightful in this regard (Lee, Burch, & Mitchell, 2014). In general, though the results show that when the work environment is one in which withdrawal, thoughts of withdrawal, and actual quitting are higher, antecedents such as alternatives, search, and withdrawal cognitions have even more positive effects, whereas factors that typically prevent turnover (e.g., high performance, longer tenure and higher pay) have weaker (i.e., closer to zero) effects. We conclude that Hypothesis 4 is generally supported.

Overall, we conclude that the personal fit, job market, and turnover contagion perspectives received the greatest support. When examining how employees arrive at a possible turnover decision, it seems prudent then to take into consideration their (dis)similarity to others, perceived or actual job alternative availability, and the extent to which others in the organization have quit or are signaling their intent to do so. The moderation results present a relatively

conservative effect in that we did not tease apart contextual nuances. It is possible that the context plays a greater role than what is shown here, due to the imprecise nature in which we were able to operationalize it. Contextual influences should be greater, the more narrowly defined the context becomes. For example, an employee might consider looking for a new job when immediate coworkers in the same department do the same but would not exhibit such tendencies when coworkers in a different department engage in search. Our analyses operationalized context at the sample level, and although we excluded samples of employees from multiple organizations, heterogeneity in each sample's composition could be quite high because some spanned multiple departments, business units, teams, or even geographical locations (though still from the same organization). Meanwhile, other contexts were more narrowly defined. We suspect that viewing the context in narrower ways would yield more significant moderation effects and possibly also provide more support for the attitudinal climate perspective.

3 | STEP 3: WHERE ARE WE GOING? AN INTEGRATIVE, FUTURE-ORIENTED DISCUSSION

The overarching goal of this study was to assess the current state of the turnover literature, considering the progress made resulting from the substantial growth of empirical work in the 21st century and what that progress means going forward. To these ends, we conducted the most comprehensive turnover meta-analysis to date and identified those predictors that most meaningfully contribute to individual quitting decisions. Second, guided by theory, we conducted a series of moderator analyses to gain more insight into the contextual nature of turnover. In this discussion, we consider the theoretical and practical implications of the results and integrate our findings by developing a research "road map," so to speak, for the future of turnover study. Specifically, after reviewing hundreds of articles, we feel it is important to discuss what we believe to be the most necessary conceptual and methodological challenges and opportunities going forward, so as to improve theory testing and prediction.

3.1 | Theoretical implications

A primary goal of this study was to aid in interpreting the vast turnover landscape with an eye toward future research. Part of this task was accomplished with a thorough initial meta-analytic review (Step 1). Still, we believe there is benefit in offering a general picture of these results. In order to summarize all tested relationships, we offer a scatter plot, shown in Figure 1, which organizes each antecedent according to its absolute corrected meta-analytic correlation, on the Y-axis, and its corresponding standard error, on the X-axis. The former metric describes effect size magnitude, whereas the latter describes effect size variability (computed as $SD/\sqrt{N}$, see Table 2). The number of studies, k, for each antecedent is also included in Figure 1, distinguished by point marker quartiles. Using this plot as a visual aid, we seek to identify potentially stronger and weaker areas of inquiry and areas that require further study. Although these results cannot speak to how any given effect might operate in a multivariate model, when a turnover researcher or human resources analytics team is considering what variables to include in their study, and survey length constraints are a factor, Figure 1 is a useful starting point.

A few variables stand out as they are the most predictive and tend to exhibit relatively low variability across contexts. These variables are shown in the upper-left area of the figure and include proximal work perceptions and behaviors such as withdrawal cognitions, job search, organizational commitment, job satisfaction, rewards offered beyond pay, justice, embeddedness, and performance. It also includes distal factors such as age, tenure, and children. Interestingly, a greater number of antecedents in this area of the plot are more proximal rather than distal.

Most of these antecedents also have been researched quite extensively (i.e., 3rd or 4th quartile of study). We are therefore quite confident about the robustness of these findings and would anticipate such variables will be consistently predictive of quitting. If a holistic account of why people quit were a researcher or practitioner's goal, we would recommend that inquiry start with these predictors. Equally, if a researcher seeks to predict turnover with a
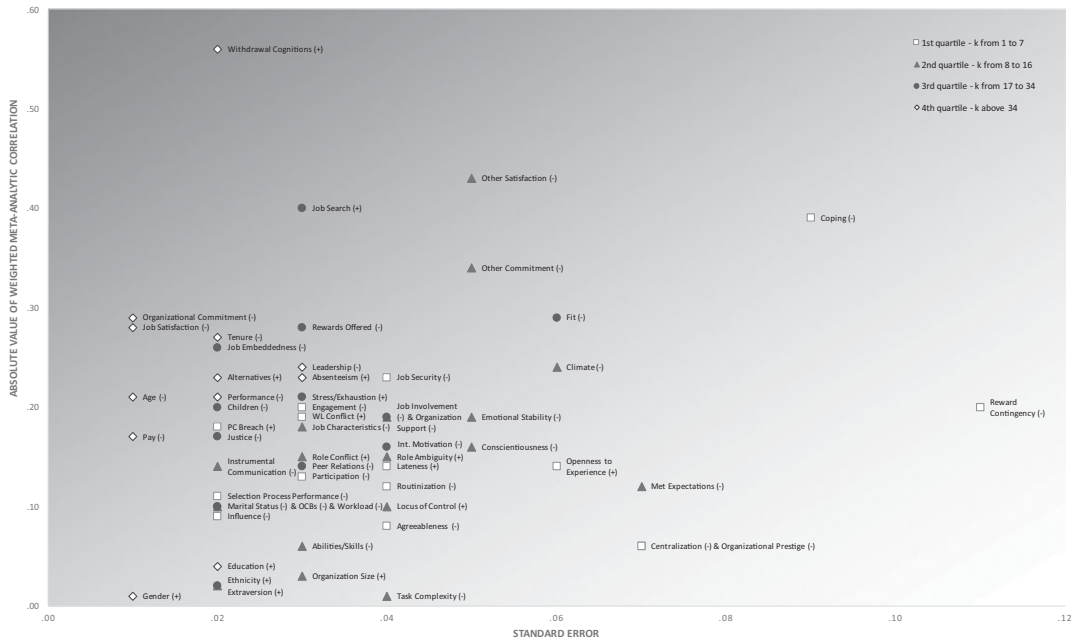
FIGURE 1 Summary of meta-analytic turnover antecedent estimates (as effect sizes-by-standard errors)
Note. Correlation signs indicated in parentheses. OCB = organizational citizenship behavior. PC breach = psychological contract breach. Due to visual overlap, we note extraversion, OCBs and organizational support are in the 2nd quartile of studies (*k*) accumulated; ethnicity, job involvement, marital status and workload are in the 3rd quartile.

new construct, it would be helpful to first incorporate these antecedents as controls or covariates. For example, when the job embeddedness construct was first introduced, Mitchell et al. (2001) demonstrated evidence of its incremental predictive validity beyond that of similar constructs like job satisfaction and organizational commitment.

Viewing standard errors as a signal of the context-dependent nature of turnover reveals antecedents showing moderate-to-strong predictive magnitude but also relatively high variability. These antecedents are located toward the top-right of Figure 1. They include constructs such as fit, climate, coping, other satisfaction, and other commitment. Thus, although job satisfaction and organizational commitment tend to be predictive across occupations and samples, more narrow predictors like career satisfaction and occupational commitment might be more context specific. Also possible, such variables may emerge as more consistent predictors when matched to their respective target (e.g., occupational commitment predicting occupational turnover; Blau, 2007).

Another inference from the data markers is that many effect sizes must be interpreted with caution. For instance, it is not especially surprising that coping and reward contingency are outliers, given their limited study. At the same time, one cannot interpret this finding as a reason to dismiss their effects entirely, or conversely, to automatically include them as study controls. Clearly, more empirical work is needed on these variables in order to draw firmer conclusions.

Noting both fit and climate having both sizable effect sizes and high standard errors speaks to the general conclusion of our research that the organizational context matters and should be explored in more depth (Johns, 2006). Our moderation tests reported here were aimed to more systematically guide and inspire such efforts. Specifically, we found numerous fit-related boundary conditions, revealing that when an employee makes a claim that he or she "fits" with the company culture, this perception can be made in terms of demographics or attitudes/cognitions, as shown here, as well as in terms of job demands relative to personal abilities or personal needs relative to job supplies (Kristof-Brown et al., 2005). Researchers might capitalize on methodological advancements in the form of response surface analysis and self–other congruence indices to further explore these promising ideas (Edwards, 2007).

We also note antecedents with higher standard errors, coupled with smaller relative effects. These antecedents are plotted toward the bottom right of Figure 1. Met expectations, centralization, and openness to experience stand out as examples. Although these constructs' effect magnitudes are comparatively lower, their higher variability suggests that they might be especially impactful in certain contexts yet matter little in others. From both theoretical and practical perspectives, there is value in identifying what such boundary conditions might be, and why they emerge for a given antecedent. Unfortunately, we were unable to perform moderator tests for these variables due to insufficient primary studies, which also makes it challenging to draw firm conclusions as to population parameter values. For example, lateness has a modest magnitude ($\rho = .14$) but a relatively larger standard error ($SE = .04$). Lateness has a longstanding history in turnover theory but has received surprisingly limited empirical attention. Given the criticality of lateness behavior to "progression of withdrawal" models (Berry et al., 2012; Harrison et al., 2006), where lateness is viewed as a minor form of withdrawal, progressing in increasing severity to absenteeism and eventually quitting, greater scrutiny of this mediated process seems warranted. Such models argue that absenteeism fully mediates lateness–turnover relationships, such that a direct lateness–turnover effect is nonsignificant. However, such models essentially ignore the heterogeneity of lateness–turnover direct effects, such that its relationship might indeed matter under particular circumstances, varying as a function of dispositional factors, the organizational climate, company policies, job market conditions, or contagion pressures.

It is also worthwhile to address antecedents with relatively lower effect magnitudes and lower standard errors. One might be quick to dismiss these variables; however, we would caution against such hasty value judgments, instead alerting researchers to reflect on their necessity as controls and to make theory-driven decisions regarding their use as predictors. Employee sex, education, and ethnicity all have small effect sizes, along with lower standard errors. We noticed that these demographics are often included in turnover studies, although most are not discussed in detail within the context of prediction but rather are included as controls. The compiled evidence here suggests that such automatic practice might not be compulsory. Becker (2005) recommends eliminating controls that have little or no relationship with outcomes, offering $|r| < .10$ as a potential cut-off for inclusion. Ultimately, considering one's research question and the study context, these variables can sometimes prove useful, and careful thought should go into each variable modeled—for as Kurt Lewin professed, "nothing is so practical as a good theory" (1945, p. 129). For instance, we see that sex does actually matter for turnover when considering fit (i.e., the sex makeup of the organization). Yet, it might be less impactful in other situations. On the other hand, age, tenure, and having children do seem to matter across a wider breadth of contexts. Controlling for these latter demographics might therefore be more routinely appropriate (Becker et al., 2016; Bernerth & Aguinis, 2016).

3.2 | A road map for future turnover research

So far, this paper has focused on the past and present. We solidified past findings, learned about new promising predictors, and highlighted the contextual nature of turnover by finding support for our fit, job market, and contagion arguments. The final goal of this paper was to use the results to identify the most pressing needs for future research. We present the following 10 recommendations targeted toward study design and broader methodological improvement.

3.2.1 | Recommendation #1: Measure voluntary turnover behavior

During our primary article search, it was surprising to find a large portion of studies that treated cognitions or intentions to quit as proxies for turnover behavior or studies that failed to distinguish voluntary from involuntary exit. First, although intentions to engage in a behavior are the best predictor of engaging in that behavior (Ajzen & Fishbein, 1980), our estimated effect size for withdrawal cognitions and turnover ($\rho = .56$) demonstrates that the two are not identical and should not be treated as such. We would argue that doing so might mislead researchers as to what antecedents are most predictive (i.e., not all things that predict cognitions similarly predict behavior) and could yield improper conclusions if mediation tests model intentions as the endogenous outcome. In a predictive sense, if one were to examine the relationship between job attitudes and turnover intentions, this effect size would likely decrease as a function of increased time between measurement, based on common method variance issues (Podsakoff, Mackenzie, Lee, &

Podsakoff, 2003). However, if behavior were the outcome, this relationship would likely *increase* with greater measurement separation (Holtom, Tidd, Mitchell & Lee, 2013), as more employees would have had more time to make a quit decision. As such, although intentions can indeed be a legitimate research interest—to identify those who might be on the verge of leaving, in order to intervene—it is fallacious to assume that the presence of one (i.e., thoughts) assures the other (i.e., behavior). Instead, for the literature to progress, researchers must measure turnover behavior *qua* turnover behavior and must test their research questions appropriately (e.g., with hazard modeling, logistic regression, or network analysis, as opposed to ordinary least squares methods; Hom et al., 2017). As we mentioned earlier, there is also room for more research on why intentions *do not* lead to action (e.g., Allen, Weeks, & Moffitt, 2005; Vardaman, Taylor, Allen, Gondo, & Amis, 2015).

3.2.2 | Recommendation #2: Pursue combinational approaches

Merely studying a motley collection of predictors does not capture the likely complexity among relationships. As such, we recommend that researchers emphasize predictor combinations in more thoughtful, theory-driven ways. For instance, Hom et al. (2012) proposed a combinational approach by introducing the idea of "proximal withdrawal states." They proposed four proximal withdrawal states derived from two dimensions: the employee's desire for leaving or staying and the employee's perceived control over this decision (whether they can freely act on their desire or are bound by external factors). Crossing these two dimensions leads to four groups of employees: those who want to leave and can (enthusiastic leavers), those who want to leave but think they cannot (reluctant stayers), those who want to stay and can (enthusiastic stayers), and those who want to stay but think they cannot (reluctant leavers). Although empirical work has begun to test this theory (Li, Lee, Mitchell, Hom, & Griffeth, 2016), such ideas are a compelling example of the kinds of combinational approaches the literature needs to pursue in order to increase prediction precision. Alternatively, one could consider creating employee profiles: For example, Stanley, Vandenberghe, Vandenberg, and Bentein 2013 developed profiles based on employee reports of affective, normative, and continuance commitment, found this to improve turnover prediction (see also Maertz & Campion, 2004).

3.2.3 | Recommendation #3: Augment the standard predictive design

In the 1970s, the turnover literature made a significant leap by adopting what was later called the "standard research design" (Hom et al., 2017; Steel, 2002). Since then, the typical turnover study measures predictors at Time 1 and turnover at Time 2, typically with a 3- to 12-month lag (the mode study lag time we observed was 12 months). This design has advantages in terms of temporal separation to assess causality but also has drawbacks: By not capturing what happens between measurement points, data useful to prediction are foregone (e.g., attitudinal shifts, disruptive events, unexpected job offers). Furthermore, by measuring most all predictors at Time 1, one cannot meaningfully depict the process of how individual turnover unfolds—that is, the process of accumulating dissatisfaction, lessening commitment and embeddedness, searching for alternatives, and other withdrawal feelings and behaviors. Although our results help identify what predictors hold promise in a general sense, the standard predictive design constrains many conclusions that can be gleaned from reality. With the next three recommendations, we therefore broadly propose some methodological shifts so that future meta-analytic work can move to more complex multilevel path models.

3.2.4 | Recommendation #4: Capture turnover processes in real time to establish temporal order

Considering that most studies measure predictors at one time and turnover at another, the field is not currently in a strong position to draw definitive conclusions regarding the temporal nature of exit as it unfolds. We appear to be in need of studies that measure multiple turnover-related predictors at multiple measurement points to establish the proper temporal sequence of variables, to and more precisely ascertain the complete chain of employee withdrawal. For example, a study that simultaneously measures the most predictive proximal constructs—cognitions, satisfaction, commitment, embeddedness, search, lateness, absenteeism, and alternatives—at multiple times would be a great starting point to determine which sequence fits the data best (and also perhaps assess how and why this might vary between individuals). Such a study may not seem very sexy because it would not introduce any new constructs, but a clearly

defined temporal model that competitively tests theoretical sequences could offer significant insight into how these mediators interrelate and interact (for example, do employees engage in job search and lateness simultaneously, does one precede the other, for whom does either apply, when does it apply, and why?). Such knowledge would also increase our ability to develop more precise practical counsel regarding the avoidance of unwanted turnover in the workplace.

3.2.5 | Recommendation #5: Measure events

The typical predictive design of assessing predictors at Time 1 and turnover at Time 2 does not allow one to capture key events that may occur between measurement periods. Increasing the number of measurement points, coupled with qualitative or quantitative inquiry about events, can help alleviate this concern (*cf.* Morgeson, Mitchell, & Liu, 2015). For example, the unfolding model of turnover suggests that shocks significantly alter how employees think about attachment to an employer. Hale, Ployhart, and Shepherd (2015) recently showed how specific individual employee and/or manager turnover events can negatively affect the collective performance of bank branches, as turnover disrupts a unit's core processes, and the branch must subsequently recover by rebuilding its lost social and human capital. Relatedly, Ballinger, Lehman, and Schoorman (2010) found that if a leader were to quit, subordinates who had high-quality exchange relationships with that leader would be more likely to quit themselves after the succession event, although they would be more likely to stay if no leader succession event had occurred (see also Ballinger & Schoorman, 2007; Shapiro, Hom, Shen, & Agarwal, 2016). Given these findings, those who report high satisfaction at Time 1 may suddenly quit due to unforeseen events (e.g., unsolicited offers, health concerns, pregnancy, turnover of a close colleague, manager turnover, etc.) that occur subsequent to that assessment. However, the majority of current work would not capture such dynamics, thereby limiting explained turnover variance.

3.2.6 | Recommendation #6: Examine predictor change over time

Whereas Recommendation #4 was a call to examine various turnover-related variables over time in order to improve the accuracy of temporal sequences regarding how quitting unfolds, Recommendation #6 is an explicit call for researchers to continue to examine how predictors change over time (i.e., growth, decline, or stagnancy). Most research providing the foundation for this meta-analysis has taken a static approach, where turnover at Time 2 is regressed on an individual's predictor level at Time 1 (Steel, 2002). Such an approach assumes that predictors remain largely stable over time. Yet, research has established that this assumption is flawed, many variables do fluctuate significantly, and such changes are systematically meaningful (Kammeyer-Mueller, Wanberg, Glomb, & Ahlburg, 2005). Chen, Ployhart, Thomas, Anderson, and Bliese (2011) found that job satisfaction and turnover intentions both changed over time for many participants. Similarly, Hausknecht, Sturman, and Roberson (2011) found employee justice perceptions varied over the course of just one year. Even seemingly stable dispositions may undergo change (Judge, Simon, Hurst, & Kelley, 2014; Wille, Hofmans, Feys, & De Fruyt, 2014). Thus, adopting a dynamic view and accounting for trajectories (i.e., using latent growth modeling) can significantly increase turnover variance explained. Notably, Liu et al. (2012) calculated employee job satisfaction trajectories, incorporating reports at three time points. Such trajectories predicted turnover after controlling for average levels. Bentein, Vandenberg, Vandenberge, and Stinglhamber (2005) also found that employees experiencing steeper declines over time in their organizational commitment were more likely to quit. We encourage future researchers to build on such results. For instance, studies might examine whether and how other predictors (e.g., absenteeism, job search, job embeddedness, work–life conflict) exhibit similar change over time, and whether this change predicts quitting. Equally, the degree of change over time is also worth consideration: More measurement occasions at shorter intervals could enhance precision as to just how much some predictors fluctuate compared to others. For example, whereas Hausknecht et al. (2011) looked at justice change on the order of one year, Matta, Scott, Colquitt, Koopman, and Passantino (in press) found justice perceptions to vary within just a 3-week period. Researchers might also consider the interaction of mean predictor levels and predictor change over time in predicting exit. It is possible that before deciding to quit, an employee reference past changes over time in his or her attitudes (i.e., as having improved, worsened, or stayed the same) as a means of putting present attitude levels into context. Such an interaction might account for unique variance beyond mean levels or trajectories alone.

3.2.7 | Recommendation #7: Increase attention to additional antecedent–turnover moderators

We strongly encourage researchers to systematically continue to examine predictors with high effect variability (i.e., $SE \geq .03$), such as fit, climate, and job security. For example, for fit, Vidyarthi, Erdogan, Anand, Liden, and Chaudhry (2014) found that for employees with two leaders, satisfaction and turnover were influenced by the fit (mis)alignment in relationships that employees have with their two leaders. For many of these predictors, simply an increase in the quantity of studies can be illuminating insofar as additional data bolsters confidence about the stability (or fragility) of effect sizes across contexts. It would also be valuable to explore the interactive effects of these predictors with other turnover-relevant concepts. For example, there is evidence that locus of control moderates the withdrawal cognitions-turnover link such that those with an external locus are more likely to turn thoughts into behavior (Allen et al., 2005).

3.2.8 | Recommendation #8: Scrutinize contextual multilevel influences

We noted earlier that many predictors capture contextual and social influences on employee turnover. Constructs such as peer relations, climate, and leadership highlight how others' behaviors and attitudes can influence how employees interpret their own work situation. Plus, our moderation analyses provide support for the general notion that context can attenuate or amplify certain variables, especially as it highlights personal fit, the job market, and workforce withdrawal tendencies. With advancements in multilevel analytical techniques, we are now in a position to model the influence of social networks, other-rated perceptions, and meso/macro-contextual indicators of turnover. A compelling example of this type of research comes from Liu et al. (2012), who not only examined individual-level satisfaction trajectories but also unit-level satisfaction agreement effects. We advocate for future research to take similar multilevel approaches and possibly even combine them with other recommendations we have offered.

3.2.9 | Recommendation #9: Test meta-analytic moderators within the scope of the data

Although we intended to be as comprehensive as possible, it was also important to only examine moderators with sufficiently accumulated effects so as to draw reliable conclusions (i.e., to limit Type I and Type II error rates). Ultimately, this issue speaks to broader concerns about moderator testing in meta-analysis, and for the turnover literature in particular: Due to the nature of meta-analytic data, moderation tests are limited in the types of questions that can be answered. If moderation is an interaction among two or more variables in predicting an outcome, a primary study would need only create a product between an independent variable and moderator (or moderators), and regress that outcome on the main effects and product (Cohen, Cohen, West, & Aiken, 2013). This is relatively straightforward because data from each respondent on each variable are usually available (barring missing responses). With meta-analysis, however, one must rely on summary data (e.g., correlations, sample averages) not individual data points. As such, this regression approach cannot be performed in a meta-analysis unless correlations are provided for both main effects–outcome *and* interactions–outcome—and they rarely are. Thus, meta-analytic interactions must be computed differently. One alternative is to correlate each study's moderator level with a respective predictor–outcome effect size; another is to separate effect sizes into subgroups, such as with results from individualistic versus collectivistic cultures (Choi, Oh, & Colbert, 2015), public versus private organizations (Jiang et al., 2012), military versus civilian samples (Griffeth et al., 2000), or others. Yet, there are problems with these two methods, particularly with regard to not appropriately weighing studies by sample size, as well as statistical power concerns (Aguinis, Beaty, Boik, & Pierce, 2005; Aguinis et al., 2013; Steel & Kammeyer-Mueller, 2002).

To remedy these issues, we recommend that researchers test for meta-analytic moderators using a WLS regression approach as done here and in other recent meta-analyses (e.g., Heavey, Holwerda, & Hausknecht, 2013), and to limit moderation tests to only those variables on which a sufficient number of primary studies are based. At the time of this writing, the subgroup method seems to be most popular approach. Yet, many such subgroup tests are based off of an already-limited number of studies, so dividing them further weakens our confidence in true effect size differences, because lower-k estimates have wider confidence intervals that are more likely to overlap with the other

subgroup (Quiñones, Ford, & Teachout, 1995). Further, such tests make little sense if one subgroup is disproportionately represented. Should Subgroup A have 14 studies and Subgroup B only 1 study, it becomes difficult to confidently say that the two subgroup effect sizes truly differ, even if a Z-test is significant. This realization led us to a final recommendation.

3.2.10 | Recommendation #10: Further study underexplored cultures and occupations

Most turnover studies we reviewed came from Western countries (e.g., the United States and Canada), with fewer explorations in other cultures (e.g., East Asia, Africa, Latin America and South America) that likely have different norms and construct meanings. With more studies in such contexts, we might see different estimates. For instance, in China and other paternalistic cultures, leadership often extends beyond consideration, including benevolent behaviors such as helping employees with problems at home (Chen, Eberly, Chiang, Farh, & Cheng, 2014). As such, leadership might more strongly affect turnover in such cultures. Yet, with only 7 out of 42 leadership-turnover studies from outside the United States, it is difficult to determine just how much they differ. Similarly, only 4 out of 55 studies on rewards besides pay were conducted outside North America. Perhaps this effect is downwardly biased, and would be stronger with more international samples, for instance, given the more generous benefits mandates in Europe compared to the United States, like paid maternity/paternity/sick leave and health insurance (Glassdoor Economic Research, 2016). Even among these Western samples, though, we also noticed a high degree of occupational homogeneity, with most studies coming from civilians in private organizations performing white-collar or hospital work. For the pan-occupational and cross-cultural generalizability of our results to hold, it is imperative that future work focuses on turnover prediction in other cultures and occupations, to determine if, when, and how certain constructs operate differently, but most importantly, why they do so.

3.3 | Practical implications

Organizations are rightfully interested in curtailing unexpected or unwanted turnover to protect social capital and organizational memory, and to reduce sizable expenses of onboarding newcomers (Allen et al., 2010). Our results offer useful insights for the practitioner community.

Allen et al. (2010) discussed how managers commonly believe that employees quit in order to take higher paying positions elsewhere. We find that the relationship between pay and turnover has increased since Griffeth et al. (2000), suggesting that the influence of pay on exit decisions is perhaps stronger today than it was two decades ago. Yet, as with the Griffeth et al. findings, many other predictors more readily controlled by managers can be more important than pay. One such factor includes rewards besides pay (e.g., training or promotional opportunities, bonuses, and non-cash benefits). Other predictors include job characteristics, leadership, climate, and organizational support. A prevailing thought many managers hold about turnover is that it is mostly due to dissatisfaction with the work itself or low pay, and because "the job is the job," rarely can anything be done to remedy high quit rates (or, they simply accept them as bearable). To the contrary, our results corroborate the notion that often, "employees quit bosses, not jobs," and that at least as much, turnover can be due to toxic work climates or feeling unsupported by the organization. In these respects, we would argue that leadership development programs might be especially valuable to retention. Such efforts might not just focus on how leaders can develop strong relationships with followers but also on how leaders can serve as a bridge between subordinates and higher-up organizational stakeholders, as well as on how to build a climate that leverages the idiosyncratic strengths of the company (Hackman & Wageman, 2007).

Equally important are the relatively strong effects for withdrawal attitudes, job market perceptions, and withdrawal behaviors. In this regard, gauging ratings on such predictors would be highly informative. Problematically, though, many employees might be reluctant to share such revealing information with their organizations, and any responses they do share might be biased so as to appear socially desirable. Some viable options to obtain such data might include ensuring response anonymity, obtaining other reports of these perceptions/behaviors, bringing in neutral consultants to administer surveys, cultivating a work environment where employees know they will not be reprimanded for sharing

opinions, or by using unobtrusive measures like absenteeism rates or monitoring how employees use company time (Kerlinger & Lee, 2000).

Another major focus of retention efforts in today's organizations is work engagement (Bersin, 2014; Graber, 2015). Companies like Gallup and Deloitte continuously update managers about employee engagement levels: One survey found that 79% of businesses are seriously worried about engagement and retention (Adkins, 2016; Deloitte Consulting Group, 2014). Employee engagement is a relatively new phenomenon within the turnover literature, and we could only identify four primary studies linking it to turnover behavior. Although we must be careful about overinterpreting this effect based on the limited evidence, engagement does appear to be a useful predictor. Thus, efforts to increase engagement would likely be valuable. Specifically, the major psychological drivers of engagement include experienced meaningfulness of work, psychological safety of the work environment, and availability to engage one's personal resources at any given moment (Crawford, Rich, Buckman, & Bergeron, 2014; Kahn, 1990). We identified a host of antecedents that would align with these drivers, such as improving job characteristics and rewards/pay to improve meaningfulness, monitoring leader behaviors, climate, justice, and job security to facilitate safety, and a (lack of) work–family conflict, role conflict, or stress to sustain availability. To the degree that managers attend to one or more of these drivers, we would expect engagement to improve, along with subsequent employee retention rates.

It is also prudent to discuss what our results mean for employee selection. Of course, due to equal employment opportunity concerns, we cannot advise organizations to select individuals based on their age, marital status, or how many children they have. However, other prehire predictors may be quite useful to curb turnover. For instance, managers might evaluate job applicants' perceived fit with the organization in terms of values and personality. Dispositionally, motivational metrics such as performance efficacy or personal goal setting would also be promising, and we would echo Zimmerman's (2008, p. 335) claim to the utility of personality inventories focusing on emotional stability and conscientiousness (as well as CSEs, as noted earlier). Other studies (e.g., Barrick & Zimmerman, 2005) have also advocated for using biographical data for selection. We concur, specifically for things like past job tenure. Given that companies often hire hundreds of new employees each year—even more for those with regularly high turnover rates—differentiating applicants even by one point on these metrics could meaningfully reduce turnover, resulting in substantial cost savings. There have also been efforts to predict commitment propensity and quit intentions during selection (Lee, Ashford, Walsh, & Mowday, 1992), so similar steps could be taken to select individuals prone to higher attitudes or embeddedness (Barrick & Zimmerman, 2005; Choi et al., 2015; Judge, Heller, & Mount, 2002).

3.4 | Limitations

There are limitations to this paper that must be acknowledged. First, treating dichotomous turnover behavior as continuous might be considered controversial, as correcting correlations can lead to an increase in sampling error variance of the adjusted meta-analytic effect distribution (Hunter & Schmidt, 2004). As such, this procedure may have inflated the results. We would advise readers to consider the uncorrected and corrected correlations when interpreting the data.

Second, as a meta-analysis, our results may be biased in favor of the most studied constructs, despite newer, equally predictive variables being overshadowed. Although we did try to incorporate many into our model, this article is not the final word as to what employees consider when they decide to quit. Exciting developments in the areas of dyadic work relationships (Chiaburu & Harrison, 2008), commitment reconceptualized as identification or internalization (Klein, Molloy, & Brinsfield, 2012), and emotional labor (Kammeyer-Mueller et al., 2013) may prominently figure into future discussions of employee turnover and empirical study.

Third, and as noted in Recommendations 9 and 10, although we aimed to be as comprehensive as possible in testing moderation hypotheses, we were unable to examine every moderation relationship, and some tests still had low power, despite inclusion restrictions. Thus, although many predictors showed significant effect size variability, we were only able to test hypotheses on a selection of them. Future studies should expand our moderation tests to the full range of predictors and study how results might differ as a function of other contextual factors.

4 | CONCLUSION

This paper reports the most comprehensive analysis of the individual-level voluntary turnover literature to date. In surveying this dense forest, we revealed an array of distal and proximal factors that contribute to exit decisions, while highlighting the context-sensitive nature of this phenomenon. Given the results, as well as the challenges in the existing literature uncovered throughout this work, we advocate for a paradigm shift in turnover research that embraces cutting-edge methodologies to capture the dynamic and multilevel ways by which turnover decisions unfold. It is our intention that this study not be a conclusive statement to turnover study, but rather a checkpoint, to take stock of where we have been and to offer a practical guide as to the most promising avenues for future inquiry.

NOTES

[1] From this point forward, when we use the term turnover, we refer to individual-level voluntary turnover.

[2] In these analyses, we did not consider multiorganization samples, unless the article separately reported mean levels of a variable for each organization studied. Employees are likely only influenced by their own organizational context, such as the mean level of job satisfaction of their own organization. Mean job satisfaction across multiple organizational samples is therefore less meaningful to examine.

[3] Due to the substantial number of studies ($k = 316$) included in the meta-analysis, we only include those references that are cited in text. The full coded article list is available upon request from the first author.

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