

An Updated and Expanded Meta-Analysis of Nonresident Fathering and Child Well-Being

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Since Amato and Gilbreth's (1999) meta-analysis of nonresident father involvement and child well-being, nonmarital childbirths and nonresident father involvement both have increased. The unknown implications of such changes motivated the present study, a meta-analytic review of 52 studies of nonresident father involvement and child well-being. Consistent with Amato and Gilbreth, we found that positive forms of involvement were associated with benefits for children, with a small but statistically significant effect size. Amounts of father-child contact and financial provision, however, were not associated with child well-being. Going beyond Amato and Gilbreth, we analyzed the associations between different types of fathering and overall child well-being, and between overall father involvement and different types of child well-being. We found that nonresident father involvement was most strongly associated with children's social well-being and also was associated with children's emotional well-being, academic achievement, and behavioral adjustment. The forms of father involvement most strongly associated with child well-being were involvement in child-related activities, having positive father-child relationships, and engaging in multiple forms of involvement. Moderator analyses demonstrated variation in effect sizes based on both study characteristics and demographic variables. We discuss the implications of these findings for policy and practice.

Keywords: child well-being, father involvement, meta-analysis, nonresident fathers

In 1999, Amato and Gilbreth published *Nonresident Fathers and Children's Well-Being: A Meta-Analysis*. Since its publication, the Institute for Scientific Information's 2011 Web of Science database reports that this article has been cited over 250 times, and there is no way of estimating how frequently it has been referenced by scholars of fathering, divorce, and child well-being, as well as policymakers, the media, and others interested in the effects of fathers on children. Numerous changes in policies and families in the years since its publication, however, suggest that a new look at this issue would be beneficial.

The "new era of fatherhood" has entered into both research and popular discourse (Gregory & Milner, 2011). Increasingly, fathers are expected to be more than breadwinners, and positive nonresident fathering is associated with financial, psychological, physical, academic, behavioral, and social benefits for children (Marsiglio, Amato, Day, & Lamb, 2000). Fathers' financial contributions remain important too; without them, children are more likely to live in poverty and have poorer academic, behavioral, and emotional outcomes (Marsiglio et al., 2000).

At the same time, barriers continue to exist for nonresident fathers' positive involvement with their children. The number of

children born and raised outside of marriage continues to grow; approximately 40% of all births are nonmarital, with up to two thirds of African American children born outside of marriage. Such numbers partly reflect an increase in cohabiting, unmarried couples (Martinez, Daniels, & Chandra, 2012), but the majority of U.S. children still are likely to experience a single-parent household at some point (Wu, 2008). The number of families with joint legal custody arrangements is increasing, reflecting a gradual policy shift from viewing nonresident fathers as sources of child support to viewing them as nurturing and involved parents (Meyer & Garasky, 1993). However, the vast majority of children in single-parent households still live with their mothers (Grall, 2006). Amato, Meyers, and Emery (2009) found that levels of nonresident father involvement have increased significantly over the last three decades; they also noted that this might or might not be beneficial for children, depending on context and the quality of the involvement.

With these changes and issues in mind, we conducted an updated and more nuanced meta-analytic review of the literature in this area. In addition to updating the literature, we also analyzed differences in effects according to types of father involvement and forms of child well-being, as well as moderation of these associations by demographic and study characteristics.

Defining Nonresident Fathering and Its Effects on Child Well-Being

Researchers have conceptualized and operationalized father involvement in a variety of ways, which complicates the process of determining which forms of fathering are most beneficial to chil-

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dren. In addition, child well-being encompasses a vast array of outcomes, from academic to behavioral to emotional. In the present study, we use the conceptual framework proposed by Palkovitz (2007) to organize the forms of father involvement included in our study: affective climate (e.g., connection, being there, involvement), behavioral style (e.g., monitoring, control), and relational synchrony (e.g., sensitive parenting, teaching). Palkovitz (2007) proposed that these three forms are “the things that matter in father–child relationships” and which transcend context (p. 193). We chose to use this general framework, rather than a specific theoretical lens, to capture the broadest possible swath of research on the topic of nonresident fathering. Limiting analyses to studies that used only a particular theoretical lens would decrease the number of studies included and thus limit the utility of this meta-analysis.

We operationalized the affective climate of fathering as including the quality of father–child relationships and involvement in children’s activities. Both fathers and children frequently mention the importance of fathers “being there” for their children (Roy, 1999), and we suggest that fathers being there is highly salient for the affective climate of nonresident fathering. High quality relationships between fathers and children reflect an emotional climate where children believe that their nonresident fathers will be there when they need them. In addition, father involvement in children’s activities (e.g., helping with homework or attending school functions) further creates a positive emotional climate surrounding fathering.

Much research regarding nonresident fathers has focused on their behavioral style. Here, we conceptualize this dimension as the level of fathers’ contact and financial provision. Palkovitz (2007) originally included “provision” as part of affective climate, but we chose to conceptualize it as part of fathers’ behavioral style for two reasons. First, provision reflects a behavior more than an emotional quality. Second, provision is closer conceptually to the amount of time spent than to the variables included in affective climate, such as father–child relationship quality. We did not assess relational synchrony, as we found no studies that assessed nonresident fathers’ roles as mentors or teachers.

The Importance of Study Quality and Characteristics

In addition to analyzing direct associations between nonresident fathering and child well-being, we also were interested in whether the strength of these associations (i.e., effect sizes) varied according to methodological and sample characteristics. Methodological characteristics of interest here were whether the data were nationally representative, who reported on father involvement, and year of data collection. Sample representativeness was relevant given the increasing number of nationally representative datasets that have become available to fathering scholars over the past 20 years (e.g., National Longitudinal Survey of Adolescent Health, National Survey of Families and Households). We investigated possible differences in effect size based on report of father involvement because mothers typically underreport and fathers typically overreport father involvement, especially in families where fathers are nonresident and mothers cannot always directly observe fathers’ behaviors (Wical & Doherty, 2005). We included year of data collection to address possible cohort effects, as both societal

norms and formal policies regarding nonresident fathers have changed substantially.

We included sample characteristics in our moderator analyses when possible. Prior research has suggested that fathering behaviors differentially affect children depending on factors such as child sex, age, and ethnicity (e.g., Fagan & Palkovitz, 2007), and maternal, paternal, and child characteristics also are related to attitudes toward and levels of father involvement. Unfortunately, few studies included consistent sample information; we conducted moderator analyses for sample characteristics when they were available for at least 10 studies. Sample-related moderators included here were average age of children, percentage of non-White mothers, and average mothers’ household income. Unfortunately, few studies included information about fathers’ characteristics, so we could not test paternal moderators.

The Present Study

Given the numerous changes in society and families over the past 15 years, the aim of the present study was to add to our understanding of the ways in which nonresident fathering is associated with various aspects of children’s well-being. Like Amato and Gilbreth (1999), we first examined overall associations between nonresident father involvement and child well-being. The present study expands upon Amato and Gilbreth’s meta-analysis by conceptualizing both nonresident father involvement and child well-being (rather than just father involvement) as multidimensional constructs. Thereafter, we examined associations between overall father involvement and specific types of well-being and between specific types of involvement and overall well-being. Finally, we examined the influence of potential moderating variables on each set of associations, in the form of study and sample characteristics, to investigate circumstances under which associations between fathering and well-being might be strengthened or diminished. Each of these analyses fills important gaps in our understanding of nonresident fathers and their children. Such analyses also allow underresearched areas to be recognized and recommended for future investigations so the field can continue to move forward.

Method

Literature Search

We identified potential studies via a search of social science (PsycINFO, ERIC, Sociological Abstracts, Academic Search Premier, MasterFILE Premier, Psychology and Behavioral Sciences Collection, and Social Work Abstracts) and law (LexisNexis) databases for the terms “father,” “child,” “nonresident,” “divorce,” “never married,” “unmarried,” “noncustodial,” “child outcomes,” and/or “child well-being.” We also reviewed articles included in Amato and Gilbreth (1999). We chose to include studies from the original meta-analysis, provided they met our criteria, for two reasons. First, including studies from the original meta-analysis provided the most complete test of our research questions regarding changes in the relationship between nonresident father involvement and child well-being over time. Including only studies published since 1998 (the last year of studies included in the original meta-analysis) would have severely limited our ability to investi-

gate that question and also would have limited the power of our analyses. Second, the current study not only updates the original meta-analysis but expands it considerably by dis-aggregating forms of father involvement and types of child well-being and testing additional moderating relationships. Such analyses were not performed by Amato and Gilbreth; therefore, including studies from the original meta-analysis enabled us to conduct these additional analyses on the full sample of literature.

Inclusion and Exclusion Criteria

We initially reviewed 208 abstracts to determine the relevance of each study. From those, we retrieved 82 studies for further review, including only peer-reviewed journal articles or book chapters. Of these, we retained only those studies in which authors tested quantitative associations between at least one type of non-resident father involvement and at least one type of child well-being. Studies were excluded if they presented undifferentiated analyses for both resident and nonresident fathers (10 studies) or if there was insufficient information to calculate effect sizes (16 studies). We contacted the authors of the latter 16 articles in an attempt to obtain additional data, and three additional articles were included in this way; 13 authors did not respond or were unable to obtain the information we requested. This resulted in a final sample of 52 studies (164 effect sizes). Table 1 shows descriptive information for each study.

Coding Procedures

The first author coded all of the articles; the second author independently coded 16 articles (28 effect sizes) to establish interrater reliability. Interrater percentage agreement was 90% for coding of effect sizes. There was 100% agreement for all moderators, except for 93.75% agreement on reporter of father involvement. Disagreements were resolved through discussion and both raters agreed on final codes.

Father involvement. Father involvement was coded into five categories, representing two of the three fathering themes proposed by Palkovitz (2007; themes are noted in parentheses after each category). Involvement in activities (*affective climate*) assessed how frequently fathers engaged in various child-related activities such as putting the child to bed, helping with homework, changing diapers, or attending school functions. Father-child relationship quality (*affective climate*) included measures of father-child closeness, trust, and support. Father-child contact (*behavioral style*) typically included the number of times fathers had seen, talked to, or otherwise had contact with children. Financial provision (*behavioral style*) was assessed via items such as child support and buying clothing, food, or toys. A fifth category was created for multiple forms of involvement when a study combined involvement types into a single variable. We originally included categories for decision-making and positive parenting behaviors, but insufficient studies assessed these forms of involvement for them to be meta-analyzed. In all categories, higher scores indicated greater involvement.

Child well-being. Child outcomes were coded into four categories, all coded such that higher scores indicated greater child well-being: psychological/emotional well-being (e.g., life satisfaction, depression [reverse coded], anxiety [reverse-coded]); cogni-

tive well-being/academic achievement (e.g., grades, test scores, IQ); behavioral outcomes (e.g., delinquency, externalizing behavior [both reverse-coded]); and social outcomes (e.g., peer and romantic relationships). Originally, we also included physical well-being (e.g., obesity, illness), but only two studies assessed this outcome, therefore, it could not be included in analyses.

Moderator variables. We coded several methodological and sample characteristics as moderators of the associations between involvement and well-being. Study characteristics included whether the study sample was representative (yes/no; based on the study describing use of a nationally representative sample), the reporter of father involvement (mother, father, child, combination, or other), and the year of data collection (if not provided, coded as 2 years prior to publication). Sample characteristics included average age of children, percent of mothers who were non-White, and average household income for mothers. Although many other variables would have been interesting to test (e.g., coparenting conflict or father characteristics), these were the only variables for which there were enough studies to conduct moderating comparisons.

Calculation of Effect Sizes

We used the Pearson product-moment correlation (r) as our effect size. Bivariate correlations were available for 33 studies; for three others, we converted other effect sizes into correlations using formulas from Lipsey and Wilson (2001). Eleven studies reported nonsignificant findings without specific r values; for these, we set $r = 0$. For meta-analyses that utilize correlations as effect sizes, we used the recommended practice of transforming correlations prior to analyses using Fisher's Z_r transformation and, after analyses, converting the values back to the original metric using the inverse (Hedges & Olkin, 1985; Lipsey & Wilson, 2001).

Analysis Plan

Treatment of dependent effect sizes. Our original research questions concerned overall effect size as well as differences in effect size based on types of child outcomes and father involvement. Forty-two studies included more than one type of child outcome or father involvement. Of these, 18 studies contributed two effect sizes, seven contributed three, and 17 contributed four or more, up to a maximum of 12 (two studies). Combining all of these effect sizes together in one analysis would have introduced considerable nonindependence (i.e., studies with more than one outcome would be "counted" more than once and, thus, given more weight; Hedges, Tipton, & Johnson, 2010), which can bias results. Statistical procedures for dealing with such nonindependence, such as multivariate meta-analyses, have been introduced very recently (e.g., Hedges et al., 2010). These techniques, however, work best when each study contributes the same number and type of effect sizes so that direct comparisons can be made. The number of studies meeting those criteria was too small for us to conduct these types of analyses. Therefore, we instead conducted several univariate analyses on groups of studies to approximate multivariate results. This strategy avoided the problem of nonindependence but did not allow for direct comparisons (e.g., between outcome categories). We tested overall associations between non-resident fathering and child well-being, between a combined mea-

Table 1
Effect Sizes and Values on Selected Moderators for Studies Included in the Meta-Analysis

Authors (Year)	Data year	N	Effect size				Father involvement	
			Psyc	Cog	Beh	Soc	Reporter	Types
*Amato (1986)	1983	72	-0.08				3	3, 4
*Amato & Rezac (1994) [†]	1988	1,285			-0.11		2	3, 5
*Baydar (1988) [†]	1981	1,071	0.02				2	4
*Baydar & Brooks-Gunn (1994) [†]	1988	437		0.09			2	2, 4
*Bogenschneider (1997)	1988	598		-0.13			3	1
*Brand et al. (1988)	1986	40	0		0		2	4
*Bray & Berger (1990)	1985	98	0.09		-0.15		5	3, 4
*Brody & Forehand (1990)	1988	60	-0.19		0		3	3
*Bronstein et al. (1994)	1992	54	-0.33	-0.34	-0.37	-0.18	5	4
*Clark & Clifford (1996)	1994	102	-0.26			0.13	3	4
*Clingempeel & Segal (1986)	1984	40	0		0		2	4
Coley & Medeiros (2007) [†]	2001	647			-0.18		3	3, 5
Dunn et al. (2004) [†]	1997	162	-0.26		-0.19		3	3, 4
Flouri (2006)	2002	520			-0.13		3	4, 6
*Furstenberg & Harris (1993)	1987	253	0	0	0		3	4
*Furstenberg et al. (1987) [†]	1981	227	0.04	0.01	-0.01		2	3, 4
*Graham et al. (1994) [†]	1988	1,110					2	2, 4
Gunnoe & Hetherington (2004)	1999	143	-0.05				3	4, 5
Harper & Fine (2006)	1997	129	-0.01				1	3, 4, 6
Hawkins et al. (2007) [†]	1996	3,394	-0.12	-0.08	-0.09		3	2, 5
*Healy et al. (1990)	1998	121	0.07		0.04		3	3, 4
*Hess & Camara (1979)	1977	16	-0.07	-0.13	0.03	-0.08	4	4
*Hodges et al. (1979)	1977	26	0.06		0.52	0	2	4
*Hodges et al. (1983)	1981	30	0		0		2	4
*Hoffman (1995)	1993	106			-0.36		1	5
Jackson et al. (2009)	2006	86		-0.24	-0.24		2	4
*Jacobson (1978)	1976	38	-0.05	-0.13	-0.24		2	1, 4
*Johnston et al. (1989)	1983	100	0.42				1	4
Jones (2004)	2001	25		-0.46			3	3, 4
Jones et al. (2003)	2001	25	0.13				3	4
*Kalter et al. (1989)	1987	56	0		0		2	4
King & Heard (1999) [†]	1988	1,172	-0.06		0		2	4
King & Sobolewski (2006) [†]	1994	453	-0.08	-0.14	-0.06		3	2-4, 6
*Kline et al. (1991)	1985	97	0.04		0.06		5	3, 4
*Kurdek (1986)	1984	91	0.01		-0.01		2	2, 4
*Kurdek & Berg (1983)	1981	70	-0.12				2	4
*Lee (1997)	1995	58			-0.05		2	4
*Machida & Holloway (1991)	1989	58	0.12	-0.15	0.1		2	4
*MacKinnon (1989)	1987	48				-0.25	2	3
*McLanahan et al. (1994) [†]	1988	844		0.05			2	2, 4
Menning (2006) [†]	1996	2,544		0.15			3	1-5
Mitchell et al. (2009) [†]	1996	4,663	0	0	0		3	1-4
*Simons et al. (1994)	1992	134	-0.12		-0.16		5	2, 6
*Southworth & Schwarz (1987)	1985	52				-0.23	3	3, 4
Sprujit et al. (2004)	1997	164	-0.19		0		3	3, 4
Stewart (2003) [†]	1996	1,469	-0.10	-0.17	-0.04		3	1, 3, 6
*Stolberg & Bush (1985)	1983	82	-0.12	0	0		2	4
*Thomas & Forehand (1993)	1991	58	-0.30		-0.26		1	3, 4
*Thomas et al. (1996)	1992	503			0		3	4
Vogel et al. (2006) [†]	1995	1,804	-0.19		-0.17		3	1, 3, 4
*Warren et al. (1987)	1983	86			-0.34		2	4
*Zill (1988) [†]	1981	1,084	0	0	0		2	4

* Denotes a study that was included in the Amato and Gilbreth (1999) meta-analysis. † Denotes that a study drew from a representative sample (although all such studies focused on subsamples of the original representative sample; e.g., children with nonresident fathers). For reporter of father involvement, 1 = father, 2 = mother, 3 = child, 4 = other, 5 = multiple. For father involvement types, 1 = involvement in activities, 2 = financial/material provision, 3 = father-child relationship quality, 4 = contact, 5 = multiple types of father involvement combined, 6 = positive parenting behaviors.

sure of involvement and specific child outcomes, and between specific forms of involvement and a combined measure of well-being. Taken together, these three sets of analyses approximate the results of multivariate meta-analysis. Alone, each analysis is untrustworthy and would be an inadvisable strategy. Combined,

however, they gave a preliminary test of the nuanced relationships we were interested in testing. Further, inspection of the joint distribution of effect sizes based on form of father involvement and child outcome type showed that categories of father involvement were relatively well distributed across child outcomes (i.e.,

studies measuring one kind of father involvement were not more or less likely to measure a specific child outcome).

Models. We chose to compute a random effects mean, which assumes that effect sizes vary systematically due to characteristics other than sampling error (i.e., moderators; Cooper, 2010). We then examined heterogeneity of effect sizes using Q (Hedges & Olkin, 1985) and I^2 (Higgins & Thompson, 2002). The Q statistic tests whether any particular effect sizes differ from the others only by sampling error (i.e., whether moderator analyses are warranted). It is underpowered in meta-analyses with few studies, however, and overpowered with many studies; therefore, we used an additional index of heterogeneity, the I^2 (Higgins & Thompson, 2002). Similar to an intraclass correlation, I^2 represents the amount of variability in a set of effect sizes due to between-studies variation (i.e., true heterogeneity) and assesses the magnitude of heterogeneity.

Moderator analyses can use either fixed effects or mixed effects models. A fixed effects model assumes that the variation in effect sizes can be explained by characteristics of the effect sizes or studies themselves (Lipsey & Wilson, 2001) and any remaining variation is due to sampling error. A mixed effects model assumes that some variation in effect sizes can be explained by moderators, but there is remaining random variation. These models often are conceptually more plausible but their use substantially decreases statistical power to detect moderator effects (Hedges & Pigott, 2004). Thus, we used a fixed effects model here.

For categorical moderators, we used the meta-analytic equivalent of analysis of variance (Hedges, 1982), which tests homogeneity of effect sizes within moderator categories and differences between effect sizes in each category. For continuous moderators, we used meta-analytic regression (Hedges & Olkin, 1985). To facilitate interpretation, average child age and average mothers' household income were grand mean centered, such that the intercept represented the effect size at the average value across studies. Household income was divided by 100 prior to centering to make variable scales more congruous. We did not grand mean center percentage of non-White mothers, as that had a meaningful 0 value (i.e., only White mothers included in the sample). For year of data collection, we subtracted 1976 (the earliest year of data collection) from each study's value so the intercept represented the effect size

for the earliest year of data collection. We conducted all analyses using Wilson's (2002) macros in SPSS 19.0.

Results

Main Effects of Nonresident Father Involvement on Child Well-Being

We first investigated the overall effect size for the relationship between nonresident father involvement and child well-being. We conducted analyses in this area on three separate groups of studies: the overall group of studies, by child outcome type, and by father involvement type. Table 2 shows the results for each of these analyses, described below in further detail.

Overall sample of studies. First, we computed the average effect size for each study across all forms of child outcome and father involvement type. This analysis preserved independence of effect sizes but did not allow for comparisons across either child outcome or father involvement type. The mean effect size was small but statistically different from zero; nonresident father involvement was positively associated with child well-being. Homogeneity analyses indicated significant variation across effect sizes and supported moderator analyses; about half of the variation was attributable to nonsampling-related sources.

Child outcome type. Second, we calculated mean effect sizes by child outcome. Because many studies contributed more than one outcome (e.g., grades and anxiety), we separated studies by outcome type, resulting in four univariate meta-analyses. Within each outcome type, some studies included several types of father involvement (e.g., contact and relationship quality). In those situations, we averaged across father involvement types to maintain one effect size per study. Nonresident father involvement was most strongly associated with child social well-being ($M_{ES} = 0.15$). Effect sizes for the other three outcomes (academic, behavioral, and psychological) were small but also statistically different from zero. Within each outcome type, effect sizes were heterogeneous and, thus, moderation analyses were appropriate.

Father involvement type. Third, we calculated mean effect sizes according to father involvement type through five univariate

Table 2
Mean Effect Sizes and Homogeneity Analyses for Overall Group, by Child Outcome Type, and by Father Involvement Type

Group of studies (<i>k</i>)	Effect size			Homogeneity analyses	
	Min	Max	Mean (95% CI)	Q_{Total}	I^2 (95% CI)
Overall (52)	-0.45	0.60	0.05 (0.03, 0.07)	109.73**	53.52 (36.30, 66.09)
Child outcome type					
Academic (17)	-0.01	0.60	0.04 (0.02, 0.08)	33.82**	52.69 (17.82, 72.77)
Behavioral (30)	-0.58	0.38	0.05 (0.02, 0.08)	60.72**	52.24 (27.50, 68.54)
Psychological (35)	-0.45	0.27	0.03 (0.01, 0.06)	56.31**	39.62 (9.25, 59.83)
Social (8)	-0.13	0.96	0.15 (0.01, 0.29)	18.99**	63.14 (20.79, 82.85)
Father involvement type					
Activities (5)	0.02	0.32	0.09 (0.03, 0.16)	11.17*	64.19 (5.79, 86.39)
Contact (45)	-0.45	0.69	0.02 (0.00, 0.06)	147.29**	70.13 (59.59, 77.92)
Financial Provision (8)	-0.09	0.34	0.06 (-0.05, 0.18)	264.83**	97.36 (96.19, 98.17)
Multiple (5)	0.05	0.38	0.11 (0.04, 0.18)	19.24**	79.21 (50.60, 91.25)
Relationship quality (20)	-0.05	0.96	0.11 (0.04, 0.16)	111.95**	83.03 (74.90, 88.53)

* $p < .05$. ** $p < .01$.

analyses. A few studies included effect sizes from multiple child outcomes (e.g., academic and behavioral); for those, we averaged across outcome. Mean effect sizes for father involvement in activities, father-child relationship quality, and multiple types of father involvement all were positive and significantly different from zero; mean effect sizes for contact and financial provision were not. Within each group, we rejected the null hypothesis of homogeneity of effect sizes and so proceeded with moderator analyses.

Moderation Analyses

As with the overall analyses, we conducted moderator analyses using the overall group, by child outcome, and by father involvement type. Data on sample representativeness, reporter of father involvement, and year of data collection were available for all studies. Analyses of associations among moderators showed differences in year of data collection by reporter of father involvement, $F(3, 47) = 7.92, p < .001$; data in studies using child reports were more recent ($M_{\text{year}} = 1994$) than data from studies using mother reports ($M_{\text{year}} = 1985$). Across study and sample characteristics, an association existed between year of data collection and percentage of non-White mothers in samples ($r = .371, p = .031, k = 34$); more recent samples were more racially diverse. No other associations were significant.

Demographic sample information was available for a much smaller number of studies, and in some subgroups too few studies reported the characteristic for us to conduct moderator analyses. When we were able to conduct moderator analyses using sample characteristics, we conducted univariate analyses for each predictor. The number of studies including information about any one sample characteristic was small; the number of studies with combinations of any two characteristics was too small to include multiple sample characteristics in one equation. Further, only one of six correlations between sample characteristics was significant; mothers' average income was correlated with average child age, $r(14) = .67, p = .007$.

The small number of studies in many subgroups also decreased the power available for many moderator analyses. Such results should be interpreted with caution, as multiple univariate tests increase the possibility of Type I error (false positives). However, we decided that this was a worthwhile risk in order to start taking a more complex look at nonresident father-child relationships, with the recommendation that future studies attempt to replicate our findings as the literature expands and more primary studies are available to permit multivariate meta-analyses.

Overall group of studies. Table 3 displays results of moderator analyses for the overall group of 52 studies, where we averaged across all child outcome and father involvement types to obtain one effect size per study. We found statistically significant differences in effect sizes by sample representativeness; studies with representative samples had smaller effect sizes than those using convenience samples. The Q_{within} value for each group was statistically significant, indicating there was unexplained variation in the effect sizes. Tests of the continuous moderators are shown at the bottom of Table 3. The B value can be interpreted as a regression coefficient representing change in the effect size with a one-unit increase in the predictor variable. Standardized beta-values (β) quantify the amount of variability in effect sizes asso-

Table 3
Moderator Analyses for all Studies ($k = 52$)

Categorical moderators				
Moderator (k)	Q_{between}	Subgroup ESs	Q_{within} by group	
Representative sample	8.03**			
No (36)		0.07**		72.02**
Yes (16)		0.02**		29.67*
Reporter of father involvement	2.74			
Father (4)		0.06		37.45**
Mother (23)		0.03		28.25
Child (21)		0.02		39.13**
Multiple (3)		0.11		2.16
Continuous moderators				
Moderator (k)	Constant ^a	B	β	Q_{model}
Year of data collection (52)	0.00	0.01	0.13	1.94
Average child age (38)	0.04**	-0.01	-0.28*	5.36*
Percent of mothers non-White (34)	0.02	-0.001	0.01	0.01
Average mother's household income (19)	0.01	0.00	0.27	3.25

* $p < .05$. ** $p < .01$.

ciated with the moderator, and Q_{model} indicates whether the variable predicted significant variation in effect sizes. Child average age was the only significant moderator. A 1-year increase in the average child age was associated with a -0.01 change in effect size (i.e., nonresident father involvement was less strongly associated with child well-being among samples of older children).

Child outcome type. We next conducted moderator analyses within academic/cognitive, behavioral, and psychological outcome categories. We did not conduct moderator analyses for child social outcomes given that only eight studies included this type of outcome. Below we present our main findings (tables of all findings are available from the first author).

Academic and cognitive achievement. Effect sizes differed according to reporter of involvement (father vs. child; $Q_{\text{b/w}} = 3.69, p = .05$), such that the mean effect size was different from zero for child reports ($k = 7; M_{\text{ES}} = 0.05, p < .01$) but not father reports ($k = 9; M_{\text{ES}} = 0.01, p = .56$). Effect sizes did not differ based on representative samples ($Q_{\text{b/w}} = 1.56, p = .21$). We were able to test all continuous moderators for this subgroup of studies, but only average mothers' household income was significant. At the average mother's household income, the effect size was 0.04 ($k = 10; p = .001$). An increase of one unit (\$100) was associated with a 0.003 increase in effect size ($\beta = 0.51, p = .02$); the association between nonresident father involvement and child academic/cognitive outcomes was stronger in studies where mothers had higher average household incomes. Changing the scale shows that this translates into a substantial increase in effect size of 0.3 for an increase of \$10,000 in income.

Positive behavioral outcomes. Effect sizes differed by reporter of father involvement ($Q_{\text{b/w}} = 16.00, p = .001$). For father reports ($k = 13$), involvement was not associated with behavior, but studies using mother reports ($k = 2$) or multiple reporters ($k = 3$) had larger effect sizes (0.28 and 0.16, respectively) compared with child reports ($k = 12; M_{\text{ES}} = 0.02$). However, the few studies in each group warrants cautious interpretation. Effect sizes also

differed by use of representative samples ($Q_{b/w} = 4.13, p = .04$). Studies using convenience samples had larger effect sizes ($k = 22; M_{ES} = 0.06, p = .002$) versus representative samples ($k = 8; M_{ES} = 0.02, p = .03$); both were significantly different from zero. Of the three sample characteristics, average child age was the only significant moderator. At the mean for the 26 studies including average child age (11.29 years), the effect size was 0.08, $p = .003$. An increase of 1 year in average child age was associated with a decrease in effect size of 0.01 ($\beta = -0.40, p < .001$); the relationship between involvement and behavior was weaker in samples of older children.

Psychological and emotional outcomes. Effect sizes did not vary based on sample representativeness ($Q_{b/w} = 0.07, p = .79$) or the reporter of father involvement ($Q_{b/w} = 4.08, p = .26$). Of the continuous moderators, year of data collection and average mother's household income were significant predictors. At the earliest year of data collection (1976), the effect size was not significantly different from zero ($M_{ES} = -0.02, p = .34$), but data collected 1 year later was associated with an increase of 0.03 in effect size ($\beta = 0.31, p = .01$). As such, more recent studies reported stronger associations between involvement and psychological outcomes. The effect size at the average level of household income across studies was not different from zero ($M_{ES} = 0.00, p = .89$), but an increase of \$100 was associated with an increase in effect size of 0.005 ($\beta = 0.45, p = .01$), or an increase in effect size of 0.5 for an increase of \$10,000 in income. Associations between nonresident father involvement and child emotional/psychological well-being were stronger among samples of mothers with higher average incomes.

Father involvement type. We then conducted moderator analyses within each type of father involvement. To conserve space, we again present only main findings here.

Father-child relationship quality. Among categorical moderators, effect sizes varied significantly based on use of representative samples ($Q_{b/w} = 10.78, p = .001$) and reporter of involvement ($Q_{b/w} = 25.03, p < .001$). Studies using representative samples again had smaller effect sizes ($k = 8; M_{ES} = 0.05, p < .01$), compared with studies not using representative samples ($k = 12; M_{ES} = 0.16, p < .001$). With regard to reporter of father involvement, a much larger effect size was observed for mother reports ($k = 2; M_{ES} = 0.38, p < .001$) compared with either child ($k = 13; M_{ES} = 0.04, p < .001$) or father reports ($k = 3; M_{ES} = 0.09, p = .002$), but only two studies used mother report, therefore, results should be interpreted cautiously. For continuous moderators, only year of data collection was significant. At the earliest year of data collection (1976), the effect size was 0.15, $p < .001$; each 1 year increase was associated with a decrease in effect size of 0.01 ($\beta = -0.20, p = .03$). The association between father-child relationship quality and child well-being was weaker in more recent studies.

Father-child contact. For categorical moderators, effect sizes differed by use of representative samples ($Q_{b/w} = 5.84, p = .02$), with nonrepresentative samples again having higher mean effect sizes ($k = 32; M_{ES} = 0.03, p = .06$) than representative samples ($k = 13; M_{ES} = 0.01, p = .08$). Effect sizes also varied by reporter of father involvement ($Q_{b/w} = 12.56, p = .006$), with only studies using mother report ($k = 3; M_{ES} = -0.14, p = .02$) and child report ($k = 8; M_{ES} = -0.03, p = .002$) having mean effect sizes different from zero. It is noteworthy that these effect sizes were

negative, indicating that greater contact was associated with poorer well-being. Among continuous moderators, average child age and percent of non-White mothers were significant predictors. For studies with the average child age ($k = 33$), there was a small but significant effect size of 0.03, $p = .003$. A 1 year increase in average child age in the sample was associated with a 0.01 decrease in effect size ($\beta = -0.26, p = .05$); samples with older children had weaker effect sizes. For racial/ethnic background ($k = 27$), studies including only White mothers had an average effect size of 0.06, $p = .03$. A one percentage point increase in the proportion of non-White mothers in the sample was associated with a decrease of 0.002 in effect size ($\beta = -0.25, p = .01$). Between studies with the lowest percentage of non-White participants (0%) and those with the highest percentage (92%), there was a difference in effect size of .18. Notably, this represents a negative effect size (i.e., contact was associated with poorer well-being) among studies with higher proportions of non-White mothers.

Father financial provision. No differences in effect sizes were found based on use of representative samples ($Q_{b/w} = .81, p = .36$) or reporter of father involvement ($Q_{b/w} = 3.21, p = .14$). For continuous moderators, percentage of non-White mothers in the sample and average mothers' household income were significant. Among studies including only White mothers, the average effect size was 0.56, $p < .001$. A one percentage point increase in the proportion of non-White mothers was associated with a decrease in effect size of 0.01 ($\beta = -0.32, p < .001$). As such, from the lowest to highest percentages of non-White mothers across studies, the effect size decreased by .92, again resulting in negative effect sizes (financial provision being associated with poorer well-being) among studies with large proportions of non-White participants. Among studies having mothers' household income at the average, the mean effect size was 0.07, $p < .01$. A \$100 increase in household income was associated with an increase in effect size of 0.005 ($\beta = 0.24, p < .001$); a \$10,000 increase was associated with an average effect size increase of 0.5.

Discussion

Direct Associations

The results of this study confirm and build upon the findings of Amato and Gilbreth (1999) that nonresident father involvement can have positive effects on children, but the quality of such involvement matters more than the quantity. Our meta-analyses indicated that overall, nonresident father involvement continues to be positively associated with a variety of child outcomes (social, behavioral, academic/cognitive, and emotional/psychological). Effect sizes in the present study were modest but consistently statistically different from zero, and the strength of the present effect sizes generally were comparable with those found by Amato and Gilbreth.

The amount of contact and financial provision, however, were not associated with overall child well-being. Our finding regarding contact was similar to Amato and Gilbreth's but our finding regarding financial provision was different, as they found statistically significant associations. Our findings indicated that involvement in children's activities and high quality father-child relationships were the most influential forms of involvement. These correspond to the affective climate aspect of Palkovitz's (2007)

conceptualization of fathering. Father–child contact and financial provision, constructs reflecting behavioral style, were not associated with well-being. Taken together, these findings partially echo those of Amato and Gilbreth (1999) that it is the types of activities in which nonresident fathers are involved and the quality of the time spent that matter, not the simple amount of time itself. The present findings did not support that financial provision was associated with benefits for children. This change from the findings of Amato and Gilbreth might reflect the recent policy shift that has occurred. Ensuring that fathers paid child support was a primary policy focus during the 1980s and 1990s, whereas the new millennium has ushered in a greater emphasis on fathers being involved and nurturing parents.

When related back to Palkovitz's themes, our findings suggest that children's well-being is tied less to fathers' general behaviors (spending time or money) and more to the affective climate created by the quality of those behaviors. Fathers matter not when they just spend time with their children, but when they spend time being involved in activities with their children and nurturing the father–child relationship. We found the strongest effect sizes in studies assessing father–child relationship quality and those using multiple measures of father involvement, which supports a move away from the traditional policy foci of visitation and child support. Instead, fathers should be encouraged to be involved with their children in a variety of positive ways.

Children's social outcomes were the most strongly associated with nonresident father involvement, although this outcome had the fewest studies. This is consistent with studies of resident fathers that have found fathers are influential to children's peer relationships (e.g., Jia, Kotila, & Schoppe-Sullivan, 2012). Although we could not test which forms of involvement were most strongly associated with social outcomes, attachment research offers a possible explanation for our findings. Children's attachment to fathers is important for later social relationships (Mattanah, Govern, & Loiez, 2011), and attachment can be disrupted by divorce (Faber & Wittenborn, 2010). Attachment might be less disrupted when nonresident fathers maintain positive father–child relationships and engagement in children's activities; such fathers model positive relationships that can buffer children against poor social outcomes.

Moderators

Methodological characteristics. The importance of sample representativeness is consistent with findings in many meta-analyses. By their nature, convenience samples take advantage of unique sample characteristics and tend to have larger effect sizes, whereas representative samples, by definition, should have values closer to the true population mean. Interestingly, studies where fathers reported on their own involvement had consistently smaller effect sizes (compared with child reporters for cognitive achievement outcomes and mother and multiple reporters for behavioral outcomes). This runs somewhat counter to previous literature indicating that fathers tend to overreport levels of involvement, although levels are not the same as correlations. Particularly in the cases of mothers and multiple reporters, however, the number of studies using such reports was very small (two and three, respectively). As such, average effect sizes for these reporters likely will regress toward the population mean as more studies are conducted.

On a related note, nonresident fathers typically are a difficult population to reach for research purposes (e.g., Teitler, Reichman, & Sprachman, 2003). By virtue of their participation in the studies sampled here, even those fathers included in representative samples likely include nonresident fathers who are the most highly involved with their children. Greater efforts are needed to obtain more representative samples of nonresident fathers.

Demographic/sample characteristics. In analyses combined across child outcomes, only average child age significantly predicted variation in effect sizes; studies with samples of older children had weaker effect sizes. This finding likely was driven by the large number of studies examining behavioral outcomes; more discussion of this below. In analyses of separate child outcomes, findings were less consistent, but more recent studies with younger children and higher average maternal household incomes had stronger effect sizes. In analyses separated by father involvement type, year of data collection was a significant predictor of effect sizes in studies for father–child relationship quality (in this case, older studies had stronger effect sizes), and studies of father contact with fewer non-White mothers had stronger effects.

Child age moderated the association between father involvement and behavioral outcomes, with weaker effects among older samples of children. This might be due to the fact that, for some externalizing behavior problems like aggression and opposition, children typically exhibit fewer behavior problems as they mature (Reef, Diamantopoulou, Meurs, Verhulst, & van der Ende, 2010), leaving less variability for father involvement to predict. Another important consideration is that studies rarely report either the length of time fathers have been nonresident, or the child age at which fathers first became nonresident. Older children who never lived with their fathers likely have different experiences than older children who had a long history of coresidence before their father left. These factors should be examined in future studies.

Maternal income consistently has been associated with maternal support of father involvement (e.g., Kulik & Tsoref, 2010); here, it might represent a proxy for the level of mothers' supportiveness toward and encouragement of nonresident father involvement. Not only might such encouragement increase levels of father involvement, supportive mothers also might engage in additional emotion work with the child on the father's behalf (Seery & Crowley, 2000), which could strengthen the influence of nonresident father involvement when it occurred. Too, due to patterns of assortative mating, mothers likely paired previously with fathers of similar income and education levels. Fathers with more education and higher income tend to have greater resources to devote to their children and are more likely to have shared parenting plans in place (Nielsen, 2013). Because greater shared parenting is associated with benefits for children (Nielsen, 2013), this likely strengthens the association between nonresident father involvement and child well-being.

The present findings suggest that cohort effects might differ depending on the focus of the study. Interestingly, findings indicated opposite effects (stronger effect sizes in recent years for emotional well-being and cognitive achievement, but weaker effect sizes in recent years for father–child relationship quality) for variables that typically are highly correlated; high quality father–child relationships generally are associated with greater child emotional well-being (Allgood, Beckert, & Peterson, 2012). Such incongruous findings are possible because for the two sets of

analyses (outcomes vs. type of involvement) effect sizes were averaged across different factors. Therefore, interpretation is difficult and of limited utility until a sufficient number of studies makes multivariate meta-analyses and direct comparisons possible.

It also is possible that the greater use of child reports in more recent studies account for some of these cohort effects. Father-child relationship quality frequently is assessed via child reports, a relatively recent phenomenon. Such child-reported outcomes frequently were paired with father- or mother-reported father involvement, reducing shared informant bias and, thus, the association between involvement and well-being. Finally, it might be that involvement in particular activities (e.g., talking about problems, helping with homework) is associated with specific outcomes such as emotional well-being and cognitive achievement, but are less attached to broad measures of father-child relationship quality. For example, helping with homework can directly improve children's grades, but might not reflect an overall high quality relationship. As such, we again recommend reassessing such associations when more primary studies are available and multivariate analyses can be performed.

When demographic characteristics were compared across years, more recent samples were more likely to include non-White mothers (i.e., samples were more racially diverse); there were no differences found in terms of child age or maternal income. This likely is due both to greater inclusion of non-White families in social science research generally in recent years and to the greater availability of representative samples. Increased sample diversity allows for greater generalization and trustworthiness of the findings and so marks a positive trend in recent studies. Because Amato and Gilbreth (1999) only noted categorically whether a sample was 100% African American, 0% African American, or between 1% and 99% African American, direct comparisons with their findings cannot be performed.

It is interesting that no differences were found in income level across study years, as more representative samples presumably also would include greater economic diversity. However, single mothers always have been at greater economic risk than married mothers, potentially creating less economic diversity to sample. Also noteworthy is that studies with fewer non-White mothers had stronger effect sizes between father-child contact and child well-being. Racial differences have been found in levels of father involvement (minority fathers typically are more involved in child rearing and caretaking than are White fathers; Hofferth, 2003), but to our knowledge, no studies have investigated racial differences in maternal support for father involvement. Rates of nonresident fathering are considerably higher among minority families and particularly among African American families (Wu, 2008) and views of single motherhood are more favorable among minority men and women than among White women (Goldscheider & Kaufman, 2006). Among non-White mothers who see nonresident fathering as more "optional," a lack of involvement might be less detrimental to children's well-being, weakening the association. Also, recall that more recent samples were more racially diverse. As such, these factors (support for nonresident fathering, cohort effects, and cultural views of nonresident fathers) likely interact to influence associations between nonresident father involvement and child well-being. Overall, our findings emphasize the importance of context on both levels and influence of father involvement and the need for more studies that investigate such associations.

Future Directions

An important contribution of this article is the guidance it provides for where future research should focus—specifically, which forms of nonresident father involvement and what sorts of child outcomes remain largely unexamined. Further research regarding frequency of contact seems unnecessary, with the exception of associations between contact and understudied outcomes such as physical health or psychological pain (Laumann-Billings & Emery, 2000). Both Amato and Gilbreth's (1999) meta-analysis and our own findings strongly confirm that operationalizing fathering as contact is not useful and provides limited benefits to children.

Financial provision. Interestingly, Amato and Gilbreth (1999) found significant associations between financial provision and child well-being, whereas we did not. Research investigating possible cohort effects could be useful, although our moderator analyses did not reveal differences in effect sizes by year of data collection. Associations between provision and understudied outcomes such as child health also are recommended, as logical connections exist between financial resources and children's health.

Affective climate. In general, the present study indicates that scholars should focus on the overall affective climate provided by fathers and what sorts of parenting behaviors promote a positive climate. For example, research has noted that nonresident fathers have high levels of recreational involvement, due to the restrictive nature of noncustodial parenting (Stewart, 1999). It often is assumed that recreational involvement is "less than" other forms of involvement such as caretaking (reflected in semiderogatory names such as Disneyland Dads or the "fun parent"), although research supports the importance of leisure activities for father-child relationships (Jenkins, 2009) and involvement in play has been associated with greater social competence for children of resident fathers (Jia et al., 2012). Beyond recreation, less is known about other forms of nonresident father involvement, including involvement in decision making or positive parenting behaviors. Given that joint legal custody is now the default in most states and the few existing studies support the importance of authoritative parenting by nonresident parents (Karre & Mounts, 2012), these are key forms of involvement to investigate in this population.

Children's health and social outcomes. We found only two studies that investigated children's health outcomes, which is a notable gap as recent research has suggested that resident fathers have a unique influence on outcomes such as child obesity (Wake, Nicholson, Hardy, & Smith, 2007). Childhood obesity is a worldwide intervention focus, so scholars should explore the influence of nonresident fathers on this area of child well-being. Similarly, few studies have examined children's social outcomes. Given the potential impact of children's attachment to nonresident fathers on their later social relationships, this is another area in which scholars should focus future research efforts. This is particularly salient given that the strongest effect sizes were associated with social outcomes. Overall, more studies are needed that investigate multiple types of father involvement and their effects on a variety of child outcomes, enabling more complex interactions between involvement and child well-being.

Relational synchrony. No studies were found that examined the third theme of Palkovitz's (2007) framework, relational syn-

chrony. Future studies should examine the potential influence of nonresident fathers as teachers, mentors, and developmentally appropriate parents.

Reporting of potential moderating variables. There is a dearth of studies that assess important moderating variables. The field needs more complex investigations that examine not only whether nonresident father involvement is associated with child well-being, but the contextual variables which strengthen or weaken such associations. Researchers need to investigate factors such as family structure, custody statuses, number of mothers' and fathers' other children (resident and nonresident), child age, and levels of conflict, and how children are impacted by different forms of father involvement in such families.

Investigation of nonlinear effects. A related methodological concern is that we could not test for nonlinear associations between nonresident fathering and child well-being, as the studies being analyzed tested only linear effects. It might be that some minimum level of father involvement must be reached before fathering behaviors matter. As noted above, we also could not test moderating effects of coparental conflict, as only two studies assessed such conflict. Research suggests nonlinear patterns of interparental conflict (King & Heard, 1999), and high interparental conflict has been strongly associated with poorer outcomes for children (Amato, Loomis, & Booth, 1995). A meta-analysis of custody status and child well-being (Bauserman, 2002) also found positive associations between joint custody and child well-being. However, because joint custody tends to be voluntary, custody becomes confounded with conflict; parents who have less conflict are more likely to agree to joint custody.

Addressing reporter bias. To the extent that studies used the same reporters for father involvement and child well-being, single informant bias is possible. We were unable to test this possibility as most studies had multiple reporters of child well-being (e.g., teacher reported behavior problems and child reported self-esteem); this made possible combinations of reporters too complex and analyses underpowered. The predominance of multiple informants within studies, however, protects against single informant bias and suggests that this is unlikely to be a significant source of bias in our results. Future studies should attempt to untangle these effects.

Direction of effects. It is important to note that the effect sizes included in this meta-analysis were correlations. Thus, it cannot be concluded that positive fathering causes child well-being. It could be that well-adjusted children draw nonresident fathers into being more involved. Longitudinal studies should attempt to tease apart these reciprocal influences.

Conclusion

Overall, our findings confirm the continuing importance of nonresident fathers for their children. To promote child-well-being, policymakers and practitioners should focus on the quality rather than the quantity of fathering, as mere time and dollars spent appear to mean little for children's outcomes. This has important policy implications, because although time and money are the simplest items to legislate, our findings suggest that an exclusive focus on custody/parenting time and child support will be largely ineffective in promoting child well-being. The present study goes beyond earlier work that viewed nonresident fathering and child

well-being as one-dimensional constructs and suggests that different forms of involvement differentially affect children, and various aspects of child well-being are differentially affected by involvement. Just as children can be at risk in all areas of well-being when fathers are absent, our findings indicate that nonresident father involvement can benefit children in all areas as well, particularly when fathers are involved in children's activities and in multiple ways, and especially in terms of children's social skills and relationships. Still, much remains to be done. It is our hope that these findings will aid scholars in the creation of future research endeavors and continue to inform policymakers and practitioners about the best ways to encourage nonresident father involvement with their children, thereby promoting children's well-being.

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