On Thursday mornings, we find ourselves sitting across from each other in our corner booth at Elmo’s Diner in Durham, North Carolina. We nearly disappear into the roomy, slightly tattered green leather cushions, anchored in the comfort of the history we share—that is, our long engagement as friends and coconspirators in the world of kinship, poverty, and ethnography. Despite our access to the long menus that flank the table, the server who greets us knows that we won’t bother to look because we always order the Egg Deal. We find solace in the predictability of the setting, our conversations, and the Egg Deal.

Between us, we have amassed about seventy-five years of experience as ethnographers, working and communing with low-income rural and urban families across generations, time, and place. Even before Linda’s orange juice and Carol’s coffee are delivered to the breakfast table, we spread out our fieldnotes and memos, aligning the salt, pepper, and other condiments so that they are equidistant to our reach. We approach our ongoing disciplined discussions about our research on poverty and families in much the same way. We are eager to launch our discussion on this particular day because Margaret Nelson, Anita Garey, and Rosanna Hertz have invited us to contribute a chapter to an edited volume that will allow us to talk frankly about aspects of our work that we have rarely had the forum to provide details about—that is, how slow ethnography and disruptions rooted in the politics of...
of science shaped the pathways we followed in constructing narratives about our observations of low-income males and their labor in families.

We joke with each other, suggesting that Margaret, Anita, and Rosanna have been stealthy, silent partners in our ongoing breakfast conversations, and that our current writing project for them is a reflection (though only in part) of their numinous presence. You see, in nearly fourteen months of breakfast tête-à-têtes, we have dutifully challenged each other to “get a handle on” how the processes of slow ethnography and disruptions owing to the politics of science have moved us along at a snail’s pace in writing about something we actually know quite a bit about—boys, men, and families. We ask each other on a fairly consistent basis: why is it taking us so long to write a full-monty narrative about the boys’ and men’s lives that are so deeply entrenched in our ethnographies and theoretical discourse on poverty and kinscripts?

“Kinscripts,” an article we wrote, now some two decades ago (Stack and Burton 1993), depicts one of the ways that we craft theory about kin processes through ethnography. For this effort, we combined our individual longitudinal ethnographic datasets on mostly African American, but also some White and Latino, low-income urban and rural families. We painstakingly culled through reams of fieldnotes and transcripts over a period of six years, recalling vivid images of the families, women, men, and children we studied at every step and harkening back to their voices and their understandings of kin roles and relations and how they work. Throughout the process, we invoked our credo of holding each other accountable for how we interpreted the data as a whole. It is because of this credo that as we wrote the present chapter, we decided to make readers privy to reflexive insights about us that likely shaped kinscripts (Naples 2003).

We invented the kinscripts framework to organize and interpret observations of: (1) the temporal and interdependent dimensions of family role transitions; (2) the creation and intergenerational transmission of family norms; and (3) the dynamics of negotiation, exchange, and conflict within families as they construct the family life course. Across our ethnographies, we could see that the dominant lens for understanding families and their outcomes was to assess their kin-work (the labor individuals do in families), kin-time (the temporal nature of individuals’ engagement in labor and the expectations from others about them doing so), and kinscription (how individuals are ushered or recruited into doing family labor). This perspective helped us
to interpret and understand the family work of women in our studies whose lives we chose to write about in greater detail first. But, our gaze both in the field and as we wrote was also fixed on the labor of boys and men trudging through childhood, adolescence, and adulthood, both inside and outside of families.

As we breakfast at Elmo’s week after week, we speak feverishly about the boys and men we came to know and understand using the kinscripts lens and about what we have and haven’t done in telling their full stories even though they were active participants in our ethnographies from day one. As such, in this chapter, we humbly reveal a confluence of forces that sometimes drove us to park boys and men in the background of our narratives on the labor of kin. We talk about why understanding the lives of low-income males, particularly adolescents, required a slower ethnographic timetable that involved us “being there” and “lingering” in the field for very long periods of time. And, we also reflect on how the kinwork of males inside low-income families has been marginalized in the scientific and public policy discourse on poverty, and consequently created disruptions for us in bringing boys and men to front and center in the theoretical and empirical evolution of kinscripts.

Kinscripts: A Reflexive Lens

As we move forward in our breakfast deliberations from week to week, we have decided to focus our current efforts on bringing the family labor of adolescent boys to center stage before we develop detailed narratives about their fathers and the other adult men in their lives. As we make these kinds of decisions, our discussions occasionally shift to our early years as collaborators as well as our own life course experiences with families and adolescent males. Our journey as ethnographic partners and vessels of kinscripts began on February 25, 1985, when Linda called Carol out of the blue—we had never met—with a straightforward, yet naive, request. She wanted to come to Durham, North Carolina, to work with Carol. Carol agreed without knowing Linda from Adam. We both simply trusted our instincts. Our ethnographic collaboration on the work of kin was launched in the summer of 1985.

When Linda came to Durham to work with Carol, we had two ethnographic studies on family networks between us: Linda’s dissertation, “Early and On-Time Grandmotherhood in Multigeneration Black Families”
(Burton 1985), and *All Our Kin* (Stack 1974), the book based on Carol’s dissertation. The worldviews we showcased in those works emerged from our academic disciplines (sociology and anthropology), the times, and our own family backgrounds. Carol was born in the East Bronx in New York City. Her family migrated first to Albuquerque, New Mexico, and then on to Southern California, where her sister was born. Carol grew up in a large, extended, working-poor, immigrant Jewish family whose members gathered around one another in the San Gabriel Valley in Southern California. Carol married as a young graduate student, had one son, and divorced. She spent the early part of her academic career at Duke University before moving to the University of California at Berkeley, where she lived and taught for several decades. Upon retiring from Berkeley, she returned to Durham.

Linda grew up in a working-poor African American family that migrated from New Orleans, Louisiana, to Los Angeles, California. She had two sisters and spent most of her life in South Central Los Angeles and Compton, California, also around a sizable extended family network. Linda married directly out of high school, had four children (three girls and one boy) rather quickly, and divorced. Her academic career took her first to Penn State University, from where she initially traveled to Durham to meet with Carol. Linda remained at Penn State for over twenty years before moving to Duke University nearly seven years ago. With this move, Carol and Linda were reunited in their adopted “homeplace,” Durham.

We both grew up with strong mothers and very generous fathers, and ironically, throughout our childhood and early adult years had resided at different ends of the 110 Freeway—a bypass that connected northeast and southwest Los Angeles. Life was much quieter and safer in South Pasadena for Carol than it was in Compton’s “Bloods and Crips territory” for Linda. At one point, Linda lived across the street from Stanley “Tookie” Williams, the leader of the Crips.

In the early 1950s, Carol spent time on her father’s bread truck near the community where Linda would later go to elementary school. Linda’s father was janitor in a Mexican café in East Los Angeles. Both our fathers traversed diverse cultures and spaces on a daily basis and were quite adept at doing so. They were lifelong caregivers, attentive and generous to others and to us to a fault, even though they were always financially in dire straits. As such, it is not hard to detect the commonalities we shared as we came of age as single mothers; as we watched our own mothers struggle to make do with the meager resources our fathers provided; and as we took part in the care our fathers
provided to other family members, friends, and sometimes strangers off the street. These life experiences, no doubt, shaped the ways we approached our research.

**Coming Together in Front Porch Dialogues**

Linda recalls the day she met Carol in person:

> Our first serious conversation was on Carol’s front porch in North Carolina (where she was teaching at Duke). It was so candid and unexpected, much like ethnography. We had no history together. We had just met. But an immediate path was forged for us on that day—a path characterized by mutual trust, honesty, and an unwavering sense of responsibility to represent and interpret the lives we became a part of in our work. We both wanted our research to mirror our respondents’ realities. I knew then that this path would be a long engagement requiring more conceptual, emotional, and physical labor than any graduate class that I had ever taken or job I had ever worked.

Carol’s thoughts:

> At the time I was the single mom of a teenage son, and I was also in the middle of doing an ethnography on African American return migration to the rural South. Summertime was for research, and I fully intended to spend my summer in northeast North Carolina. I listened closely to what Linda had in mind, and thank goodness I said yes. Whatever our initial intentions were, we soon began talking about our basic frustration with language and terminology in our respective fields that made it so hard to portray what we were observing. The formal kinship terminology in anthropology left me without a satisfactory way to describe the intricacies and dynamics of caregiving and care-receiving among the extended families in my study.

On Carol’s front porch we relived our data, and we talked, and talked, and talked. We shared the everyday practices we saw in low-income families in a midwestern city and in South Central Los Angeles. We also began to talk about our ethnographic practices, and seriously engaged the topic of nuanced, understated, and often-misunderstood aspects of families and
poverty. Through our conversation, we found our own blind spots and missing pieces of the jigsaw puzzles in our ethnographies. Carol’s ethnographic gaze was wide. As she crafted her dissertation, she and her young son had moved in and among a stretch of kin networks, living in harsh conditions that were labeled in political circles as a culture of poverty. She began with an interest in the strategies that large extended families used to survive urban poverty, the networks they formed, and their individual and cultural sense of what they owed to themselves and what they owed to others. Linda’s research eyes were directed toward time, place, generational depth, and recurring family roles and patterns across the life course. This work tethered her initially to the field of aging and the role of young grandmothers caring for children in the throes of urban poverty and neighborhood violence. Carol wrote about social and cultural patterns within families. Linda wrote about family structures, roles, and communities that created an accelerated life course for poor African Americans. But neither of us had found fitting language to describe the paradoxes and complexities of poverty and family labor that we had come to know through our work. Carol’s memory of these discussions is that “we stripped away any pretense of claiming that we had done justice to or succeeded in describing the kin networks that characterized our work. I came from a long tradition of kinship studies in anthropology, and I tangled with long-standing kinship terminology I had inherited from my field as I was writing my book.” Linda noted that “I was handed a store of concepts in graduate school—some still inform my work. But, generations, the normative expectable life course, role theory, and theories of poverty and gender didn’t offer enough plasticity or agency for me to situate what women and men were saying about family labor and what they were actually doing about it, and why.”

Looking back on those early days, as we linger in our booth at Elmo’s, we realize that we have been talking together about the work of kin for nearly thirty years now. The times have changed. As friends, scholars, and colleagues, we continue to revisit our data, and we consistently mull around in a sea of theories about poverty and family life. Kinscripts emerged from these early musings, but our cycle of discovery and rediscovery around the labor of kin was never quite satisfied by this initial product. As times changed, and as we eventually made forays into the politics of science and public policy, which we will address shortly, we have slowly been moving our storyline on the work of kin from a focus on girls and women to the necessary spotlight on boys and men.
Adolescent Boys and Slow Ethnography

As we consider the question of why it has taken us so long to produce detailed narratives about the lives of males using the lens of kinscripts, we assert that we have never excluded them in our ethnographies or our written work. Their stories, however, have been sprinkled throughout much of what we write and talk about rather than positioned center stage. During our breakfasts at Elmo’s, we debate with ourselves and with each other about why this is so, particularly because males have been part and parcel of our ethnographies from the start. One hypothesis that we entertain frequently is that the creation of ethnographies of adolescent males and the labor of kin is very slow-going. We know from our fieldwork experiences that women studying adolescent males require excessive amounts of time and physical and emotional labor to carefully identify gendered behaviors and their meanings in context (Burton, Garrett-Peters, and Eaton 2009). It also necessitates traversing expansive temporal and physical spaces and having some theoretical mastery of adolescent development to disentangle the paradoxical demands that impoverished families (and mothers, in particular) often place on their adolescent male children (Burton 2007; Stack 2001). We both understood these issues from our initial ethnographic work when we, as young mothers and budding scholars (albeit at different points in historical time), sought to participate in the lives of and build relationships with adolescent boys inside families.

Not surprisingly, in the course of our engagements with adolescent boys and their families, we became inevitably enmeshed in social relationships with them by virtue of participating in their everyday lives (Asher and Fine 1991). To develop these relationships, we quickly understood that we had to “be there” and “linger” in our research sites over extended periods of time. Being there allowed us to see behaviors other researchers who were short-timers in the field may not have normally seen, such as the nuanced and sometime covert rules and courses of action in families’ daily routines. We exerted tremendous effort in identifying and trying to understand the rules and routes of families’ everyday lives and how families present themselves to others in the situations they encounter (Goffman 1959). This was not an easy task by any means. Nonetheless, being in the field over long periods through different seasons and at different times of the day and night (see Burton 1991) allowed us to observe how adolescent boys learned about and
assumed family caregiving roles that were in stark contrast to what others believed about them after observing only the boys’ public personas.

As we have noted, the lives of many teen boys in poor families have been part of our ethnographic purview in all our studies (see for example Burton 1990, 1997, 2007; Burton, Garrett-Peters, and Eaton 2009; and Stack 1974, 1996, 2001). When we met these young men, they were often characterized as problems at school by their teachers and principals. But, when we stayed with them long enough, we found that the very same boy the principal labeled as no good was in charge of a toddler in the family every day after school, or worked two jobs to help his mother pay the light bill and rent.

Linda’s (Burton 1991) ethnographic work with adolescent males and their families in a midsized northeastern city provides a poignant example of the points we are attempting to make here. In the context of a local high school with a 25 percent graduation rate for African American teen males, Linda was introduced to James, an eighteen-year-old tenth grader who was labeled by school administrators as a “salty walking illiterate disaster.” Linda came to know James over the course of several years, but not in the context of school. Rather, she interacted with him through a web of relationships that she built with his peers. Within several months, Linda learned that James was a “lite weight drug dealer” who sold “street pharmaceuticals” on a schedule that was not consistent with others who were also doing so. James sold drugs in three two-hour shifts: one in the morning, one in the afternoon, and one at night. After observing James’s pattern, Linda queried him as to why he sold drugs in shifts. He told her that he would tell her when he was ready.

At the end of the first year, James invited Linda to his home to meet his family. When she arrived at James’s home, she found him caring for his ailing grandmother, Ada, who was lying comfortably in a hospital-like room set up in the small apartment’s dining area. Swaddled in a daisy-patterned comforter, Ada, dying of cancer, appeared to be all of seventy pounds. “I’m the only one who can take care of her,” James said. “No one else needs to know about this. I figured that I could trust you and that maybe I could talk to you about this without you letting my homies know.” Indeed, James carried the contradictions of his street persona and his gentle spirit of family caregiving on what an observant parent might see as “fragile man-child shoulders.” Linda was able to gather these insights about James because she was there over time and present when he was ready to reveal the breadth of his life and family obligations, on his own terms.
Through our work, we have also come to know that there are circumstances that occur in single-mother households that demand considerable labor from adolescent sons. Robert Weiss (1979), a longtime mentor to both of us, has written eloquently about these issues in the lives of single divorced mothers and their children. The issue we have frequently observed in trying to identify our own “ethnographic truths” about these situations is that daughters are slightly more likely to receive “public credit” from their mothers for their kinwork while sons are frequently caught in variable webs of complex demands and mixed messages about their labor in ways that could lead a fast-paced ethnographer who does not linger in the field (e.g., spends merely one month there) to arrive at erroneous conclusions about these sons’ contributions to their families. We have recognized the potential for such errors in our own fieldwork relative to what we know we would have missed if we had not lingered in the families in which adolescent boys had assumed the role of “the man of the house.”

For example, Carol met Victor, a self-proclaimed man of the house, when he was working in a fast-food restaurant in Oakland (Stack 2001). This was not his first fast-food job, or even his very first job, as he had lied about his age and worked as a roofer when he was fifteen. That year he turned his earnings over to his mother and he beat up his stepfather, who had abused him and his mother for many years. “I guess he [the stepfather] thought I was never going to grow up,” Victor said. Victor did grow up, and fast. He was, in his own words, “self-supporting” by the time he was fifteen.

Before long, Victor started dressing like a gangster and associating with gangsters. He found one job after another, and made good money. Victor gave his mother half of each paycheck, and he still had more money than he knew what to do with. By sixteen, between working and attending to his girlfriends and worrying about his little brothers, he had no time to go to school. Despite his mother’s protests, Victor began to sleep in the daytime and run hard all night. He told his mother that he was “in charge of his own life.” “I started working young,” he wrote in his diary, and like a writer of a memoir, he added in bold letters that “a 15-year-old is very close to adulthood.”

Later on in Carol’s study, which followed her respondents over six years, Victor was working in a fast-food restaurant six hundred miles north of Los Angeles. When his mother decided to send his wayward thirteen-year-old brother, Santos, to live with him in Oakland, Victor confessed that he was excited. He wrote in his diary, “The main reason he is come to stay with
me is because he is mess up by following my foot steps and joining a gang. . . . He is trying to be like me. I don’t want him to do the same mistake and things I did.” Victor repeatedly told Carol that he wanted his brother to “learn from my mistakes.” Through her long engagement with Victor, Carol was able to document how an adolescent male who flirted with the life of a gangster also provided valuable kinwork to his family as he transitioned from boy to man.

Fifteen-year-old Antoine, one of Linda’s adolescent male respondents, took on this role of man of the house at age thirteen when his mother, Sandra, started a job as a cafeteria worker in compliance with welfare-to-work regulations (see Burton 2007). Sandra’s job required an hour-long commute by bus each way, long work hours, and very low pay. The considerable time Sandra spent being at and getting to and from work greatly decreased the amount of time she had to parent her eight sons (Antoine was the second oldest) and sustain relationships with a partner and her friends. As her partner and friend relationships dwindled, and her availability for parenting her sons all but disappeared, Antoine increasingly became her “spouse-apparent,” serving as her confidante, consultant, and co-parent to his younger siblings. Antoine’s fate as child-as-mate was sealed when his older brother, Dwayne, was arrested and jailed for attempting to contribute to the family economy by selling drugs.

Longitudinal observations of and interviews with Antoine and Sandra indicated that Antoine more than carried his weight as man of the house for several years, until at fifteen years of age he grew weary of his responsibilities. He had been responsible for getting his younger brothers and himself off to school every morning, and one day, according to him, “I just decided to stop.” He and his brothers stayed home from school for almost two months before his mother was alerted to their truancy by school officials and was forced to quit her job and stay at home to ensure that her sons went to school every day. While Antoine’s parenting responsibilities diminished somewhat because of the truancy incident and his mother’s displeasure with his “failure as a co-parent,” Sandra seemed to need him more than ever as an emotional confidante after she quit her job. She became increasingly depressed and anxious about the family’s financial situation and struggled not to turn to drugs as an option to “settle her nerves.”

Like Antoine and Victor, sixteen-year-old Alex served as the man of the house, providing solace and financial support to his mother and parenting for his fifteen-year-old sister and her one-year-old son. Alex’s mother,
however, was very ambivalent about his role and often sent him mixed messages about the importance of his contributions to the family (see Burton 2007). Whenever his mother started a relationship with a new boyfriend, she would “demote” Alex, requiring that he cease being her peer and “act like a child” in the presence of her boyfriend. On one occasion, his mother’s boyfriend demanded that Alex take out the trash “like a good boy.” Alex threw the trashcan at the boyfriend and demanded that the boyfriend leave his house “because he was the man of the house and helped his mother to pay the bills and raise his nephew.” Alex’s mother responded by telling him to go to his room. Instead, Alex left the house and moved in with a twenty-five-year-old woman down the street. He said, “Now, I am really the man of the house.”

The point we are making here is that all these young men were engaged in extensive family labor that we, as ethnographers, would not have seen or been able to contextualize accurately if not for our long stay in the field and our attention to how adults in these boys’ lives thought about their development and recruited them to or released them from family roles. In following James, Victor, Antoine, and Alex across various landscapes over time, we also became very aware of their public personas and how others perceived them in, for example, school settings. These young men were often labeled as “trouble.” Working through the “trouble” label in our analysis is what led us to experience major disruptions in disseminating our narratives about them. Although the perspectives of social scientists and policy-makers about these teens and the work they do in families has shifted somewhat over time, during the earlier years of kinscripts some audiences found our insights on boys to be “too maternal,” “potentially patronizing,” and “likely exaggerated.” Below, we discuss these points of view relative to the politics of science and how they shaped our kinscripts narrative on adolescent males.

The Politics of Science and Policy as Disruptions to Kinscripts

Indeed, in family after family, place after place, and study after study, we consistently observed adolescent boys engaging in the labor of kin. We lingered in the field and in families’ and sons’ lives long enough to witness the ungendered nature of their kinwork, to observe how expectations and practices around that labor were transmitted to boys sometimes as young as five years old, and to discover how boys’ kinwork was often treated by institutional
outsiders (e.g., school principals) as “fabrications” because males’ laboring in poor families seemed counterintuitive to these boys’ public personas. Most importantly, in our kinscripts journey, we experienced numerous disruptions in sharing these boys’ narratives with scholarly and public policy communities. During the early and middle years of our work, the dominant discourse about poor men and boys, particularly those of color, never quite provided us with a comfortable academic entrée or a policy audience that would take this work seriously.

The dominant discourse across many years had cast urban poor minority boys and men into personas that made them irrelevant and almost invisible in families (Burton and Snyder 1998). They were regularly characterized in the scientific literature and in the media as human forms whose lives were pathological idioms tied to criminal justice systems, violence, unemployment, and a manifested disinterest in education and social mobility. As a consequence, our work on men and boys was often viewed as questionable and counterintuitive, or as anecdotal anomalies. It didn’t help that we were women and mothers who prominently inserted ourselves in a discourse dominated by conservative male academics and policy-makers who saw no virtue in the family labor of men beyond that of family breadwinner.

Carol’s engagement with disruptions in representing the labor of boys and men in families began in the 1970s when she and others, such as Joyce Ladner (1973), brought their ethnographic research on poor families to the academic and political debates surrounding the Moynihan thesis and the culture of poverty. Typically, these debates, which relied on relatively thin data, stigmatized poor men, mostly African Americans, as pathological deterrents to establishing “normative family forms” because of their histories of unemployment, family absence, and general malaise in caring for the children they produced. Alice O’Connor (2001, 269), in her now classic volume Poverty Knowledge, keenly described the disruptions Carol experienced in her attempts at integrating her research into the discourse of the time:

[Unlike much of the existing research about men and poor families,] the pathology Stack and others emphasized lay not within the family [and men], but within racism and unemployment and a welfare system that made stable monogamous relationships between [men and women] difficult to sustain. Nor did “The Flats” [Carol’s ethnographic research site] suffer from the absence of fathers—fathers were integrated within the kin networks, albeit hidden from conventional research when they
were not playing the “traditional” role of breadwinner or household head. By writing about gender relations from a female perspective, Stack complicated the [prevailing discourse] of wandering unemployed men and lone welfare-dependent women that had usually been told by and about men. . . . [Male scholars,] however, incorporated none of Stack’s insights into the analysis of men in extended families.

Some of the initial dismissal of Carol’s work went even further than O’Connor’s account. Carol recalls, for example, that shortly after the publication of All Our Kin, she gave a talk on strategies for survival among low-income families residing in The Flats. A well-known economist in the audience banged his hand on the conference table after her talk, saying, “The trouble with ethnography is that it takes too long to find quick policy solutions, and I would add [that it is also] wrong-headed.”

Nearly two decades later, Linda fared no better than Carol in using her longitudinal ethnographic research to put the labor of boys and men front and center in the discourse on poverty and families. By this time, the discourse had taken a slightly different slant, largely because of Wilson’s (1987) The Truly Disadvantaged. The lens shifted dramatically toward neighborhood effects, violence, drugs, and gangs as producers of dysfunctional adolescent boys who were bound for lives in the criminal justice system. Ethnographies such as Sullivan’s (1989) Getting Paid: Youth Crime and Work in the Inner City, Anderson’s (1990) Streetwise, MacLeod’s (1995) Ain’t No Making It, Bourgois’s (2003) In Search of Respect: Selling Crack in El Barrio, Venkatesh’s (2008) Gang Leader for a Day, Bergmann’s (2008) Getting Ghost: Two Young Lives and the Struggle for the Soul of an American City, and Harding’s (2010) Living the Drama: Community, Conflict, and Culture among Inner-City Boys dominated the academic and public policy literature and airwaves. Linda vividly recalls the reaction of one senior White male academic when she presented her work on the labor of adolescent boys in families at a conference. He publicly scolded her:

You have no idea what you are talking about. You are a Black woman studying Black boys in dangerous neighborhoods. There is no way you can gather the kind of data you present. What self-respecting man would tell you about changing diapers when he is a gangbanger? You need to stick with talking about women and that kinscripts stuff. That’s something you probably know about. And, you only have subtle
intelligence so let the men handle the hard work. By the way, you will never get tenure if you start trying to convince people you know something about Black men. They will say you are biased and too emotionally invested to tell an accurate story.

Needless to say, Linda took this man aside and let him know that if he were ever going to garner an ounce of intelligent understanding about the lives of African American boys and men in families “in the ’hood,” he should go into the field with her and see which one of them would come out unscathed and appropriately enlightened. She said: “Growing up in Compton, coupled with excellent graduate training, can provide one with fierce and accurate survival, observation, and analytic skills. Thus, as any good scientist should know, one should do his/her homework before making the clearly misinformed, ignorant, and disrespectful statements you made.”

Unfortunately, this scholar’s reactions to discussions of adolescent boys as kinworkers were fairly common among some public intellectuals during this time. It was more than difficult to engage these scholars and policy-makers in meaningful and contextually informed dialogues about young boys providing valuable family labor “in the ’hood.” Such personas seemed inconceivable to many who could only conceptually and empirically configure these young men as readying themselves to become the next cohort to enter the criminal justice system.

The next phase of disruptions in Carol’s and Linda’s attempts to disseminate narratives about kinscripts and males appeared when scholars and policy-makers seriously turned their attention to the topic of fatherhood in the late 1990s. The major hiccup at that point, however, was that research on males’ kinwork focused almost exclusively on how men participated financially, socially, and psychologically in families as fathers (see Booth and Crouter 1998). This narrow framing of men’s family labor was encouraged by a national “call to arms” for research on responsible fathering and was reflected in (see Cabrera et al. 2000): (1) federal and private funding agencies redirecting their funding streams toward research on fathering (Federal Interagency Forum on Child and Family Statistics 1998); (2) rapid growth in the number of handbooks, anthologies, and special issues of scientific journals focused on fatherhood (Cabrera and Tamis-LeMonda 2002; Lamb 1987, 1997; Marsiglio 1995; Parke 1996); (3) the creation of several national research and information clearinghouses on fathering, such as the National Center on Men and Families at the University of Pennsylvania; (4) a notable rise
in town hall meetings and activists’ initiatives that championed responsible fatherhood, particularly among low-income men of color; and (5) the eventual launching of a specialty journal on the topic, aptly named *Fathering*.

Indeed, this flurry of attention and activities pushed scientific, political, and policy agendas on fatherhood forward at a rapid pace, but it did so almost to the exclusion of considering the other types of kinwork men and boys negotiated and performed in families (see, for example, Raley, Bianchi, and Wang 2012). We had observed in our ethnographic work that other forms of kinwork, such as providing care for younger siblings or ailing grandparents, were intricately connected to men’s fathering roles. In fact, their kinwork as young boys and adolescents seemed to serve as “training grounds” for their future roles as biological and surrogate fathers. These data posed a dilemma for both of us. As we remained mindful of the focus of the times—which, by the way, also had a strong emphasis on “deadbeat dads”—it was not clear to us how we could present case studies of adolescent males’ kinwork without once again engaging the politics of science that questioned whether poor adult men had the capacity to raise their children, especially if their sons were doing heavy lifting in their families that some deemed “a father’s duty.” We also knew from our work that the efforts of men were often minimized by women, who in much of the extant research were the primary informants about fathers’ contributions to families, and who, in general, wanted much more from men than the U.S. economy and educational system could and would provide for them. So, again, we waited to launch our narratives on adolescent boys and kinscripts with these considerations in mind.

Fortunately, as the first two decades of the millennium have progressed, the field of study around kinscripts, poverty, and men and families is widening. The conservative, male-dominated points of view are at long last dissipating somewhat and new scholars are taking up the gauntlet of kinscripts and the labor of men in families. These scholars include, for example, Constance Dallas at the University of Illinois at Chicago (see Dallas and Kavanaugh 2010; Dallas et al., 2009; Dallas, Wilson, and Salgado 2000), and Kevin Roy at the University of Maryland, College Park (see Madhavan and Roy 2012; Marsiglio and Roy 2012; Roy 2004; Roy and Burton 2007; Roy, Messina, Smith, and Waters [forthcoming]; and Roy and Smith 2013). We are extending invitations to Constance and Kevin and others to join us for breakfast at Elmo’s to discuss these issues. Perhaps the legacy of our work will indeed live on in the work of these trailblazers and others who find a
theoretical home in the kinscripts perspective. We will continue to breakfast at Elmo’s every Thursday and move forward in holding ourselves accountable for our work and in engaging with our younger colleagues who are clearly opening the door for us and others to proceed with this dialogue. It is such a good feeling when a plan comes together—even when it takes over thirty years. That’s the magic of Elmo’s!

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