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Reconsidering the theory on adolescent-limited and life-course persistent antisocial behaviour

Abstract:
This article presents a critical review of the taxonomic theory of *adolescent-limited and life-course persistent antisocial behaviour* (Moffitt 1993) and its empirical evidence. This influential theory suggests that there are two qualitatively distinct types of offenders that require distinct theoretical explanations. Moreover, the empirical evidence for the typology is considered to be strong, at least by some. I discuss along three lines. First, to what extent the taxonomy should be interpreted literally. Second, whether the suggested mechanisms are likely to produce the hypothesized groups. Third, whether some of the most important empirical evidence really does support the theory. I conclude that the theoretical arguments are surprisingly unclear on key issues and that the empirical evidence is highly problematic.

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Introduction: Controversies in Criminal Careers Research

The now classic study of Wolfgang, Figlio and Sellin (1972) showed that a small group of 'chronic offenders' were responsible for about half of the offences in a birth cohort. These chronic offenders were defined by using a cut-off point of five or more offences by the age of 17, and they constituted six per cent of the cohort. Similar findings have been reproduced subsequently in a large number of studies, although the size of the group varies. In the 1980s, the Panel on the Research on Criminal Careers outlined what is now known as the *criminal career paradigm* as the study of characterization of the longitudinal sequence of crimes committed by individual offenders (Blumstein *et al.* 1986). They argued that it was necessary to disaggregate the course of offending into onset, duration, cessation, frequency, etc. and that each of these might have separate causes. One key issue in this paradigm was early prediction of chronic offenders and consideration of the effect of selective incapacitation of this group (see, e.g., Blumstein, Farrington and Moitra 1985). An organizing device was to distinguish between 'innocents', 'desisters' and 'chronics'. The chronic offenders have been shown to differ from the normal population and lower-level offenders on a range of characteristics. They have an earlier onset, longer duration and higher offending frequency, and they also differ on social characteristics and psychological measures. It is therefore tempting to suggest that these persons are radically different from normal persons, and their causes of offending might be very different from those with a later onset. However, this interest in offender types, and especially chronic offenders, was largely driven by empirical considerations and policy concerns, while the theoretical justifications were less clear (see also Osgood 2005).

In particular, Gottfredson and Hirschi took issue with the criminal career paradigm and argued that offenders differed in *degree*, not *kind*. Furthermore, they suggested that a latent trait, referred to as low self-control, could account for onset and duration as well as the other parameters of the career paradigm (Gottfredson and Hirschi 1986; 1990). A problematic point in early criminal career studies was how to define chronic offenders empirically. It is clear that a cut-off point of, for example, five or more offences was arbitrary, and that different cut-off points would lead to different numbers of chronic offenders. Critics argued that it was not necessary to create categories, and that a continuous latent trait might account for the diversity (Greenberg 1991; Rowe, Osgood and Nicewander 1990). The proponents of the criminal career paradigm maintained that disaggregation, into both offender types and the previously mentioned career parameters, was useful, and a methodological debate followed (see, e.g., Barnett *et al.* 1992; Land 1992).

Two major contributions that followed in the wake of this debate put the categorical approach ahead of further longitudinal studies of offending. First, Moffitt (1993) offered a taxonomic

theory of antisocial behaviour that filled a major gap in the criminal career literature by providing a theoretical justification for studying distinct categories of offenders. In short, this taxonomy suggests that there are two distinct types of antisocial individual. The 'adolescent-limited' type (AL) comprises normal adolescents and has social causes, but the 'life-course persistent' type (LCP) consists of persons with inherited or acquired neuropsychological deficits leading to an antisocial personality. Thus, each antisocial 'type' has its own distinct explanation, and while the AL type desists from antisocial behaviour by adulthood, the LCP type continues throughout the entire life-course. The second contribution was methodological. Nagin and Land (1993) offered an innovative solution for the empirical definition of subpopulations of offenders. The sample could be divided into discrete groups who displayed a similar offending pattern over time based on objective statistical criteria. This methodology is now known as semi-parametric group-based modelling (Nagin 1999; 2005). A large number of empirical studies have since identified subpopulations with offending trajectories in accordance with Moffitt's taxonomy, and the overall evidence in favour of the taxonomic theory is presented as being quite strong (Moffitt 2003; 2006).

Despite that the typological theory has proved to be very influential[1] parts of the taxonomic theory is surprisingly unclear on key issues, and much of the empirical evidence is embedded in controversial and complex methodological debates. In this article, I critically review some main characteristics of Moffitt's taxonomic theory and its empirical evidence. I do so along three lines. First, I ask whether the offender types are meant literally or not. This is a basic premise for the discussion, but it is not entirely clear in Moffitt's works; I suggest that a rather literal interpretation is reasonable. Second, I discuss the main mechanisms suggested by the theory, and ask whether it is likely that meaningfully distinct groups will arise from these mechanisms. I also ask on what grounds the causes suggested for one of the groups would not also apply to the other. Third, I discuss the kind of empirical evidence that arises from group-based modelling, questioning whether the evidence is as strong as some seem to think.

Moffitt's Taxonomy

At first, the taxonomy resembles other typologies in the sense that it suggests that there might be different causes for different kinds of offender. While 'general' theories of crime suggest that the same fundamental causes apply to all persons, although they may be differently exposed to these risks, the

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[1] The importance of this theory is reflected by that ISI Web of Science Citation Report shows that her 1993–article has been cited 1,911 times (by 13. mai 2009). Moffitt was also awarded the Stockholm Criminology Prize in 1997 on the basis that her *"discovery of 'adolescent-limited' versus 'life-course persistent' offenders in a birth cohort of New Zealand residents has been a major stimulus to research on patterns of offending."* (www.criminologyprize.com)

taxonomic approach argues that offending cannot be reduced to a single theoretical process[2] (see also Brennan, Breitenbach and Dietrich 2008; Mealey 1995). This is exemplified by Lykken's complaint (Lykken 1995: 180) that 'there seems to be an irresistible tendency for criminological theorists to oversimplify the causes of crime'. In a similar vein, Moffitt does not dismiss general theories but argues that 'the processes they describe may fit better for different types of delinquents' (Moffitt 1993: 694).

Thus, Moffitt argues that there are two distinct types of offender. The first is the high-rate offender who shares many characteristics with the 'chronic offender' (i.e., Blumstein *et al.* 1986), while the other is the antisocial type who does not differ from normal adolescents in other respects, and who desists from offending after a short period. These types are labelled life-course persistent (LCP) and adolescent-limited (AL) antisocial types, and can be defined by their antisocial (crime, aggression, etc.) trajectories. Moffitt not only describes these two patterns but also suggests that they are distinct types of person. In short, she suggests that 'temporary versus persistent antisocial persons constitute two qualitatively distinct categories of individuals (…) each in need of its own distinct theoretical explanation' (Moffitt 1993: 674). The LCP type is caused by neuropsychological deficits established before birth and through early childhood, while the AL type consists of normal children behaving antisocially during adolescence as a response to their experience of a 'maturity gap', mimicking the LCP type. In short, LCP 'has its origins in neurodevelopmental processes; it begins in childhood and continues thereafter. In contrast, adolescent-limited offenders' antisocial behavior has its origin in social processes; it begins in adolescence and desists in young adulthood' (Moffitt 2006: 570). One empirical implication is that the aggregate crime rate conceals that there are two distinct offender types: one chronic and one limited to adolescence. These are distinct, both in causes and outcomes, and identifiable through their developmental trajectory.

It is important to recognize that Moffitt's theory is to some extent deterministic considering the LCP type. The theory does not predict that LCPs will continue *offending* through the life-course, but it does predict that they will maintain *some kind* of antisocial behaviour at all stages of life. The LCPs will then typically display both strong heterotypic and homotypic continuity, but there may be changes in the types of antisocial behaviour they undertake. Nevertheless, it is implicitly stated that homotypic continuity is common, and change in lifestyle is unlikely as *'... a new job furnishes the chance to steal, and new romance provides a partner for abuse'* (Moffitt 1993: 684).

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[2] This kind of argument is found in various forms, but it is not quite clear why the general approach would imply that there is only *one* theoretical process. Admittedly, some have presented their theory in rather absolute terms (see, e.g., Gottfredson and Hirschi 1990), but most would only claim to highlight some main mechanisms (Akers 1973; Becker 1993; Becker 1963; Laub and Sampson 2003; Merton 1968), and it is hard to see that any of them would be incompatible with other theories. The general approach includes therefore *many* theoretical processes.

One of the most attractive features of the dual taxonomy is that it explicitly discusses the fact that a small proportion of the population seem to embark on a chronic criminal career (see also Blumstein *et al.* 1986), while some criminality through adolescence is actually fairly common among normal persons. For the LCP type, we need to explain their *continuity* in offending, while for the AL type it is necessary to explain *change*.

Literal types?

Although Moffitt talks about 'types', it is not always clear how literal these types are and how distinct their behavioural patterns are. We often use the terminology of 'types' for heuristic purposes, without assuming there are any clear distinctions between the types in real life. We use such typologies all the time in daily life and also as social scientists. Sociologists might talk about categorical constructs such as 'social class' or 'the conservatives', without necessarily assuming these to be clear-cut categories. Sociologists would also refer to such types as *Weberian ideal types* (Weber 1967 [1903–17]), but it is far from clear that Moffitt's typology is meant in this looser analytic sense.

We might add that our *understanding* of the nature of a taxonomy is itself on a continuum that ranges from *Weberian ideal types* to ontologically distinct subtypes of humans. The extreme notion of a typology would then perhaps resemble Lombroso's theory of 'the atavistic man' (see, e.g., Lilly, Cullen and Ball 2007), but most would probably interpret Moffitt's typology as being somewhat closer to ideal types. Although Moffitt's notion of types must be interpreted as being somewhere in between these extremes, I suggest that it is quite common to interpret the theory as being closer to the *literal* understanding than to being *ideal types*.

However, there are also reasons *not* to understand these types in a literal sense, as Moffitt has also referred to these types as 'hypothetical prototypes' (Moffitt 2006: 570). It is also hard to see how any of the risk factors and processes discussed by Moffitt (1993; 1997; 2006) can lead to truly discrete types. Moffitt also suggests that the AL type does *not* differ from other non-offending children. This implies that AL does *not* denote an interval of antisocial potential between non-offenders and the LCP type, and this does not fit the idea of a continuous distribution. As has been noted by others, if LCP is just another word for high-rate offenders, then it is hard to see what is gained by the new label (Sampson and Laub 2005a: 20).

One reason to take the typology rather literally is that the theory has been presented to contrast with earlier typologies that are merely descriptive (Moffitt 1993: 674), and the offender types have then been presented as being 'qualitatively distinct categories of individuals' (Moffitt 1997: 11). This use of terminology suggests that the types are meant to be distinct. Furthermore, if not taken literally, then there is no point in discussing whether more types exist, such as low-level chronics or

adult-onsets (see Moffitt 2006). Moreover, the taxonomy is presented in *contrast* to both general theories (Moffitt 1997: 43) and earlier merely descriptive typologies (Moffitt 1993: 674). The taxonomy aims to overcome the limitations of these theories, so it is unlikely that the types are intended in such a heuristic way.

I might be misinterpreting Moffitt on this point, but in that case, it is a common mistake and is in itself a reason to discuss this position critically. For example, in a well-established text-book it is asked whether 'offenders can be divided into two—and only two—types' (Lilly, Cullen and Ball 2007: 325). Others have questioned whether the types really are so entirely different (Smith 2002) and have generally discussed the dangers of accepting such offender types (Osgood 2005; Sampson and Laub 2005a). The typology has also been contrasted with 'general theories' (Blokland 2005; Brennan *et al.* 2008; Moffitt 1997:43). Similar accounts of the typology are also found elsewhere (Ezell and Cohen 2005; McDermott and Nagin 2001; Osgood 2005; Piquero, Farrington and Blumstein 2007: 139). These discussions are only meaningful if the types represent distinct entities, so even if this should be a misinterpretation of Moffitt's intentions, it is sufficient reason for discussing this position.

Same or Different?

The typological theory rests on a limited set of main mechanisms. Importantly, these mechanisms are not entirely new, but, as Moffitt states: 'What is new is the way in which many different theories of delinquency have been integrated under a taxonomic umbrella' (Moffitt 1993: 694). First, there are inherited and acquired traits that form an initial disposition towards developing an antisocial personality. Second, there are 'snares', where past behaviour limits opportunities and hinders the person from returning to prosocial behaviour. Third, there is a process of 'mimicry', where normal adolescents who would otherwise behave well nevertheless engage in antisocial behaviour. I will first discuss each of these processes in turn, and relate them to alternative explanations of the same phenomena.

Initial antisocial dispositions

The initial difference between AL and LCP types is that the LCP type suffers from neuropsychological deficits that prevent healthy development of prosocial skills. These risk factors are found in severe childhood conditions and include inherited traits in interaction with unfavourable environments. The environmental risk factors are the usual ones: poverty, harsh and incompetent parenting, poor neighbourhood, etc. None of these risk factors is entirely new to criminology, and most criminologists would agree that the risk factors are relevant for explaining offending. In fact, these factors are very similar to those proposed by self-control theory (Gottfredson and Hirschi 1990), but they are also well

known from other theories (Conger *et al.* 1994; Sampson and Laub 1993). The inherited (or acquired prenatal) risk factors include both genetic dispositions and prenatal events that might damage the foetus, such as maternal alcohol and substance abuse, which in turn damage neuropsychological development. Children are then born with varying vulnerabilities that might protect against, or reinforce, the impact of environmental risk factors.

Persistent offending is explained by having an LCP personality, while time-limited offending is explained by having an AL personality. The structure of the argument is then very similar to self-control theory (Gottfredson and Hirschi 1990) in the sense that a personality structure established early in life explains all later offending. Most sociologically orientated theories of crime would probably not dispute that there might be differences in children's initial dispositions but would treat such dispositions as 'unobserved heterogeneity' or selection processes that one should try to take account of.

So far, one could say that Moffitt's theory has added inherited traits to the list of risk factors, but the taxonomic assumption differs from other general theories by suggesting that the heterogeneity in personality traits is best conceived as two distinct types, rather than differences in degree. However, it is hard to see how any of the risk factors and processes discussed by Moffitt (Moffitt 1993; 1997; 2006) can lead to truly discrete types. Neither inherited or acquired neurological deficits, maternal drug use while pregnant, criminogenic environment, poor parenting and harsh discipline nor any of the other single factors discussed by Moffitt are likely to be discrete phenomena. All of them must be present to a greater or lesser extent. Moreover, it must be the *combination of these effects* that generates the two types. Adding together a number of continuous factors is not likely to lead to two literal discrete types unless there is some tipping point or extreme concentration of risk factors, but no such thresholds are discussed nor specified by Moffitt. It is then not clear how LCP and AL can be discrete types. Could it be that the LCP type just denotes the tail of a continuous distribution? In that case, it would be a theory of differences in *degree* of exposure to a range of serious risk factors, and the LCP type is simply those who have been the most exposed at an early age. However, in that case, the types would differ in the level of exposure and would not be *qualitatively* different. We could then interpret the LCP as the upper percentile of some distribution. In that case, it might be reasonable to interpret the AL type as being much less exposed to these risk factors but more exposed than non-offenders. The recently suggested 'low-level chronics' type (Moffitt 2006) seems to occupy an interval in between the AL and LCP types on the level of exposure to risk factors.

An additional feature of the AL theory is that most of them will desist from offending as they get older. When they move into adulthood, they will resettle into a conventional lifestyle, as they do not have any lack of prosocial skills that would prevent them from doing so. As their motivation for

offending is found in the experience of a 'maturity gap', this motivation will diminish as they get older. Furthermore, healthy youth are adaptive and will change behaviour with changing contingencies. Desistance from offending is therefore rooted in their prosocial personality (which is established early in life). This argument is structurally very similar to self-control theory (Gottfredson and Hirschi 1990), where adolescents are said to grow out of crime with changing circumstances and opportunities. Although this theory argues that there is a decline in offending for all persons, those with the lowest level of self-control will continue offending, while those with a medium level of self-control will terminate their criminal career. This process is also compatible with the age-graded theory of social control (e.g., Sampson and Laub 1993), where the transition to adulthood brings with it changing agencies of social control and opportunities.

Snares

The argument of personality structures explaining all later offending is a static explanation, suggesting that no events later in life have an effect on offending. A dynamic element is introduced to the taxonomic theory by the notion of 'snares'. Moffitt suggests that initial individual differences may lead to children missing out on opportunities to learn prosocial skills because of rejection by peers and adults. Vulnerable children are then in danger of 'becoming ensnared by the consequences of antisocial behaviour' (Moffitt 1993: 684). As these mechanisms occur already in early childhood, they are also part of the mechanism that forms personality structure. Those children who are not initially vulnerable (the AL type) might also be captured by similar snares. Even if the initial motivation for antisocial behaviour is not due to an antisocial personality, the negative consequences of delinquency might reinforce further delinquent behaviour. This occurs through damaging the relationships with parents and peers, provoking responses from adult authorities (Moffitt 1993: 688).

Although Moffitt claims that the causes of LCP and AL are fundamentally different, it seems that the mechanism of reinforcement by the negative consequences of delinquency is similar for both LCP and AL. However, the consequences of such 'snares' are different at different ages. In particular, if the cumulative process of reinforcement starts early, it will hamper learning of social skills and contribute to the development of an antisocial personality. Note, however, that this depends on the reactions from others, so that problematic behaviour can be ameliorated by actions such as effective parenting.

Interestingly, such cumulative processes are no different from those of classical stigma theory (Becker 1963) and Sampson and Laub's (1993) argument about cumulative disadvantages. Rather, Moffitt emphasizes that the consequences might be a little different at early ages because of the hampering of social skills development. A more important point is that such mechanisms introduce

further continuously distributed variability, and it becomes even more difficult to see how there can be discrete types of offender. If the first delinquent act occurs at different ages for different people, then the reinforcing mechanisms must also vary to a similar extent. Again, as no threshold effect is mentioned by Moffitt, this must be gradual variability, making the 'distinctly different' aetiology between AL and LCP a sliding scale.

Mimicry

According to Moffitt, there is one mechanism that applies only to the AL type. They are characterized as normal children with a prosocial personality. The cause of offending for these children is the experience of a 'maturity gap' when they reach puberty. They feel more adult than they are treated, and they seek to engage in behaviours that signal adult roles. They also perceive their antisocial peers as more autonomous, doing what they like, engaging in offending, drinking and being more sexually active, etc. This appears to be a successful strategy, and the AL type mimics this behaviour to such an extent that during adolescence, it is hard to distinguish between AL and LCP types. However, neither the frustrations of experiencing a 'maturity gap' nor mimicry is new to criminology, although mimicry is more usually referred to as *learning* (Akers 1973).

It appears that the LCP type does not experience a maturity gap and that this mechanism is not at all important to these individuals. Neither is it clear why the LCP type would not be able to learn from others (or 'mimic'). I find it hard to see that exposure to early risk factors and experiencing frustrations due to the 'maturity gap' should be mutually exclusive processes, and Moffitt does not present any argument for this being the case. One might perhaps suggest that as the LCP type would behave antisocially in any case and as this behaviour does not need to be learned because it comes naturally for these individuals, so additional frustrations at this age would not have any further effect on offending. This is reasonable if all LCP adolescents had already realized their full antisocial potential, but as this is far from clear, I see no reason why there would not be an additional effect also for the LCP type.

I am unaware of whether there has been empirical testing to determine whether LCP offenders experience such a maturity gap and mimic others. However, *if* this is the case, the lack of any additional effect for them could of course also be explained by the fact that they are already characterized by a deviant lifestyle. There might be a declining effect of *additional* risk factors, and any further increase in exposure will have negligible additional effect on behaviour. In other words, the effect of a maturity gap (or any other risk factor) will depend on the total exposure to risk factors and possibly also on age. In this case, it is quite possible that certain risk factors may seem more pronounced for some adolescents than for others, without evoking the notion of distinct types.

10

The Empirical Evidence

The above discussion suggests that the dual taxonomy to a large extent summarizes uncontroversial mechanisms that are compatible with traditional general theories. However, an important contribution is that if certain social skills are not learned at an early age, then it is harder to learn them later in life, and that certain neurological deficits make some children particularly vulnerable. What remains as a distinct feature of Moffitt's theory is the taxonomy itself: the division of the population into two distinct types. It seems, then, that it boils down to the *existence* of antisocial types. Although I have argued that the theoretical arguments are not sufficiently clear, the existence of 'types' can also be seen as an empirical question. I will therefore discuss some of the empirical evidence for the existence of the AL and LCP types.

The identification of types and the classification of persons into these types are also important for further tests of the taxonomy. In fact, Moffitt has proposed several testable hypotheses about the two types, but these hypotheses are conditional on the identification of the types.

> If appropriate research designs fail to yield the predicted individual natural histories (or growth curves), at or near the predicted base rates, then the theory is wrong. However, if subjects are found who match the natural histories of this taxonomy, then the following hypothesis may be tested about differential predictors and outcomes. (Moffitt 1993: 694)

The first challenge then is to define the two types, and it should be recognized that in the empirical literature reviewed by Moffitt (2006), a particular statistical technique holds a prominent position to this end. Many of the empirical studies she lists as finding support for the taxonomy are using the so-called 'semi-parametric group-based modelling' technique (SPGM), and she explicitly recommends this approach for future studies (Moffitt 2006). She has repeatedly argued that this technique gives a more objective basis for assessing the number of subpopulations in a sample (see, e.g., Moffitt 2006: 576, 579, 581), as is clear from the following.

> First, the methods are agnostic with respect to taxonomic theories, and thus results are relatively free from investigator bias. Second, the methods can search a longitudinal data set to ask whether there is indeed more than one developmental trajectory in it, as a taxonomy implies. Third, they can ascertain (…) whether the taxonomic theory has specified the right number of developmental subtypes in the population. (Moffitt 2006: 585)

This methodology is a statistical technique that identifies clusters of persons who behave in similar ways through time, and is often referred to as a method capable of testing for the existence of subpopulations that are not directly observable (Nagin 1999: 139; 2005: 11). Simulation studies have shown that correct classifications can be made in this way (Brame, Nagin and Wasserman 2006).

Although this is a well-documented technique based on sound statistical considerations, its uses and interpretation have been matters of considerable controversy, and this kind of interpretation has been highly contested (see, e.g., Bauer 2007; Eggleston, Laub and Sampson 2004; Nagin and Tremblay 2005; Raudenbush 2005; Sampson and Laub 2005a; Skarðhamar 2009). Despite these methodological concerns, SPGM seems to be very important to empirical assessment of the taxonomy. I will therefore limit this discussion to the empirical evidence from SPGM studies.

It has been claimed that the hypothesized types are found in all studies (Moffitt 2006). However, most of the studies conclude more ambiguously in relation to the typology than Moffitt's review suggests. A more common conclusion in the literature is that the results are in accordance with some aspects of the typology but also with general theories (e.g., Brame, Mulvey and Piquero 2001; Fergusson, Horwood and Nagin 2000). Piquero's review of SPGM studies includes more than 80 studies on trajectories of offending and aggression, and concludes that the findings are in accordance with Moffitt's taxonomy (Piquero 2007: 49). He also states that as between three and five groups are consistently identified across studies, this 'certainly suggests some sort of generality in the findings', but it is less clear whether the generality of the findings is the existence of offender types, or merely that a small number of latent classes can capture the observed variability in the trajectories. As the latter interpretation would not be very remarkable, one gets the impression that there is at least some support for the notion of meaningful 'types'. Many of the studies are also critical of at least some notions of the typology (e.g., Blokland et al. 2005; Brame et al. 2001; Ezell and Cohen 2005; Laub and Sampson 2003), and most researchers are reluctant to use the terms AL and LCP, and prefer to invent new labels for their groups. Many studies aim not only to identify the types but also to test other hypotheses derived from the typology, usually by comparing the group profiles. Some find mixed evidence for the typology in this way (e.g., Blokland et al. 2005; Fergusson, Horwood and Nagin 2000), while others find rather strong support (e.g., D'Unger et al. 1998; Nagin, Farrington and Moffitt 1995; Odgers et al. 2007). Nevertheless, such further tests of the theory rest on the initial identification of types, and I limit the discussion to this point, as this seems to be the main concern also for Moffitt (2006).

The number of groups varies a great deal across studies. Even though most studies identify four or five offender groups (D'Unger et al. 1998; Piquero 2007), there are also studies that find three (Nagin and Land 1993), six (Ezell and Cohen 2005), or seven groups (Bushway, Thornberry and Krohn 2003). Some studies find multiple chronic groups of various shapes (D'Unger et al. 1998; Ezell and Cohen 2005; Laub and Sampson 2003; Nagin and Land 1993) or multiple time-limited groups (Bushway et al. 2003; D'Unger et al. 1998). The LCP type seems to have been of primary interest, but even though 'all studies that have set out to identify a persistent antisocial type have found one'

(Moffitt 2006: 593), this does not necessarily mean they have found the same type. The results diverge considerably. For example, the trajectory of the LCP type (or the equivalent highest rate or persistent group) might display a peak at age 13 (Fergusson *et al.* 2000), age 15 (Bushway *et al.* 2003), age 18 (Nagin *et al.* 1995; Nagin and Land 1993; White, Bates and Buyske 2001), age 22 (Piquero *et al.* 2007), age 21 or 29 (Ezell and Cohen 2005), or even age 37 (Laub and Sampson 2003). The LCP trajectory is bell shaped with a peak in late adolescence or early adulthood in most studies (Bushway *et al.* 2003; D'Unger *et al.* 1998; Nagin and Land 1993), but it can also rise sharply towards the early twenties and then flatten out (Blokland 2005), be flat throughout adolescence (Wiesner and Windle 2004) or even *decrease* throughout adolescence (Fergusson *et al.* 2000; Odgers *et al.* 2007). The LCP type might offend at a considerably higher rate than all other groups at all times (Blokland 2005; Nagin and Land 1993; Wiesner and Windle 2004), or other groups might offend at a higher level at some ages (Bushway *et al.* 2003; Fergusson *et al.* 2000).

The studies use very different kinds of data, measures of crime, and lengths of follow-up (see Piquero 2007 for a more thorough overview). Some use conviction data (Blokland 2005; Piquero *et al.* 2007), others use self-reported crimes (Fergusson *et al.* 2000), and some use a broader measure of delinquency or conduct problems (Odgers *et al.* 2007; Wiesner and Windle 2004). Some studies cover the whole lifespan (Blokland *et al.* 2005; Laub and Sampson 2003), others about two or three decades (Piquero *et al.* 2007; White, Bates and Buyske 2001), and yet others about a decade (Fergusson *et al.* 2000; Odgers *et al.* 2007) or only a few years (Wiesner and Windle 2004). These differences in data are likely to affect the number of latent classes (D'Unger *et al.* 1998; Eggleston *et al.* 2004) and make it harder to judge whether the hypothesized groups are reproduced across studies. Nevertheless, findings from various types of study have been summarized as being in support of the typology without much discussion about possible incomparability (Moffitt 2003; 2006). An exception is Piquero (2007), who compares similar studies as far as possible.

One study uses reasonably comparable data from three cities (London, Philadelphia, Racine) and deserves special attention for that reason. The authors state that the latent classes are reproduced across sites, and that the groups are 'meaningful in and of themselves (…) rather than merely representing a discrete approximation to an underlying continuous distribution' (D'Unger *et al.* 1998: 1622). There are clearly some similarities across sites, but as the authors also recognize, there are some marked differences as well. For example, while the high-rate chronics in the London cohort are convicted at about twice the rate of the AL type, in the Philadelphia cohort, similar high-rate chronics have police contact at a rate similar to the low-rate AL type, while the late-onset chronic group from the Racine cohort (felony rates) is not reproduced at the other sites at all. It is not clear what it takes

for these groups to be reproduced, and the ambiguity of how to interpret the trajectory groups is reflected in the authors' warning against 'overinterpretation' (p. 1623).

Some of the differences between studies can probably be explained by the fact that the studies use different kinds of data or measurements. However, they can also be due to the groups being artefacts of the method rather than reflecting some substantial entities, and the varying data sources just make it harder to see the incongruence. It is not clear what degree of difference between studies can be accepted while retaining the assertion that the types are reproduced. One reason why the findings are accepted as supporting the typology (at least by some) could be that the theoretical definitions of the types are rather vague. The LCPs offend at a *somewhat* persistent and high rate (although low-rate LCP is also accepted), and the ALs offend at a *somewhat* time-limited and low rate (although high-rate AL is also accepted). It is then hard to see how such findings could *not* support the typology at some level. In conclusion, the large number of empirical studies using SPGM, many of which are interpreted by proponents of taxonomic theories as supporting the taxonomy, have *not* convincingly reproduced the hypothesized types.

A related question is whether the recommended SPGM technique really is capable of testing taxonomic theories, as it is the subject of controversial and complicated methodological debates (Bauer 2007; Nagin and Tremblay 2005; Sampson and Laub 2005b). A thorough discussion of these debates would exceed the scope of this article, but some points that are largely uncontroversial should be mentioned. First, SPGM will *always* find groups whether they truly exist or not. Furthermore, there are no direct tests of 'distinctness', so it is hard to know the extent to which the underlying distribution is continuous or discrete (see also Bauer 2007). Given the empirical evidence, the individual differences are just as likely to be *gradual* on a sliding scale. Importantly, most authors of SPGM analyses are aware of these caveats. It is my impression that almost all SPGM analyses refer to Moffitt's typology as a motivation for the analysis, but I have seen few analyses with a clear conclusion regarding the typology. It is then puzzling how these findings can be summarized as giving strong support for the typology. It is also unclear how SPGM can be used to substantiate Moffitt's typology. Raudenbush has explained how such small mechanisms are capable of producing all kinds of trajectories that might not be predicted so easily in advance (Raudenbush 2005). This has also been shown in a simulation experiment where continuous heterogeneity in combination with state dependence and random events is sufficient to generate similar aggregate patterns that appear to be distinct trajectory groups when analysed using SPGM (Skarðhamar 2009). It is therefore reasonable to suggest that the empirical patterns found in the empirical literature fit equally well with a general, mechanism-based approach and with the categorical approach. It then turns out that the empirical

evidence for the taxonomic theory that has applied SPGM (which is a substantial proportion of the evidence) should at best be regarded as imprecise.

Conclusion

I have argued that although Moffitt's taxonomy has become very influential in criminology and has inspired a large amount of empirical work, it is vague on key issues. First, it is unclear to what extent the notion of 'types' is meant literally, even though one key contribution of the theory is to integrate a multitude of mechanisms under a taxonomic umbrella (Moffitt 1993: 694). Second, it is hard to see how the suggested mechanisms would produce any meaningful groups at all, particularly as no threshold effects are mentioned. Third, it is not clear why the causes would be fundamentally different across the types and not merely differences in exposure and perhaps age-graded effects. From the theoretical arguments, it would therefore be more reasonable to assume gradual differences rather than discrete types. Moreover, it is hard to see why one needs to invoke the notion of two distinct types to discuss the more specific mechanisms and risk factors included in the theory.

Finally, I have argued that the empirical evidence for the taxonomy from group-based modelling is not compelling. The findings differ markedly across studies, and the trajectory groups are reproduced across studies only if one is willing to accept quite extreme variation in the observed patterns. The empirical evidence is also embedded in methodological controversies, and there is serious doubt that this kind of methodology is able to distinguish between general and taxonomic theories at all (Bauer and Curran 2004; Sampson and Laub 2005a; Skardhamar 2009). This is in sharp contrast to the way in which empirical evidence has been presented previously (Moffitt 2006). To summarize the findings from all these studies as supporting the taxonomy ignores the fact that the vast majority of these studies have conclusions that are more ambiguous about the taxonomy.

This discussion is based on a rather literal interpretation of the taxonomic theory, and I have argued that there are ample reasons for doing this. It might nevertheless be that the taxonomy was not initially intended this way. Importantly, the usefulness of any typology depends on its purpose. It may be more convenient to describe the world in terms of categories rather than as a continuous distribution. For example, one would often refer to 'the rich' instead of 'those earning more than the 90th percentile of the income distribution'. Although the latter is more precise, the former is more convenient and sometimes *sufficiently* precise. In a similar way, it might be convenient to refer to 'chronic criminals' rather than some elaborate definition of whom we are actually talking about. To use the notion of 'types' in this loosely descriptive sense may be appropriate depending on the setting, but it implies that it is possible to be more precise. One may, for instance, describe the entire distribution.

If the typology is meant as an *explanatory* theory, then individuals' observed behaviour is explained by having one of the proposed personality types. For example, the behaviours of high-rate offenders are explained by being an LCP type, and desistance from crime in adolescence are explained by being an AL type. If the taxonomic theory is explanatory rather than descriptive, the usual demands for clarity, specificity and testability apply. I have here assessed Moffitt's taxonomy as an explanatory theory, and my criticism would naturally diminish if the taxonomy did not include such theoretical ambitions.

It follows that one should be cautious about basing practical policy on taxonomic theory, and in particular debates about incapacitation. There is a tradition of combining the idea of offender types (i.e., chronic offenders) with selective incapacitation strategies (Blumstein *et al.* 1986), and Farrington made an argument for incapacitation through an analogy to medicine by stating that *'Many illnesses can more easily and effectively be prevented than cured. One method of preventing the spread of diseases is through quarantine'* (Farrington 1987: 89). The belief in a truly chronic offender type, such as the LCP, who is not likely to change behaviour, will easily lead to support for incapacitation rather than wasting time on rehabilitation for this group. It may still be reasonable policy to target high-rate criminals, but this can be done without considering the high-rate offenders as fundamentally different from other people. Another policy implication is early intervention to prevent children from developing an LCP personality. Although most researchers and practitioners alike would agree that early intervention is generally important, it is not necessarily advisable solely because otherwise they will become future LCP criminals. Early intervention would in most cases be sufficiently justified by welfare considerations based on solving children's immediate problems,[3] but intervening early with the intention of identifying future chronic offenders is to treat troubled children as future criminals even before they have had the chance to offend. A belief that early problems are risk factors for future problems does not entail the idea of distinct offender types.

In sum, the taxonomic approach does not offer much that is not already included in the various general theories, although Moffitt emphasizes learning of prosocial skills and the development of an antisocial personality. However, the theory has not presented any arguments that a typology is truly needed, as similar patterns can also be interpreted in light of established general theories. The taxonomy is certainly valid in so far as there exist people who behave antisocially throughout their lives, and that they differ from others on a range of characteristics, but it is nevertheless more reasonable to suggest that the differences between the law-abiding and the highest-rate offenders are

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[3] Note that the risk factors discussed by Moffitt (1993) are all problems from both the child's and the parent's perspective.

on a sliding scale. Although the extremes may appear to be black and white, there are considerable shades of grey in between.

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