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Abstract

Whereas most research on substance (ab)use by adolescents studies only the effects of *personal* networks of adolescents, we propose a theoretical approach that allows for predictions on the effects of the *macro*-level social network structure on usage rates. We model alcohol use as risk-dominant but inefficient behaviour in a coordination problem, given that adolescents face incentives to align their behaviour with their friends. We propose a game-theoretical model in which actors choose behaviour in a repeated coordination game in an endogenous network. Predictions on levels of alcohol use depending on initial network structure are based on computer simulations of this model. We test the predictions using longitudinal data on alcohol use and friendship choices in school classes in Dutch high schools. We replicate the predicted ‘catalysing’ effect of initial network density on the development of alcohol use but the predicted opposing effect of centralization could not be confirmed.

Keywords

Adolescents, game theory, simulation, social networks, substance abuse

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Introduction

Adolescence is a life stage in which many forms of problematic behaviour reach their peak (Steinberg and Morris, 2001), including delinquency and substance abuse. Even though there is little evidence that problematic behaviour in adulthood *originates* from behaviour during adolescence (Moffitt, 1993), these types of behaviour may have considerable impact on adolescents themselves and on society in general. Substance abuse, especially, on which we focus in this article, is associated with problems in other areas, including delinquency, mental health problems, and problems with educational attainment (Newcomb and Bentler, 1989). Social influence by peer groups has often been named as one of the important factors that can trigger various types of problematic behaviour, including alcohol and drug abuse (e.g., Hawkins et al., 1996; Moffitt, 1993; Newcomb and Bentler, 1989). Consequently, relationships among adolescents have been the focus of a considerable body of literature (Giordano, 2003).

Issues of peer influence and selection have been of major concern within this context. On the one hand, it has been found that adolescents are sensitive to the influence of peers (Bot et al., 2005; Graham et al., 1991; Swadi, 1999). On the other hand, it has also been recognized that not only are adolescents influenced by their social environment, but they also choose peers as friends who are similar to themselves, a phenomenon known as homophily (Lazarsfeld and Merton, 1954; McPherson et al., 2001). Disentangling these two simultaneous processes poses an ongoing theoretical and methodological challenge (Bauman and Ennett, 1996; Kirke, 2004).

Small and medium-sized groups of adolescents have long been a popular setting for sociologists to study social networks, and the emergence of cultures and norms. Schools, in particular, are an attractive setting to study these topics because they constitute relatively well-delineated social contexts, in which complex processes can be observed relatively easily (some prominent examples include Bearman et al., 2004; Coleman, 1961; Epstein and Karweit, 1983). In this sense, a school constitutes a kind of 'social microcosm', or, as Coleman (1961: 9) put it, a 'society within society'.

Research on diffusion dynamics has shown that the overall structure of a network has important consequences for emerging patterns of behaviour (e.g., Centola, 2010; Centola and Macy, 2007; Granovetter, 1973; Watts, 2002). Yet, while studies on adolescent behaviour often emphasize the importance of social networks, most research focuses on *individual-level* explanatory factors such as attributes of personal networks (e.g., Graham et al., 1991) or individual network positions (e.g., Ennett and Bauman, 1993). A possible reason for this divergence is that theorizing on the effects of the macro-level network structure requires the specification

of the mechanisms that connect the macro-level network structure with micro-level individual behaviour, and conversely, micro-level behaviour with macro-level collective outcomes. Given the interdependencies involved in interpersonal influence processes, specifying these macro-to-micro and micro-to-macro mechanisms is not trivial (cf. Coleman, 1990), and the inclusion of co-evolving network processes complicates matters further.

Theoretical tools that are particularly suited to deal with interdependencies of individual action and the transitions between different levels of aggregation are game theory and agent-based modelling. In this article, we follow an approach based on these tools to study the effects of the macro-level network structure on alcohol use. We aim to explain differences in average alcohol use between *groups* of adolescents (in this case, school classes) by means of the social network structure in the group at the start of the influence-selection process. We formulate the following research question:

To what extent can properties of the initial overall network structure explain differences in average alcohol use between school classes?

In answering this question, we use a theoretical model based on *strategic* interaction, in which we model social influence as a coordination game. In this model, we explicitly account for endogenous evolution of the network.

In recent years, research on social networks has blossomed in economics, spawning an extensive literature of game-theoretical models of network effects, network dynamics, and co-evolution of networks and behaviour (for good introductions to this literature see Goyal, 2007, and Jackson, 2008). The potential significance of this literature is considerable. In particular, it may be capable of bridging the 'theory gap' in social networks theory, by providing very explicit mechanisms through which individual, goal-directed action leads to macro-level outcomes in networks (Corten, 2009; Granovetter, 1979).

For our scenario, the model by Jackson and Watts (2002) is particularly relevant. In fact, the theoretical model used in this article can be considered as a variation of the Jackson and Watts (2002) model. Although influential, however, game-theoretical network models are relatively rarely tested empirically. A probable cause for this lack of empirical validation is that game-theoretical models of networks typically predict many equilibria or stable states. This article contributes to this literature by developing an approach that circumvents the above problem and thereby allows for a first test of the Jackson and Watts (2002) model in a natural setting. In short, our approach consists of using simulation techniques, applied in an earlier article by Buskens et al. (2008), to predict properties of stable states depending

on initial conditions, and testing these predictions on the relation between initial conditions and outcomes using group-level empirical data.

Before we explain how our approach complements existing approaches to co-evolution of networks and behaviour, we outline the theoretical model as analysed by Jackson and Watts (2002) and Buskens et al. (2008). We then briefly summarize the simulation analyses by Buskens et al. (2008), which provide the basis for our predictions. Subsequently we describe the data and methods used to test these predictions, and present the results of our analyses. In the final section, we reflect on the implications of the results for our approach and how it relates to other approaches, and discuss alternative specifications of the theoretical model.

Coordination, influence, and alcohol use

In developing our model, we assume that when deciding whether to use alcohol, adolescents have incentives to choose the same behaviour as the peers they interact with, for reasons that we outline below. This implies interdependence between adolescents’ decisions resembling the strategic structure of a *coordination game*. We assume that the outcome of this ‘game’ determines the utility that a student derives from each of his friendship relations. The outcome of the game, in turn, depends on the behavioural choices of both students involved in a friendship relation. The game is shown in Figure 1, in general form and with numerical payoffs (only the relative values of the pay-offs matter). In a friendship *network*, students play this game with multiple friends simultaneously. For ease of exposition, however, we first discuss the two-player setup and afterwards generalize this to a network setting.

Each player in the game has two options – either to drink or not to drink alcohol. This game is a coordination game because it has two Nash equilibria in which both players choose the same action. Thus, the players prefer to play the same action as their interaction partner, reflecting the basic idea that adolescents face a *pressure for conformity* in their behaviour.

There are several reasons why such a peer influence might exist. First, there may be some intrinsic reason why an activity brings more utility if it

		Player 2		Player 2	
		Drink	Don't drink	Drink	Don't drink
Player 1	Drink	a,a	c,b	$14,14$	$8,0$
	Don't drink	b,c	d,d	$0,8$	$20,20$

Figure 1. Alcohol use as a coordination game, in general form and with numerical payoffs. $b < c < a < d$; $(a-b) > (d-c)$.

is coordinated with others. A trivial example is games – playing ball is more fun if you can have someone to play with you. Similarly, drinking alcohol is most likely a social behaviour, in the sense that the utility of use is higher if it is shared with someone else. A second incentive for coordination is *imitation*. During adolescence, people go through important changes, and consequently face many uncertainties. As a result, adolescents may look at their peers as a reference to help them determine which behaviour is appropriate (Marsden and Friedkin, 1993). Third, there may be norms among groups of adolescents that promote conformity in general, as well as in the area of substance use (Sherif and Sherif, 1964). These three distinct pressures all lead to incentives for adolescents to coordinate their behaviour. In the coordination game in Figure 1, this is represented by the fact that the payoffs for both players are higher when they choose the same action than when they choose different actions (i.e., $a > b$ and $d > c$).

The preference to coordinate does not imply that students are necessarily indifferent about whether to use alcohol or not. In the structure of the game, we assume that students prefer abstinence to using alcohol, given that they coordinate their behaviour. In Figure 1, this is reflected by the fact that $d > a$. This mirrors the assumption that the disadvantages of using alcohol in terms of the financial costs, long-term health risks, and possible sanctions by parents and teachers are higher than the short-term gains.

Another feature of this game is that the ‘punishment’ for failure of coordination differs between the actions. In our numerical example, if Player 1 chooses to drink and Player 2 chooses not to drink, the payoff of Player 1 is 8 while the payoff of Player 2 is 0. In other words, in a situation where one drinks and the other does not, this is worse for the one who does not drink. This assumption of asymmetric actions signifies that the use of alcohol has a number of effects on social behaviour that have a negative impact on the social environment of the user, rather than on the user herself. For instance, drinking can lead to inflation of the ego, increased risk-taking, or downright aggression (Steele and Josephs, 1990). In situations where some use alcohol and others don’t, such behaviours can have negative consequences, especially for the non-users. In this sense, we can say that abstinence involves a higher risk on a lower payoff, such that the equilibrium in which both players drink can be classified in game-theoretic terms as a *risk-dominant* equilibrium (Harsanyi and Selten, 1988). In Figure 1, this feature is established by the assumption that $(a-b) > (d-c)$.

The game in Figure 1 provides a simple model for two actors in a friendship relation, deciding whether or not to drink. In reality, such choices take place in friendship *networks*, in which students maintain relations with several friends. We can extend our two-player model to a network model by

assuming that every player plays with *several* interaction partners simultaneously. Each player can choose one action against all interaction partners (i.e., either consume alcohol or not), and receive the payoff as in Figure 1 from every interaction. Thus, students receive utility from every friendship relation separately, but must also adjust their behaviour to several friends simultaneously.

A crucial assumption of this model is that actors cannot differentiate their behaviour between different interaction partners: a student cannot choose to drink with one friend and not with the other. The rationale for this assumption is the idea that by choosing to drink in some situations, students make a general decision to 'be a drinker', and thereby influence *all* their relations. Obviously, this is a simplification. In reality, it is entirely conceivable that students behave differently with one friend than with another. However, such differentiation is likely to be more costly in terms of effort than the situation in which one can simply use one mode of behaviour. By considering alcohol use as an individual attribute rather than as relational choice, we also conform to the standard in the literature (e.g., Bot et al., 2005; Graham et al., 1991; Kirke, 2004), in which substance use is typically analysed as an individual characteristic and not as a relational attribute.

In order to include selection, we assume that actors can choose with whom to interact. We assume that the coordination game is played repeatedly, and that in every iteration of the game, actors can choose both their behaviour in the game, and with whom to play. When updating their behaviour or their network links, actors choose the optimal response to what their interaction partners did in the previous interaction. Figure 2 illustrates this process. Actor i plays the coordination game with her two 'neighbours' j and k . Actors i , j , and k play one type of behaviour, while actor l plays the other type of behaviour. At the same time, i and l have the opportunity to form a

Figure 2. A coordination game in a dynamic network.

new link. We assume that maintaining social relations costs time and effort, such that every actor has to pay some cost for every link she maintains. The link between i and l is only formed if the expected benefits will outweigh the costs for both i and l .

Thus, we have sketched the outlines of a simple game-theoretic model for selection and influence in a dynamic network. We use a slightly more complex version of this basic model to derive specific hypotheses to be tested in the context of alcohol use. We first discuss how such a model could contribute to understanding influence and selection processes in comparison to earlier approaches.

Approaches to the study of selection and influence

Disentangling the simultaneous effects of influence and selection has been the focus of considerable research effort in the past decade. The basic problem was already recognized by Cohen (1977) and Kandel (1978), who noted that the effect of influence is likely to be overestimated if selection effects are ignored (the reverse also holds). A major breakthrough in the study of influence and selection was achieved by the introduction of actor-oriented, simulation-driven statistical models for longitudinal network data, implemented in the SIENA software (Snijders, 2001). In brief, this method works as follows: given a set of subsequent observations of a social network, these panel data are considered to constitute 'snapshots' of an underlying dynamic process. It is assumed that this process is driven by actors trying to optimize an *objective function* (roughly, a random utility function), both with regard to their own network position and with regard to their own behaviour. The aim of the method is to estimate the components of these objective functions based on the observed 'snapshots' of this process. This is achieved by simulating the underlying process, and optimizing the fit between the simulated process and the observations based on maximum likelihood criteria. The effects of influence and selection can be identified as separate coefficients in the estimated objective function. Detailed expositions of the SIENA method can be found in Snijders (2006) and Snijders et al. (2007). Examples of applications of the method studying co-evolution of behaviour and networks can be found in Steglich et al. (2006), Light and Dishion (2007), and Knecht (2008), among others.

Using the data we also analyse in this article, Knecht et al. (2011) applied SIENA to study the co-evolution of friendship networks and alcohol use to disentangle influence and selection effects. The results showed a clear selection effect (adolescents select friends based on similarity in drinking behaviour), but provided only weak evidence for social influence.

In this article, we take a somewhat different approach. We first use the coordination model to derive predictions about the relation between the initial state of a group (in this application, a school class) in terms of network structure and behaviour and aggregate behaviour at a later stage. We compute aggregate statistics on the groups in our data to create a dataset of *groups*, each observed at two different time points. We then test statistically whether the groups in the data developed in the way that was predicted by the model.

To explain how this approach compares to SIENA, we highlight the most important differences and similarities. First and foremost, our method tests predictions at the *macro level* (that is, at the level of a whole network), while SIENA tests predictions at the individual level (namely, hypotheses about components of the utility function). In a sense, we can say that SIENA tests hypotheses on how the process works at the micro level, while we test hypotheses on the *outcomes* at the macro level, assuming a specific theory on how the process works at the micro level (also see Currarini et al., 2010, for a somewhat similar approach). We explicitly use initial states of the co-evolution process to predict outcomes, while SIENA merely conditions its simulations on the initial states. In this sense, the approaches are complementary. In the treatment of the data, the main difference is that we use only aggregate measures of network structure and behaviour to test our predictions, while SIENA needs individual-level information.

A second difference between our approach and the SIENA approach is that rather than estimating properties of the network dynamics from the data, we assume a very *explicit* model of network formation and co-evolution. That is, we specify in detail the precise strategic nature of the interaction by means of the coordination game. In this respect, our model is more detailed in the specification of actors' incentives than SIENA. Similarly, we do not estimate the rate at which network changes can occur (the *rate function* in SIENA terminology), but instead make specific assumptions on this rate (Buskens et al., 2008 show that outcomes are robust under different assumptions on the speed of network dynamics).

Besides differences, there are also a number of similarities between our analysis and a typical SIENA analysis. First, and most importantly, both methods assume the possibility of an underlying co-evolution process in which both individual characteristics and the network change. Second, both methods rely on simulation to handle the complexity implied by a co-evolution process. Third, both methods assume that changes take place in 'microsteps' – only one link, or one actor's behaviour, changes at a time.

By testing macro-level hypotheses, we avoid two disadvantages of SIENA. The first problem concerns the theoretical interpretation of SIENA

results; the second (related) problem concerns data requirements. Consider a co-evolution process based on coordination in a dynamic network. Suppose that, at some point, the network has reached a stable state in terms of behaviour – no actor can improve her utility by changing her behaviour, but some *can* improve their utility by changing ties. We observe the network for a few more ‘snapshots’, in which very little change in behaviour is observed (because it is already in, or close to, a stable state), but some tie changes are observed. SIENA results on these observations would probably indicate that there is no influence going on, only selection. The theoretical implication of this would be that only selection plays a role in the considerations of the actors, even though stability in behaviour is implied by a coordination game. In other words, social influence can only be identified as change, although theoretically social influence could also be expressed by *stability*. The second problem is related to the first. Estimating effects of selection and influence in a SIENA model requires that enough changes in both the network and behaviour are observed in the data. If an observed network is close to stability on one of the dimensions, this may lead to estimation problems in SIENA, forcing the analyst to drop these observations from the data (cf. Knecht et al., 2011, who can analyse only 78 out of 120 school classes).

The alternative approach we use here suffers less from these issues. Because our approach relies on examining *outcomes* of an underlying co-evolution process, it circumvents the problem of the interpretation of results that we identified above. Even if, in a given network, only relational changes are observed, our model still provides predictions that follow a model that assumes both selection and influence. Thus, networks that are observed in a stable state can also be compared with the predictions, because the theoretical model *predicts* what stable states should look like. As a result, networks that are relatively stable also contribute to the test of the hypotheses, while they would lead to estimation problems in SIENA. In the following, we apply our model to predict properties of stable states from initial conditions in terms of behaviour and network structure. Results on this relation between initial conditions and outcomes can be used to identify selection and influence effects. As we will show in the analysis, we find evidence that the emerging distribution of alcohol use is influenced by the *initial* network structure. We argue that this is a strong indication for the existence of influence: if the process were driven only by selection, the network should only adapt to the distribution of behaviour, and not vice versa. In principle, this logic could also be applied to identify selection effects: in that case, the model would predict an effect of the initial distribution of behaviour on the emerging network structure. In this article, however, we restrict the analysis

to emerging behaviour as the dependent variable because we are mainly interested in explaining differences in alcohol use.

Because our model predicts properties of stable states, our method does not require that enough changes in ties and behaviour are observed in each network to make estimation possible. As a result, we are able to use a larger share of the available data to test our hypotheses. However, our approach *does* require that enough variation in initial conditions and outcomes is present in the data. In this study we meet this demand by using data on a large number of groups. Another disadvantage of our approach is that we cannot directly test hypotheses on individual decision processes, as is possible with SIENA.

Although our theoretical model takes into account that behaviour and the network co-evolve, it provides the most informative predictions on the emerging distribution of *behaviour*, and less in terms of the emerging *network*. The analyses in Buskens et al. (2008) show that the emerging network is almost perfectly determined by the emerging distribution of behaviour: actors maintain links only with other actors with the same behaviour. These predictions can be tested at the individual level, and therefore an empirical analysis of network formation based on our model would not provide additional insight as compared to a SIENA analysis. Moreover, from a substantive point of view, we are mainly interested in explaining differences in alcohol use, and less in explaining network structures.

More recently, a debate has flared up on the identifiability of influence effects in networks (Shalizi and Thomas, 2011; VanderWeele, 2011), triggered in part by the remarkable findings of Christakis and Fowler (2007) and Fowler and Christakis (2008) on the contagion of obesity, happiness, and other traits. The main concern in this debate is whether *causal* effects can be identified in observational studies, as they are likely to be confounded with other processes. Our position in this debate, as far as the current article is concerned, is that we do not claim that we directly show causal effects. Rather, our approach is explicitly deductive: we specify a theoretical model with rather strong assumptions on the underlying causal relations of the social process, and derive hypotheses on expected regression coefficients from this model. Naturally, confirmation of our hypotheses does not *prove* that the assumed causal relations hold, but vice versa – if the assumptions are true, then the hypotheses must be confirmed. Thus, our approach is aimed at falsification of a causal model rather than identification of causal effects.

Predictions

The theoretical model outlined above describes a dynamic process in which the network and behaviour co-evolve. It can be shown analytically that this

process may converge to a large variety of stable states (Berninghaus and Vogt, 2006; Buskens et al., 2008; Jackson and Watts, 2002). Thus, by itself, this model does not yet provide precise predictions on which stable states will occur. To obtain more informative predictions on which stable states are more likely to occur than others, one can use computer simulations. Buskens et al. (2008) ran extensive computer simulations of the same model as we use here, varying the payoff structure of the game, initial distribution of behaviour, and the structure of the initial network. The simulations resulted in a large dataset of initial conditions and resulting outcomes. This dataset was subsequently analysed using conventional regression analysis methods, yielding predictions on how outcomes relating to aggregate behaviour depend on initial conditions, in terms of the initial distribution of behaviour and the initial network structure. In Table 1, we reproduce the main results of this analysis insofar as they are relevant to the present study. The table shows coefficients of a regression analysis predicting the proportion of actors playing the payoff-dominant action Y (corresponding to *not drinking* in our application in this article). The predictors included in the table are the initial proportion of actors playing Y , features of the initial social network structure (density, centralization, segmentation, segregation, number of components, network size) and interactions between the initial proportion and density and centralization. The analysis was performed separately for two different payoff structures: one where the payoff-dominant action Y is also risk-dominant, and one in which Y is

Table 1. Selected logistic regression coefficients on the proportion of actors choosing payoff-dominant action ‘ Y ’ interactions computed with variables centred at their means (simulation results from Buskens et al., 2008).

Model parameter	Y is risk-dominant	Y is risk-dominated
	Coeff.	Coeff.
Proportion Y at t_0	16.728	16.778
Size	-0.004	-0.006
Density	1.479	-1.414
Centralization	-0.413	0.453
Segregation	-0.281	0.134
Segmentation	-0.058	-0.182
Number of components	-0.004	0.004
Prop. $Y \times$ Density	31.080	30.845
Prop. $Y \times$ Centralization	-23.000	-22.538
Constant	-8.144	-8.196

risk-dominated. The latter case corresponds to our model of drinking as presented above.

These simulation results can be summarized as follows. First, they show that the initial distribution of behaviour (i.e., how many actors choose one action or the other at t_0) has a strong positive effect on the outcome, which is not surprising. Second, they show that there are main effects of the initial network structure, in particular of density and centralization, and that the directions of these effects depend on the payoff structure of the game. That is, the main effect of density is positive if Y is risk-dominant, and negative if Y is risk-dominated. Third, there are interaction effects between initial behaviour and initial density and centralization, in the sense that these network properties can strengthen or weaken the effect of initial behaviour. For further detail on the setup of the simulations and the derivation of the above results, we refer the reader to Buskens et al. (2008).

We use these results to derive specific hypotheses on the development of alcohol use among adolescents in school classes. To connect the simulation model with our empirical setting, we need to make a number of assumptions. First, we assume that alcohol use for adolescents is essentially a coordination game, as explained above: adolescents have incentives to display the same behaviour as those they interact with; jointly using alcohol has (*ceteris paribus*) a lower utility than abstinence; and the risk involved in unilaterally using alcohol is lower than the risk of not unilateral abstinence. The network is the friendship network between adolescents in a school class. The underlying assumption is that the group of classmates constitute a salient interaction context for adolescents. Because they spend a considerable share of their time at school among peers, we expect that they adjust their behaviour to interaction partners from this group. Second, this implies that we also assume that adolescents are not influenced by relations they might have *outside* their class. Admittedly, this is probably an unrealistic simplification of the situation. However, there is evidence that the students have most of their friendships and also their most important friendships in school classes (Friemel and Knecht, 2008).

Third, we need to assume that, at the last observation point, the process is sufficiently close to a stable state. Given that our last point of observation is only one year removed from the start of the process, it is obviously unrealistic to assume that at that point behaviour and networks will have completely converged. Nevertheless, it seems reasonable to assume that, if the empirical process behaves as assumed in the model, the outcomes should at least be considerably closer to a stable state than the initial conditions. In that case, the predicted relations between initial conditions and outcomes should still hold, albeit possibly less strongly than in the simulation results.¹

We observe school classes at four different points in time. Applying our model, we aim to predict the behaviour in the last observed period from the first observed period. Accordingly, all hypotheses are formulated in terms of the effects of properties of a group (school class) at t_1 on properties of the group at t_4 . A key group-level property at t_1 is the initial distribution of the *propensities* to choose alcohol use or abstinence. This propensity determines the likelihood that a student will use alcohol in the first ‘round of the game’. The distribution of propensities determines only how the process starts; in subsequent time points, actions are exclusively the result of interaction in the coordination game.

As to the effects of the initial network structure, we focus on the effects of network *density* and network *centralization*. In the simulation analyses by Buskens et al. (2008), these measures proved to have the largest effects on emerging behaviour. *Density* refers to the proportion of ties present in the network, given the number of members of the network. *Centralization* is the extent to which ties are concentrated with relatively few individuals, rather than distributed uniformly among the network members (Snijders, 1981).

The first hypothesis serves as a ‘baseline’ hypothesis, and relates the initial propensity to use alcohol to the resulting behaviour at the end of the process:

Hypothesis 1: *The higher the average propensity to use alcohol in a class at t_1 , the higher the proportion using alcohol at t_4 .*

The next two hypotheses concern the effects of the *initial density* of the network. Buskens et al. (2008) report that a higher initial density leads to a higher proportion of actors choosing the risk-dominant action, when starting from a situation in which the initial propensity is 50%. The intuition is that because the risk-dominant action is a stronger ‘attractor’ (Young, 1998), more interaction at the start of the process (i.e., a higher density) leads more easily to convergence to the risk-dominant equilibrium. However, when the initial propensity is skewed, a higher initial density favours the action towards which the process already tended from the start.

This implies a hypothesis about a *main effect* of initial network density, and a hypothesis about an *interaction effect* between initial density and the initial propensity to use alcohol.

Hypothesis 2: *The higher the density of the network in a class with an equal distribution of initial propensity to use alcohol at t_1 , the higher the proportion of students using alcohol at t_4 .*

Hypothesis 3: *The higher the density of the network in a class at t_1 , the stronger the effect of the proportion of students using alcohol at t_1 on alcohol use at t_4 .*

We expect similar effects of *centralization* of the initial network as for density, but in the opposite direction:

Hypothesis 4: *The higher the centralization of the network in a class with an equal distribution of initial propensity to use alcohol at t_1 , the lower the proportion of students using alcohol at t_4 .*

Hypothesis 5: *The higher the centralization of the network in a class at t_1 , the weaker the effect of the propensity to use alcohol at t_1 on alcohol use at t_4 .*

These two hypotheses signify that centralization of the network helps to counter the forces of the initial distribution of behaviour and risk dominance. If the network is initially more centralized, there are actors in the network having relatively many interactions. Those actors are more influential, and if those actors happen to choose the *risk-dominated* behaviour (i.e., *not* drinking), they are more likely to pull the rest of the network in this direction. Thereby, it is easier to ‘escape’ from the ‘pull’ of the risk-dominant behaviour if the network is more centralized initially.

Data

Data collection

The data for this study were collected in a longitudinal survey project on 14 Dutch secondary schools, conducted in 2003 and 2004 (Knecht, 2004, 2008; Knecht et al., 2011). From each school, all first-year classes were selected (between five and 14 classes per school, with an average of nine), and in each of these classes, all students were surveyed at regular intervals using written questionnaires. On average these students are 12 years old at the beginning of the academic year. In the Netherlands primary and secondary schools are separate institutions, leading to the formation of new classes. The first measurement took place shortly after the students entered the secondary school from primary education. The students were then surveyed again after three months, for a third time after another three months, and for a fourth and last time after another three months, resulting in a total of four waves. In total, 120 classes participated in all four waves. The survey included questions on the personal characteristics of students, on various

types of behaviour (including alcohol use) and opinions, and various network measures.

The questionnaires were administered at school, with the students from each class together in a classroom. A researcher or research assistant was present at each session. Because the survey sessions were held during normal school hours, it could happen that not all students of one class were present. Moreover, some students may have joined a class between the observation points. Because of this, the number of students per class in the data may slightly differ across the different waves.

Variables and measures

Individual-level measures

Personal networks. Social relations in classes were measured using various *name generators*. In each wave, students were asked to name their best friends in class, the classmates with whom they spent leisure time, and those with whom they discussed personal matters. For each of these questions, they were allowed to name up to 12 classmates, using a list of codes for all classmates provided with the questionnaire. The maximum of 12 nominations was used only very rarely (up to 2% of the observations for any of the network measures), which indicates that this maximum was not a limitation on the measurement of nominations.

On the basis of these individual-level measures, we construct networks at the class level. To verify that the results do not depend too much on the specific construction method, we use two different methods and report results using both methods.

For the first method, we combine measurements on three different name generators to identify interactions. We use nominations of 'best friends', spending leisure time together and discussing personal matters. We require that, *taken together*, nominations on the three variables are reciprocated. Thus, we assume that two students interact if they nominate each other, each on at least one of the three variables. This method takes into account that interpretations of friendly relations may differ between students: while student *i* might consider student *j* as one of her best friends, *j* might nominate *i* merely as someone with whom she discusses personal matters. As we are only interested in the extent to which students interact, we think that such mutual nominations, even though the interpretations of the relation slightly differ, can be interpreted as mutual interactions.

For the second method, we use only nominations from one name generator of best friends (as is most common in the network literature), and assume that if one student nominates another, the two interact. That is, we do not

require that nominations are reciprocated, and we interpret every directed tie as a symmetric relation. Bilateral nominations are treated the same as unilateral nominations.

Both methods result, at the aggregate level, in a *non-directed* network in which all ties are bilateral. This is required to adequately test the predictions from our model, which explicitly assumes that adolescents have incentives to coordinate their behaviour if they *interact*. By definition, interaction is non-directed, which implies a non-directed network.

Alcohol use. The use of alcohol by the students was measured in different ways in the different waves of data collection. In the first wave, students were asked how often they used alcohol in the preceding three months. Answers could be given on a five-point scale: 'never', 'once', '2–4 times', '5–10 times', and 'more than 10 times'. In waves two, three, and four, students were asked how often they had used alcohol in the preceding three months *with friends*, with the same answer categories as in the first wave. Thus, the measurement differs between the first wave and the other three waves in that the question of the first wave does not ask specifically about drinking with friends, but rather about drinking in general.

For this reason, Knecht et al. (2011), who analyse the same dataset, can use only the last three waves. Here we take the difference to be an advantage. In the context of our model, the measure in waves 2 to 4 represents alcohol use as far as it happens in a context of interaction with friends. This fits well in our theoretical framework, in which we assumed that alcohol use is a choice in a game of social interaction.² The measure used in wave 1, in contrast, we interpret as an indicator for the *individual propensity* to use alcohol before the influence/selection process that takes place among the students within one class starts. This corresponds with initial alcohol use at t_1 in our theoretical model. We feel that this is appropriate because the data collection in the first wave took place shortly after the start of the first year in secondary school. Thus, the three months mentioned in the question would refer for the largest part to the period just before the students entered secondary school, before they were influenced by the friendship network in their class. For this reason, it is not problematic that the measure of the first wave does not measure alcohol use with friends only.

Interpreting the measures in this way has the advantage that the complete time span between the four waves of data collection can be used in the analysis. The disadvantage is that the measure of wave 1 cannot be directly compared with the measures in the later waves. We can, however, use the measure of wave 1 as a predictor of alcohol use as measured in the later waves in a multivariate analysis (as we explain below).

Network-level measures

Aggregate network measures. Using the two operationalizations of interaction between students, we can construct a friendship network for each class at each time point. To be able to test our hypotheses, we compute network measures for each of these networks. The hypotheses are concerned with density and centralization. *Density* is defined as the number of existing ties divided by the number of possible ties, given the size of the network (Wasserman and Faust, 1994: 101). For *centralization*, we use the measure proposed by Snijders (1981), which is based on the (normalized) degree variance. Besides the measures needed for testing the hypotheses, we compute a measure of *relative network change* for descriptive purposes. This measure describes the extent to which the networks change between the different waves and is defined as the proportion of dyads in a network that have changed status (i.e., created a tie or deleted a tie) from one time point to the other. The measure is only defined as long as the set of nodes in the network does not change. Consequently, we cannot compute this measure for all networks at all time points. The number of networks for which we can compute the measure is at least 90 at each time point. Moreover, for the networks of ‘non-reciprocated friendship ties’, (method 2) we also report the proportion of nominations in these networks that are actually reciprocated.

Aggregate measures of alcohol use. We aggregate the individual measures of alcohol use into aggregate measures of alcohol use per class in two steps. First, we dichotomize the individual-level measures between ‘1’ (never) and ‘2’ or higher (once or more). This is done to make the measures more consistent with the theoretical model, which assumes that only two different actions are possible. We choose this specific dichotomization because it is substantively clear: we now distinguish between those who do not drink at all and those who drink sometimes. Moreover, the empirical distribution on this variable is such that the large majority of the students do not drink. Taking this group as a distinct category therefore seems most appropriate. In the second step, we calculate the proportion of students drinking (‘1’ on the dichotomized variable) per class.

Methods of analysis

Our analytical strategy is set up as follows. We start the analysis with some descriptive statistics on the development of behaviour and the network across the four waves. We then turn to regression analysis to test the hypotheses. In line with the analyses in Buskens et al. (2008) and Corten and Buskens (2010), we conduct an analysis at the macro level, using classes as

the units of analysis. The basic aim is to explain the level of alcohol use at the last observed time point, using measures characterizing the initial state per class as predictors. We use (linear) regression analysis, with the proportion of students that uses alcohol per class as the dependent variable. Because the classes were not independently sampled but are nested within schools, we use multilevel random intercept regression (Snijders and Bosker, 1999) with a random intercept at the school level.

Using a linear regression model to analyse a dependent variable that is a proportion is not without problems. We see a number of reasons why, in this case, using a linear model is not problematic. First, the distribution of our dependent variable does not show peaks at the edges of the distribution. In fact, the distribution closely resembles a normal distribution. Second, our models do not predict impossible outcomes, that is, values below 0 or higher than 1. Third, standard regression diagnostics do not indicate severe violations of model assumptions. Fourth, additional analyses (not reported here) using a logistic transformation of the original dependent variable do not lead to qualitatively different results. Buskens et al. (2008) and Corten and Buskens (2010) use logistic regression for grouped data for their analyses at the macro level. In those studies, use of logistic models was necessary because of the highly skewed distribution of the dependent variable. Because our dependent variable here is approximately normally distributed, we do not suffer from this problem. We instead prefer to use the somewhat simpler linear model, which allows for better treatment of the multilevel structure of the data. To verify whether the results are robust against different specifications of the network variables, we repeat the regression analyses for the two specifications discussed in the section 'Variables and measures'.

To test hypotheses 3 and 5, we construct two interaction terms by multiplying the initial proportion drinking with density and centralization of the initial network, respectively. To facilitate the interpretation of the respective main effects, we subtract 0.5 from the initial propensity before multiplication. This is necessary to test hypotheses 2 and 4, because these hypotheses predict the effects of the network structure given that the initial propensity is 0.5. To ensure that the main effect of the initial propensity can be meaningfully interpreted, we centre the values of initial density and centralization at their respective means before multiplication.

Thus, the interaction between the initial propensity to use alcohol and initial network density is computed as:

$$\text{Interaction} = (\text{initial propensity alcohol use} - 0.5) \\ (\text{density} - \text{mean}(\text{density}))$$

This construction ensures that, in the above case, the main effect of density in the regression equation can be meaningfully interpreted as referring to the situation in which the initial propensity is 0.5, while the main effect of the initial propensity can be interpreted as referring to the situation with average initial density.

Results

Descriptive results

Table 2 provides the means and standard deviations on key measures at the individual level: the original five-point measure on drinking behaviour, the dichotomized version of this measure, and the number of nominations of best friends, classmates with whom the respondent discusses personal issues, and classmates with whom the respondent spends leisure time. These three network variables are, at the individual level, *directed* measures; they measure the number of 'outgoing' ties of a student.

The two measures of alcohol use show a rather consistent pattern: the average alcohol use decreases from wave 1 to wave 2, and then steadily increases. The initial decrease reflects the difference in measurement between wave 1 and 2 (see the section 'Variables and measures'): because the initial measurement in wave 1 does not focus exclusively on alcohol use with friends but also captures drinking in other situations, the figures are somewhat higher. Overall, the averages are fairly low.

The average number of 'best friends' nominations shows a slight increase over the first three waves, and then decreases again in the fourth wave. The

Table 2. Descriptive statistics at the individual level, per wave.

Variable	Wave 1		Wave 2		Wave 3		Wave 4	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Drinking (5-point scale)	1.854	(1.205)	1.459	(0.945)	1.636	(1.128)	1.756	(1.230)
Drinking (dichotomous)	0.424	(0.494)	0.242	(0.429)	0.301	(0.459)	0.342	(0.475)
'Friends' nominations	3.591	(2.585)	3.926	(2.715)	4.303	(2.652)	4.068	(2.510)
'Personal' nominations	1.225	(1.571)	1.710	(1.841)	2.037	(1.887)	2.048	(1.962)
'Leisure' nominations	1.466	(1.488)	2.046	(1.812)	2.510	(1.959)	2.636	(2.089)
Number of obs. (listwise)	2,826		2,723		2,768		2,781	

other two network measures increase consistently over the four waves, but are clearly lower than the number of friends nominations.

In Table 3, we summarize the trends in measures at the aggregated (network-) level. Thus, in this table, the unit of analysis is the *class* rather than the individual student as in Table 2. The proportion of students drinking per class (as measured by the dichotomized variable) naturally shows the same pattern as the individual-level statistics, but has a smaller standard deviation (the aggregate measure might be interpreted as a weighted average of the individual-level measure). Both density and centralization are computed on the two different types of networks constructed by the methods described in the section on individual-level measures. Averaged over classes, we do not see much of a trend in either of the two density measures. If anything, the figure shows a small increase over waves 1 to 3, and a decrease in wave 4, which is consistent with the results at the individual level with regard to friends in Table 2. Centralization is rather low on both measures, and does not show much of a trend. Each network size is stable over time, and shows very little difference between the two methods. We report also the average change per class compared to the network in the preceding wave. The results on network change indicate that most of the changes occur between waves 1 and 2, and that the friendship network is slightly more dynamic than the network according to the combined measures.

In Table 4, we report the pairwise correlations between the dependent and independent variables to be used in the regression analyses. Because the

Table 3. Descriptive statistics at the class level, per wave ($N = 120$).

Variable	Wave 1		Wave 2		Wave 3		Wave 4	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Prop. drinking	0.427	(0.133)	0.245	(0.120)	0.306	(0.136)	0.347	(0.151)
<i>Combined network</i>								
Density	0.092	(0.026)	0.103	(0.032)	0.110	(0.028)	0.107	(0.033)
Centralization	0.112	(0.054)	0.128	(0.056)	0.132	(0.057)	0.124	(0.053)
Network size	25.300	(4.076)	25.233	(4.140)	25.275	(4.122)	25.225	(4.103)
Change	–	–	0.094	(0.026)	0.082	(0.025)	0.079	(0.025)
<i>Friendship network</i>								
Density	0.202	(0.053)	0.226	(0.052)	0.232	(0.050)	0.222	(0.052)
Centralization	0.115	(0.073)	0.152	(0.071)	0.154	(0.064)	0.146	(0.059)
Network size	25.275	(4.079)	25.217	(4.141)	25.183	(4.095)	25.200	(4.095)
Change	–	–	0.167	(0.044)	0.133	(0.036)	0.127	(0.038)
Reciprocity	0.579	(0.084)	0.586	(0.090)	0.571	(0.090)	0.581	(0.086)

Table 4. Pairwise correlations between dependent and independent variables ($N = 120$).

	1	2	3	4	5	6
1. Prop. drinking, – Wave 1						
2. Prop. drinking, – Wave 4	0.528**	–				
3. Density (combined)	0.1	0.098	–			
4. Centralization (combined)	0.089	0.233**	0.464**	–		
5. Density (friendship)	0.083	0.057	0.769**	0.522**	–	
6. Centralization (friendship)	0.028	0.103	0.493**	0.658**	0.633**	–
7. Network size	–0.034	–0.12	–0.591**	–0.593**	–0.628**	–0.544**

** $p < 0.05$.

two size measures are nearly identical, we report only the results on the size of the combined networks. The results show that there are weak to modest significant correlations between the various network variables and across the behavioural variables. The fact that the measures for density and centralization are correlated between the different construction methods suggests that these construction methods do not lead to very different results. The correlation between density and centralization within each construction method is remarkably high, given that the measure for centralization is controlled for density. Closer inspection of the data shows that these correlations are caused by a small number of classes that have both high density and high centralization. Exclusion of these outliers, however, does not lead to different results of the regression analyses. Network size correlates significantly with all the other network measures, but not with behaviour.

Multilevel regression using combined network measures

In this analysis, we use the networks as constructed by our first method, in which several types of name generators are combined. We conduct random intercept regression with average drinking behaviour in wave 4 as the dependent variable, and class-level properties in wave 1 as predictors. We estimate three different models. In Model 1, we include only the main effects of the initial proportion using alcohol, the initial density, and also

control for network size. In Model 2, we add a term for the interaction between the two predictors. In Model 3, we add a main effect and interaction effect of centralization.

The results are displayed in Table 5. Model 1 shows a positive and strongly significant effect of drinking behaviour in wave 1 as expected (Hypothesis 1), but no significant effect of initial density. We also find no significant effect of network size. In Model 2, the additional interaction effect is positive and significant, in accordance with Hypothesis 3. The main effect of initial density can in this model be interpreted as the effect of density for cases in which the initial proportion using alcohol is 0.5. This effect was expected to be positive (Hypothesis 2). The coefficient, however, is in the opposite direction as expected but not significant. In Model 3, we add both the main effect of centralization and the interaction effect of centralization with drinking behaviour in wave 1. Although both effects are not significant, we note that while the interaction effect is in the expected direction (Hypothesis 5), the main effect of centralization is opposite to the predicted direction (Hypothesis 4). Moreover, the likelihood ratio test also indicates that although Model 2 is a significant improvement over Model 1, Model 3 does not further improve on Model 2. We therefore rely on Model 2, and conclude that only Hypotheses 1 and 3 are confirmed in this analysis.

Multilevel regression using non-reciprocated friendship ties

To examine to what extent our results depend on the specific network construction method, we repeat the analysis of the previous section using our second construction method, using (unreciprocated) friendship nominations as ties. Apart from this, the two analyses are identical.

Table 6 presents the results of this analysis. Overall, the results are consistent with the results in Table 5. We again find highly significant effects in the expected direction of the initial propensity and the interaction effect with initial density. Also, we again find no significant effects of initial density (main effect), centralization, or size, while the main effects of initial density and centralization are again in the opposite direction as expected.

We also find a number of differences between this and the previous analysis. First, in Model 6, with the effects of centralization included, the interaction term with density remains significant, in contrast with Model 3. A likelihood ratio test, however, indicates that Model 6 is no significant improvement over Model 5. Second, the effect of the interaction term with density in Model 6 is considerably smaller than in Model 3.

All in all, the results of this analysis are comparable to the results of the analysis with combined network measures. The likelihood ratio tests again

Table 5. Multilevel regression predicting average drinking behaviour in wave 4 using combined network measures.

Variables	Prediction	Model 1			Model 2			Model 3		
		Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p
Prop. drinking, wave 1	+	0.613***	(0.079)	0.000	0.609***	(0.077)	0.000	0.606***	(0.077)	0.000
Density	+	-0.833	(0.525)	0.112	-0.300	(0.557)	0.591	-0.334	(0.575)	0.561
Network size		-0.004	(0.004)	0.266	-0.003	(0.003)	0.333	-0.003	(0.004)	0.428
Drinking * Density	+				7.917**	(3.155)	0.012	7.642**	(3.349)	0.022
Centralization	-							0.083	(0.278)	0.765
Drinking * Centralization	-							0.215	(1.433)	0.881
Constant		0.257**	(0.127)	0.043	0.192	(0.126)	0.128	0.177	(0.137)	0.195
Variance components										
Var(Constant)		0.004	(0.002)		0.005	(0.002)		0.005	(0.002)	
Var(Residual)		0.012	(0.002)		0.011	(0.002)		0.011	(0.002)	
Log Likelihood		84.425			87.442			87.450		
Likelihood ratio test: χ^2		48.20***			5.99**			0.08		
Number of observations		120			120			120		

$p < 0.1$; ** $p < 0.05$, *** $p < 0.01$. Likelihood ratio tests refer to the comparison with the previously estimated model; the likelihood ratio test for Model 1 refers to the comparison with the unrestricted model.

Table 6. Multilevel regression predicting average drinking behaviour in wave 4 using combined network measures.

Variables	Prediction	Model 4			Model 5			Model 6		
		Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p
Prop. drinking, wave 1	+	0.610***	(0.080)	0.000	0.593***	(0.077)	0.000	0.591***	(0.076)	0.000
Density	+	-0.352	(0.257)	0.171	-0.094	(0.262)	0.719	-0.137	(0.288)	0.635
Network size		-0.004	(0.004)	0.311	-0.003	(0.003)	0.330	-0.002	(0.004)	0.494
Drinking * Density	+				4.518***	(1.540)	0.003	6.193***	(1.874)	0.001
Centralization	-							0.081	(0.203)	0.689
Drinking * Centralization	-							-1.972	(1.423)	0.166
Constant		0.246*	(0.132)	0.063	0.192	(0.129)	0.136	0.163	(0.129)	0.207
Variance components										
Var(Constant)		0.004	(0.017)		0.004	(0.002)		0.004	(0.002)	
Var(Residual)		0.012	(0.008)		0.011	(0.002)		0.011	(0.002)	
Log Likelihood		84.141			88.205			89.298		
Likelihood ratio test: χ^2		47.63***			8.13**			2.9		
Number of observations		120			120			120		

$p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Likelihood ratio tests refer to the comparison with the previously estimated model; the likelihood ratio test for Model 4 refers to the comparison with the unrestricted model.

indicate that Model 5, which includes the interaction term with density but no effects of centralization, is the preferred model. Most importantly, these results do not lead to different conclusions with regard to the hypotheses. The results suggest that the substantive conclusions are robust against the different specifications of the network variables.

Additional analyses

The analysis of non-reciprocated ties differs from the analysis of combined network measures in two respects: we changed from the combination of various network measures to friendship nominations only, and from reciprocated nominations to non-reciprocated nominations. For two combinations on these two dimensions, we found no substantive differences in results, but there are two combinations remaining – reciprocated friendship ties, and non-reciprocated combined ties. Results on these two combinations (not reported here) do also show no qualitative difference with the two analyses just discussed.

Our results consistently show an effect of the initial network structure, which leads to conclusions different from those of Knecht et al. (2011) (see the concluding section). A possible explanation of this difference is the different use of data: because alcohol use was measured in a slightly different way in the first wave of data collection, Knecht et al. (2011) could not use this part of the data and instead used the second wave as the first observation. In contrast, we interpreted the measure of alcohol use in the first wave as a pre-existing tendency for drinking, and used the first wave as the first time point. To check the validity of this explanation, we replicated the analyses of combined network measures, using the data of the *second* wave of data collection. Although coefficients are in the same direction and of comparable magnitude, we find no significant results. Therefore, we cannot rule out that the differences in results are driven by a different choice of data, that is, by the difference in the measure of alcohol use or the fact that we look at a longer period.

Discussion

In this article we aimed to contribute to the understanding of selection and influence processes in the dynamics of alcohol use among adolescents. We did so by adopting a theoretical approach that interprets alcohol use as a coordination game in a dynamic network. Relying on simulation analyses of a game-theoretic model, we formulated hypotheses on the effects of initial conditions in terms of network structure and initial tendencies for alcohol

use on resulting levels of alcohol use per school class. We tested these hypotheses using longitudinal data on alcohol use and social networks of 12-year-old students in Dutch high schools. Using various specifications of the independent variables, we were able to consistently confirm two hypotheses.

First, we find that the average initial propensity to use alcohol per class has a positive effect on average alcohol use per class at a later stage. Second, we find that this effect becomes stronger, the higher the initial density of the social network in a school class. That is, in line with expectations, the density of the network *amplifies* the initial behavioural tendency.

We also predicted that initial network density should have a positive effect on alcohol use for classes that start with an average propensity to use alcohol of 0.5. However, we did not obtain any significant results on this hypothesis, and moreover, the estimated effect was consistently in the opposite direction than expected. Also, we were not able to obtain significant results on the effect of initial network *centralization*, where we predicted that centralization should have a negative main effect on alcohol use and should negatively interact with the initial behavioural tendency. While the direction of the estimated main effect is opposite to the prediction, the interaction effect is in the expected direction.

How should our findings be interpreted? Although we were not able to test all hypotheses thoroughly, the fact that we found effects of the initial network structure on resulting behaviour is revealing. The implication of this finding is that *influence* must play a role in the co-evolution process of alcohol use and network formation. If only selection drove the process, then network formation would depend on the distribution of behaviour, but not vice versa: the emerging behaviour should be independent of the initial network. Instead, we find that there *is* an effect of the initial network. This conclusion contrasts with the findings of Knecht et al. (2011), who found only evidence for selection using the same dataset. We return to this issue below.

The predictions on direction of the main effects of density and centralization depend on the assumption that using alcohol is the risk-dominant action in the coordination game. The facts that both effects were not significant, and were consistently estimated in the opposite direction than predicted, suggest that this assumption might need to be revised. Such a revision might take two directions. First, the finding that the effect of density was consistently negative while the effect of centralization was positive suggests that abstinence, rather than using alcohol, is the risk-dominant action. On the basis of this assumption, we would expect that a larger number of observations at the class level would yield significant effects for both variables. A

second alternative assumption is that this coordination game is actually risk-neutral, in the sense that neither of the two equilibria is risk-dominant. In that case, we would indeed expect no main effects of density or centrality.

A cautionary note is in order here, however. The predictions of these main effects all refer to the situation in which the initial propensity is 50%. This means that the sizes and directions of these effects rely crucially on the exact definition of this majority, and therefore on the dichotomization of the dependent variable. While we think that our particular specification is well founded, arguments for different specifications are certainly conceivable. Therefore, one should be careful to draw strong conclusions based on the results on these main effects. Note, however, that the predictions on the *interaction effects* depend much less on the exact specification of the dependent variable. Moreover, a different dichotomization would shift both main effects up or down, but could not reverse the directions of both effects at the same time. Thus, the ability to discriminate between the two different versions of the model in terms of risk dominance, as discussed above, does not depend on this specification.

In sum, therefore, we conclude that although the model with the assumption that alcohol consumption is the risk-dominant action has to be rejected, the results are not inconsistent with an alternative specification of the model in which alcohol consumption is payoff-dominated *and* risk-dominated. This alternative specification should be the focus of future research, testing both the macro-level predictions of such a model more thoroughly, and providing the micro-level foundations of this alternative payoff function.³

Our analysis thus highlights how simulation results can also be used to test predictions from game-theoretical models in cases where such models predict multiple stable states. We proposed a solution to this problem by using predictions on how outcomes depend on initial conditions, and testing these conditions on a relatively large number of network-level cases. Although we did not have enough significant results to say the final word on the proposed model, the above discussion illustrates how such results can be used to argue about the specific assumptions of the game-theoretical model, such as the specification of payoff functions *even if we do not measure individual payoffs directly*. Instead, the simulation results of Buskens et al. (2008) allow us to test implications of these assumptions.

Finally, why do we find evidence for influence effects, while Knecht et al. (2011) found only weak evidence? Part of the explanation could be that our approach indeed solves some of the problems of the SIENA approach applied by Knecht et al. (2011), as we outlined above. That is, we were also able to use relatively stable classes for testing the hypotheses, and

could therefore use four compared to three waves. Besides the general methodological approach, however, there are some other differences between the two studies that could potentially account for the different findings. A first difference we already discussed concerns the use of data. Additional analyses showed that we cannot rule out this explanation of the differences in findings. Thus, it could be that the time from wave 2 to wave 4 is too short to observe network influence effects. Another possible source of those differences is that we were able to analyse a larger number of classes.

Second, the theoretical interpretation of influence is somewhat different between the two studies. Knecht et al. (2011) analysed the *directed* network, assuming that an adolescent is influenced by those peers she nominates as a friend. Thus, the influence is assumed to work in only one direction. In our model, we assume that influence takes place through *interaction*, which is by its nature undirected. While this difference is theoretically important, it should be noted that it is not exclusively a consequence of the different methodological approaches; a model with 'two-way influence' would also be possible within the SIENA framework.

Third, partly as a consequence of the differing theoretical conceptualization of influence, our measures of the network are somewhat different. Knecht et al. (2011) use directed 'best friend' nominations, while we use various combinations of network variables, including 'best friend' nominations but also measures of spending leisure time together and discussing personal matters.

Given these differences, it is not clear how the differences in findings should be judged. We clearly encounter a discrepancy between findings at the macro level, where we do find evidence for influence effects, and the micro level, where such effects could not be observed. This discrepancy poses a new puzzle that deserves more attention in future research on this topic. Such research should focus on the development of theoretical models that are consistent with empirical research on micro-level behaviour and the macro-level findings as presented in this study (see also Corten and Buskens, 2010).

Overall, we conclude that the focus on effects of macro-level network effects contributes to the explanation of emerging differences between classes and adds interesting new insights into the study of co-evolution processes.

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Notes

1. Conversely, we do not need to assume that the conditions in the first observed period are literally 'initial' in the sense that the relations and substance use of the adolescents are not influenced by a common history (even though it is relatively unlikely that they are). The model makes no assumptions on how the initial conditions came into being – it just takes them as given, and makes predictions based on the initial conditions as they are. Moreover, the simulation analyses by Buskens et al. (2008) show that the predictions are robust over a wide range of initial conditions.
2. This assumption does not imply that we believe that there are no individual factors influencing alcohol use. In this study, however, we largely disregard these factors because we are interested in the social dynamics of alcohol use.
3. The reader might wonder why, given that the results arguably provide more support for this alternative model than for our original specification, we have not reformulated our model and predictions to avoid presenting negative results. However, we feel that our original specification with alcohol consumption being risk-dominant was *ex ante* more intuitive and credible, and therefore deserved to be tested explicitly.

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