

**Evaluating the UK National Pilot of The Real Game: Technical Report on the
Quantitative Analysis of Learning Outcomes**

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Note

A summary of the data reported in detail here is included in the main report on the NICEC evaluation of The Real Game. This also includes other quantitative and qualitative data.

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1. Design of the study

The quantitative part of the study offers two kinds of evidence: first, opinions of, or attributions of effects to, The Real Game obtained directly from participating pupils; and second, a quasi-experimental examination of its effects on career-related beliefs and knowledge.

The quantitative study was confined to 16 of the 37 schools in the field trials. As with the qualitative study, it was conducted during the 'start-up' phase of The Real Game in the UK and is not, therefore, an examination of a well-established programme in which teachers have become experienced.

It is also important to note that rigorous experimental control could not be exerted, so that the objective was limited to one of obtaining reasonably robust, but indicative, results. Experimental control was lacking in several important respects. First, it was not exerted over the schools to be included in the pilot. Second, neither the time, nor the resources, were available to include samples drawn in non-Real Game schools or to collect the data and conduct the forms of analysis which would be required, should such a course of action otherwise have been practicable. Third, control was not exerted over either the number of young people participating in The Real Game, or the way in which they were allocated to it in each school. Fourth, it was not exerted over the *manner* in which The Real Game was implemented in the schools. These initial constraints dictated that this would be an 'intact groups' study in which one or more mixed-ability tutor groups would participate in The Real Game in each pilot school.

A simple 'before and after' study would have been open to the accusation that any changes it demonstrated might be attributable to repeated questioning using the same questionnaires (reactivity), to the sorts of changes which do, in any event, take place in the thinking and opinions of developing young people (maturation), or to other aspects of their schooling and general experience over the trial period (history). One way to counter this is to examine relative effectiveness according to degree of implementation. But several factors militated against this. First, whilst it was anticipated that the manner of implementation might vary, this was not predictable with any confidence. Second, it was not known whether there would be significant *quantitative* variation in the extent to which The Real Game was implemented. Third, in order to be convincing, such studies require either that firm control be exerted over precisely the two factors just mentioned, or that very large samples be taken, not merely of young people, but also of *schools*. Thus designs of this type had to be dismissed.

Control subjects were, obviously, required. But these were available only on the same mixed-ability, intact-group basis as Real Game participants. A simple 'after-only' design would, therefore, be open to the accusation that ostensible Real Game effects reflected prior differences between the young people participating in The Real Game and the control samples to which they were compared (sample selection bias).

Given these circumstances, the safest course of action was to employ a 'before and after', non-equivalent control group design.¹ This 'quasi-experimental' approach meant that it was desirable to pursue pupil data (sex, age within year, eligibility for free school meals, SAT scores, ethnic group) which would permit gross forms of sample bias to be detected and, if necessary, controlled statistically. In addition, the inclusion of such factors in the analysis might make any genuine Real Game effects stand out. However, these additional forms of information were available for only a minority of schools, so that analyses involving them are confined to small sub-samples.

¹ Other possibilities, such as the Solomon design, were precluded, not only on time/cost grounds, but because they would geometrically increase the difficulty of coping with factors such as non-random selection and differential degrees of implementation (see later).

In the absence of experimental control it was obviously necessary to consider non-Real Game inputs to samples. Control samples were exposed to the usual pattern of teaching in the schools in which they were drawn and this varied between schools. A key question was, therefore, ‘were control samples exposed to *other* forms of Careers Education and Guidance during the trial period?’ However, the possibility could not be precluded that Real Game samples might also be exposed to *additional* forms of Careers Education and Guidance over the same period. What the actual pattern would be was not predictable at the outset, so this information was gathered in the course of the qualitative study. Our best estimate is that in no school were either Real Game or comparison sample members exposed to additional or alternative forms of Careers Education and Guidance *during the trial period*. Thus this threat to validity may be set aside. This does not, of course, mean that the starting positions of samples might not have been influenced by earlier Careers Education and Guidance work.

Some important matters of detail should also be noted. Both the pre-test (t1) and post-test (t2) administration of questionnaires were to be conducted by teachers, according to a common script. This reinforced the anticipation that sample attrition might be high. Attrition is common in longitudinal, school-based studies – due, for example, to student absence at t2, to the erroneous belief that it is more important to collect data from experimental than control subjects, and to the break-down of arrangements which tends to occur in the last few weeks of the school year. Only two safeguards were practicable: first, to draw sufficiently sizeable initial samples to accommodate balanced attrition across experimental and control samples (over-sampling) and, where samples were *systematically* lost, to reconstruct adequate t1/t2 samples; and second, to remind schools repeatedly of both the importance of the post-test and the equal importance of the control sample. As will be seen, the first of these steps subsequently proved to have been vital.

In addition, steps were taken to record the degree of Real Game exposure and implementation at two levels. First, teachers were asked to record absences from Real Game sessions, so that experimental subjects not actually exposed to the full diet could, if necessary, be identified and compared with others – although, of course, they could not be excluded unilaterally from the experimental sample for this reason, as to do so in one sample, but not in the other, might introduce biases against truants, those suffering illness, etc. In point of fact, given the numbers for whom this information was available, the lack of variability in attendance, and the complications associated with adjustment for degree of implementation, this information proved to be of limited value. Second, information was gathered on the extent of Real Game implementation in each school, both as a safeguard and in case it might prove possible to effect simple comparisons between schools. However, whereas The Real Game is designed to occupy 25 to 30 hours, almost all pilot schools actually devoted substantially less time to it. Thus no clear basis emerged for adequate comparisons to be made. The important implication is that, as well as being a study of The Real Game in its ‘start-up’ phase, the present study must also be regarded as an evaluation of partial implementation of The Real Game, not of The Real Game as intended by its designers.

Moreover, we cannot assume that The Real Game was abbreviated in a similar manner in each school. Thus this is a study of the effects, not only of *partial*, but also of *heterogeneous* implementation. The implications can be illustrated in relation to young people’s beliefs about what they learned from The Real Game. As noted above, the questions they were asked were based on Real Game learning objectives. If implementation had been complete, substantial differences in the responses to questions would give an indication of what The Real Game seemed to them to do best or most strikingly. But at low levels of implementation, participant opinions may tell us rather more about what teachers chose to omit. A similar point may be made with regard to t1-t2 (pre-/post-test) comparisons. Where effects are found, they may be smaller than in the case of full implementation, and ‘null’ results may reflect failures of implementation, rather than the limitations of The Real Game as its designers intend it.

2. Instruments

Two questionnaires were employed in this study. The first was already included in the Real Game programme of activities, covering both factual knowledge and opinions which it is the objective of The Real Game to convey or to influence. Following the initial (t1) administration of this questionnaire in the first schools to enter the pilot (220 subjects), patterns of response were inspected and the number of items reduced from fifty to thirty. This was in order to avoid a 'ceiling effect', or the production of a false null due to the inability of the instrument to record positive change, following very high levels of correct or positively evaluated responses at t1. It is important to emphasise that this reduction was conducted without regard to content and exclusively according to patterns of response. At t2, this reduced version was re-administered. However, twenty participant opinion questions were added to the variant for use only with Real Game sample members. These questions asked subjects to record how much they believed they had learned, and were derived from Real Game learning objectives. They were added to the end of the questionnaire, in order to avoid a 'prompting' effect upon the original Real Game questionnaire items and to maintain equivalence of the versions used with each sample.

The second questionnaire was developed for the study according to the requirements of the evaluation sponsor. The salient points are as follows. First, existing instruments with known properties (see Killeen *et al.*, 1994) were of US origin, long (many items), and, in the main, protected by copyright. They could not be adapted and applied in a short time-scale. It was therefore necessary to create new instruments, without an opportunity for pilot. It was plainly not possible to develop *tests* of knowledge or skills; thus instruments were self-reports of beliefs, of confidence (self-efficacy) and of attitudes to career information and self-reported access to information sources. The scales which were developed covered:

- Beliefs in the utility of job exploration, self-awareness and self-exploration in relation to jobs, and of career planning.
- Self-efficacy for (confidence in ability to do or develop) job exploration, for self-awareness in relation to career decision-making and for career planning.
- Beliefs related to 'employability'.
- General need for career information, need to engage in specific career information search activities, and knowledge of related sources.

On the basis of the first 220 cases at t1, some items were deleted, but none, of course, could be added. Within each of the first two major groups, the order of presentation of sub-scale items was randomised; in the first and third major groups, both positive and negative items were employed.

3. Implications of quasi-experimentation for analysis and results

It is important to note that the constraints operating on the quantitative study have serious implications for the conclusions which can be formed. Samples were not randomly assigned and sub-samples within schools vary in size; in more complicated analyses, cells (e.g. school by sex by Real Game/comparison sample) are often unbalanced and cell numbers may be quite small. The first of these contravenes the assumptions of many standard procedures, and the second and third are irksome. In addition, standard procedures often make assumptions (notably concerning normality and equality of variance, or the 'shapes' and the comparability of 'shapes' of distributions) about the populations from which sub-samples are drawn. Moreover, the *way* in which optimal models are specified depends upon the *nature* of the relationships between their constituents.

The analyses employed standard procedures, as the alternatives would have been extremely time-consuming. Standard procedures can be seen as a brake on over-optimistic interpretation. However, in view of the design of the study, it is possible to outline a common-sense approach to interpretation which has a basis in probability. Obviously, the larger any difference between Real Game and comparison sample gains or losses from t1 to t2, the more credible they are. However, we can add two further criteria of adjudication. First, the general hypothesis is that participation in The Real Game leads to *gains* relative to the comparison sample. This means that we hypothesise t1 to t2 gains in the Real Game sample, and that (in view of the possibility of gains in the comparison sample) these should be greater than in the comparison sample. Thus, if statistically significant differences open up because comparison sample scores *decline*, the result is less consistent with our general research hypothesis. Second, *consistency* of results across schools is at a premium. If The Real Game has no effect, then we would not expect to see the same thing happening in each school; rather, we would expect to see random differences between the fairly small samples within schools, with the Real Game sample sometimes appearing to do a little better, and sometimes a little worse, than its corresponding comparison sample. If The Real Game has an effect, then we would expect to see this repeated from school to school – the larger the effect, the more consistently so.

Thus, the most convincing evidence is of *consistent* differences between samples in each school, which arise because the Real Game sample has made a *gain*, or a larger gain than the comparison sample, relative to its own starting position.

4. Samples

Samples of young people were accumulated as schools entered the pilot. Eventually, sixteen schools were included. In each school, mixed-ability tutor groups were selected (by school staff) to act as a comparison to the mixed-ability groups participating in The Real Game. Thus *nominal* samples were approximately balanced within most schools, giving totals of 546 Real Game participants and 559 comparison subjects. Balancing of numbers within schools was, however, sometimes far from perfect: there was, for example, a clear excess of comparison subjects in two schools (Schools 11 and 15).

However, these are only *nominal* samples. As we have been at pains to point out, the study was conducted in the expectation of both individual and school-level (systematic) sample attrition. Schools were asked to administer two questionnaires, both and at t1 and t2. At t2, one of these questionnaires had two variants – one for the comparison sample and a second for use with the Real Game sample, to which items eliciting opinions of the Real Game had been added. Some schools omitted to administer one or both questionnaires at t1 or, more commonly, at t2. This omission sometimes affected both samples and sometimes only one sample. Some schools did not use the questionnaire variant to which supplementary questions had been added.

In addition, only a minority of schools provided ancillary information on the sex, precise age, ethnic group and SAT scores of sample members. Thus, samples are substantially reduced when these factors are considered.

These omissions are in addition to conventional sample attrition at the individual level, which is modest by comparison. Thus the effective sample differs *systematically* according to the comparisons being made, and sample attrition must be considered in the light of this systematic variation.

In addition, of course, the problem of item non-response must be considered when additive scales are employed as dependent (criterion) variables. Non-response distributions were examined and cut-offs established, beyond which cases were deemed wholly missing and

excluded from the analysis. Thereafter, a small number of key analyses were conducted twice: first, for subjects answering *all* relevant questions; and second, for the somewhat larger numbers of subjects answering the *minimum* number of items. In the latter case, missing item values were imputed from the subject's own mean scores on the items they actually answered. As those who missed one or more items tended to have slightly lower mean scores for those they answered, this is something of a safeguard against biased estimates. The procedure is of practical significance, however, only in analyses where it is essential to retain the maximum number of sample members, i.e. where ancillary pupil data, which are at a premium, are to be used.

It follows that *effective* samples must be distinguished from the *nominal* samples of young people involved in the Real Game pilot. The ways in which samples are constructed and effective rates of sample retention are calculated will be outlined fully for Real Game participant opinions and for responses at t1 and t2 to the Real Game questionnaire. Thereafter, shorter summaries will be given.

Effective sample: Real Game participant opinions. Two schools (Schools 2 and 12) failed to administer the relevant t2 questionnaire to the Real Game sample and two more (Schools 7 and 10) administered only the basic variant, which lacked opinion questions. Thus data are available for twelve of the sixteen pilot schools. In most of them, attrition was between 10% and 20%. It is possible that higher rates of attrition (School 13, 30%; School 15, 28%) may be due to failure to administer the questionnaire to part of the sample. However, on the conservative supposition that only schools failing to administer *any* correct t2 variants should be excluded from the denominator, 74% of the available initial (t1) Real Game sample (337 of 456 cases) were present at t2 and answered *all* opinion questions. 82% (374) missed no more than five questions and are included in the analysis.

Effective sample: retained Real Game questionnaire items. Table 1 shows both those answering *all* relevant items and those who answered *most* (at least 25) Real Game items on both occasions. Five schools (Schools 2, 8, 12, 14 and 15) failed to administer the relevant questionnaire on at least one occasion and are excluded from the table. Two schools (Schools 10 and 16) are shown to have re-administered the questionnaire only to one sample. All of these schools are, therefore, excluded both from the analysis and from the calculation of effective sample attrition. Three schools (Schools 1, 6 and 9) provide seriously unbalanced samples, in each case favouring The Real Game over the comparison sample. They are, however, retained. It is noteworthy that in some schools (e.g. School 1) sample size increases substantially when those answering *most* questions on both occasions are included. This may reflect different standards of questionnaire administration. Without imputing values, the total sample size is 476 (Real Game = 251, Comparison = 227). However, when those answering most items are admitted to the sample (by imputing values to missed items) the total sample size increases to 602 (Real Game = 309, Comparison = 293).

In addition to excluding seven schools, we set aside a handful of individual pupils in the nine remaining schools who were absent at t1 but present at t2. We do, however, include a very small number who missed more than 5 items at t1 in our calculation, so that in this respect it is conservative. Table 2 shows the position in detail: of those answering *any* relevant questions at t1, 78% of Real Game and 74% of comparison sample members are retained without imputation at t2. When values are imputed at t2, 85% of Real Game sample members and 80% of comparison sample members are retained.

Table 1: Effective samples – full and minimum completion of retained Real Game questionnaire items at both t1 and t2

p

G		R		F	
G	R	F	G	R	F
1	2	0	4	7	7
3	2	0	5	6	6
4	2	5	0	0	0
5	2	2	4	4	4
6	0	9	2	2	2
7	6	3	3	3	3
9	9	8	2	2	2
0		5	5	5	5
1	2	6	8	8	8
3	2	2	3	3	3
6	7		7	7	7
Σ	0	2	0	0	0

G		R		F	
G	R	F	G	R	F
1	2	5	7	7	7
3	2	2	6	6	6
4	7	3	0	0	0
5	2	2	9	9	9
6	2	2	3	3	3
7	2	9	0	0	0
9	9	9	2	2	2
0		6	6	6	6
1	6	3	0	0	0
3	0	6	6	6	6
6	8		8	8	8
Σ	3	9	0	0	0

a

G				
G	1	m	a	N
1				0
2				6
3				1
5				3
9				3
3				1
5				1
2				1
2				1
A				4
Σ	1	m	a	3
1				3
2				5
4				1
5				1
6				1
A				0
Σ	2	m	a	5
Σ	4	m	a	1
Σ	1	m	a	3
Σ	5	m	a	1
Σ	1	m	a	3
Σ	1	m	a	3

s

G			
N	%	N	%
2	3	0	2
6	3	7	9
1	3	1	3
3	8	1	3
		1	3
		1	3
1	3	1	3
		1	3
4		6	
3	9	4	
5	3	4	1
1	3	1	3
4		1	3
1	3		
		1	3
0	2	7	5
2	5	3	8
4	1	4	1
		1	3
1	3		
1	3	2	5

As indicated earlier, samples are reduced in analyses which require ancillary data. In order to keep exposition within bounds, it will be briefly noted here that:

- Information on individual absence from Real Game sessions cannot be employed in analyses in which the comparison sample is also involved, for obvious reasons.
- Ethnic group varies too little to be useful in the analysis. It is set aside.
- Of the available attainment measures, only SAT English score is related to Real Game questionnaire outcomes; other measures are set aside.

Although sample size varies according to analysis, the best test of Real Game effects which we are able to perform controls not only for school, but also for sex, age and ability (SAT score in English). The sample which can be constructed to perform this test is small, as *all* of these items of information must be available. The minority of schools which provided it tended to be ones in which initial samples were small and the task less onerous. One school (School 13) did not provide information for control subjects and for this reason must be excluded. Hence we have nearly-complete information for relevant subjects in just four schools (Schools 3, 5, 6 and 7) (Table 3). In order to calculate sample retention, subjects making any response at t1, even if unusable, are included in the divisor, as before. Sub-samples are as follows: without imputation, Real Game sample retention is 84% (n = 81) and comparison sample 77% (n = 69); with imputation of up to five values, Real Game sample retention is 93% (n = 89) and comparison sample 86% (n = 77). It should be emphasised that these young people are best considered as a small sub-sample in four schools. Within these schools, attrition and exclusion from analysis due to missing data are modest.

		g		a		b		h 3	
				s					
h	h	3	N		1	2	2		
R	h	3	N		1	2	2		
			%		3%	6%	6%		
		5	N	5	2	0	2		
			%						
		6	N	1	1	0	2		
		%							
	7	N	1	4	3	8			
		%	6%						
	h		N	7	8	8	8		
			%						
C	h	3	N	1	1	2	2		
			%						
		5	N	1	3	2	0		
			%						
		6	N	7	3	7	7		
		%							
	7	N	4	1	6	2			
		%							
	h		N	3	8	0	9		
			%						

h O

a

Effective samples for self-efficacy, career beliefs, employability beliefs and career information items. These groups of items were contained in a single questionnaire. Almost all nominal sample members completed it at t1, but a disproportionate number who failed to do so were concentrated in the comparison sample in one school. The effect was, however, to bring sample sizes into closer balance in that school (School 11). Six schools (Schools 2, 8, 9, 10, 12, 16) failed to administer the questionnaire at t2 and the effective sample is thus drawn from the remaining ten schools (Table 4). Real Game and comparison subjects are approximately balanced in all but two schools (Schools 6 and 7). Effective samples are determined by individual absences and failures to complete all, or sufficient numbers of, questionnaire items in each group of questions.

For the self-efficacy scales, item non-response was such as to suggest that values should be imputed. The aggregate effective sub-samples are as follows: without imputation, Real Game = 270 and comparison = 260; with imputation, Real Game = 344 and comparison = 335. Effective sample retention is calculated as before, excluding 21 subjects who were nominally part of the samples in the ten schools but made no reply at t1, but including those failing to answer a sufficient number (at least 18 of the 22 retained items) at t1 in the divisor. The effective rates of sample retention are as follows: without imputation, Real Game 65% and comparison 61%; with imputation, Real Game 83% and comparison 79%.

Four of the schools shown in Table 4 did not provide ancillary data (Schools 1, 4, 11 and 14). Thus six schools are the basis for the effective sample when sex, age and ability are taken into account (Schools 3, 5, 6, 7, 13 and 15). Sample sizes are therefore reduced as shown in the results section. Sample retention is not, however, so important an issue, as in no case is the conclusion altered by this reduction of samples.

Calculation of effective sample retention for the remaining groups of items is based on the same schools as for self-efficacy. For career beliefs, sample retention in the Real Game sample was 78% (n = 324), and in the comparison sample 73% (n = 310). These figures were judged sufficient and missing values were not imputed. For the independent inspection of sub-scales, there were small increases in the available samples. The position is very similar for the remaining item groups. For employability beliefs, sample retention for the Real Game sample was again 78% (n = 324), and for the comparison sample 74% (n = 314). Finally, for need for career information and knowledge of information sources, which will be assessed at the sub-scale level, Real Game sample retention varies between 76% and 83% (n = 315 and n = 344), and comparison sample retention between 70% and 80% (n = 291 and n = 338).

When we move away from the large numbers nominally included in the study, and consider effective retention for the comparisons made, these are in the usual range for studies of this type. Despite the resource constraints, which were severe, the samples were of a similar order, or larger, than those generally reported in the refereed literature on the effectiveness of careers education and guidance.

5. Results: participants' opinions about their own learning

Results will be presented in three main sections: participant opinions (reported learning); effects measured using the Real Game's World of Work Questionnaire; and, finally effects measured using the questionnaire on beliefs and self-efficacy. This reflects the way in which the effective samples are composed.

			F				E	
			t	n	n	n	n	
R	B	1	3%	5%	2%	2%	9%	
		3			2%	2%	2%	
		4	0%		2%	9%	9%	
		5	5%	1%	5%	6%	2%	
		6	2%		2%	9%	3%	
		7	1%		4%	7%	2%	
		1	6%	2%	7%	3%	3%	
		3	9%	1%	2%	2%	4%	
		4	6%	1%	4%	4%	3%	
		5	6%	3%	6%	0%	5%	
			8%	3%	7%	2%	5%	
C	B	1	1%	3%	2%	2%	5%	
		3	1%	1%	2%	2%	2%	
		4	6%	1%	3%	6%	9%	
		5			9%	7%	2%	
		6	5%		5%	9%	9%	
		7	4%		2%	6%	2%	
		1	8%	5%	5%	9%	8%	
		3	8%	1%	0%	6%	5%	
		4	7%	1%	3%	3%	2%	
		5	7%	4%	4%	8%	6%	
			7%	6%	3%	2%	2%	

Real Game participants held broadly positive views of what it had taught them. Half or more said they had learned ‘quite a lot’ or ‘a lot’ about each of the twenty learning objectives concerning which they were questioned. The most common response was ‘quite a lot’ (Table 5).

The items can be ranked according to how positively young people responded. Because the most common response and the response of the ‘middle person’ is always ‘quite a lot’, we cannot rank them in these ways. Instead, items are ranked according to their respective mean (average) responses (Table 6). Ranked in this way, learning about the relationship of qualifications and occupations is the most widely perceived benefit. This was closely followed by the advantages and disadvantages of different jobs and occupations, learning about the sex-typing of occupations, the value of planning for the future whilst still at school, skills for today’s jobs, and how important issues affect communities. Many of the learning objectives which may be thought to be particularly associated with the simulation or ‘gaming’ method of instruction employed in the Real Game were also quite highly rated (e.g. listening to others, working with others against deadlines, contributing to discussion). Participants less often thought they had learned about employers’ recruitment criteria, the impact of technological change on the world of work, self-expression, and the world of work in ten years’ time.

The highest-ranking items are mostly learning about ‘present-day’ career-related matters relevant to the young people’s current personal situations. The lowest-ranking items appear to refer to learning about what might be (for them) more remote career issues, such as recruitment criteria and the future shape of work. It would be surprising if the relative positions of these groups of items were to be reversed in a future sample of similar age, but no importance should be attached to the minor differences between responses to items within the broad categories we have outlined, as they would almost certainly not be repeated in future samples.

Answers to almost every one of these questions are significantly correlated with every other, but an exploratory analysis indicated that it would be unwise to form a simple additive scale and that to do so might obscure relationships. A principal components analysis extracted two factors (eigenvalue = 1) explaining 40% of item variance. Young people answering at least 15 of the 20 opinion items were included in this analysis, and item (not personal) means were substituted for missing replies in order to retain them all in the sample. Table 7 shows that one dimension of young people’s opinions is dominated by, or underlies their answers to, items referring to working collaboratively, communication skills, the future of work, etc. (QQ108 down to QQ96), whereas the second dimension relates to what might be regarded as the more traditional or conventional objectives of careers education (QQ93 to QQ92). That is to say, there appear to be some underlying consistencies in the ways in which the young people tended to evaluate their learning about these two groups of things.

In view of this, a preliminary and purely exploratory examination was made of the relationship of ‘factor scores’ (not summated scale scores) to a small number of possible influences; following this, the items themselves were considered.

In that part of the sample for which we have more detailed information, the sex and ability (SAT scores) of the young people were not related to their opinions about what they had learned.

Sex and opinion data (at least 15 of 20 items) are available for 265 members of the Real Game sample. There was no association between sex and opinion factor scores. It is true that