Women and Social Capital: A Comment on Hall’s ‘Social Capital in Britain’

VIVIEN LOWNDES*

Hall’s interesting article contains the bold, but undeveloped, argument that ‘social capital has been sustained in Britain largely by virtue of the increasing participation of women in the community’. 1 Hall’s statement draws attention to the curious silence within the social capital debate about gender dynamics. Hall modestly notes that his analysis of social capital in Britain raises more questions than it resolves. The purpose of this Comment is to clarify these questions as they relate to women and social capital. The objective is not simply to ‘add women and stir’. 2 Rather, a consideration of gender dynamics illuminates key controversies within the wider social capital debate.

Analyses of social capital are based upon the claim that patterns of formal and informal sociability build up relations of trust and mutual reciprocity. The resultant ‘social capital’ enhances individuals’ capacity to join together in collective action to resolve common problems (or ensure that governments address such problems) – it ‘capitalizes’ political engagement. High levels of social capital are associated with high-performing democratic institutions, and economic success. 3 A consideration of gender dynamics throws light upon two important issues: the distribution of different types (and levels) of social capital within communities, and the nature of the link between networks of sociability and patterns of political engagement.

Hall’s bold claim about women and social capital is based upon data showing that women’s membership of associations more than doubled between 1959 and 1990 (increasing by 127 per cent). Men’s membership grew only slightly during the same period (by 7 per cent). By 1990 the levels of men’s and women’s associational membership were almost identical. 4 Surprisingly, having set the hare running, Hall does not analyse gender dynamics when looking at other settings for the generation of social capital, i.e. charitable endeavour (voluntary work) and informal sociability. Women’s

* Department of Public Policy, De Montfort University, Leicester. The author thanks Marian Barnes of the University of Birmingham for helpful discussions, and Peter Hall for reactions to an earlier version of this Comment.

4 Hall’s data provide an interesting contrast to US trends. For the period 1974–94 Putnam reports a 25 per cent fall in overall group membership, with women bearing a disproportionate share of the decline (see Robert D. Putnam, ‘Tuning In, Tuning Out: The Strange Disappearance of Social Capital in America’, PS: Political Science and Politics, 28 (1995), 665–83). Hall’s data on Britain show that men’s associational membership began to decline after 1973 (down 23 per cent by 1990), but reveal continued rises in women’s group membership during this period (up by 23 per cent).
‘increasing participation in the community’ is not explored further; it is defined solely in terms of membership of formal associations. If, as Hall argues, women have played the central role in sustaining levels of social capital in Britain, there is a need to probe further both the level and character of their involvement in other community settings.

If we return to the original sources used by Hall, interesting patterns emerge in relation to voluntary work and informal sociability. As with associational membership, men and women have similar rates of involvement in voluntary work: 34 per cent of women and 31 per cent of men undertook some voluntary work in 1994. However, there are important differences in the character of men’s and women’s involvement. More than twice as many men as women undertook voluntary work related to sports and recreation (29 per cent compared with 13 per cent). Women, by contrast, were more active in voluntary work in the fields of health, education and social services. As for the specific roles undertaken, men were more likely to occupy committee posts, while women dominated in visiting and befriending activities.5

Gender-specific patterns of activity are also evident in relation to informal sociability. The time-budget studies drawn upon by Hall show an increase between 1961 and 1984 in the time both men and women spent on leisure activities outside the home, with the biggest increase among women in full-time employment. The focus of informal sociability varied, with women spending a third of the time spent by men in sports’ activities, and only half as much time at social clubs. Men, however, spent a third of the time devoted by women to visiting friends.6

Looking beyond aggregate levels of associational membership, it is clear that while women’s involvement in the community has expanded, there are important differences in the types of activity undertaken by women and men. This observation prompts interesting questions about the nature of social capital. Are different forms (and levels) of social capital generated in different community settings? Are men and women involved in different, gender-specific ‘circuits’ of social capital? Might these serve to ‘capitalize’ political engagement in different ways?

Addressing these questions requires a broadening of the empirical and theoretical focus of the social capital debate. To date, attention has been disproportionately directed at male-dominated activities. Of the Italian local associations considered by Putnam, 73 per cent were sports clubs whilst only 1 per cent concerned health and social services.7 Hall considers in detail trends in pub attendance in Britain (where men still spent twice as much time as women in 1984), whilst relegating to a footnote increases in time spent on childcare. Like other commentators on social capital, Hall effectively disregards an entire sphere of relevant activity – that is, the social networks that involve parents (almost universally mothers) in their roles as child-carers. Such networks are produced and reproduced through a range of familiar activities, some characterized by mutuality, others by reciprocity – for instance, the ‘school run’, childcare ‘swaps’, baby-sitting, shared children’s outings, emergency care, and the taking and fetching and watching of children in their school and club activities. These networks clearly fit within Hall’s definition of social capital forming activities: ‘regular contact with others, beyond the sphere of the family or the market … the kind of face-to-face relations of relative equality associated with participation in common endeavours’.

7 Putnam, Making Democracy Work, p. 92.
While some child-care activities are relatively formalized – playgroups, after-school clubs, post-natal support groups, ‘mums and toddlers’ mornings, babysitting circles – they often lack formal constitutions and are characterized by loose and shifting memberships. Some childcare-related activity may be picked up through surveys of associations and voluntary work, but much more meets Hall’s criterion of ‘informally organized but regular activities undertaken with others’. Hall does not include child-care activities in his analysis on informal sociability, although time-budget data do exist. Between 1961 and 1984, the time spent on child-care actually doubled for both women and men (albeit from a lower base), in the context of a general reduction in domestic work and an increase in leisure time outside the home. Time spent on child-care is clearly compatible with, and could even promote, wider networks of sociability and community involvement.

Why are child-care networks not considered in analyses of social capital? The most likely explanation seems to lie with the enduring influence of the ‘public/private’ split in the consideration of politics in general, and citizenship in particular. As Susan James summarizes: ‘The cluster of activities, values, ways of thinking and ways of doing things which have long been associated with women are all conceived as outside the political world of citizenship and largely irrelevant to it’. In the context of the public/private split, it is easily assumed that activities involving women and children take place ‘within the family’, even where they involve non-related adults in extensive networks of trust and reciprocity. Child-care activities (and informal care more generally) are regarded as domestic rather than civic or community matters. Indeed, women’s maternal and care-giving roles have traditionally been seen as barriers to full citizenship. Carole Pateman argues that a gendered model of citizenship characterizes modern democracies, built upon a distinction between ‘man-the-soldier’ and ‘woman-the-mother’.

Yet it is hard to avoid the conclusion that child-care networks produce ‘really useful’ social capital, especially when compared with some other settings that attract commentators’ attention. Trust and mutuality may be associated with pub attendance (along with fighting and petty crime), but there is surely a far weaker link to social capital formation than the regular, shared and reciprocal responsibilities that characterize child-care networks? Recent sociological research argues that women are, in general, more strongly connected to neighbourhood networks than men, whilst mothers of young children enjoy particularly robust patterns of social exchange.

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9 The relationship between child-care and community involvement may itself be ‘gendered’. Drawing on Dutch time-budget surveys, de Hart and Dekker report: ‘Among men … participation falls if a great deal of time is taken up by household and family care tasks, while among women the frequency of participation in voluntary activity rises in such circumstances’ (see Joep de Hart and Paul Decker, ‘Civic Engagement and Volunteering in the Netherlands: A “Putnamian” Analysis’, in Jan W. van Deth, Marco Marafii, Kenneth Newton and Paul F. Whiteley, eds, Social Capital and European Democracy (London: Routledge, 1999), pp. 75–107, at p. 89).
access to social support and even financial help. Among employed women, relations of interdependency within local networks are an important resource for ‘the effective co-ordination of home, work and family life’. As Helen Russell argues: ‘the very building blocks of social networks are gendered … Women’s continued responsibility for caring and domestic work tends to restrict the range of social activity they are involved in, but it does provide an opportunity to build up supportive social networks in the community’. Women’s neighbourhood support networks are not a thing of the past – some rosy glow left over from the Young and Wilmott community studies of the 1950s. The utility of such networks, particularly in relation to child-care, is reinforced in the context of continuing increases in women’s labour-force participation (particularly striking for mothers of children under 5 years), the retreat from the extended family, and the very low levels in Britain of public and private child-care provision.

Hall argues for a more careful consideration of the distribution of social capital and of the kinds of social capital available to different groups in society. Hall’s analysis focuses on class and generational effects, and yet gender differences are clearly important (across and within other social categories). If, as seems likely, women have access to different forms of social capital (in different quantities) from men, what are the implications for patterns of political engagement? Perhaps commentators have steered clear of care-based networks because the social capital they create is not ‘convertable’ into political engagement, or is less strongly associated with responsive government (or economic success)? The continued male dominance of formal politics and business leadership might be taken as evidence of, or as contributing to, such a scenario. However, such a hypothesis serves principally to draw attention to the biggest question facing social capital analysts: what are the causal links within the virtuous circle of social capital – between social networks of trust and reciprocity, patterns of political engagement, and democratic and economic health? This ‘micro-logic’ remains obscure for all categories of activity that form social capital; the issue simply becomes clearer when we shift our attention from Putnam’s now familiar sports’ clubs and choral societies to the realm of school runs and baby-sitting circles.

Despite its association to date with game theory and abstract modelling, the social capital debate could make more use of qualitative case studies and individual ‘life histories’. What are the ways in which individuals, and groups, draw upon resources generated in social and civic networks when they act politically? How, in different contexts, do relations of trust and mutual reciprocity ‘capitalize’ political engagement? Here the social capital debate has much to learn from the existing literature on women’s unorthodox routes to political engagement, and from feminist perspectives on citizenship. In Britain, the classic example of the striking miners’ wives (1984),

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18 Between 1988 and 1998 the proportion of British women in paid employment outside the home grew from 52 per cent to 62 per cent. The proportion of mothers with children under 5 who work outside the home has increased from 32 per cent to 51 per cent in the same period. In 1998 only a third of mothers with children under 5 used professional or registered childcare, and over half of all working mothers relied on informal care. See *Meeting the Childcare Challenge: A Framework and Consultation Document* (London: HMSO Cm. 3959, 1998).
19 The distribution of social capital in relation to ethnic differences is another area in which research is needed.
alongside a myriad of other local case studies, shows how women have been politicized through engagement in community activity – in areas like housing, health, child-care, crime-prevention and neighbourhood regeneration. Such studies serve to illuminate the links between what Putnam calls the ‘internal’ and ‘external’ benefits of group activity: that is, the gains for individuals in terms of their personal development and political competence, and for the wider community in terms of collective services or campaigns. Research into women’s community involvement demonstrates the importance of pre-existing relationships of trust and mutuality among friends and neighbours. Shared concerns serve to mobilize self-help and campaigning activity, which in turn ‘spill over’ into the formal political arena as activists’ competence grows. Feminist political theory has long focused on what should become a central issue for the social capital debate – that is, the relationship between the ‘small democracies’ of everyday life and the ‘big democracy’ of political parties and organized government.

Overcoming the public/private split in political science is not an option for social capital analysis: it is a vital precondition. In order to investigate the links between social capital, political engagement and ‘good government’, phenomena such as friendship, caring and neighbourliness all have to be recast as legitimate objects of political enquiry. An awareness of gender dynamics and a preparedness to venture into ‘women’s territory’ will be vital components of such a project. Putnam deals the ‘heroic’ conception of citizenship a mortal blow when he makes playing sport and watching birds key measures of the ‘civic community’. To understand more fully the dynamics of social capital formation and mobilisation, the heroic citizen may need to be killed off completely.

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21 The ways in which friendship can become a political resource is discussed in relation to the women’s movement by Martha A. Ackelsberg, ‘Sisters or Comrades? The Politics of Friends and Families’, in Irene Diamond, ed., *Families, Politics and Public Policies* (New York: Longman, 1983); Marian Barnes considers the role of friendship within disabled people’s groups and groups of mental health service users. See Marian Barnes, *Care, Communities and Citizens* (Harlow: Longman, 1997).


23 Hall himself makes an important contribution in attempting to measure trends in informal sociability.
I am, of course, extremely pleased that my M + 1 hypothesis has received so much attention. Under the principle of ‘as long as you spell my name right’, I appreciate every single citation. Nevertheless, I have been surprised at some of the interpretations of my argument. Browne and Patterson offer me an opportunity to clarify my position.  

My interest is in the causal mechanisms that produce political phenomena, not in comparing outcomes against some normative standard of rationality. I analysed the convergence of electoral outcomes towards an M + 1 equilibrium and argued that the mechanism that produced this convergence was not rational calculation but learning. I did not intend M + 1 to be used as a standard of rational behaviour nor to impugn the intelligence of Japanese candidates or political parties for having failed to accomplish it rapidly. I found, and still find, it hard to sustain the idea of rational calculation as the mechanism that produced the observed behaviour.

The primary basis of my argument was the observation that it took several elections before equilibrium was reached. The mechanism must have been a dynamic process of some sort. Rational calculations based on full information would not have taken so long. I therefore hypothesized that the actual mechanism was trial-and-error learning. Browne and Patterson are perfectly correct in criticizing my work for not presenting a full-fledged model of how this learning occurs. I had planned to publish a follow-up piece entitled ‘How Do Parties Learn?’ in which I would lay out the cues, heuristics and decision rules followed by parties in deciding how many candidates to run. After several years of work, I must admit I still do not understand how parties learn. I continue to work on the problem and have made some progress, but the task is proving more difficult than I had imagined.

Browne and Patterson appear to address the issue of decision rules but their analysis fails to address my learning hypothesis for three reasons. First, they drop the first five elections from their analysis. They begin their analysis in 1958 after most of the learning had already occurred. It is difficult to address the question of how equilibrium is reached if you analyse the system only after it nears equilibrium. Secondly their analysis is static. One cannot find learning without analysing change over time. Thirdly, their analysis is retrospective, based on information that the actors could not have possessed when they made their decisions. If one is interested in evaluating outcomes against a standard of rationality, it makes sense to assume full information: is the outcome what would be expected if the actors were fully informed and rational? If, however, one is interested in the decision rules that people actually use, the idea that they begin by accurately predicting the future is simply not plausible.

Browne and Patterson’s argument with Cox and Niou appears to be over what should

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* Faculty of Policy Studies, Chuo University.
be the standard of rationality and thus what should be counted as an ‘error’. Essentially Browne and Patterson argue that Cox and Niu’s definition is too narrow and too strict. They argue that if you make the test of rationality easier, more cases will pass, and that indicates that people are smarter than we thought. I find Cox and Niu’s definition of an ‘error’ quite useful but do not think either that an ‘error’ indicates stupidity or that an optimal strategy indicates intelligence.

Like Cox,4 I find Browne and Patterson’s analysis interesting but disagree with their interpretation. I interpret their findings to mean that parties are not necessarily punished for pursuing suboptimal strategies. I would add that parties are not necessarily rewarded for pursuing optimal strategies. If Browne and Patterson find that a party did not lose a seat they might have won, it means that they followed a rational strategy. If a party does lose a seat, it indicates that the party must have been maximizing something other than the number of seats. These two decision rules make it difficult to find a strategic error. I think both decision rules are wrong.

Browne and Patterson’s example of Kagawa 1st district in 1958 is an example of a suboptimal strategy that did not cost the Liberal Democratic Party (LDP) any seats. No one would recommend nominating four candidates in a three-member district as the optimal way of maximizing seats. The decision to nominate four was probably perfectly rational in many ways, several of which Browne and Patterson do not consider. The 1958 election was the first for the LDP, newly formed from the merger of the Liberal and Democratic parties. Nominating four candidates could have been perfectly rational even if it had cost the party a seat. The primary goal may well have been to keep the peace within the party and prevent defections. The LDP was virtually guaranteed a majority in 1958 as long as they maintained their unity. Unity was more important than any single seat. Browne and Patterson are perfectly correct in arguing that rationality should not be judged on the basis of seats alone. However, you can always explain everything if you take everything into consideration. More importantly, why should political scientists be interested in judging the rationality of nominating behaviour?

According to Cox and Niu, the Japan Socialist Party (JSP) made a relatively large number of overnomination ‘errors’ in 1969. Does this indicate that the JSP in 1969 was stupid? The party did not change their nomination strategy in 1969. They failed to foresee a substantial drop in their vote and their standard decision rules unexpectedly proved disastrous. No one forecast the drop in the socialist vote at the time and, even with the benefit of hindsight, we still do not have a satisfactory explanation of why it happened. It is unreasonable to judge the JSP’s intelligence based on this failure of foresight. But why should political scientists be involved in evaluating the intelligence of political parties at all?

I have learned from repeated experience that any suggestion that some rational choice theory or assumption might not be accurate often produces an emotional defensive response. I find the assumption of full information and perfect foresight implausible and many seem to interpret that position as an attack on human intelligence. They rise to the defence of humanity. For the record, I like humanity and do not intend to insult it. Moreover, I suspect that trial-and-error learning is ‘better than rational’5 so no criticism of humanity was implied by the hypothesis that the actual mechanism is learning, not rational calculation.

More fundamentally, my idea of science does not involve evaluating either behaviour or outcomes. I want to understand the mechanisms that produce political phenomena and use that understanding to predict future phenomena. Rational choice is often quite useful in this pursuit. I am certain that if we knew the decision rules that people actually use, those rules would make sense. However, I find it hard to get interested in debates over the proper standards for judging rationality or about whether some particular behaviour lives up to those standards or not. I mean no criticism of people who are interested in these philosophical questions, but any statement of mine that appeared to be a contribution to either of these debates was purely unintentional. I apologize for the confusion.