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We report a laboratory experiment in the context of the December 2008 riots in Greece, after the killing of a 15-year-old student by a policeman. Our sample comprised 266 students from the University of Thessaloniki. We tested whether media reports can affect people's willingness to harm those in opposing groups by examining the way students allocated money between themselves and others of various professions, including police, in modified dictator games. Exposure to media reports decreased giving to police, but only when choices were private. Laboratory behaviour was correlated with self-reported participation in demonstrations, supporting the external validity of our measure. Media exposure appears to have affected behaviour by different pathways than those proposed in the existing literature, including "spiral of silence" and "frame alignment" theories.

In December 2008, after the killing of fifteen-year-old schoolboy Alexandros Grigoropoulos by a member of the police force, Greece was shaken by a series of demonstrations, which swiftly turned violent. Participants fought the police and destroyed property. Although mass violence has subsided at the time of writing, there have been subsequent terrorist attacks targeting individual on-duty police officers and mob attacks against police stations.[1] The December events continue to resonate in current Greek politics and society.

We use Greece as an example to examine the behavioural roots of conflict between opposing groups in a democratic system. This paper reports an experiment to test the specific hypothesis that intergroup discrimination and potentially conflict may be fostered by messages from the media. Our participants were Greek students, who were asked to allocate real money between themselves and others, including members of the Greek police force. This complements existing research by demonstrating that exposure to media may affect behaviour as well as, or instead of, changing opinions. To increase confidence in external validity, we correlated behaviour in our experiment with self-reported participation in demonstrations against the police.

Commentators and academics have blamed the media for fomenting civil and political conflict in cases such as the civil war in the former Yugoslavia (Oberschall 2001, 2000; Kaufman 2001; Ignatieff 1998); twentieth-century race riots in the United States (Bauerlein 2001); and the Rwandan genocide (Gourevitch 1999).[2] A key observation from these case studies is that not all participants in conflict are necessarily willing. As one Rwandan eyewitness stated: "ten percent helped; 30 percent were forced to kill; 20 percent killed reluctantly; 40 percent killed enthusiastically" (Mamdani 2001). Indeed, psychologists have hypothesized that intergroup violence is aided by the creation of within-group norms supporting it (Bar-Tal 1990). Similarly, some

1 See e.g. "Policeman Shot Dead in Greece," BBC News, June 17, 2009, http://news.bbc.co.uk/1/hi/world/europe/8104799.stm, "Greek unrest takes worrying turn," BBC News, January 5, 2009, http://news.bbc.co.uk/1/hi/world/europe/7811914.stm.

2 By contrast, the literature on social movements has not tended to give the media a leading causal role in mobilizing protest (although see Gamson 1992; Walgrave and Manssens 2000; Cooper 2002).

observers argue that the media was crucial to the escalation, outcome, and public perception of the December 2008 events in Greece. Some media outlets presented a very emotional account of the boy's death and its consequences for democracy (on the Greek media's emotional presentation of events in general, see Patrona 2006, 24); others tried to downplay the social importance of the riots, presenting and condemning them as outbreaks of random violence (Michael-Matsas 2010). For this reason, Greece offers an interesting context to examine media effects on behaviour towards a specific group in a democratic regime.

Public opinion research offers theories that could explain the role of the media in this process. *Framing theory* suggests that media can affect the cognitive schema individuals select to understand a situation. *Agenda setting theory* proposes that some issues can be made more salient in audiences' minds simply by covering them more often. *Priming* extends this, arguing that media coverage makes audiences more likely to consider particular issues in evaluating actors or issues (Scheufele and Tewksbury 2007). *Frame alignment theory*, an extension of framing within the social movement literature, holds that messages from social movements have a greater impact if they fit with a person's existing beliefs (Snow et al. 1986; Snow and Benford 1988). Lastly, and of particular relevance for understanding how the media can influence the formation of social norms, Neumann (1974, 44) argues that a *spiral of silence* could lead people who thought they were in a minority to conceal their true beliefs. If individuals depended heavily on mass media to learn what others think, this would give media power to determine the opinions people were willing to express, and hence public opinion; importantly, it could also affect the actions people were willing to take. Applied to the Rwandan case, the argument would be that the media created a climate in which attacks on Tutsis were publicly believed to be acceptable, or even required, behaviour.[3]

Our research does not aim to identify the psychological mechanisms behind media effects. Instead we examine a new and interesting dependent variable – that of behaviour with monetary consequences. Research on these topics has typically focused on measurement of opinions, or in the case of the spiral of silence, on measuring willingness to speak out, often via hypothetical questions.[4] This is a limitation if we are interested in behaviour such as conflict participation, because behaviour and expressed opinions may vary independently of one another. On the other hand, case studies of civil conflict (and other forms of research such as survey analysis and media content analysis) can focus on behavioural dependent variables, but it is intrinsically hard for them to demonstrate causality. We therefore chose a middle way: a laboratory experiment in which the dependent variable is behavioural. We cannot directly study participation in violent conflict in the laboratory, but we can explore behaviour that has real monetary consequences, both for the participants and for others. While such behaviour certainly offers no guarantee of external validity,[5] it does provide evidence that goes beyond the informational content of opinion questions and hypothetical questions about behaviour. Specifically, we examine (1) the effect of media on behaviour and (2) the interaction between media exposure and the publicity of subjects' decisions. This allows us to examine the "spiral of silence" effect, which predicts that individuals will be more affected by media when they know their actions will be visible to others who have been exposed to the same media.

In our experiment, participants first read a newspaper article: a treatment group read an inflammatory article dealing with the shooting of Alexandros Grigoropoulos, while a control group read a neutral article. They then allocated real money between (1) themselves, and (2) anonymous recipients identified only by profession, including members of the Thessaloniki police force. Our dependent variable is "giving discrimination": the difference between amounts given to members of the police force and amounts given to

3 Fuller literature reviews on priming, framing, and agenda-setting can be found in Weaver (2007), Scheufele and Tewksbury (2007), and Ball-Rokeach and DeFleur (1976).

4 For a review, see Scheufele and Moy (2000). Glynn et al. (1997) provide a meta-analysis of these studies and find limited effects.

5 Similar external validity issues are faced by studies on the effects of violent media that use lab measures of violent behaviour as a dependent variable. See Geen and Thomas (1986) for a review and discussion.

other groups. We can thus measure whether exposure to media affects giving discrimination. A cross-cutting treatment manipulates the publicity of allocation decisions: some subjects' decisions were fully private, while others were revealed to a neighbouring participant. This allows us to examine how the effect of the media article is moderated when subjects' decisions are made public to others. Thus, we can examine the effect of the spiral of silence on behaviour. Spiral of silence theory predicts that exposure to mass media will change individuals' views of what the majority thinks, and this will change their willingness to express their own opinion, or to take actions that reveal their opinion in public. This effect will be absent when the same actions are private.

Our participants were students at the University of Thessaloniki. Since Thessaloniki was a centre of anti-police activity, and since students were centrally involved, our sample comes from an interesting population of potential conflict participants.[6] Indeed, many subjects reported taking part in demonstrations against the police. Before making their allocations, half our students were exposed to a (real) newspaper article discussing the killing of Alexandros Grigoropoulos. The other half were exposed to a neutral article. This allows us to measure the effects of media exposure on the allocations made. Clearly, donating or withholding money in a laboratory is not the same as taking part in a potentially violent protest. However, it is an action with real consequences for both oneself and others, and this may be linked to willingness to take similar action in other contexts. In fact, we find a significant correlation between behaviour in the laboratory with self-reported protest participation during December 2008, detailed in subsection 4.3 below.

We find that media reports did indeed affect students' willingness to give to police, but only when decisions were private. When decisions were going to be made visible to another person, the media report had no effect. Thus, our hypothesis that public actions would be more influenced by the media was not supported. Also, while media reports decreased giving to police when giving was costly, they did not decrease giving when not giving was costly – that is, when participants had to pay to reduce the recipient's payoff. Thus, although exposure to media can change people's behaviour, we did not find evidence that the media can cause people to take action with material costs to themselves. Lastly, media exposure did not significantly affect people's expressed opinions. In other words, our subjects behaved differently even though their publicly expressed opinions did not change. Our results thus suggest that the link between opinion and action is complex. Media narratives may change what people do in private, for instance by legitimating selfish behaviour, without affecting the opinions they express in public. And making decisions public can (sometimes) dampen the effect of media narratives.

In the next section, we describe the background of our experiment: the Greek riots of 2008, and the role of police, students and media in them and in Greek society more generally. Section 2 sets out our design and Section 3 gives our results.

1. Background: The 2008 Riots

The December 2008 riots in Greece were the public response to the killing of a fifteen-year-old Athenian schoolboy by a policeman (USA Today 2008). The event catalysed an explosion of public discontent which was not directly connected to the boy's death but was indicative of the general mood of the society (Christofer 2008). The outrage of a traditionally highly politicised society (Alivizatos 1990) escalated into a month-long conflict between police and demonstrators, including both peaceful demonstrations and violent riots. Aggression against the police and other symbols of state and media power, such as university teachers and journalists, continued in subsequent months, along with repeated strikes against government economic policies (Smith 2008).

The violence demonstrates the anger of Greeks, particularly young people, towards government and state institutions

6 However, as is common in experimental work, our recruitment methods do not allow us to claim that our sample is representative.

(Karamichas 2009, 291). General dissatisfaction with public administration, political corruption, and unsuccessful governance has for many years lacked an effective means of expression, due to Greece's weak civil society (Mouzelis 1978, 19). The major political parties dominate social relations and expression of opinion in the Greek political sphere (Pridham 1990, 116) and have long used corruption and clientelism to secure their rule (Pappas 1999, 169–220). The on-going economic crisis added to public dissatisfaction with the dysfunctionalities and corruption of the Greek political system. The riots have been described as the first political explosion of the current world economic crisis, combining features from May 1968, and from the French *banlieu* rebellion in 2005 (Michael-Matsas 2010).

During the events of December 2008, the police were the main target of protesters' anger, not only because of their function as a law and order mechanism. Historically the Greek police have been seen as an organ of state repression. Before 1974, the police were used by both the dictatorship and elected right-wing governments to keep the masses out of politics (Veremis 1997; Demertzis and Kafetzis 1996). Support for democracy was suppressed and private life was infiltrated by a vast network of police informers (Samatas 1986, 35). The democratization of the state apparatus by the Karamanlis administration shied away from introducing major reforms out of fear of a backlash (Clogg 2002, 173). This led to the disillusionment of ordinary Greek citizens with the police (Kassimeris 2001, 262), which continued during the Third Greek Republic. In modern Greek politics the police are framed as both symptom and cause of political failure (Featherstone 2009, 2). The population's deep lack of trust in the police force (Mouzelis 1978, 133), is reinforced by the police's inability to provide good services, and the fact that the force is seen as protecting the political establishment, the two major parties. The death of Alexandros Grigoropoulos thus added to a disillusionment not only with the police, but with the entire state apparatus, including the political, economic, and social elite of the country. An opinion poll reporting trust in institutions showed that more Greeks distrust the police (46 percent) than trust them (43 percent). But the police are not the least trusted institution. Political parties come bottom, trusted by only 8 percent, while newspapers are trusted by just 33 percent.[7]

The role of the media in the December events was crucial. Greek media have a special position in the country's political life, participating as an independent political actor, more so than in other European democracies. As in other young southern European and Latin American democracies, the media in Greece are highly party-politicized (Hallin and Papathanassopoulos 2002). Media companies have invested little in developing quality journalism, which is hardly considered an autonomous profession. Compensating for their lack of professionalism, the media chose sensationalism over objective presentation of news. Outlets with different political alliances spin the news from different political perspectives. As a result, one could read wildly different interpretations of the December riots depending on the source.

The media's emotional presentation of the boy's killing and their analysis of its impact on democracy (or, on the other side, one-sided condemnation of the rioters' violence without comparable condemnations of police actions) may have affected opinions – whether privately held or openly expressed – about the actors and events of December 2008. Equally importantly, it may have affected the behaviour of those involved. For that reason we focus our analysis on the impact of the media coverage of the events.

Students are traditionally seen as a force of political change by both the students themselves and by Greek society in general. The strained relations between students and police and the students' role as the major protest group in the December riots give us a valuable opportunity to examine a strong form of outgroup discrimination – something that is extremely hard to create with minimal groups in the lab (Mummendey et al. 1992; Brewer 1999).[8] Finally, the political situation allows us to examine media effects in a naturalistic way using a real media report drawn from the December 2008 period.

7 Opinion poll conducted by Public Issue on December 17–19, 2008. Results can be found at http://www.publicissue.gr/1028/institutions-2/.

2. Experimental Design and Questionnaire

Experiments were conducted in nine sessions from April 8 to April 11, 2009, at the University of Macedonia, Thessaloniki, Greece. The sessions were held in the computer laboratory of the Economics Faculty, with adaptations for running computerized experiments.[9] Subjects were recruited via two methods: a) voluntary registration during lectures at the European and International Studies department and b) posters and leaflets distributed in and around the university and in the city centre.[10] The subjects were aware that they would be paid €2.50 for participating, and would have the potential to earn more depending on their answers. Volunteers contacted us by phone, e-mail, or in person to subscribe to the session of their choice and were informed that the sessions would run for an hour.

We employed a 2 × 2 factorial design, where we varied the publicity of the decisions and subjects' exposure to media. We applied these four treatments between subjects, so that any subject was exposed to only one of the four treatments. The experiment had two stages. In the first stage subjects read one of two newspaper articles: one dealing with the riots, and a control article about the activities of a Greek internet telephony company. In the second stage, subjects played a series of dictator games in which their decisions were either public or private. (Whether the subjects were in the *private* or the *public* treatment was determined beforehand and did not change during the experiment. This was communicated to the subjects before they made any decisions.) In each dictator game subjects could give money to people outside the lab, identified by their profession and gender. In these games we varied the recipient (the *other*) and the relative price of giving to the other. After all allocation decisions had been taken, subjects were asked to fill out an electronic questionnaire. Table 1 gives an overview of the experimental design.[11] All sessions were conducted by the same experimenters, taking the same roles in each session.

Table 1: Structure of the experiment

	Neutral media/ private	Riot media/ private	Neutral media/ public	Riot media/ public
	(N=68)	(N=72)	(N=72)	(N=54)
I	Introduction and explanation of experiment			
II	**Neutral media**	**Riot media**	**Neutral media**	**Riot media**
III	Example for choice on the budget sets			
IV	Profession chosen at random without replacement out of six professions *(stratified within subject)*			
V	Choice on linear budget set chosen randomly without replacement out of nine different possibilities *Stratified within subject and profession*			
	Price of giving (in €)	Budget (in €)		
	0.50	7.50		
	1.00	7.50		
	2.00	7.50		
	0.00	7.50		
	2.00	15.00		
	-0.50	5.00		
	-1.00	7.50		
	-2.00	10.00		
VI	Choice on step-shaped budget set			
VII				**Discuss decisions with neighbour**
VIII	**Post-experimental questionnaire**			

The columns represent between subject treatments and the rows within subject decisions. Number of observations reported for between-subjects treatments.

8 Other researchers have used ethnic groups to examine intergroup behaviour. Here is the problem that norms of inter-ethnic fairness, which are common in societies where ethnic groups must live together, may interfere with research. Indeed, experimental work with "home-grown" groups often finds weaker results than that with minimal groups (Habyarimana et al. 2007; Whitt and Wilson 2007; Goerg et al. 2008; Bernhard et al. 2006).

9 Photographs available on request.

10 As with most laboratory experiments, our recruitment methods are unlikely to give a truly random sample of the population of interest. However, there are no obvious reasons for our results to be biased in any specific direction. Since April 11 was a Saturday, the students on campus that day might be a strongly selected subset. We have too few observations on the Saturday (38 in total in four treatments) to explicitly test for selection effects, but our results are robust to the exclusion of the Saturday session (analyses available upon request).

11 The protocol and written instructions are available on request.

2.1. Media Articles

We asked subjects to read an article from a Greek newspaper and find spelling mistakes.[12] Subjects were given five minutes to complete the task. Half of the subjects read a neutral article about business activities of a large internet telephony company. The other half were presented with a news article containing a detailed description of the shooting of the fifteen-year old boy, broadly sympathetic to demonstrators and critical of the police. This article functions as a riot media treatment presenting the subjects with a subtle negative stimulus towards the police triggering a more emotional reaction. Subjects were asked to count the spelling mistakes and rewarded with 1€ for getting the correct number (which was 10). This task resembles a priming task, frequently used in psychological research to make a certain concept more salient (Bargh and Chartrand 2000; see Tajfel 1981 or Benjamin et al. forthcoming for applications to economics). We do not attempt to distinguish between the different causal mechanisms which may have been operating, including "priming," "framing," and "agenda-setting" effects. The relationship between these concepts is complex and controversial (Scheufele and Tewksbury 2007) and its discussion is not the focus of this research.

Subjects were aware that both articles came from *Kathimerini*, a widely read Greek newspaper.[13] We chose articles from *Kathimerini* because it has the reputation of being a serious, mainstream paper. Its moderately right-wing position is well known to the Greek public. More left-wing subjects might if anything be sceptical of Kathimerini's position, which would bias our results towards finding no effect of the media on behaviour. Another factor that might bias our results downward is the "hostile media phenomenon": subjects tend to perceive media as biased against their own views, whatever those views are (Vallone et al. 1985).

2.2. Recipients

Recipients were either police or members of one of five other professions: firefighter, private-sector employee, civil servant, housewife, or entrepreneur. We used multiple groups so as to avoid inducing experimenter demand effects by making the police/other distinction obvious. Firefighters in particular provide a close comparison group with police, since both are uniformed state employees with a strong group identity. This allowed us to check whether our results come from general intergroup hostility, rather than specifically from discrimination against members of the police group. Recipients were identified on the computer screen by true profession, along with fictitious names that preserved gender, and subjects were informed of this. Recipients were not present in the laboratory. Instead, money donated was sent to them by post, directly after the experiment (see section 3.3. below).

2.3. Dictator Game

In a dictator game subjects are asked to split money between themselves and a recipient. Dictator games are widely used in experimental economics to measure other-regarding preferences (e.g. Forsythe et al. 1994); behaviour in dictator games may also be affected by norms (Dana et al. 2006). Each subject played six turns of a modified dictator game – one for each profession. In each turn a profession was chosen randomly without replacement, a recipient was chosen randomly from a pool of potential recipients of the chosen profession, and the subject then made nine decisions allocating money between him- or herself and the recipient. So every subject was presented with all of the six professions in random order.

Motivated by analogous situations in civil conflict, we wished to learn how subjects behaved when discrimination carried a cost to the discriminator, and more generally how discrimination was affected by changes in its cost. Therefore, we varied the price of giving to the other person across the nine decisions.[14] Subjects were shown a series of different budget sets, with payoff to oneself on the y-axis and payoff to the recipient on the x-axis, and were asked to pick a point on the boundary of the budget set, as shown in Figure 1. Before the actual task, subjects made a non-paid trial choice to ensure that the setup was well understood.

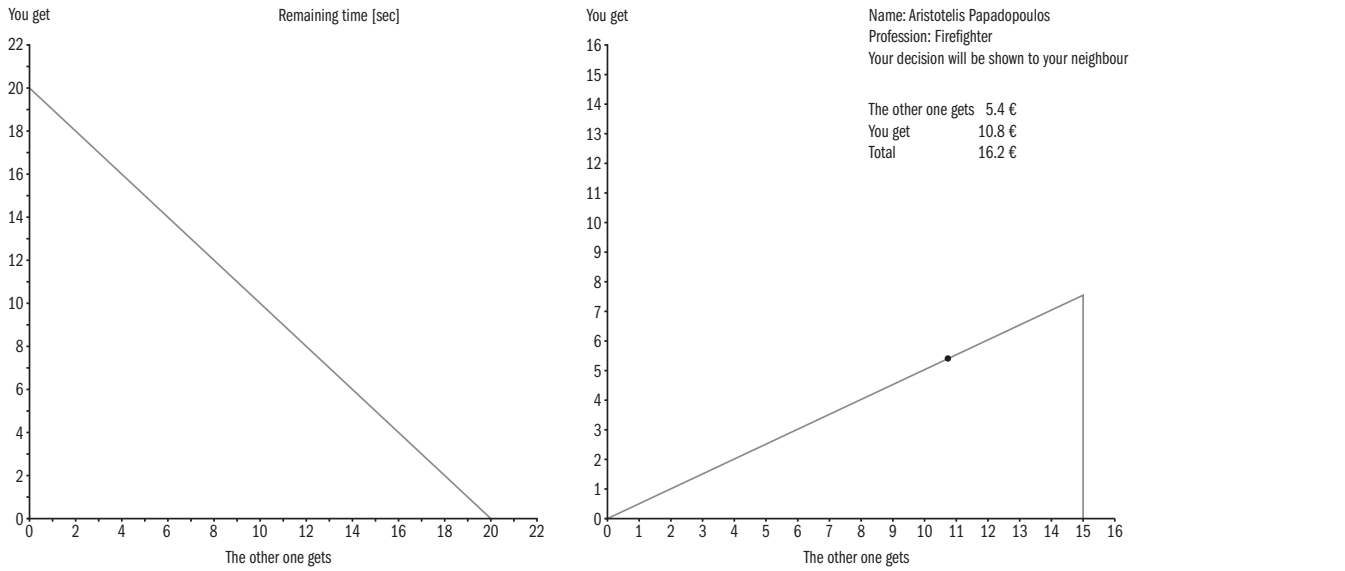
12 A translation of the articles (without spelling mistakes) can be found in the Appendix.

13 According to the newspaper *To Vima*, November 25, 2008, http://www.tovima.gr/default.asp?pid=2&artid=243941&ct=85&dt=12/10/2008.

14 For a similar approach see Andreoni and Miller (2002) or Fisman et al. (2007).

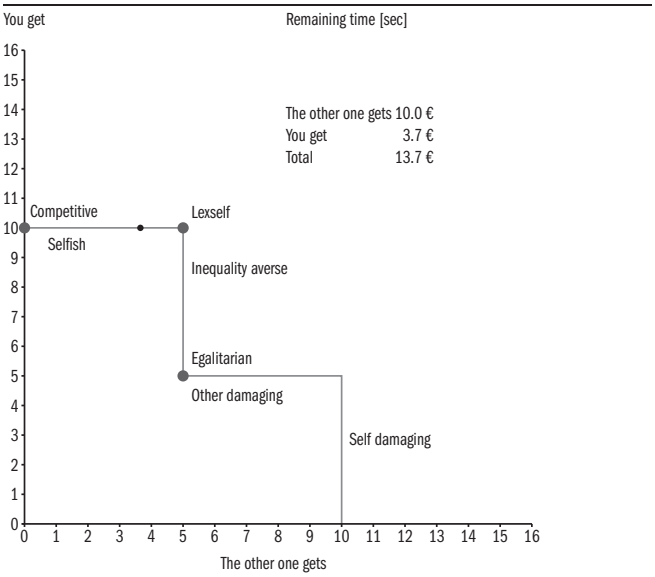
When a point was chosen, the resulting allocation was shown in figures in the top right corner of the screen. If the subject was satisfied with the decision, they confirmed the choice. There were three different kinds of budgets. Four were standard budget sets crossing the x-axis at 7.5€ or 15€, and the y-axis at 7.5€ or 15€. Thus, the price to give to the other person was either 0.5€, 1€, or 2€ and the own endowment was either 7.5€ or 15€. One budget set had a zero price of giving: the set crossed the y-axis at 7.5€, continued to (7.5€,7.5€) and then dropped to cross the x-axis at 7.5€.

Figure 1: Examples of budget sets



Note: Amount given to self is on the Y axis and amount given to the other is on the X axis. Subjects clicked on the budget line to determine the allocation. After each click they were shown the allocation, which they could then confirm.

Figure 2: Step-shaped budget set



Note: Diagram labels and emphasized points were not shown on the screen.

We call these five budgets, including the zero price budget, the "costly giving" budgets. Three budget sets had a negative price of giving, i.e. it was actually costly not to give to the other – we describe these as "costly withholding" budgets. These started at the origin, and went to either (5,10), (7.5,7.5) or 10,5), so that the price of not giving was 0.5€, 1€, or 2€. This is the closest laboratory analogue to behaviour that has costs to the actor as well as to the potential victims, such as participation in riots.

The final budget set in each profession was step-shaped (see Figure 2). The step-shaped budget set is suitable for detecting certain prototypical forms of other-regarding preferences in a non-parametric way, as choices within certain subsets on the budget line have a direct interpretation in terms of social preferences.

Competitive subjects want to maximize the difference between their income and the income of the recipient. *Selfish* subjects choose the highest possible outcome for themselves but, given this choice, do not maximize the payoff of the other. *Lexself* subjects maximize their payoff and then the payoff of the other. *Inequality averse* subjects will forego their own profit in order to reduce inequality. The *egalitarian* point indicates strong preferences for fairness. Points to the right of this point indicate other-damaging behaviour on the horizontal line, and self-damaging behaviour on the vertical line.

2.4. Private and Public Treatments

In the public treatment, after the dictator games, one set of each subject's decisions was chosen at random and displayed to a single other subject, who was seated at a neighbouring workstation in the lab; pairs of neighbours were then asked to chat (using the zTree interface) about their decisions for three minutes. In the private treatment, decisions were not displayed and there was no chat.

In neither case could we, the experimenters, connect subjects' decisions to their real identities. In both cases, subjects were informed of this in advance. Half the subjects were in the public treatment, half in the private.

2.5. Questionnaire and Payment

Immediately after the experiment, but before the payment procedure, subjects were asked to fill out a questionnaire on-screen. They were assured that their answers were anonymous, and that they were not obliged to answer any questions. The questionnaire included the following sections.

1. Open-ended questions on the purpose of the experiment, so as to check for experimenter demand effects (see below).
2. Trust in institutions, including the media and the police.
3. Questions about the riots, including multiple choice questions on who was to blame for the riots, the causes of the riots, whether violence by the police and/or students was justified, and whether policing had been effective.
4. Questions on the subject's own participation in and experience of demonstrations, including participation in violence.
5. Questions measuring the subject's level of identification with the student group.
6. Demographics.

After all subjects had completed the questionnaire, they were called up individually and paid privately before leaving the laboratory. Subjects were shown the money they had allocated to dictator game recipients being placed in envelopes for posting (but not shown the recipients' names or addresses), and one volunteer subject came with us to observe the envelopes being posted.

2.6. Credibility and Debriefing

Greece is a low-trust society, so we were concerned to establish the credibility of our experiment. Initial instructions for participants, which were read out in public, stressed that economic experiments do not use deception. In the post-experimental questionnaire, we asked subjects whether they trusted the experimenters to send the money on a scale from 1 ("not at all") to 7 ("very much"). The mean responses by treatment were: riot/private 6.02; riot/public 6.06; neutral/private 5.70; and neutral/public 5.60. The responses are quite high, and are not significantly different at a 10 percent level using rank-sum tests. Thus, even if credibility was not achieved for all participants, this will not have affected the treatments differently.

Another concern in psychological experiments is "experimenter demand" effects, which cause participants to behave in ways they think the experimenters want. This

makes it important that participants do not guess the purpose of the experiment.[15] Priming tasks can be a particular area of concern (Bargh and Chartrand 2000). By including multiple social groups as recipients, using a non-standard dictator game, and by framing the media article as a spelling task, we aimed to avoid this. As a check, our questionnaire included open questions on the experiment topic. No participant mentioned the police or the December 2008 disturbances.

2.7. Hypotheses

We use a simple measure for our dependent variable: the difference between amount given to members of the police force and amount given to other groups. We call this "giving discrimination".[16] To examine whether media can affect behaviour towards the police group, we formulate the following key hypothesis:

Hypothesis 1 *Giving discrimination will be greater in the riot media treatment.*

We split this into two sub-hypotheses, based on the different costs of giving.

Hypothesis 1.1 *Giving discrimination will be greater in the riot media treatment, in the costly giving budgets.*

Hypothesis 1.2 *Giving discrimination will be greater in the riot media treatment, in the costly withholding budgets.*

To examine the spiral of silence hypothesis, we test for an interaction effect between the publicity and media treatments. If exposure to media affects how our subjects perceive others' opinion, and they care about others' opinion when their actions can be observed in public, then:

Hypothesis 2 *The increase in giving discrimination from neutral to riot media treatment will be greater in the public treatment.*

We also wish to examine whether exposure to media affects people's expressed opinions. Our questionnaire included multiple choice questions asking which groups the subject blamed for the December disturbances.

Hypothesis 3 *Blame for the police will be greater in the riot media treatment.*

Lastly, we use the same measure to test whether behaviour is correlated with expressed political beliefs, and to test the prediction of frame alignment theory that messages are more effective if they fit with a person's existing beliefs. Thus we expect an interaction between the media treatment and our measure of beliefs.

Hypothesis 4 *Giving discrimination will be greater among subjects who blame the police for the shooting.*

Hypothesis 5 *The increase in giving discrimination from neutral to riot media treatment will be greater among subjects who blame the police for the shooting.*

3. Results

In total 184 subjects participated; the number of subjects per session varied between 12 and 28. 58.6 percent of the subjects were female. 20.2 percent of the women and 30.9 percent of the men stated that they participated in demonstrations connected to the events in December, but nobody admitted taking violent action. The experiment lasted around one hour. The average payment (including show-up fee and rewards for correct counts in the initial task) was about 10 euros.

Table 2 gives a detailed overview of the allocations to the different professions by treatment over all budget sets. The first – and not necessarily surprising – observation is that average giving over all treatments when the decision is observed increases giving to the other by around 0.50 (t-test, p-value < 0.001). This fits with research on observability in experimental games, which is known to in-

15 Participants were invited to email the researchers if they wished to learn more about the experiment after taking part.

16 This term is purely for convenience, and is not meant to imply any particular theory for the reasons behind the difference between amounts given.

crease the pressure to conform with norms such as fairness and generosity (e.g. Andreoni and Petrie 2004, Dana et al. 2006). In contrast, there is no significant difference between the riot and the neutral prime over all profession types.

Table 2: Average giving over all budget sets (pooled) by treatment (euros)

	Treatment				
	Private		Public		
	Neutral	Riot	Neutral	Riot	Total
Profession					
– Police	4.16 (0.17)	4.01 (0.16)	5.08 (0.15)	4.92 (0.17)	4.53
[All non-police]	4.41 (0.07)	4.66 (0.07)	5.00 (0.07)	4.90 (0.08)	4.74
– Civil servant	4.27 (0.16)	4.26 (0.15)	4.63 (0.15)	4.87 (0.17)	4.49
– Private-sector employee	4.41 (0.16)	4.62 (0.15)	5.06 (0.15)	4.81 (0.17)	4.73
– Housewife	4.86 (0.16)	5.07 (0.15)	5.48 (0.15)	5.34 (0.16)	5.18
– Entrepreneur	3.90 (0.17)	4.25 (0.15)	4.47 (0.16)	4.24 (0.17)	4.22
– Firefighter	4.62 (0.16)	5.12 (0.15)	5.37 (0.15)	5.24 (0.16)	5.09
Total	4.37	4.56	5.01	4.90	

Standard error of means in parentheses

In the private treatment, when subjects received the neutral prime, police and entrepreneurs received the lowest average contributions. In the riot prime, the donation to policemen was lower, at 4.01, while the contributions to the other professions were greater (or nearly the same in the case of civil servant). This is in accordance with our expectations that giving discrimination will be greater in the riot media treatment. A particularly striking observation is the rise in payments to police from the private treatment to the public treatment under the neutral media condition. They received over 0.90 more and their average payment actually exceeded the average of the other groups. This shows that subjects were functioning according to the norm of giving and non-discrimination in public, but showed their true feelings in private. This corresponds to the spiral of silence theory. In the public treatment, the riot prime decreased contributions for all profession types. The difference between police and fire service personnel we do not investigate further. We furthermore find significant differences for decisions made in the private, riot prime treatment, which we examine more closely when testing our hypothesis in the next section.

4.1. Non-parametric Tests

We measure an individual's "giving discrimination" as average giving to non-police, minus average giving to police. (Results do not change if more complex measures are used, such as the t-statistic of a police dummy in a regression of an individual's giving.) Table 3 shows average giving discrimination by treatment.

Table 3: Discrimination by media and publicity treatments

	All budget sets					
	Private		Public		Total	
	N		N		N	
Neutral	45	0.258	48	−0.079	93	0.086
Riot	48	0.658***	38	−0.021	86	0.358***
Total	93	0.466***	86	−0.053	179	0.217***
	Positive Prices					
	Private		Public		Total	
	N		N		N	
Neutral	45	0.146	48	−0.28	93	−0.073
Riot	48	0.692***	38	−0.092	86	0.427**
Total	93	0.431**	86	−0.115	179	0.169
	Negative Prices					
	Private		Public		Total	
	N		N		N	
Neutral	45	0.166	48	0.237	93	0.126
Riot	48	0.327	38	−0.369	86	0.019
Total	93	0.247	86	−0.116	179	0.073

Note: Significance levels of Mann-Whitney test whether discrimination is present: * p<0.10, ** p<0.05, *** p<0.01.

We then ran Mann-Whitney tests on discrimination in the different treatments.[17] There was a slight, but not significant, increase in discrimination in the riot treatment over the neutral treatment (p=0.127, one-tailed). Thus, Hypothesis 1 (giving discrimination will be greater in the riot media treatment) receives weak support in the data. Next, we differentiate between costly giving and costly withholding budgets.

Table 4: Preference types elicited from choices on the step-shaped set

Preference type	Private				Public			
	Neutral		Riot		Neutral		Riot	
	Police	Not police	Police	Not police	Police	Not police	Police	Not police
	% of subjects in category							
Competitive	13.33	10.67	25.00	8.57	4.17	3.72	13.16	11.40
Egoistic	26.67	18.22	18.75	12.24	14.58	13.22	13.16	11.92
Lexicographic self	35.56	47.56	29.17	41.22	45.83	58.26	34.21	33.16
Egalitarian	4.44	9.33	2.08	6.12	10.42	5.37	10.53	8.29
Equity	11.11	5.78	20.83	17.55	18.75	11.57	15.79	21.24
Other-damaging	6.67	6.22	4.17	12.65	6.25	10.42	13.16	11.92
Self-damaging	2.22	2.22	0	1.63	0	0	0	2.07
Pearson's test	5.45 (0.49)		17.17** (0.01)		4.57 (0.47)		1.61 (0.95)	

Note: Significance levels of Pearson's chi-squared test of equality of distributions between choices for the police and non-police groups on the step-shaped budget set by treatment (p-value in parentheses): * p<0.10, ** p<0.05, *** p<0.01.

Discrimination did not vary significantly between media treatments when withholding was costly (p=0.846, one-tailed). Cheap discrimination, however, is significantly stronger in the riot media treatment (p=0.009, one-tailed). Thus, Hypothesis 1.1 about the costly giving budgets is supported, but Hypothesis 1.2 about the costly withholding budgets is not: the riot cue increased the difference between giving to police and others, but only when giving was costly to the subject. Table 4 gives further support for this result for the step-shaped budget set. We categorized subjects' choices on the step-shaped set by their corresponding prototypical social preferences, as described above. The distributions of social preference types for police and non-police recipients are significantly different in the private, riot media treatment (χ^2-test, p-value: 0.01). In particular, subjects are much more likely to show competitive preferences towards police than to non-police. In the other three treatment combinations we do not observe this difference.

To examine Hypothesis 3 that blame for the police will be greater in the riot media treatment, we examined a question from the questionnaire: "Who was to blame for the December 2008 riots?" Subjects were presented with different alternatives, of which more than one could be chosen. We count those subjects who chose either "the police" or "the police leadership" as blaming the police. In a Wilcoxon rank-sum test, blaming the police was not significantly different between riot media and neutral media treatments (p-value: 0.23, one-tailed). Thus, Hypothesis 3 is rejected. By surveying opinions alone, we would conclude that the media have little short-run effect.

Turning to Hypothesis 4 (giving discrimination will be greater among subjects who blame the police for the shooting), we compare subjects who blamed the police or police leadership for the riots with those who did not. Giving discrimination is not greater among those who blamed the police – in fact it is less, but insignificantly so (0.153 vs. 0.260, p=0.921, one-tailed). The same holds for cheap discrimination. However, this pattern is reversed in the private treatment: discrimination is higher among those who blame the police, and for cheap discrimination this is significant at the 10 percent level (p=0.0576, one-tailed).

To examine Hypothesis 5 (the increase in giving discrimination from neutral to riot media treatment will be greater among subjects who blame the police for the shooting), we look whether there is a significant trend towards

17 The use of non-parametric tests is robust to non-normality of the data. However, they might have less statistical power than parametric tests like the t-test, when the normality assumption is not violated.

discrimination when changing from the neutral to the riot treatment. We find that cheap discrimination is significantly stronger (difference: 0.59, Kruskal-Wallis test, p-value: 0.06) when subjects blame the police and their decision is private. We also find an increase in discrimination in the public treatment, but this is not significant at conventional levels (difference: 0.33, Kruskal-Wallis test, p-value: 0.15). We do not find significant differences in discrimination between the riot and the neutral treatments behaviour with subjects who – according to the questionnaire – did not blame the police.

4.2. Regression Tests

The overall pattern revealed so far is that subjects only give less to police when it is not costly, and only in private. To investigate this further, we ran OLS regressions on giving to others, using clustered robust standard errors for inference, where the cluster is the individual. Table 5 reports the results. Riot and Public are dummies for the riot media and public treatments respectively.[18]

The regression analysis shows the effect of prices and endowments. While for positive prices (these are the costly giving treatments) these effects go in expected directions, i.e. higher prices reduce giving to the other and higher endowment increases it, the effect of endowment on giving goes in an unexpected direction: the higher the endowment (measured in terms of the maximum that a subject could give to him- or herself), the lower the willingness to contribute, even controlling for the price. This also drives the aggregate results in column (1) of Table 5.

The effects of both media (identified in Table 5 as Riot, indicating that the subjects read the article on the riots) and publicity (identified as Public) treatments on giving to non-police do not reach significance, except for the public treatment effect when prices are zero. For the publicity treatment this is surprising, as these findings are not in line with previous research on social distance and other-regarding behaviour (for early evidence in simple dictator games see Hoffman et al. 1996). The fact that recipients are not present in the laboratory may have dampened the effect of publicity.[19]

18 We conducted robustness checks adding the gender of the recipient into the regression. The main effect of the gender coefficient is insignificant and the coefficients of interest did not change in size and significance. Results are available upon request.

19 Findings that donations to a third party outside the lab are higher when identity is reported to a subject the lab have been found – to the best of our knowledge – only in research on charitable giving. See for example Reinstein and Riener (forthcoming) in the context of charities.

Table 5: Regressions of amount given

	(1)	(2)	(3)	(4)
	Non-zero prices	Positive prices	Negative prices	Zero price
Price	0.802*** (0.0781)	-2.333*** (0.155)	2.181*** (0.069)	
Endowment	-0.0755*** (0.0138)	0.270*** (0.0156)	-0.080*** (0.028)	
Riot	0.201 (0.254)	0.502 (0.460)	-0.199 (0.309)	0.471 (0.428)
Public	0.517 (0.275)	0.640 (0.448)	0.350 (0.288)	1.158** (0.404)
Police	-0.154 (0.199)	-0.146 (0.242)	-0.166 (0.202)	-0.711 (0.393)
Riot × public	-0.188 (0.362)	-0.159 (0.633)	-0.225 (0.436)	-1.159* (0.583)
Police × public	0.278 (0.235)	0.427 (0.314)	0.0818 (0.257)	0.668 (0.512)
Police × riot	-0.381 (0.297)	-0.547 (0.340)	-0.161 (0.326)	-0.220 (0.575)
Police × riot × public	0.363 (0.355)	0.173 (0.449)	0.614 (0.409)	-0.206 (0.769)
Downward	-4.627*** (0.336)			
Constant	7.527*** (0.319)	2.339*** (0.376)	9.511*** (0.394)	5.353*** (0.348)
Combined coefficients				
Police + police × riot	-0.536** (0.221)	-0.692*** (0.241)	-0.327 (0.256)	-0.931 **(0.420)
Police + police × public	0.124 (0 .125)	0.281 (0.200)	-0.083 (0.158)	-0.042 (0.328)
Police + police × public				
+ police × riot + police × public × riot	0.105 (0.149)	-0.092 (0.214)	0.369 (0.190)	-0.468 (0.391)
Observations	7607	4348	3259	1087
R^2	0.2278	0.0989	0.3178	0.0365

Dependent variable is amount in euros. Coefficients are unstandardized. Cluster robust standard errors in parentheses, clustered by individual.
Column (1) pools costly withholding (negative prices) and costly giving (positive prices) treatments, but excludes zero-cost giving. The step-shaped budget sets are not part of this data.
Baseline: decision private, neutral cue, non-police recipient, positive prices include zero price giving
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Examining the *Police* dummies – and their interaction with the treatments – confirms the results of the non-parametric tests. We see no evidence that subjects gave less to police in the public treatment: the combined coefficient of police plus police × public is not significantly different from zero. On the other hand, the combined coefficient of police plus police × riot is significantly different from zero and negative. Thus, Hypothesis 1 cannot be rejected for private decisions. However, publicity appears to eliminate the effect of the riot media, since the combined coefficient police + riot × police + police × public + riot × police × public is not significantly different from 0.

We now turn to Hypothesis 4. Table 6 shows a simple regression of giving discrimination on treatment dummies, crossed with a dummy for those who blamed police or police leadership. If the riot cue had more effect on this group, then the difference between "riot blame" and "neutral blame" will be greater than between "riot no-blame" and "neutral no-blame," in either the private or the public treatment. Although the direction of the effect is right (0.89–0.30 > 0.33–0.14 in the private treatment; –0.17–(-0.51) > 0.25–0.13 in the public treatment), it is not significant. Similar results (not shown) were obtained for regressions on individual giving decisions. Thus, the weak non-parametric results are not supported in a regression framework: we cannot reject the null hypothesis that the riot media treatment had no extra impact on individuals who blamed the police.

Table 6: Linear regression of cheap discrimination and blame attributions

Base: Private neutral no-blame	0.14	(0.29)
Private neutral blame	0.30	(0.46)
Private riot no-blame	0.33	(0.49)
Private riot blame	0.89*	(0.40)
Public neutral no-blame	0.13	(0.36)
Public neutral blame	-0.51	(0.39)
Public riot no-blame	0.25	(0.45)
Public riot blame	-0.17	(0.36)

Robust standard errors in parentheses. Results for downward sloping budget sets. A two-sided t-test rejects the hypothesis that in the private treatment the riot media has a different effect on subjects who blame the police and subjects who do not (p-value: 0.24).
* p<0.10, ** p<0.05, *** p<0.01

4.3. Participation in Demonstrations

Laboratory behaviour can be accurately measured, but does it correlate with behaviour in the real world? To address these concerns, our questionnaire included measures of participation in the December 2008 demonstrations. We examine how our "giving discrimination" measure correlates with these self-reports.

Around 23 percent of our subjects participated in the demonstrations. Table 7 shows average donations by treatments and groups. We see very clear and significant discrimination against police among subjects who participated in the demonstrations, but only in the private treatment. The discrimination is stronger in the riot media treatment. In the public treatment, we do not see discrimination. In contrast, the group of subjects who did not take part in the demonstrations do not appear to discriminate against the police, except in the private, riot media treatment.

Table 7: Average giving over budget sets with positive prices by treatment

Participated in demonstrations				
	Neutral/Private	Neutral/Public	Riot/Private	Riot/Public
Non-police	2.98	3.34	4.15	2.12
Police	1.98	3.85	2.46	1.69
Δ	1.00* (0.053)	-0.51 (0.240)	1.69*** (0.007)	0.434 (0.32)

Did not participate in demonstrations				
	Neutral/Private	Neutral/Public	Riot/Private	Riot/Public
Non-police	2.67	3.43	3.02	4.22
Police	2.80	3.52	2.58	4.30
Δ	-0.13 (0.660)	-0.08 (0.780)	0.44* (0.100)	-0.08 (0.830)

t-test for differences of giving between police and non-police groups. p-value in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Using the step-shaped budget set, we also examined whether subjects who chose the competitive point when the recipient was a police member were more likely to have been involved in the demonstrations. Estimating a linear probability model with participation in demonstrations on the left hand and the preference type on the right hand side of the equation, we find that those with competitive preferences were significantly more likely to have participated in the demonstrations (results available on request).

It could be that subjects misreported their participation in demonstrations so as to justify their behaviour in the experiment. We cannot rule this out completely. However, as a robustness check we examined whether the answers on participation were different between the treatments. If self-justification explained the answers, we would expect that the effect of the treatment on giving would be reflected in the answers. Fortunately, we cannot reject the null hypothesis that the distribution of answers is equal (publicity treatments: χ^2-test, p-value: 0.141, cueing treatments: χ^2-test, p-value: 0.797). Therefore, although we cannot rule out some misreporting, we believe it is unlikely that the level of misreporting is correlated with in-lab behaviour.

We expected to find that subjects who participated in the demonstrations gave less to the police in the upward sloping budget sets, but this was not the case. So, although self-reported participation in the riots was linked to laboratory behaviour, we could not replicate the kind of behaviour that has material costs and risks, such as participation in political protest. Further work with a more selected group of subjects – for example violent protesters – might address this issue. Also, we cannot say whether causality runs from protest involvement to less giving, or vice versa, only that the two behaviours were correlated.

5. Discussion and Conclusion

Many case studies of conflict propose that outgroup hatred can be whipped up by media and elite rhetoric. In our experiment, subjects exposed to an "inflammatory" newspaper article gave significantly less to police than to others in a dictator game, but only when decisions were private. Hypothesis 1, that exposure to the media would increase discrimination, therefore received qualified support. One explanation for the effect of media on people's opinions and actions is the "spiral of silence" hypothesis: subjects care about other people's opinions in forming their own, and they will infer these opinions from the media. However, in this case, the hypothesis that this will be strongly visible in the public treatment was not supported (Hypothesis 2). In fact, exposure to the media had less effect when decisions were made public. There was also no evidence that the media treatment increased subjects' expressed blame for the police (Hypothesis 3). Why did the spiral of silence not arise here? In this case, subjects may have already known that the December 2008 events were a widely discussed and contentious topic. If so, media exposure would have had little effect on their expectations of what others thought, and thus little effect on their public behaviour.

Similarly, according to frame alignment theory, messages which are aligned with subjects' existing beliefs should have more effect on their behaviour. However, when we examined the behaviour of subjects who blamed the police, we found no evidence that they gave less to the police (Hypothesis 4), and only weak evidence that their giving was more affected by the riot media in the private treatment (Hypothesis 5). Frame alignment seems not to have operated here.

This leaves open the question of exactly why media exposure did affect our subjects' private behaviour towards others, even without affecting their expressed opinions or public behaviour. While we cannot answer this question for certain, one possible hypothesis is that in our experiment the riot article's frame provided subjects with an internal justification for selfish behaviour towards the police, without changing their expressed attitudes. An analogy can be drawn with research on self-serving choice of norms (Loewenstein et al. 1993, Konow 2000): when multiple norms seem to be applicable to a situation, subjects may choose the norm which benefits them most. Here, the riot article may have allowed subjects to apply a "Lockean claim" or "punishment" norm, justifying lower generosity to police. If this were so, it would be a previously unstudied pathway by which media reports might affect behaviour in conflict situations. For this reason, we believe that existing work examining media effects on opinions ought to be com-

plemented by further experimental research into media effects on political and social behaviour. Future work could test our hypothesis on self-serving norms; experiments could also examine the effects of exposure to media over a longer period than the relatively short timeframe used here.

Laboratory experiments will always face questions of external validity. While we have no panacea for these concerns, we were able to link behaviour in the experiment to self-reported participation in demonstrations. It is possible that subjects lied or misremembered their own actions, but we prefer the simple explanation that there was a genuine correlation with real-world behaviour, especially as reported participation levels did not vary between treatments.

Clearly, our laboratory results cannot explain the causes of the December 2008 riots in Greece. However, we can make inferences about discrimination and its triggers among Greek students. When given an appropriately chosen frame to understand recent events, many students gave less to the police. Students were reluctant to discriminate in the public treatment, perhaps because they feared the disapproval of fellow participants whose political views were unknown. But in the private treatments, anti-police behaviour was elicited. These findings are still relevant two years later, as the events of December 2008 triggered general social unrest and further violence in the form of new terrorist organizations. In fact, the media themselves have become targets, with the murder of a journalist in July 2010; extremist groups see the media as part of the establishment that causes the general misery of the country (see discussions in Karakousis 2010 and Strittmatter 2010).

Based on our results we can also draw some general conclusions on conflicts between opposing groups in democratic settings. Our results show the power of the media to shape behaviour. In particular, they suggest that, surprisingly, media may affect behaviour more in private than in public. This is interesting, since many important political actions, such as democratic voting, occur in private.

More generally, we believe that experimental methods will become increasingly important in studying the motivations behind political protest, contentious politics, and civil conflict. Both field and laboratory experiments have a role. A key issue will be defining and finding the population of interest. We also hope that our work will generate interest in linking experimental and case-study approaches to these issues. As our experiment shows, the insightful hypotheses provided by qualitative work can be tested experimentally.

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Appendix: The Newspaper Articles

1. Neutral

Skype Comes to Your iPhone

Internet telephony provider "Skype" has announced plans to expand its services to mobile phones such as iPhone and Blackberry, scheduled for May.

Skype has been working for some time to make its services compatible with the most advanced mobile phones in the market. In an attempt to expand its current user base of 400 million people, the company has been offering cheap and occasionally free calls.

Skype manager, Scott Darslang, made no secret of his great expectations for the success of adaptation on the iPhone, which he said was a great piece of technology and very compatible with Skype services. "The most important request from our users is the transfer of our service on the iPhone, and this demand is constantly rising", commented Darslang in a recent interview.

Even though Skype is best known for its videocall service, the company yet to announce whether this function will be available on the iPhone. "We are very careful when it comes to quality," explained Darslang, pointing out that they must first make sure that it works perfectly, before incorporating it in the iPhone package.

From *Kathimerini*, March 30, 2009 (http://portal.kathimerini.gr/4dcgi/_w_articles_kathworld_1_30/03/2009_273107)

2. Riot

The Constitution and the Blood

December 2008. Exarchia. A special police agent, called "Rambo" by his colleagues, kills the high school student Alexandros Grigoropoulos; the bullet hits the fifteen-year-old in the chest. The numerous protest voices on television, the internet gave the police to get away with the line of "police self-defence" and "emotional turbulence." Eyewitnesses confirm that the policeman shot the boy in cold blood following a trivial verbal exchange, and departed immediately with his colleague leaving the boy to die.

Shocked by their brother's murder, students across the country protested in anger. Fully aware and bitter that their voice will not be heard they left books (Ancient Greek, novels, maths, everything a child reads) and flowers on the "unknown soldier" monument in front of the Greek parliament. Among the books we might find a copy of our constitution with two points underlined: Article 2.1: "Respect and protection of the value of the human being constitute the primary obligations of the State" and Article 5.2 "All persons living within the Greek territory shall enjoy full protection of their life, honour and liberty." The students have underlined "all persons." With their blood.

Extract from an article by Pantelis Boukalas in *Kathimerini*, December 9, 2008 (http://news.kathimerini.gr/4dcgi/_w_articles_columns_2_09/12/2008_295314)

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